When the Press Falls Silent: The Impact of Local Newspaper Closures on Corporate Greenwashing

Fangming Xu Mengyao Yu Zilu Shan Shuo Wang

May 30, 2025

Abstract

This study examines the impact of local newspaper closures on corporate greenwashing behavior. We hypothesize that the disappearance of local newspapers reduces external scrutiny by weakening journalistic oversight, thereby increasing the likelihood that firms exaggerate their environmental performance. Exploiting variation in newspaper closures across U.S. counties, we find that affected firms are significantly more likely to engage in greenwashing. The effect is particularly pronounced among large and high-reputation firms, which appear more inclined to substitute symbolic for substantive disclosure in the absence of media scrutiny. Financially constrained firms exhibit a weaker response, possibly due to resource limitations or stronger regulatory pressure. The findings are robust to alternative greenwashing measures, treatment definitions, and local socioeconomic controls, underscoring the critical role of local media in promoting corporate environmental transparency.

Keywords: Greenwashing, Newspaper Closures, Media Coverage, Information Asymmetry

JEL Codes: G38, M14, Q56, L82

1 Introduction

Corporate greenwashing, defined as the strategic misrepresentation of environmental performance, has become an increasingly pressing concern as firms respond to growing investor and stakeholder pressure related to environmental, social, and governance (ESG) standards. This study examines how the surrounding information environment, with a particular focus on local media scrutiny, influences corporate incentives to engage in greenwashing. Given that ESG disclosures are largely voluntary and unaudited, many firms are tempted to selectively report positive environmental outcomes while concealing negative ones, thereby creating misleading impressions of sustainability (Delmas and Burbano, 2011; Lyon and Maxwell, 2011). These distortions are further intensified by weak regulatory enforcement and inconsistent global standards (Marquis et al., 2016; Heese et al., 2022), leaving stakeholders dependent on corporate self-disclosures with limited assurance of their accuracy.

Firms may strategically employ ESG disclosure as a symbolic tool to manage external perceptions and sustain legitimacy. Market-based research (Cho and Patten, 2007; Ntim and Soobaroyen, 2013) often highlights the instrumental use of sustainability disclosure for image management. For instance, Cho et al. (2012) and Romero et al. (2019) argue that ESG statements frequently reflect symbolic efforts rather than substantive change. Dowling (1993) emphasizes the value of corporate image, suggesting that reputational capital can reduce firms' cost of capital or improve operational outcomes.

The decline of local newspapers presents a compelling setting to evaluate how external information shocks affect corporate environmental behavior. While firms may respond to stakeholder pressure through strategic ESG disclosure, the presence of local journalistic oversight acts as a crucial accountability mechanism. Local newspapers play a critical role by sharing corporate disclosures with broader audiences, enhancing the visibility of ESG efforts, and strengthening the incentives for environmental reporting (Dyck et al., 2010; Heese et al., 2022). For instance, Gillan et al. (2015), Spack et al. (2012), and Parguel et al. (2015) stated that greenwashing may provide financial benefits by improving a firm's ecological image, particularly when stakeholders perceive the firm as environmentally responsible.

However, the steady wave of newspaper closures across U.S. counties has created a quasi-natural experiment for studying corporate behavior. This setting offers a valuable degree of exogeneity, as closures are often driven by industry-wide technological or economic changes, rather than firm-specific factors. Jiang and Kong (2023) argue that robust information ecosystems, such as local newspapers, are essential in holding companies accountable for their environmental disclosures. Their study demonstrates that the closure of local newspapers, which serve as an important channel for monitoring corporate and environmental practices, diminishes public scrutiny and reduces corporate accountability. This finding aligns with Dyck et al. (2010), who show that media coverage can deter corporate misconduct by exposing unethical practices, and with Campa (2018), who highlights the role of consistent media presence in promoting better corporate governance. These studies suggest that the absence of adequate information dissemination channels can increase the likelihood of opportunistic corporate behaviors, such as greenwashing. Building on these insights, this study leverages the exogenous timing of newspaper closures to examine how diminished local media oversight affects firms' incentives to engage in greenwashing. By focusing on a quasi-natural experiment in the information environment, the analysis offers new causal evidence on the relationship between media scrutiny and corporate environmental disclosure.

Despite increasing academic attention to greenwashing, important gaps remain in the literature. While studies such as Li et al. (2022) and Torelli et al. (2020) have examined the drivers and consequences of greenwashing—ranging from its relationship with financial performance to its role in stakeholder communication—few works have directly investigated how external information channels shape firms' greenwashing behavior. Although prior research has considered multiple factors influencing greenwashing, less is known about how external scrutiny—especially from local news—interacts with firms' disclosure strategies. The complexity of selective reporting remains underexplored, particularly in self-regulated ESG settings where firms face weak enforcement but strong reputational pressures. This gap highlights the need for deeper investigation into how firms respond to changes in their local information environment, and how such changes shape incentives for symbolic versus substantive environmental disclosure.

Local newspapers have historically served as important monitors of corporate conduct, especially in localized settings. As Miller (2006) and Dyck et al. (2008, 2010) observe, the local press plays a unique role in holding nearby firms accountable by uncovering and publicizing misconduct, including symbolic environmental disclosures. Their close ties to the community—including access to employees, suppliers, and regional networks—enable them to detect and report practices that may escape broader national scrutiny (Engelberg and Parsons, 2011; Peress, 2014; Heese et al., 2022). For instance, Circular reported that Sainsbury's faced accusations of greenwashing after an investigation revealed that the majority of soft plastic packaging collected through its in-store recycling scheme was ultimately incinerated rather than properly recycled, despite being marketed as recyclable (Circular Online, 2020). This example illustrates how local journalism can reveal discrepancies between environmental claims and actual practices. From this perspective, the closure of local newspapers may reduce external pressure for transparency, potentially leading to more frequent or egregious forms of greenwashing.

At the same time, local newspapers may not always function as neutral arbiters. Their economic reliance on local advertisers may lead to selective coverage or favorable bias toward large employers or influential local firms (Shapira and Zingales, 2017; Gurun and Butler, 2012). To maintain subscriber loyalty and avoid alienating their readership base, local journalists may avoid negative reporting on major local firms (Shapira and Zingales, 2017). Moreover, Heese et al. (2022) document how limited staffing and investigative capacity can hinder the ability of local newspapers to uncover corporate misconduct. Thus, the actual disciplining role of local media may vary depending on context, leaving open the empirical question of whether newspaper closures systematically affect corporate greenwashing behavior.

This study examines the causal effect of local media scrutiny on corporate greenwashing by leveraging the staggered closure of local newspapers across U.S. counties between 2002 and 2017. The closure of local newspapers—driven primarily by industry-wide economic pressures rather than firm-specific environmental behavior—provides a plausibly exogenous shock to local information environments. This quasi-natural experiment allows us to evaluate how the withdrawal of journalistic oversight alters firms' incentives to selectively disclose environmental performance. We examine greenwashing as a form of selective disclosure, defined as the discrepancy between a firm's symbolic environmental claims and its substantive operational actions (Hawn and Ioannou, 2016). Firms often emphasize relatively benign environmental indicators in external communications while omitting or obscuring more material shortcomings, thereby creating a misleading impression of environmental responsibility without necessarily breaching formal disclosure requirements (Delmas and Burbano, 2011; Lyon and Maxwell, 2011; Kim and Lyon, 2015; Bowen, 2010).

To estimate this effect, we implement a difference-in-differences (DiD) framework comparing changes in greenwashing behavior between firms headquartered in counties that experienced newspaper closures and those in unaffected counties. The treatment variable is defined as a binary indicator equal to one if a firm is located in a county that experienced a closure and the observation year is strictly after the closure year. This setup enables us to capture the post-closure shift in disclosure incentives while avoiding bias from transitional dynamics or anticipatory behavior. As Jiang and Kong (2023) point out, local newspapers play a key role in increasing public visibility of corporate environmental disclosures. When these outlets disappear, external scrutiny declines, creating an environment where greenwashing can occur with less risk of detection. Our approach directly addresses a key limitation in prior greenwashing literature, which often fails to identify causal mechanisms due to endogeneity in media exposure or firm disclosure choices. By exploiting variation in a local institutional shock, we provide stronger empirical evidence on how media oversight shapes symbolic environmental disclosure.

To validate the robustness of our baseline findings, we conduct a series of additional tests. First, we include county-level controls such as per capita income, county population, and population change. While the treatment effect remains positive and statistically significant, its magnitude declines slightly, and population growth itself is positively associated with greenwashing. Second, we verify the parallel trends assumption, and conduct placebo tests using pre-closure years as pseudo-treatment periods, finding no significant effects—supporting the credibility of our identification strategy. Third, we explore alternative definitions of the treatment period by varying the length of the post-closure event window. Specifically, we examine whether the treatment effect persists when restricting the post-treatment period to 3, 5, or 7 years after the newspaper closure. The results remain directionally consistent, though statistical significance weakens under shorter windows, suggesting that the effect of media oversight erosion may accumulate gradually over time. Fourth, we confirm that our results are robust to alternative fixed effects structures and to different operationalizations of greenwashing (e.g., using lagged components or alternative datasets). Finally, heterogeneity analyses reveal that the treatment effect is more pronounced among large firms, high-reputation firms, and those with lower financial constraints.

This paper makes several important contributions to the literature on greenwashing and the informational role of local media.First, it is among the earliest studies to examine how local newspaper closures influence corporate greenwashing behavior—a form of symbolic disclosure that has grown increasingly relevant amid rising stakeholder pressure on environmental accountability. While prior work has extensively documented the media's role in curbing financial misconduct (Dyck et al., 2010; Campa, 2018), its influence on environmental misrepresentation remains largely unexamined. By explicitly linking the decline of local journalism to corporate ESG disclosure practices, this paper fills a critical gap in the greenwashing literature.

Second, the study advances the broader understanding of how firms respond to shifts in external scrutiny. Unlike existing work that views media coverage as a straightforward enforcement mechanism (e.g., (Heese et al., 2022)), this research shows that reduced media visibility does not simply remove deterrence—it also reshapes firms' reputational incentives to engage in symbolic disclosure. This insight deepens our understanding of how information environments interact with corporate communication strategies, particularly in settings where ESG disclosure remains largely voluntary.

Third, this study expands the literature on the societal implications of local news decline by linking newspaper closures to symbolic environmental disclosure—a connection previously overlooked in corporate sustainability research. While prior work has shown that media erosion weakens civic engagement and public governance (Gentzkow et al., 2011), this paper is the first to demonstrate its implications for corporate ESG behavior. The findings show that the disappearance of local newspapers not only undermines public access to environmental information but also alters firms' cost–benefit calculus in selectively disclosing green performance. By shifting the reputational landscape in which firms operate, local news closures unintentionally relax symbolic accountability, thus facilitating greenwashing.

Fourth, the empirical depth and comprehensive design of this study offer a strong foundation for causal interpretation. The analysis draws on firm-level panel data from 2002–2017 and exploits the staggered timing of newspaper closures as a plausibly exogenous shock to local information environments. The baseline difference-in-differences framework is rigorously tested through a suite of robustness checks: alternative treatment timing definitions (e.g., post-closure vs. symmetric event windows), parallel trend validation, placebo tests, and dynamic specifications that capture how treatment effects evolve over time. Importantly, the results remain stable across multiple greenwashing measures and hold after accounting for firm- and county-level covariates, including local economic conditions and population trends.

Furthermore, the study uncovers important heterogeneity in treatment effects. The results suggest that larger firms and those with strong reputational capital are more likely to greenwash following media decline—potentially due to their greater sensitivity to visibility loss—while financially constrained firms appear less responsive, likely due to limited resources or heightened regulatory pressure. These findings offer novel insights into how firm-specific characteristics and local environments jointly moderate the strategic use of ESG disclosure.

Taken together, these contributions advance both theory and practice by clarifying the informational foundations of greenwashing behavior and highlighting the critical role of local journalism in sustaining corporate environmental accountability. This paper provides new causal evidence that local newspaper closures significantly increase greenwashing behavior, particularly among firms with greater visibility and reputational sensitivity. The results underscore the importance of local journalism in shaping disclosure practices and disciplining opportunistic environmental claims. As local media infrastructures continue to erode, these findings reveal an often-overlooked externality: diminished journalistic oversight weakens firms' incentives to provide accurate and complete environmental information. This study thus offers both scholarly insight and practical guidance for stakeholders seeking to interpret ESG disclosures more critically, and for policymakers considering the broader governance implications of declining local media presence.

2 Literature Review

2.1 Corporate Greenwashing

Environmental, social, and governance (ESG) concerns have emerged as a central theme in corporate strategy and financial markets, particularly following the adoption of the United Nations Sustainable Development Goals (SDGs) in 2015 and the signing of the Paris Agreement in the same year (United Nations, 2015; United Nations Framework Convention on Climate Change, 2015). These international frameworks have heightened pressure on firms to demonstrate environmental responsibility and align with sustainability expectations. However, the increasing emphasis on sustainability has also created new incentives for greenwashing, which refers to the act of projecting a false image of compliance without undertaking substantive environmental actions. Understanding this tension is essential for evaluating the credibility of ESG disclosures.

The term greenwashing was first coined by environmentalist Jay Westervelt in 1986 to describe misleading environmental messaging in the hotel industry (de Freitas Netto et al., 2020). While early studies focused on product-level deception, such as exaggerated claims about recyclable packaging or energy efficiency, recent literature has shifted toward examining greenwashing at the firm level. In this broader context, greenwashing reflects a strategic misalignment between firms' external environmental claims and their internal operational practices (Habek and Wolniak, 2015).

According to Hawn and Ioannou (2016), greenwashing is defined as the discrepancy between a firm's "green talk" and "green walk." The former refers to symbolic ESG communications, including sustainability reports, press releases, or marketing campaigns, whereas the latter captures substantive ESG activities, such as environmental investments, emissions reductions, and compliance measures. The gap between these two dimensions forms the conceptual foundation for measuring greenwashing and is often operationalized as the difference between symbolic and substantive ESG indicators.

Recent research suggests that ESG disclosure is frequently used in a symbolic manner to enhance legitimacy or attract capital, even when actual environmental improvements are minimal or nonexistent (Cho et al., 2012; Romero et al., 2019). Berrone et al. (2017); García-Sánchez and Martínez-Ferrero (2018) argue that firms often respond to stakeholder pressure or reputational concerns by overstating their commitment to sustainability. This symbolic use of ESG language is often driven by rational cost-benefit considerations, particularly in regulatory environments where enforcement is weak but reputational returns are high.

Empirical evidence confirms the widespread nature of greenwashing. According to a 2020 global screening by the International Consumer Protection and Enforcement Network (ICPEN), 42 percent of corporate environmental claims were found to be exaggerated, false, or misleading (International Consumer Protection and Enforcement Network, 2020). Misrepresentation is particularly common in consumer-facing sectors such as fashion, cosmetics, and household goods, where marketing plays a central role in shaping public perception. Firms frequently portray themselves as environmentally responsible while offering insufficient verifiable evidence to support their claims.

A well-known real-world example is the Volkswagen emissions scandal. In 2009, Volkswagen began selling over 480,000 "clean diesel" vehicles in the United States. However, it was later revealed that the company had installed software that activated pollution controls only during laboratory tests, while disabling them during normal driving conditions. As a result, the vehicles emitted pollutants at levels up to 40 times higher than legal limits. In 2015, the U.S. government fined Volkswagen \$14.7 billion for violating emissions laws and misleading consumers. This case exemplifies how firms may use green branding to conceal environmentally harmful behavior (Speculations, 2015).

Firms' incentives to engage in greenwashing are shaped by a variety of conflicting forces. On one hand, they may benefit from enhanced ESG reputation, preferential loan terms, and long-term access to green finance (He et al., 2019; Yang et al., 2017; Yu et al., 2020). On the other hand, they face substantial obstacles, such as high costs of innovation, challenges in monitoring, and the potential for reputational damage or legal consequences if their greenwashing is uncovered (Petitjean, 2019; Zhang, 2021). This tension illustrates the fundamental trade-off that firms often face between sustainability and profit maximization (Friedman, 1970).

The credibility of ESG disclosures is further undermined by the absence of standardized reporting frameworks. Compared to traditional financial reporting, ESG disclosures often suffer from low levels of accuracy, transparency, and comparability (Del Giudice and Rigamonti, 2020). The non-financial nature of ESG data, coupled with inconsistencies in global disclosure standards, provides firms with considerable discretion to selectively present favorable information. For example, King and Lenox (2001) show that firms may exaggerate environmental progress in order to gain competitive advantage.

In the absence of credible third-party auditing or strong regulatory enforcement, media scrutiny plays a vital role in constraining opportunistic ESG reporting. By disseminating corporate environmental claims to broader audiences, the media reduces information asymmetry, enhances accountability, and imposes reputational consequences on misrepresentation, which serves as a deterrent to greenwashing and strengthens corporate transparency and discipline (Dyck et al., 2008; Miller, 2006; Dyck et al., 2010). Through investigative reporting, media outlets can expose discrepancies between firms' public disclosures and actual practices, thereby limiting the scope for opportunistic behavior. As Dyck et al. (2008) note, the media helps lower transaction costs by aggregating and distributing information to the public. Miller (2006) further argues that growing consumer demand for environmental responsibility intensifies media scrutiny, which can hold firms accountable for unsustainable practices and discourage symbolic disclosures not backed by real actions.

Nevertheless, the effectiveness of this mechanism depends on the strength of the media landscape. When media oversight declines, particularly in local contexts where information gaps are most severe, it remains unclear how corporate behavior will adjust in response. This uncertainty underscores the importance of understanding the consequences of weakening media environments for ESG credibility.

2.2 Local Newspaper Closures

While local newspapers serve as important monitors of corporate behavior, their independence can be compromised by financial dependencies. Approximately 60% of their revenue comes from local advertisers (Gurun and Butler, 2012), which may lead to positive reporting bias and reluctance to criticize key sponsors. Newspapers also rely on community subscriptions, creating additional pressure to avoid content that could alienate readers—many of whom are employed by the very firms under scrutiny (Mullainathan and Shleifer, 2005; Shapira and Zingales, 2017). Despite these limitations, local newspapers still play a meaningful role in monitoring corporate behavior and reducing information asymmetry, particularly in settings where external enforcement mechanisms are weak or inaccessible (Ma et al., 2022).

Media scrutiny deters greenwashing. Media scrutiny discourages symbolic disclosure, and regulatory institutions can further discourage such practices by imposing oversight (Kim and Lyon, 2015). Media organizations enhance transparency by aggregating and publicising corporate information, thereby reducing transaction costs and promoting accountability (Dyck and Zingales, 2005; Miller, 2006).

Newspapers act as essential watchdogs. For example, coverage by the Financial Times and the Wall Street Journal of self-dealing by Russian firms significantly reduced governance violations by damaging reputations and triggering regulatory investigations (Dyck et al., 2008). Similarly, Snyder and Strömberg (2010) argue that local newspapers, by informing citizens, may also enhance firms' environmental responsibility. Empirical evidence shows that newspaper closures lead to more corporate misconduct, as firms exploit the loss of scrutiny, confirming that media attention deters unethical behavior by increasing reputational risk (Heese et al., 2022; Dyck et al., 2010).

Due to their proximity and access to local networks, local newspapers are often better positioned than national outlets to detect greenwashing at the community level. Their familiarity with regional industries, workforce conditions, and stakeholder dynamics enables them to identify discrepancies in corporate sustainability claims that might otherwise go unnoticed (Miller and Shanthikumar, 2011; Shapira and Zingales, 2017). This localized vigilance is critical for maintaining transparency and accountability in corporate environmental communication.

Secondly, local newspapers act as external monitors for local firms. Miller (2006) documents that the national press monitors corporations for their wrongdoings by providing information about accounting fraud. Miller and Shanthikumar (2011) provide evidence that local newspapers often cover more local firms than national newspapers. Local newspapers can supply the national press with evidence of firms' fraud and misconduct, further disseminating this information. Therefore, we expect that the closure of a local newspaper will weaken its monitoring role on local firms, increasing their chances of performing greenwashing.

Building on this literature, we examine whether the closure of local newspapers influences firms' propensity to engage in greenwashing. Local newspapers frequently cover corporate sustainability claims and may serve as the first line of journalistic oversight, particularly in communities where other accountability institutions are limited. Their disappearance removes a layer of public visibility and may alter how firms approach ESG disclosure. As Miller (2006) point out, local newspapers are often better positioned to report on nearby firms due to their proximity and access to community resources. Ma et al. (2022) estimate that more than 50% of original local and regional news in the U.S. is generated by local newspapers. When these outlets shut down, the reach and depth of environmental disclosure coverage is likely to decline, weakening the broader media infrastructure that supports transparency.

The decline of local newspapers has become a critical issue with far-reaching implications for corporate transparency and accountability. Traditionally, local newspapers have played an essential role in monitoring business conduct by providing timely and localized coverage of corporate activities. Their reporting helps shed light on firms' environmental behavior and disseminates corporate disclosures to broader audiences. Research by Heese et al. (2022) finds that newspaper closures are associated with increased corporate misconduct, suggesting that firms take advantage of diminished scrutiny. Similarly, Dyck et al. (2010) demonstrate that media attention deters unethical behavior by elevating reputational risks and exposing malpractices to public view.

Moreover, the disappearance of local newspapers is likely to diminish the quality and scope of information available to stakeholders. National media often lacks the proximity and depth to report on localized issues with the same immediacy (Lyon and Maxwell, 2011). The loss of localized investigative capacity—essential for uncovering complex or context-specific corporate behavior—can reduce the likelihood that firms' symbolic ESG disclosures are scrutinized or contested. As Schiffrin (2014) notes, meaningful investigative journalism typically requires sustained local engagement and specialized knowledge that national outlets may not consistently provide.

However, the decline in media visibility following newspaper closures may also reshape the incentives underlying greenwashing. Media coverage amplifies the reputational value of ESG disclosures by broadcasting them to wider audiences and enhancing stakeholder scrutiny (Dyck et al., 2010). In the absence of this visibility, firms may perceive fewer benefits from symbolic disclosure, as their environmental messaging is less likely to reach or influence key audiences.

At the same time, the reduction in journalistic oversight may decrease the expected cost of misrepresentation. With fewer investigations into inconsistencies between reported and actual environmental performance, firms may face less reputational or regulatory risk for engaging in greenwashing (Jiang and Kong, 2023; Schiffrin, 2014). As a result, greenwashing behavior may increase due to weaker deterrence, or decline due to muted reputational rewards.

Building on these insights, this study examines whether the loss of local newspaper coverage affects corporate greenwashing behavior. On the one hand, closures reduce external scrutiny from the media, potentially enabling firms to overstate their environmental performance. On the other hand, diminished media visibility may weaken the reputational incentives for symbolic disclosure.

The net effect of these opposing forces remains an open empirical question.

H1: Local newspaper closures affect corporate greenwashing behavior by simultaneously reducing external oversight and reputational incentives. We hypothesize that the former effect dominates, such that newspaper closures lead to an increase in greenwashing.

3 Data

3.1 Sample

To quantify corporate greenwashing, we follow Hawn and Ioannou (2016) and define it as the discrepancy between a firm's external and internal environmental actions. External actions refer to public claims about environmental commitments, while internal actions involve actual practices aimed at improving environmental performance. Greenwashing arises when a firm's reported actions do not align with its real environmental efforts. We classify Thomson Reuters' Refinitiv environmental data into external and internal categories, identifying 23 external and 22 internal items. Following Hawn and Ioannou (2016), we normalize these indices to a 0–1 scale and adjust for firm size, allowing us to directly measure the gap between reported and actual environmental actions (Eccles et al., 2014; Hawn and Ioannou, 2016).

As an illustrative example, MacLean and Behnam (2010) analyze a case in which a firm's external environmental claims significantly outweighed its internal efforts. The company constructed a legitimacy façade by decoupling symbolic compliance from day-to-day operational practices. While outwardly appearing environmentally responsible, the firm's internal practices did not reflect substantive commitment. This misalignment not only generated internal skepticism and dissonance among employees but was eventually uncovered by regulators, severely damaging the firm's external credibility and legitimacy. This case exemplifies how a large gap between external and internal actions—what we define as greenwashing—can lead to reputational and regulatory consequences.

Earlier research has used two primary sources to document newspaper closures. Darr et al. (2018) relied on the *Chronicling America* database, managed by the Library of Congress, which records the founding and closing dates of thousands of newspapers. Gentzkow et al. (2011) and Gao et al. (2020) utilized the *Editor & Publisher Yearbook*, an annual publication that provides a comprehensive overview of U.S. newspapers, including information on publication frequency and geographic coverage. These studies typically define a newspaper closure as a drop in the number of daily newspapers within a county from one year to the next, and assign newspapers to counties using data from the 2010 U.S. Census.

Building on this literature, we draw on Jiang and Kong (2023), who constructed a comprehensive list of daily newspaper closures by combining both data sources and applying the above matching and closure definitions. Specifically, we adopt their Appendix Table A1, which reports 236 daily newspaper closures between 2000 and 2018. Following their approach of retaining only the first closure event in counties with multiple closures, and restricting to our analysis period, we obtain a final sample of 159 county-level newspaper closures between 2002 and 2017.

For population data, we follow the methodologies outlined by Jiang and Kong (2023) and Gao et al. (2020). Annual population and per capita income data are sourced from the U.S. Bureau of Economic Analysis (BEA) through the CAINC1 dataset. This dataset provides county-level population estimates, which are based on the U.S. Census Bureau's midyear estimates and adjusted using the intercensal method to account for decennial Census revisions. Additionally, employment data are gathered from the BEA's CAINC4 dataset, which includes annual employment and wage data at the county level. Employment figures are normalized by dividing total employment by the county's population for each year. These data allow us to control for key macroeconomic conditions at the county level, ensuring that fluctuations in population size and employment are accounted for, which are factors that could influence both newspaper closures and corporate greenwashing behavior.

Sample Composition. As shown in Table 1, my initial sample is based on firm-year observations from WRDS (Compustat) for U.S. publicly listed firms with non-missing tickers and total assets, covering the period from 1999 to 2024. This yields 221,117 firm-year observations for 23,108 unique firms. In the first step, I merge these records with U.S. firm-level data from S&P Capital IQ to obtain county identifiers. The Capital IQ dataset spans 2002 to 2022. I retain firm-year observations with non-missing county information, resulting in 74,486 observations across 6,743 firms and 408 industries (based on three-digit SIC codes).

Next, I merge the firm-level dataset with newspaper closure information from Jiang and Kong (2023). This dataset is constructed from Chronicling America and the Editor & Publisher Yearbook, covering the period 2000 to 2018. Following their approach, I retain only the first daily newspaper closure event per county.

In the third step, I incorporate county-level demographic and economic data—including total population, per capita income, and employment—sourced from the U.S. Bureau of Economic Analysis (BEA), which provides data from 1969 to 2022. These variables are matched to firms using county-year identifiers.

Subsequently, I merge in greenwashing scores from Refinitiv (ASSET4), available from 2002 to 2017. Following Hawn and Ioannou (2016), greenwashing is defined as the gap between a firm's external (symbolic) and internal (substantive) environmental actions. I exclude financial firms and match the scores at the firm-year level.

After all steps of data construction and filtering, the final sample as shown in Table 1 covers the years 2002 to 2017 and includes 12,016 firm-year observations from 1,924 unique firms across 334 industries. Before conducting the empirical analysis, I remove observations with missing values in any key regression variable to ensure robustness and consistency.

3.2 Model

Empirical Strategy: DiD Estimation Framework We employ a Difference-in-Differences (DiD) framework to assess the impact of local newspaper closures on firms' greenwashing behavior. The treatment indicator is defined as equal to one for firm-year observations where the firm is in a county that experienced a newspaper closure, and the year is strictly after the closure year.[1] This approach captures the discontinuous shift in local media scrutiny following closure events and enables comparison between treated and untreated firms over

[1] In robustness checks, we also experiment with alternative treatment windows of fixed duration (e.g., 3-, 5-, and 7-year periods post closure), following the event-study structures used in Jiang and Kong (2023) and Heese et al. (2022). While shorter windows yield weaker statistical significance, the direction and magnitude of effects remain consistent, providing additional support for the baseline specification.

time.

This definition is consistent with identification strategies used in prior research that examines the consequences of local institutional changes, including Agirdas (2015), Liu (2023), and Chen et al. (2024). By structuring the treatment variable to reflect the post-closure period, we aim to isolate the change in firms' symbolic environmental disclosure behavior attributable to the loss of journalistic oversight.

Our specification includes firm fixed effects to control for time-invariant firm characteristics, state-by-year fixed effects to account for regional economic conditions and shocks, and industry-by-year fixed effects to control for sector-specific trends in environmental disclosure norms. Standard errors are clustered at the county-by-year level to address potential within-cluster correlation.

The specific model to test whether newspaper closures affect corporate greenwashing behavior is shown as below:

$$y_{ikt} = \alpha_0 + \alpha_1 \text{Treatment}_{kt} + \alpha_i + \alpha_{st} + \alpha_{it} + \text{Controls} + \epsilon_{ikt} \tag{1}$$

We define the dependent variable y_{ikt} as the standardized greenwashing score of firm i, located in county k, in year t. The key explanatory variable, Treatment_{kt}, is a binary indicator equal to 1 if the firm is in a county that experienced a newspaper closure and the observation year is strictly after the year of closure; it is set to 0 otherwise. This definition follows empirical implementations from prior studies examining post-event effects of local news closures (Agirdas, 2015; Liu, 2023; Chen et al., 2024).

We define the treatment period as beginning in the year following a newspaper closure ($t = \text{ClosureYear} + 1$), thereby capturing corporate responses after the institutional shock has materialized. The closure year itself ($t = 0$) is excluded from the treatment period to avoid confounding effects stemming from anticipatory behavior or transitional adjustments that may occur during the event year.[2] This approach is consistent with prior studies employing event-study frameworks in similar contexts (e.g., Jiang and Kong (2023)), which also omit the closure year to enhance identification. By adopting this specification, the analysis captures post-closure dynamics without imposing an arbitrary window length, allowing for more flexible estimation of persistent effects.

Following prior studies (e.g., Marquis et al. (2016); Berrone et al. (2017); Arouri et al. (2021); Jiang and Kong (2023)), we control for a comprehensive set of firm- and county-level characteristics that may affect greenwashing behavior and are potentially associated with local newspaper closures. Prior research suggests that firm size influences both stakeholder attention and the capacity to manage external perceptions. Larger firms tend to receive more media coverage, are embedded in broader networks, and are more likely to be subject to external scrutiny (Reitz et al., 1979; Singh, 1986). In addition, they have greater access to financial markets, which reduces the cost of implementing environmental initiatives (Innes and Sam, 2008; Herbohn et al., 2014). In this study, we measure firm size using the natural logarithm of total employees, which captures not only organizational scale but also the influence of internal stakeholders. Employees are widely recognized as an important stakeholder group that can affect corporate decision-making (Barnett, 2007). Firms with larger workforces may face stronger internal expectations for social and environmental responsibility and greater external visibility and reputational exposure. This specification allows us to capture the dual effects of firm scale and stakeholder dynamics on corporate disclosure behavior.

According to (Delmas and Burbano, 2011), firms' size and profitability can shape their propensity to adopt greenwashing strategies. Firm performance is measured by return on assets, calculated as operating income before depreciation divided by the average of total assets based on the most recent two periods. More profitable firms are likely to be more operationally efficient and able to allocate resources to environmental or symbolic strategies

[2] In Appendix Table A1, as a robustness check, I examine a variant where the treatment period begins from the closure year itself ($t \geq \text{ClosureYear}$). This alternative yields a statistically insignificant estimate, suggesting that firms may not adjust greenwashing behavior immediately, but only after the institutional shock is realized. This supports the baseline definition that focuses on post-closure dynamics.

(Dowling and Pfeffer, 1975; Meyer and Rowan, 1977). Fixed Asset, also known as capital intensity, is measured as net property, plant, and equipment scaled by total assets (Marquis et al., 2016; Arouri et al., 2021; Jiang and Kong, 2023). This variable reflects a firm's potential to cause environmental impact and the likelihood of scrutiny over environmental performance. In addition, capital intensity can capture important intra-industry variation in operational structure and disclosure practices. To account for firms' financing needs, we control for the debt ratio (total debt over assets). Higher leverage often implies greater pressure from creditors, which may incentivize firms to avoid disclosing negative environmental performance (Zhang, 2023; Tian et al., 2025; Kim et al., 2021). We also include capital expenditures scaled by total assets as a proxy for long-term strategic investment, which may relate to strategic disclosure. Firms engaging in larger capital outlays may be more inclined to shape stakeholder perceptions through disclosure practices.

Firm-level risk is further measured by the inverse of the debt-to-equity ratio, capturing firms' solvency. Prior literature shows that financially constrained or distressed firms may strategically use symbolic disclosure to maintain legitimacy under external pressure (Mateo-Márquez et al., 2022). However, heightened risk may also induce caution: firms facing solvency challenges may avoid making unverifiable environmental claims to reduce exposure to reputational or regulatory backlash. Thus, the expected effect of risk on greenwashing is theoretically ambiguous and likely context-dependent.

Corporate reputation is captured using a binary indicator equal to one if the firm is listed among *Fortune's 100 Best Companies to Work For in America* in a given year (Marquis et al., 2016; Arouri et al., 2021). This measure reflects a firm's perceived standing among stakeholders and its efforts to cultivate a socially responsible image. High-reputation firms may have stronger incentives to maintain consistent ESG narratives and therefore engage in greenwashing to meet public expectations. Conversely, their established trust capital might reduce the need for exaggerated claims, as stakeholders already perceive them favorably. As such, reputation can either amplify or dampen symbolic disclosure depending on how firms weigh reputational gains against risks of being exposed.

At the county level, in line with Gentzkow et al. (2011); Gao et al. (2020); Jiang and Kong (2023), we include income per capita (expressed as the natural logarithm of county income per capita), county population (measured as the natural logarithm of population for each county-year), and population change (the percentage change in population from the previous year) to account for the macroeconomic conditions in each county. Echoing the insights of Gentzkow et al. (2011), we include population size and per capita income due to their historical significance in determining newspaper market dynamics, thereby safeguarding our study against potential confounders that could be related to both newspaper closures and corporate greenwashing. These county-level economic indicators are also crucial in excluding the possibility that the economic conditions of a county are driving both the shutdown of its newspapers and the greenwashing practices of local firms.

Further, prior literature emphasizes the role of economic development in shaping environmental expectations. Some scholars (De Soto, 1989; Husted, 2005) argue that wealthier and more developed regions are more likely to demand environmental accountability and foster sustainability. For example, Grant and Gnyawali (1996) finds that individuals in economically advanced areas are better informed and more likely to pressure companies for responsible behavior. However, other studies (Chapple and Moon, 2005; Ioannou and Serafeim, 2012) highlight that economic development is not the sole determinant of corporate social performance across regions, as variations may also arise from differences in institutional frameworks, cultural norms, and information environments.

All continuous control variables are winsorized at the 1st and 99th percentiles to minimize the influence of outliers. Detailed definitions of all variables are provided in the Appendix.In addition, we apply entity fixed effects to control for time-invariant characteristics at the firm level, state-year fixed effects to account for time trends across states, and industry-year fixed effects to capture industry-wide shifts over time. These fixed effects help control for omitted factors at the firm, state, and industry levels, ensuring the robustness of our identification strategy. Specifically, α_i represents entity fixed effects, α_{st} denotes state-year fixed effects, and α_{it} refers to industry-year fixed effects. Robust standard errors are clustered at the county-by-year level.

Sample Distribution Table 2 reports the annual distribution of firm-year observations by treatment status from 2002 to 2017. Column "0" corresponds to observations in counties without a newspaper closure in a given year, while column "1" refers to those in counties where a closure occurred. The total number of observations rises steadily over time, from 58 in 2002 to over 1,000 in both 2016 and 2017, reflecting the expansion of the matched sample as more firms satisfy data availability criteria in later years.

A notable pattern is the increasing number of treated firm-year observations over the sample period. In 2002, only 26 firm-year observations fall into the treated group, while by 2017 this number grows to 359. This upward trend is consistent with the gradual spread of local newspaper closures across U.S. counties and indicates a staggered adoption structure in the treatment variable.

The proportion of treated observations also increases substantially over time. While treatment observations account for approximately 45% of the sample in the early years, they comprise around 35–40% of the total from 2008 onward. This variation highlights the need for a difference-in-differences approach that accounts for staggered treatment timing, where units enter treatment in different years.

Overall, this table underscores the temporal heterogeneity in treatment exposure and supports the empirical strategy adopted in the paper.

4 Results

4.1 Baseline Specification Firm-Level

Table 4 reports the baseline regression results assessing the effect of local newspaper closures on corporate greenwashing behavior, using firm-level covariates and a comprehensive fixed effects strategy—controlling for firm, state-year, and industry-year heterogeneity. Column (1) reveals that the coefficient on Treatment is positive and statistically significant at the 5% level (0.121, $p < 0.05$), indicating that firms located in counties that experienced local newspaper closures are significantly more likely to engage in symbolic environmental disclosure. This result is consistent with the notion that the disappearance of local journalistic oversight reduces reputational pressure, thereby encouraging firms to exaggerate their environmental commitment without corresponding substantive actions. In the absence of investigative coverage and public accountability typically facilitated by local media, firms may perceive a diminished threat of reputational penalties for misrepresentation (Jiang and Kong, 2023; Heese et al., 2022; Dyck et al., 2008).

Among firm-level characteristics, FirmSize enters with a positive and significant coefficient (0.100, $p < 0.05$), suggesting that larger firms are more prone to greenwashing. One possible explanation is that large firms are more visible to the public, investors, and regulators, and thus face heightened expectations to maintain an environmentally responsible image. The pressure to maintain social legitimacy may lead larger firms to adopt symbolic disclosure strategies even when actual environmental improvements are limited (Marquis et al., 2016; Berrone et al., 2017; Zhang, 2023).

The coefficient on FixedAsset is negative and statistically significant (-0.601, $p < 0.1$), implying that capital-intensive firms are less likely to greenwash. This could be attributed to their operational transparency and environmental traceability—physical operations involving significant fixed assets (e.g., factories, plants) are easier for external stakeholders to monitor through alternative oversight mechanisms such as regulatory inspections, satellite imagery, or environmental NGO scrutiny. In such cases, symbolic claims that lack substantive backing are more likely to be exposed, which increases the reputational and regulatory cost of greenwashing (Marquis et al., 2016; Arouri et al., 2021).

The R&D Intensity also shows a negative and significant association with greenwashing behavior (-1.178, $p < 0.05$), implying that innovation-intensive firms are less likely to rely on symbolic environmental claims. This aligns with the idea that firms investing more in R&D are building long-term capabilities and may prefer to align their disclosure with substantive environmental performance. Alternatively, these firms might be more cautious in their communication strategies to avoid reputational risks if expected outcomes from R&D do not materialize (Delmas and Burbano, 2011; Yu et al., 2020).

On the other hand, variables such as ROA, DebtRatio, ScaledCapitalExpenditure, and Risk do not exhibit statistically significant relationships in the baseline model. This suggests that short-term profitability and financing structure may not play a dominant role in shaping symbolic disclosure decisions once firm, industry, and regional fixed effects are accounted for.

Taken together, these findings establish that local newspaper closures significantly increase the likelihood of corporate greenwashing, especially among firms that are larger in size and less constrained by operational transparency. These baseline estimates provide a foundation for the subsequent analyses, which investigate whether the observed effects vary with local socioeconomic conditions or specific firm characteristics that influence reputational incentives.

4.2 County-Level Controls

To assess the sensitivity of the baseline estimates to regional economic and demographic conditions, I augment the core specification with county-level control variables. Column (1) of Table 4 reports the baseline model using only firm-level controls, while Column (2) introduces three additional covariates: Ln Income Per Capita, Ln County Population, and Population Change. The inclusion of these variables slightly reduces the treatment coefficient from 0.121 to 0.102, though the result remains statistically significant at the 10% level. This indicates that the estimated impact of newspaper closures is not driven by unobserved county-level characteristics, and the core finding is robust to the inclusion of local contextual factors.

Population dynamics. The coefficient on Population Change is positive and statistically significant ($\beta = 7.301$, $p < 0.05$), suggesting that firms located in faster-growing counties tend to exhibit higher levels of greenwashing. This pattern may reflect increased economic activity and market competition in expanding regions, where firms have stronger incentives to use symbolic environmental disclosure to attract consumers, investors, or talent. Additionally, population growth may temporarily outpace the development of institutional oversight, creating a strategic window for firms to engage in reputation management without proportional scrutiny.

Overall, these results strengthen the conclusion that local newspaper closures contribute to greenwashing behavior, and this relationship persists even when controlling for county-level heterogeneity.

4.3 Robustness tests

Parallel Trend Test. The validity of the difference-in-differences design relies on the parallel trends assumption, which requires that, absent the newspaper closures, treated and control firms would have exhibited similar trends in corporate greenwashing behavior. Following Jiang and Kong (2023) and Heese et al. (2022), I conduct an event-study analysis to assess the plausibility of this assumption.

Specifically, I estimate the following dynamic specification by replacing the post-treatment indicator with a series of dummy variables capturing the relative years around the newspaper closure:

$$y_{ikt} = \alpha_0 + \sum_{m \neq -1} \beta_m D_{m,kt} + \alpha_i + \alpha_{st} + \alpha_{it} + \text{Controls} + \epsilon_{ikt} \tag{2}$$

where $D_{m,kt}$ is a set of indicator variables equal to one if firm i is located in a county that experienced a newspaper closure, and the year t is m years relative to the closure year. The event window includes $m = -3, -2, 0, 1, 2, 3$, and year $m = -1$ is omitted as the reference period to avoid multicollinearity. Firm fixed effects (α_i), state-year fixed effects (α_{st}), and industry-year fixed effects (α_{it}) are included, along with firm-level control variables. Standard errors are clustered at the county-by-year level.

Figure 1 plots the estimated coefficients and their 95% confidence intervals. The coefficients on the pre-closure years ($m = -3$ and $m = -2$) are statistically insignificant and close to zero, supporting the parallel trends assumption. In the post-closure period, the coefficients begin to rise, becoming statistically significant in year +3, indicating a delayed but meaningful increase in greenwashing behavior among treated firms.

For presentation clarity, we focus on a symmetric six-year window from three years before to three years after the newspaper closure event. The omitted baseline is the year prior to the event ($t = -1$). Results using a wider window (±5 years) yield qualitatively similar conclusions and are available upon request.

Placebo Test. To further evaluate the parallel trends assumption and rule out spurious correlations, I conduct a placebo test following Roberts and Whited (2013) and Jiang et al. (2016). Specifically, I create pseudo newspaper closures by artificially shifting each actual closure year three years earlier than its true timing. I then re-estimate the baseline model (Model 1) using this placebo treatment indicator to compare the greenwashing behavior of firms in the treatment and control counties in the pseudo post-closure period.

Table 5 reports the results from this falsification test. The estimated coefficient on the placebo treatment is small in magnitude and statistically insignificant, indicating that pseudo closures do not affect corporate greenwashing. This finding suggests that treated and control firms exhibit similar trends in the absence of actual newspaper closures, supporting the identifying assumption of parallel trends.

Dynamic Effects of Newspaper Closure. To validate the parallel trends assumption and investigate the dynamic impact of local newspaper closures on corporate greenwashing behavior, I estimate an event-study specification spanning five years before to five years after the closure event. The omitted category is the year prior to the closure (i.e., year –1).

The results show that the estimated coefficients for the pre-treatment period (*Dm5* to *Dm2*) are small in magnitude and statistically insignificant ($p > 0.4$), which supports the validity of the parallel trends assumption—there is no evidence of systematic differences in greenwashing trends between treated and control firms before the closure.

In the treatment window, I observe no immediate response in the year of closure (*D0*), as the coefficient is close to zero and not significant ($p = 0.756$). Starting from year +2 (*Dp2*), however, the estimated effect begins to rise and becomes statistically significant in year +3 (*Dp3*, coefficient = 0.204, $p = 0.045$) and remains significant in year +4 (*Dp4*, coefficient = 0.201, $p = 0.050$). In year +5 (*Dp5*), the effect remains positive and marginally significant ($p = 0.071$), suggesting that the greenwashing effect is persistent and does not dissipate quickly.

This pattern suggests that the impact of local newspaper closures on corporate greenwashing emerges gradually over time rather than immediately. The delayed response is consistent with the notion that firms adjust their public environmental disclosures with a lag following reductions in local media scrutiny.

These findings indicate that the reduction in media scrutiny following newspaper closures does not immediately change firms' behavior, but rather leads to a gradual and sustained increase in greenwashing over time. The delayed and growing treatment effect supports the interpretation that diminished public oversight weakens firms' incentives to maintain environmental transparency.

Varying the Length of the Event Window. To assess whether the baseline findings are sensitive to the choice of event window, I re-estimate the static difference-in-differences (DID) specification using alternative time windows surrounding the newspaper closure year. The full-sample specification uses all firm-year observations from 2002 to 2017 (7,207 observations), yielding a *Treatment* coefficient of 0.121 (standard error = 0.045, $p < 0.05$), suggesting that, on average, local newspaper closures increase the firm's greenwashing intensity by 0.121 standard deviations.

However, this full-sample estimate assumes that the treatment effect is homogeneous across all post-closure years, which may be unrealistic if the effect is concentrated shortly after closure or diluted by long-run trends. To address this concern, I follow prior literature (Gao et al., 2020; Heese et al., 2022; Jiang and Kong, 2023) and restrict the treated sample to symmetric windows around the closure year: ± 3 years (column 1), ± 5 years (column 2), and ± 7 years (column 3). This approach, commonly used in event-study designs, helps focus on the period most likely affected by the shock while avoiding the inclusion of potentially confounding post-treatment dynamics.

The estimates across these alternative windows remain consistent in both magnitude and significance. In the ± 3-year window (5,144 treated observations), the coefficient is 0.124 ($p < 0.10$); in the ± 5-year window (5,654 observations), it increases to 0.154 ($p < 0.05$); and in the ± 7-year window (6,061 observations), it is 0.146 ($p < 0.05$). These results suggest that the estimated effect is not sensitive to the length of the event window, reinforcing the robustness of the main conclusion.

In addition, using narrower windows helps isolate short- and medium-term treatment effects and reduce potential contamination from unrelated long-term macroeconomic, technological, or policy trends. The fact that the effect persists across different specifications is consistent with prior evidence that firms respond relatively quickly to changes in the local information environment (Gurun and Butler, 2012; Jiang and Kong, 2023), and supports the interpretation that local newspaper closures causally increase greenwashing intensity in the years surrounding the event.

Fixed-effect choice. The choice of fixed effects is pivotal in a difference-in-differences design, as the specification must absorb exactly those dimensions of unobserved heterogeneity that may be correlated with treatment assignment. In firm-level studies of corporate environmental behavior, it is common to include firm and year fixed effects to control for time-invariant firm characteristics and macroeconomic shocks. However, such a structure may fall short when treatment exposure—such as a local newspaper closure—is determined by geographic factors and may affect firms differently across sectors.

This study therefore adopts a more comprehensive fixed effects structure that includes firm, state-by-year, and industry-by-year fixed effects. This triple-fixed-effects model controls for three critical sources of heterogeneity: firm-specific traits, regional time trends, and sector-specific disclosure norms that evolve over time. The inclusion of state-by-year fixed effects addresses the concern that newspaper closures tend to occur in counties facing economic downturns or enforcement budget cuts, which could independently influence firms' reporting incentives. The industry-by-year fixed effects control for changes in sectoral ESG expectations and disclosure practices that may co-vary with the timing of closures.

Table 8 illustrates the importance of this modeling choice. When only firm and year fixed effects are included (column 1), the estimated effect of *Treatment* is positive but not statistically significant, indicating limited precision. In contrast, when state and industry fixed effects are added (column 2), the treatment coefficient becomes statistically significant at the 10% level. This contrast highlights the need to control for both geography and industry heterogeneity to obtain a credible estimate of how firms respond to diminished media oversight.

Alternative Treatment Definition. To assess the robustness of the baseline findings to alternative definitions of treatment timing, I re-estimate the main specification using several variants of the treatment window. Specifically, I vary whether the treatment period includes the year of newspaper closure (ClosureYear), and whether it captures the subsequent three, five, or ten years. As reported in Table 9, the estimated coefficients on the treatment indicator remain generally small and statistically insignificant when restricting the treatment window to shorter periods (e.g., $year >$ ClosureYear and $year \leq$ ClosureYear + 3 or +5). However, when extending the post-closure window to ten years, the coefficient becomes positive and weakly significant. These results suggest that the effect of newspaper closures on greenwashing behavior may accumulate gradually and is more discernible over a longer horizon, while also demonstrating that the main finding is not overly sensitive to modest changes in treatment timing.

Alternative Greenwashing Measurement. To test whether the findings are robust to alternative definitions of greenwashing, I compare three well-established measures in the literature. GW2 is the benchmark measure from Hawn and Ioannou (2016), defined as the difference between symbolic and substantial CSR disclosures measured in the same year. GW3 follows the same authors but introduces a one-year lag for substantial disclosures, capturing the idea that firms may disclose symbolically after waiting to assess the outcome of substantive CSR activities (e.g., green innovation). This lag structure is motivated by theoretical arguments suggesting a delayed response in symbolic reporting.

GW1, on the other hand, is based on Marquis et al. (2016) and measures greenwashing through selective disclosure, calculated as the difference between absolute and weighted disclosure ratios, using Trucost data.

As shown in Table 10, the treatment effect remains positive and statistically significant across all three definitions. The coefficient on *Treatment* is positive and significant at the 5% level under both GW2 and GW3. While GW3 introduces a lagged design and leads to a smaller estimation sample, the effect size and significance are largely preserved. This supports the idea that symbolic responses may follow substantive CSR decisions with a delay, yet the overall pattern holds.

GW1, which is based on a different data source and concept (selective rather than symbolic-substantive mismatch), also yields a positive and significant coefficient. Because GW1 covers the period 2005–2020, the earlier firm-years in the baseline sample are excluded. Despite this difference in measurement and sample coverage, the results remain directionally consistent and statistically significant.

Taken together, these findings reinforce the robustness of the main result: local newspaper closures are associated with increases in greenwashing behavior, regardless of how greenwashing is operationalized.

4.4 Additional Tests

Heterogeneous Effects by Firm Size. To explore whether the effect of local newspaper closures varies by firm size, I conduct a heterogeneity analysis by interacting the treatment variable with a dummy that equals one if a firm's employment is above the median level in the sample. Column (1) of Table 11 replicates the baseline regression using the full sample. Column (2) introduces the interaction term between the treatment indicator and the large-firm dummy. The coefficient on the interaction term is positive and statistically significant at the 5% level, suggesting that the treatment effect is primarily driven by large firms.

The effect of treatment for small firms (i.e., those below the median size) is statistically insignificant, indicating that these firms are less responsive to changes in local newspaper oversight. This pattern may reflect differences in public exposure, regulatory scrutiny, or reputational vulnerability. Larger firms are more likely to be in the public eye and face stronger stakeholder expectations, which may increase their incentive to engage in symbolic environmental disclosure when local monitoring weakens. These results are consistent with the notion that greenwashing is a strategic response more prevalent among firms with higher external visibility.

Heterogeneity by Financial Risk To examine whether financial risk moderates the relationship between newspaper closures and greenwashing, I interact the treatment variable with a high-risk dummy, defined as firms with above-median risk, measured by the inverse of the solvency ratio (i.e., stockholders' equity divided by total liabilities). As shown in Table 12, the interaction term is significantly negative at the 5% level (coefficient = –0.149), indicating that high-risk firms reduce their greenwashing following local newspaper closures.

This finding runs counter to the expectation that less media oversight may relax constraints and encourage opportunistic behavior. A plausible explanation is that financially vulnerable firms already face greater regulatory scrutiny or financing pressure and may respond to reduced external monitoring by becoming more cautious, rather than risk amplifying symbolic disclosures. Alternatively, resource-constrained firms may lack the capacity to engage in more greenwashing. Thus, the results highlight a heterogeneous mechanism where financially weaker firms adopt a more conservative disclosure strategy in response to declining media scrutiny.

Heterogeneity by Reputation To further explore heterogeneity in firms' greenwashing responses, I examine whether the effect of local newspaper closures varies with firms' reputational standing. Corporate reputation is captured using a binary indicator equal to one if the firm is listed among *Fortune's 100 Best Companies to Work For in America* in a given year (Marquis et al., 2016; Arouri et al., 2021). This measure reflects a firm's perceived standing among stakeholders and its efforts to cultivate a socially responsible image.

As shown in Table 13, the interaction term between the treatment indicator and high reputation is positive and statistically significant. This suggests that firms with stronger reputational concerns are more responsive to the decline in local media oversight, potentially due to their higher visibility or pressure to maintain a consistent ESG narrative. It is worth noting that the reputation data is only available from 2004 onward. Accordingly, I restrict the sample period to 2004–2017, which reduces the number of observations from 7,207 to

7,066. Nevertheless, the results remain robust, indicating that the main findings are not driven by sample selection.

Heterogeneity by County-Level Economic Conditions To examine whether the effect of newspaper closures on greenwashing behavior varies by local economic conditions, I interact the treatment indicator with a high-income county dummy. The dummy equals one for firms located in counties where income per capita is above the sample median. As shown in Table 14, the interaction term is positive but statistically insignificant, suggesting no strong differential effect between richer and poorer regions. This indicates that local income levels do not materially shape the way firms adjust their greenwashing behavior in response to the loss of local media oversight. In addition, county-level variables such as LnIncomePerCapita, LnCountyPopulation, and PopulationChange are included as controls. Notably, counties experiencing faster population growth show a significantly positive association with greenwashing, highlighting the potential influence of local demographic dynamics.

Heterogeneity by County Population Growth To further explore whether the effect of newspaper closures varies with regional demographic dynamics, I interact the treatment variable with an indicator for counties experiencing above-median population growth. As shown in Column (2) of Table 15, the coefficient on population growth remains significantly positive, suggesting that firms in faster-growing areas are more likely to engage in greenwashing behavior. This may reflect heightened market competition or increased pressure to attract consumers and talent in rapidly expanding regions.

However, the interaction term between treatment and high population growth is statistically insignificant. This indicates that the marginal effect of local newspaper closures is not stronger in counties with faster population growth. One possible explanation is that alternative information channels (such as digital media or online platforms) may partially substitute for traditional newspapers in high-growth regions. Another possibility is that firms' greenwashing behavior is primarily driven by internal factors—such as resources or strategic goals—rather than external population dynamics.

5 Conclusion

This dissertation provides a comprehensive analysis of how the decline in local journalistic oversight—measured by daily newspaper closures—affects corporate environmental disclosure behavior, with a particular focus on greenwashing. Greenwashing is defined as the discrepancy between firms' symbolic (external) and substantive (internal) environmental actions. Leveraging a unique panel dataset of publicly listed U.S. firms from 2002 to 2017, combined with county-level newspaper closure data and regional demographic and economic indicators, I employ a difference-in-differences strategy to identify the causal effect of media oversight erosion on firms' symbolic environmental disclosure.

Baseline results reveal that firms located in counties experiencing a newspaper closure are significantly more likely to engage in greenwashing. This effect remains robust after controlling for firm fixed effects, state-by-year fixed effects, and industry-by-year fixed effects, suggesting that the absence of local media reduces reputational pressure and lowers the cost of symbolic disclosure without corresponding action. Firms appear to exploit this institutional void by strategically inflating their environmental claims in the absence of local journalistic scrutiny.

Event-study analyses confirm the parallel trends assumption and show a delayed but persistent increase in greenwashing following newspaper closures. Rather than responding immediately, firms gradually adjust their behavior as reputational risk diminishes over time. This temporal pattern is consistent with the idea that the weakening of external oversight alters firms' disclosure strategies in a progressive manner. Placebo tests, alternative event windows, various fixed effects structures, and different operationalizations of greenwashing all reinforce the robustness of the main findings.

Heterogeneity tests reveal that the treatment effect is more pronounced among large firms and those with strong reputational standing. These firms, being more visible to stakeholders, are particularly responsive to the relaxation of local scrutiny. In contrast, financially constrained firms tend to behave more conservatively in the face of reduced oversight, possibly due to limited resources or regulatory pressure. Interestingly, local income levels and population growth do not significantly moderate the treatment effect, suggesting that the erosion of media oversight has a broadly uniform influence across economic contexts.

Theoretically, this study contributes to several literatures. First, it integrates media economics into corporate environmental behavior research by documenting the disciplining role of local journalism in ESG disclosure. Second, it introduces a novel empirical approach to quantifying greenwashing, combining symbolic and substantive dimensions of environmental performance. Third, it bridges institutional theory and non-market strategy by illustrating how information infrastructure—particularly local media—functions as an informal governance mechanism constraining opportunistic disclosure.

From a policy perspective, the findings highlight the importance of independent local journalism as a non-regulatory enforcement mechanism. As ESG reporting increasingly relies on firms' voluntary disclosure, the disappearance of local news outlets may create blind spots in the informational environment, weakening external monitoring and enabling symbolic compliance. Revitalizing local media ecosystems may thus serve as a complementary governance tool to improve the integrity of ESG reporting.

Several limitations should be acknowledged. Although the identification strategy is robust, unobservable confounders or time-varying omitted variables cannot be entirely ruled out. The measure of greenwashing, while theoretically grounded, is constructed from available structured data and may not capture all dimensions of misrepresentation. Moreover, the study focuses on U.S. public firms; generalizability to private firms or other institutional contexts remains an open question.

Future research could explore how alternative media channels—such as digital platforms or social media—substitute for traditional newspapers in shaping corporate behavior. Scholars may also investigate how managerial traits (e.g., CEO reputation or background) influence firms' disclosure strategies in low-oversight environments. Extending this analysis to other CSR domains, such as labor rights or community relations, may reveal whether similar greenwashing mechanisms operate beyond the environmental dimension. Finally, incorporating textual analysis could offer deeper insights into the linguistic patterns and strategic framing of symbolic ESG disclosures.

In sum, this dissertation provides empirical evidence that local newspaper closures causally increase corporate greenwashing behavior. It underscores the institutional value of local journalism in promoting environmental transparency and highlights the broader societal consequences of media decline in the accountability landscape.

References

Agirdas, C. (2015). How do corporate political connections impact financial reporting? evidence from the newspaper industry. Working Paper.

Arouri, M., El Ghoul, S., and Gomes, M. (2021). Greenwashing and product market competition. *Finance Research Letters*, 42:101927.

Barnett, M. L. (2007). Stakeholder influence capacity and the variability of financial returns to corporate social responsibility. *Academy of Management Review*.

Berrone, P., Fosfuri, A., and Gelabert, L. (2017). Does greenwashing pay off? understanding the relationship between environmental actions and environmental legitimacy. *Journal of Business Ethics*, 144(2):363–379.

Bowen, F. (2010). *After Greenwashing: Symbolic Corporate Environmentalism and Society*. Cambridge University Press.

Campa, P. (2018). Press and leaks: Do newspapers reduce toxic emissions? *Journal of Environmental Economics and Management*, 91:184–202.

Chapple, W. and Moon, J. (2005). Corporate social responsibility (csr) in asia: A seven-country study of csr web site reporting. *Business and Society*, 44(4):415–441.

Chen, X., Li, W., and Xu, F. (2024). Media and environmental accountability: Evidence from local newspaper closures. Working Paper.

Cho, C. H., Freedman, M., and Patten, D. M. (2012). Corporate disclosure of environmental capital expenditures: A test of alternative theories. *Accounting, Auditing and Accountability Journal*, 25(3):486–507.

Cho, C. H. and Patten, D. M. (2007). The role of environmental disclosures as tools of legitimacy: A research note. *Accounting, Organizations and Society*, 32(7-8):639–647.

Circular Online (2020). Sainsbury's and tesco 'misleading customers' over recycling. `https://www.circularonline.co.uk/news/sainsburys-and-tesco-misleading-customers-over-recycling/`. Accessed May 29, 2025.

Darr, J. P., Hitt, M. P., and Dunaway, J. (2018). Newspaper closures polarize voting behavior. *Journal of Communication*, 68(6):1007–1028.

de Freitas Netto, S. V., Sobral, M. F. F., Ribeiro, A. R. B., and Soares, G. R. d. L. (2020). Concepts and forms of greenwashing: A systematic review. *Environmental Sciences Europe*, 32(1):1–12.

De Soto, H. (1989). *The Other Path: The Invisible Revolution in the Third World*. Harper and Row. Originally published in Spanish as "El otro sendero".

Del Giudice, A. and Rigamonti, S. (2020). Does audit improve the quality of esg scores? evidence from corporate misconduct. *Sustainability (Switzerland)*, 12(14).

Delmas, M. A. and Burbano, V. C. (2011). The drivers of greenwashing. *California Management Review*, 54(1):64–87.

Dowling, G. R. (1993). Developing your company image into a corporate asset. *Long Range Planning*, 26(2):101–109.

Dowling, J. and Pfeffer, J. (1975). Organizational legitimacy: Social values and organizational behavior. *Pacific Sociological Review*, 18(1):122–136.

Dyck, A., Morse, A., and Zingales, L. (2010). Who blows the whistle on corporate fraud? *Journal of Finance*, 65(6):2213–2253.

Dyck, A., Volchkova, N., and Zingales, L. (2008). The corporate governance role of the media: Evidence from russia. *Journal of Finance*, 63(3):1093–1135.

Dyck, I. J. A. and Zingales, L. (2005). The corporate governance role of the media. *SSRN Electronic Journal*.

Eccles, R. G., Serafeim, G., and Krzus, M. P. (2014). Market interest in nonfinancial information. *Journal of Applied Corporate Finance*, 26(3):48–56.

Engelberg, J. and Parsons, C. A. (2011). The causal impact of media in financial markets. *The Journal of Finance*, 66(1):67–97.

Friedman, M. (1970). A friedman doctrine—the social responsibility of business is to increase its profits. *New York Times Magazine*, 6:33, 122–124. Retrieved from `http://www.nytimes.com/`.

Gao, H., Lee, G., and Murphy, D. (2020). Financing dies in darkness? the impact of newspaper closures on public finance. *Journal of Financial Economics*, 135(2):445–467.

García-Sánchez, I. M. and Martínez-Ferrero, J. (2018). How do independent directors behave with respect to sustainability disclosure? *Corporate Social Responsibility and Environmental Management*, 25(4):609–627.

Gentzkow, M., Shapiro, J. M., and Sinkinson, M. (2011). The effect of newspaper entry and exit on electoral politics. *American Economic Review*, 101(7):2980–3018.

Gillan, S. L., Hartzell, J. C., Koch, A., and Starks, L. T. (2015). Firms' environmental, social and governance (esg) choices, performance and managerial motivation. *Journal of Business Ethics*, 132(4):641–660.

Grant, J. H. and Gnyawali, D. R. (1996). Strategic process improvement through organizational learning. *Strategy & Leadership*, 24(2):4–10.

Gurun, U. G. and Butler, A. W. (2012). Don't believe the hype: Local media slant, local advertising, and firm value. *Journal of Finance*, 67(2):561–598.

Habek, P. and Wolniak, R. (2015). Factors influencing the development of csr reporting practices: Experts' versus preparers' points of view. *Engineering Economics*, 26(5):560–570.

Hawn, O. and Ioannou, I. (2016). Mind the gap: The interplay between external and internal actions in the case of corporate social responsibility. *Strategic Management Journal*, 37(13):2569–2588.

He, L., Wu, C., Yang, X., and Liu, J. (2019). Corporate social responsibility, green credit, and corporate performance: an empirical analysis based on the mining, power, and steel industries of china. *Natural Hazards*, 95:73–89.

Heese, J., Pérez-Cavazos, G., and Peter, C. D. (2022). When the local newspaper leaves town: The effects of local newspaper closures on corporate misconduct. *Journal of Financial Economics*, 145(2):445–463.

Herbohn, K., Walker, J., and Loo, H. Y. M. (2014). Corporate social responsibility: The link between sustainability disclosure and sustainability performance. *Abacus*, 50(4):422–459.

Husted, B. W. (2005). Risk management, real options, corporate social responsibility. *Journal of Business Ethics*, 60(2):175–183.

Innes, R. and Sam, A. G. (2008). Voluntary pollution reductions and the enforcement of environmental law: An empirical study of the 33/50 program. *Journal of Law and Economics*, 51(2):271–296.

International Consumer Protection and Enforcement Network (2020). Greenwashing: Half

of green claims lack evidence. https://icpen.org/news/1146. Accessed May 02, 2025.
Ioannou, I. and Serafeim, G. (2012). What drives corporate social performance? the role of nation-level institutions. *Journal of International Business Studies*, 43(9):834–864.
Jiang, J. X. and Kong, J. (2023). Green dies in darkness? environmental externalities of newspaper closures. *Review of Accounting Studies*.
Jiang, Y., Oron, T. R., Clark, W. T., Bankapur, A. R., D'Andrea, D., Lepore, R., and Radivojac, P. (2016). An expanded evaluation of protein function prediction methods shows an improvement in accuracy. *Genome Biology*, 17(1):184.
Kim, E. H. and Lyon, T. P. (2015). Greenwash vs. brownwash: Exaggeration and undue modesty in corporate sustainability disclosure. *Organization Science*, 26(3):705–723.
Kim, M., Stice, D., Stice, H., and White, R. M. (2021). Stop the presses! or wait, we might need them: Firm responses to local newspaper closures and layoffs. *Journal of Corporate Finance*, 69:102035.
King, A. A. and Lenox, M. J. (2001). Does it really pay to be green? an empirical study of firm environmental and financial performance. *Journal of Industrial Ecology*, 5(1):105–116.
Li, W., Li, W., Seppänen, V., and Koivumäki, T. (2022). Effects of greenwashing on financial performance: Moderation through local environmental regulation and media coverage. *Business Strategy and the Environment*.
Liu, Z. (2023). The disciplining effect of the press: Evidence from environmental misconduct. Working Paper.
Lyon, T. P. and Maxwell, J. W. (2011). Greenwash: Corporate environmental disclosure under threat of audit. *Journal of Economics and Management Strategy*, 20(1):3–41.
Ma, Z., Stice, D., Stice, E., and Zhang, Y. (2022). Local newspaper closures and bank loan contracts. *SSRN Electronic Journal*.
MacLean, T. L. and Behnam, M. (2010). The dangers of decoupling: The relationship between compliance programs, legitimacy perceptions, and institutionalized misconduct. *Academy of Management Journal*, 53(6):1499–1520.
Marquis, C., Toffel, M. W., and Zhou, Y. (2016). Scrutiny, norms, and selective disclosure: A global study of greenwashing. *Organization Science*, 27(2):483–504.
Mateo-Márquez, A. J., González-González, J. M., and Zamora-Ramírez, C. (2022). An international empirical study of greenwashing and voluntary carbon disclosure. *Journal of Cleaner Production*, 363:132567.
Meyer, J. W. and Rowan, B. (1977). Institutionalized organizations: Formal structure as myth and ceremony. *American Journal of Sociology*, 83(2):340–363.
Miller, G. S. (2006). The press as a watchdog for accounting fraud. *Journal of Accounting Research*, 44(5):1001–1033.
Miller, G. S. and Shanthikumar, D. M. (2011). Geographic location, media coverage and investor reactions. *SSRN Electronic Journal*.
Mullainathan, S. and Shleifer, A. (2005). The market for news. *American Economic Review*, 95(4):1031–1053.
Ntim, C. G. and Soobaroyen, T. (2013). Black economic empowerment disclosures by south african listed corporations: The influence of ownership and board characteristics. *Journal of Business Ethics*, 116(1):121–138.
Parguel, B., Benoît-Moreau, F., and Russell, C. A. (2015). Can evoking nature in advertising mislead consumers? the power of 'executional greenwashing'. *International Journal of*

Advertising, 34(1):107–134.

Peress, J. (2014). The media and the diffusion of information in financial markets: Evidence from newspaper strikes. *The Journal of Finance*, 69(5):2007–2043.

Petitjean, M. (2019). Eco-friendly policies and financial performance: Was the financial crisis a game changer for large us companies? *Energy Economics*, 80:502–511.

Reitz, H. J., Pfeffer, J., and Salancik, G. R. (1979). The external control of organizations: A resource dependence perspective. *The Academy of Management Review*, 4(2):309.

Roberts, M. R. and Whited, T. M. (2013). Endogeneity in empirical corporate finance. In *Handbook of the Economics of Finance*, volume 2, pages 493–572. Elsevier B.V.

Romero, S., Ruiz, S., and Fernandez-Feijoo, B. (2019). Sustainability reporting and stakeholder engagement in spain: Different instruments, different quality. *Business Strategy and the Environment*, 28(1):221–232.

Schiffrin, A. (2014). *Global Muckraking: 100 Years of Investigative Journalism from Around the World*. The New Press, New York.

Shapira, R. and Zingales, L. (2017). Is pollution value-maximizing? the dupont case. *SSRN Electronic Journal*.

Singh, J. V. (1986). Performance, slack, and risk taking in organizational decision making. *Academy of Management Journal*, 29(3):562–585.

Snyder, J. M. and Strömberg, D. (2010). Press coverage and political accountability. *Journal of Political Economy*, 118(2):355–408.

Spack, J. A., Board, V. E., Crighton, L. M., Kostka, P. M., and Ivory, J. D. (2012). It's easy being green: The effects of argument and imagery on consumer responses to green product packaging. *Environmental Communication*, 6(4):441–458.

Speculations, G. (2015). Volkswagen emissions scandal: What we can learn from history. `https://www.forbes.com/sites/greatspeculations/2015/10/06/volkswagen-emissions-scandal-what-we-can-learn-from-history/`. Accessed May 02, 2025.

Tian, L., Song, X., Du, M., and Xu, B. (2025). The disciplinary impact of capital market internationalization on corporate esg greenwashing: A study of a-shares' inclusion in the msci index. *International Review of Financial Analysis*, 103:104202.

Torelli, R., Balluchi, F., and Lazzini, A. (2020). Greenwashing and environmental communication: Effects on stakeholders' perceptions. *Business Strategy and the Environment*, 29(2):407–421.

United Nations (2015). Transforming our world: the 2030 agenda for sustainable development. `https://sdgs.un.org/goals`. Accessed: 2025-05-02.

United Nations Framework Convention on Climate Change (2015). Paris agreement. `https://unfccc.int/process-and-meetings/the-paris-agreement/the-paris-agreement`. Adopted at COP21 in Paris on 12 December 2015.

Yang, Z., Fan, M., Shao, S., and Yang, L. (2017). Does carbon intensity constraint policy improve industrial green production performance in china? a quasi-did analysis. *Energy Economics*, 68:271–282.

Yu, E. P., Luu, B. V., Chen, C. H., and Segot, T. L. (2020). Greenwashing in environmental, social and governance disclosures. *Research in International Business and Finance*, 52:101192.

Zhang, D. (2021). Green credit regulation, induced r&d and green productivity: Revisiting

the porter hypothesis. *International Review of Financial Analysis*, 75:101723.
Zhang, D. (2023). Does green finance really inhibit extreme hypocritical esg risk? a greenwashing perspective exploration. *Energy Economics*, 121:106688.

Tables

Table 1: Sample Composition

This table summarizes the construction of the final analysis sample. Industry is defined at the 3-digit SIC level. The final sample consists of firm-year observations with available greenwashing scores, matched county-level data, and newspaper closure information.

Sample selection steps	**Observations**	**Firms**	**Industries**
All plant-year observations from Compustat for U.S. firms with non-missing tickers and total assets (1999–2024)	221,117	23,108	–
Matched with S&P Capital IQ U.S. firm data, ticker and county non-missing (2002–2022)	74,486	6,743	408
Merged with newspaper closure data from Jiang and Kong(2023) (2000-2018)	74,486	6,743	408
Merged with county-level population data from the U.S. Bureau of Economic Analysis (BEA) (1969-2022)	74,486	6,743	408
Merged with greenwashing scores from ASSET4, excluding financial firms(2002–2017)	**12,016**	**1,924**	**334**

Table 2: Yearly Distribution of Treated and Control Firms

This table shows the yearly distribution of firm-year observations by treatment status. Column "0" indicates firms located in counties without a newspaper closure in that year, while column "1" indicates those in counties where a newspaper closure occurred.

Year	No Closure (0)	Closure (1)	Total
2002	32	26	58
2003	50	33	83
2004	90	63	153
2005	122	74	196
2006	137	83	220
2007	169	106	275
2008	241	136	377
2009	297	162	459
2010	316	174	490
2011	319	178	497
2012	316	179	495
2013	320	184	504
2014	316	192	508
2015	501	278	779
2016	707	372	1,079
2017	675	359	1,034
Total	**4,608**	**2,599**	**7,207**

Table 3: Summary statistics firms.

This table reports the summary statistics on an annual basis of the variables used in our analyses. All variables are defined in the Appendix.

Firm-Years Sample

($N = 12,016$)

Variable	Mean	Std.	Min.	P25	Median	Max.
FirmSize	2.049	1.837	-6.908	0.998	2.175	7.741
ROA	0.121	0.135	-3.046	0.067	0.121	1.289
FixedAsset	0.240	0.242	0.000	0.045	0.148	0.975
RD	0.024	0.069	0.000	0.000	0.000	2.163
DebtRatio	0.259	0.218	0.000	0.100	0.227	3.769
ScaledCapitalExpenditure	0.039	0.047	-0.033	0.008	0.026	0.733
Risk	1.199	4.959	-2.914	0.283	0.640	312.111

Table 4: Effect of Newspaper Closures on Greenwashing

Stepwise Inclusion of Controls (Firm vs. Firm + County Level)		
	Dependent variable: *GW2_std*	
VARIABLES	(1) Firm-Level Controls	(2) Firm + County-Level Controls
Treatment	0.121**	0.102*
	(0.045)	(0.048)
FirmSize	0.100**	0.090*
	(0.046)	(0.048)
ROA	−0.095	−0.104
	(0.178)	(0.182)
FixedAsset	−0.601*	−0.569*
	(0.302)	(0.284)
RD	−1.178*	−1.349**
	(0.517)	(0.477)
DebtRatio	0.174	0.150
	(0.142)	(0.148)
ScaledCapitalExpenditure	0.110	0.076
	(0.725)	(0.693)
Risk	0.012	0.009
	(0.010)	(0.010)
LnIncomePercapita		0.266
		(0.269)
LnCountyPopulation		−0.533
		(1.016)
PopulationChange		7.301**
		(2.712)
Constant	−2.516***	1.933
	(0.140)	(13.670)
Fixed Effects	Firm, State-Year, Industry-Year	
Observations	7,207	6,950
Adj. R-squared	0.671	0.675

Robust standard errors clustered at the county by year level .

*** p¡0.01, ** p¡0.05, * p¡0.1

All control variables are winsorized at the 1st and 99th percentiles.

$$y_{ikt} = \alpha_0 + \alpha_1 \text{Treatment}_{kt} + \alpha_i + \alpha_{st} + \alpha_{it} + \text{Controls} + \epsilon_{ikt} \tag{3}$$

This table presents the estimates of the baseline model. The dependent variable is the standardized greenwashing ratio (GW2_std). The variable of interest is Treatment, a binary variable equal to 1 if the firm is located in a county that experienced a local newspaper closure; equals 0 otherwise. All control variables are included in the model and are defined in Appendix 1. All control variables are winsorized at the 1st and 99th percentiles. Fixed effects for SP_ENTITY_ID, SP_STATE × Year, and SP_SIC_CODE × Year are accounted for. Robust standard errors clustered at the county-by-year level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 5: Placebo Test: Pre-treatment Falsification Using Artificial Closure Timing

This table reports a placebo test, which examines whether there is a difference in greenwashing between treated and control firms in the absence of actual newspaper closures. We create pseudo closure events by artificially shifting the closure year three years earlier and re-estimate the baseline specification using the same design. Variable definitions are provided in Appendix A. Robust standard errors clustered at the county-by-year level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable: *GW2_std*	
VARIABLES	(1) Placebo Closure (–3 years)
placebo_Treatment3	-0.013
	(0.073)
FirmSize	0.100^{**}
	(0.046)
ROA	-0.084
	(0.181)
FixedAsset	-0.580^{*}
	(0.302)
RD	-1.084^{**}
	(0.508)
DebtRatio	0.168
	(0.144)
ScaledCapitalExpenditure	0.078
	(0.736)
Risk	0.013
	(0.009)
Constant	-2.489^{***}
	(0.145)
Fixed Effects	Firm, State-Year, Industry-Year
Observations	7,207
Adj. R-squared	0.670

Robust standard errors clustered at the county-by-year level.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

All control variables are winsorized at the 1st and 99th percentiles.

Table 6: Dynamic Effects of Newspaper Closures on Greenwashing

This table presents the dynamic treatment effects estimated using an event-study specification around the year of local newspaper closure. The omitted category is the year immediately prior to the closure ($t = -1$). We use a symmetric ten-year window covering five years before and five years after the event ($t = -5$ to $t = +5$). The results suggest that while no significant differences are observed in the pre-treatment period, greenwashing begins to increase in the post-treatment years, with statistically significant effects emerging in year $t = +3$ and persisting through $t = +5$. Variable definitions are provided in Appendix A. Robust standard errors clustered at the county-by-year level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable: *GW2_std*	
VARIABLES	(1) Dynamic Specification
Dm5	0.115
	(0.090)
Dm4	−0.035
	(0.108)
Dm3	−0.073
	(0.085)
Dm2	−0.063
	(0.077)
D0	−0.027
	(0.083)
Dp1	0.067
	(0.067)
Dp2	0.135
	(0.080)
Dp3	0.204**
	(0.093)
Dp4	0.201**
	(0.095)
Dp5	0.191*
	(0.099)
Fixed Effects	Firm, State-Year, Industry-Year
Controls	Yes
Observations	5,336
Adj. R-squared	0.688

Robust standard errors clustered at the county-by-year level are reported in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
All control variables are winsorized at the 1st and 99th percentiles.

Table 7: Varying the Length of the Event Window

This table presents robustness tests by restricting the sample to alternative event windows centered around the year of newspaper closure. The main specification uses the full sample from 2002 to 2017, while columns (1) to (3) limit the treated group to observations within ±3, ±5, and ±7 years of the closure year, respectively. All regressions include the same set of control variables and absorb Firm, State-Year, and Industry-Year fixed effects. Robust standard errors are clustered at the county-by-year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

VARIABLES	(1) $[-3, -1], [1, 3]$	(2) $[-5, -1], [1, 5]$	(3) $[-7, -1], [1, 7]$
Dependent variable: *GW2_std*			
Treatment	0.124*	0.154**	0.146**
	(0.059)	(0.066)	(0.065)
FirmSize	0.024	0.054	0.050
	(0.059)	(0.059)	(0.052)
ROA	-0.090	-0.094	-0.095
	(0.238)	(0.243)	(0.230)
FixedAsset	-0.693*	-0.770**	-0.714*
	(0.332)	(0.346)	(0.356)
RD	-1.665**	-1.441**	-1.393**
	(0.583)	(0.586)	(0.627)
DebtRatio	0.129	0.154	0.168
	(0.191)	(0.184)	(0.178)
ScaledCapEx	0.388	0.200	0.059
	(0.734)	(0.703)	(0.696)
Risk	0.010	0.010	0.012
	(0.014)	(0.013)	(0.010)
Constant	-2.304***	-2.366***	-2.369***
	(0.189)	(0.185)	(0.170)
Fixed Effects	Firm, State-Year, Industry-Year		
Observations	5 144	5 654	6 061
Adj. R-squared	0.681	0.674	0.664

Table 8: Alternative Fixed Effects Specifications

This table reports robustness checks using alternative fixed effects structures. The dependent variable is the standardized greenwashing score (*GW2_std*). Column (1) includes firm and year fixed effects; column (2) includes firm, state, and industry fixed effects. The coefficient on *Treatment* remains positive and significant across both specifications, confirming the robustness of the main result.

VARIABLES	(1) Firm + Year	(2) Firm + State + Industry
Dependent variable: *GW2_std*		
Treatment	0.025	0.098^{*}
	(0.048)	(0.051)
FirmSize	0.099^{**}	0.121^{***}
	(0.036)	(0.039)
ROA	−0.078	−0.189
	(0.152)	(0.155)
FixedAsset	−0.165	−0.054
	(0.214)	(0.286)
RD	−0.473	−0.553
	(0.444)	(0.540)
DebtRatio	0.075	0.300^{**}
	(0.106)	(0.128)
ScaledCapitalExpenditure	0.056	−0.446
	(0.470)	(0.499)
Risk	0.009	0.011
	(0.011)	(0.011)
Constant	-2.591^{***}	-2.705^{***}
	(0.097)	(0.097)
Fixed Effects	Firm, Year	Firm, State, Industry
Observations	8,824	8,818
Adjusted R-squared	0.589	0.549

Robust standard errors clustered at the county-by-year level.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

All control variables are winsorized at the 1st and 99th percentiles.

Table 9: Alternative Definitions of Treatment

This table examines whether the estimated effect of newspaper closures on greenwashing is robust to alternative treatment definitions. Each column defines the treatment period differently, varying in the inclusion of the closure year and the length of the post-closure window. The dependent variable is the standardized greenwashing score (*GW2_std*). All regressions include firm, state–year, and industry–year fixed effects, and the same set of control variables. Robust standard errors are clustered at the county-by-year level.

VARIABLES	(1) *GW2_std*	(2) *GW2_std*	(3) *GW2_std*	(4) *GW2_std*	(5) *GW2_std*
Treatment Definition					
$year \geq ClosureYear, \leq +3$	X				
$year > ClosureYear, \leq +3$		X			
$year \geq ClosureYear, \leq +5$			X		
$year > ClosureYear, \leq +5$				X	
$year > ClosureYear, \leq +10$					X
Treatment Coef.	−0.039	0.002	−0.017	0.022	0.076*
Std. Error	(0.037)	(0.030)	(0.040)	(0.030)	(0.041)
Fixed Effects	Firm, State–Year, Industry–Year				
Observations	7,207				
Adj. R-squared	0.670	0.670	0.670	0.670	0.671

Robust standard errors clustered at the county-by-year level.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

All control variables are winsorized at the 1st and 99th percentiles.

Table 10: Alternative Measures of Greenwashing

This table compares results across three greenwashing measures. GW2 is the benchmark definition (same-year symbolic minus substantial disclosures); GW3 incorporates a one-year lag for substantial items; GW1 follows Marcus et al. (2016) and is based on the Trucost dataset. All regressions include firm, state–year, and industry–year fixed effects. Robust standard errors are clustered at the county-by-year level.

VARIABLES	(1) GW2_std	(2) GW3_std	(3) GW1
Treatment	0.121**	0.112*	5.924**
	(0.045)	(0.056)	(2.440)
FirmSize	0.100**	0.144**	1.152
	(0.046)	(0.054)	(2.378)
ROA	-0.095	-0.100	3.342
	(0.178)	(0.194)	(9.196)
FixedAsset	-0.601*	-0.340	10.033
	(0.302)	(0.355)	(13.922)
RD	-1.178**	-0.497	79.264**
	(0.517)	(0.793)	(31.057)
DebtRatio	0.174	0.226	8.733
	(0.142)	(0.186)	(6.978)
ScaledCapitalExpenditure	0.110	0.408	45.829
	(0.725)	(0.813)	(36.222)
Risk	0.012	0.035*	0.878
	(0.010)	(0.019)	(0.968)
Constant	-2.516***	-2.597***	-26.099**
	(0.140)	(0.166)	(9.071)
Fixed Effects	Firm, State–Year, Industry–Year		
Observations	7,207	5,821	4,262
Adj. R-squared	0.671	0.591	0.662

Robust standard errors clustered at the county-by-year level.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

All control variables are winsorized at the 1st and 99th percentiles.

Table 11: Heterogeneity by Firm Size

This table examines whether the impact of local newspaper closures on greenwashing behavior differs by firm size. Column (1) reports the baseline specification. Column (2) adds an interaction term between the treatment indicator and a large-firm dummy (equal to one if firm employment is above the median). The significant positive interaction coefficient suggests that larger firms respond more strongly to local newspaper closure.

VARIABLES	(1) GW2_std	(2) GW2_std
Treatment	0.121**	-0.032
	(0.045)	(0.067)
Treatment $\times$ Large Firm		0.322**
		(0.130)
FirmSize_above		-0.081
		(0.063)
Controls	Yes	Yes
Constant	-2.516***	-2.496***
	(0.140)	(0.141)
Fixed Effects	Firm, State–Year, Industry–Year	
Observations	7,207	7,207
Adj. R-squared	0.671	0.674

Robust standard errors clustered at the county-by-year level.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

All control variables are winsorized at the 1st and 99th percentiles.

"Large Firm" is defined as firms with above-median employment.

Table 12: Heterogeneity by Firm Risk Level

This table investigates whether the impact of local newspaper closures on greenwashing behavior varies by firms' financial risk levels. Column (1) reports the baseline results. Column (2) introduces an interaction between the treatment indicator and a high-risk dummy (equal to one if the firm has above-median risk, measured as the inverse of the solvency ratio). The significant negative coefficient on the interaction term suggests that high-risk firms reduce their greenwashing behavior following the loss of local newspaper coverage, potentially due to resource constraints or heightened regulatory and financial pressures.

VARIABLES	(1) GW2_std	(2) GW2_std
Treatment	0.121**	0.214***
	(0.045)	(0.071)
Treatment × High Risk		-0.162**
		(0.070)
High Risk		0.021
		(0.043)
Controls	Yes	Yes
Constant	-2.516***	-2.523***
	(0.140)	(0.137)
Fixed Effects	Firm, State–Year, Industry–Year	
Observations	7,207	7,207
Adj. R-squared	0.671	0.672

Robust standard errors clustered at the county-by-year level.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

All control variables are winsorized at the 1st and 99th percentiles.

High Risk is defined as firms above the median of the inverse solvency ratio.

Table 13: Heterogeneity by Corporate Reputation

This table investigates whether the effect of local newspaper closures on corporate greenwashing behavior varies by firms' reputational standing. Column (1) replicates the baseline specification. Column (2) includes an interaction term between the treatment indicator and a binary variable for high-reputation firms, which equals one if the firm appears in *Fortune's 100 Best Companies to Work For in America* in that year. The positive and statistically significant interaction term suggests that high-reputation firms exhibit a stronger greenwashing response following newspaper closures.

VARIABLES	(1) GW2_std	(2) GW2_std
Treatment	0.121**	0.109**
	(0.045)	(0.043)
Treatment × High Reputation		0.279**
		(0.117)
Reputation		-0.104
		(0.081)
Controls	Yes	Yes
Constant	-2.516***	-2.562***
	(0.140)	(0.132)
Fixed Effects	Firm, State–Year, Industry–Year	
Observations	7,207	7,066
Adj. R-squared	0.671	0.682

Robust standard errors clustered at the county-by-year level.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

All control variables are winsorized at the 1st and 99th percentiles.

High reputation defined by *Fortune's 100 Best Companies to Work For in America.*

Table 14: Heterogeneity by County-Level Economic Conditions

This table examines whether the impact of local newspaper closures on corporate greenwashing behavior varies with local economic characteristics. Column (1) reports the baseline model. Column (2) includes an interaction term between the treatment indicator and a high-income county dummy (equal to one if income per capita is above the sample median). The results show no significant heterogeneity across income levels, suggesting that the main effect does not differ between richer and poorer counties. All regressions include the same set of firm-level controls and fixed effects.

VARIABLES	(1) GW2_std	(2) GW2_std
Treatment	0.102*	0.040
	(0.048)	(0.056)
HighIncome		-0.002
		(0.050)
Treatment × HighIncome		0.091
		(0.066)
FirmSize	0.090*	0.091*
	(0.048)	(0.048)
ROA3	-0.104	-0.106
	(0.182)	(0.183)
FixedAsset	-0.569*	-0.562*
	(0.284)	(0.287)
RD	-1.349**	-1.359**
	(0.477)	(0.481)
DebtRatio	0.150	0.149
	(0.148)	(0.148)
ScaledCapitalExpenditure	0.076	0.048
	(0.693)	(0.701)
Risk	0.009	0.009
	(0.010)	(0.010)
LnIncomePercapita	0.266	0.256
	(0.269)	(0.271)
LnCountyPopulation	-0.533	-0.611
	(1.016)	(1.027)
PopulationChange	7.301**	7.268**
	(2.712)	(2.701)
Constant	1.933	3.136
	(13.670)	(13.843)
Fixed Effects	Firm, State–Year, Industry–Year	
Observations	6,950	6,950
Adj. R-squared	0.675	0.675

Robust standard errors clustered at the county-by-year level.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

"HighIncome" is defined as counties with income per capita above the sample median.

Table 15: Heterogeneity by County Population Growth

This table investigates whether the effect of local newspaper closures on greenwashing behavior varies across counties with different population growth rates. Column (1) reports the baseline regression with county-level population growth as a control variable. Column (2) includes an interaction between the treatment variable and a dummy for counties with above-median population growth. While population growth itself is positively associated with greenwashing, the interaction term is statistically insignificant, suggesting that the marginal impact of newspaper closures is not stronger in faster-growing regions.

VARIABLES	(1) GW2_std	(2) GW2_std
Treatment	0.102*	0.121**
	(0.048)	(0.056)
Treatment × High Population Growth		-0.034
		(0.042)
FirmSize	0.090*	0.091*
	(0.048)	(0.048)
ROA3	-0.104	-0.102
	(0.182)	(0.183)
FixedAsset	-0.569*	-0.565*
	(0.284)	(0.286)
RD	-1.349**	-1.350**
	(0.477)	(0.477)
DebtRatio	0.150	0.152
	(0.148)	(0.148)
ScaledCapitalExpenditure	0.076	0.068
	(0.693)	(0.692)
Risk	0.009	0.009
	(0.010)	(0.009)
LnIncomePercapita	0.266	0.250
	(0.269)	(0.269)
LnCountyPopulation	-0.533	-0.487
	(1.016)	(1.016)
PopulationChange	7.301**	7.808**
	(2.712)	(2.805)
Constant	1.933	1.472
	(13.670)	(13.644)
Fixed Effects	Firm, State–Year, Industry–Year	
Observations	6,950	6,950
Adj. R-squared	0.675	0.675

Robust standard errors clustered at the county-by-year level.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

All control variables are winsorized at the 1st and 99th percentiles.

"High Population Growth" is a dummy equal to one for counties above the median population growth rate.

Figures

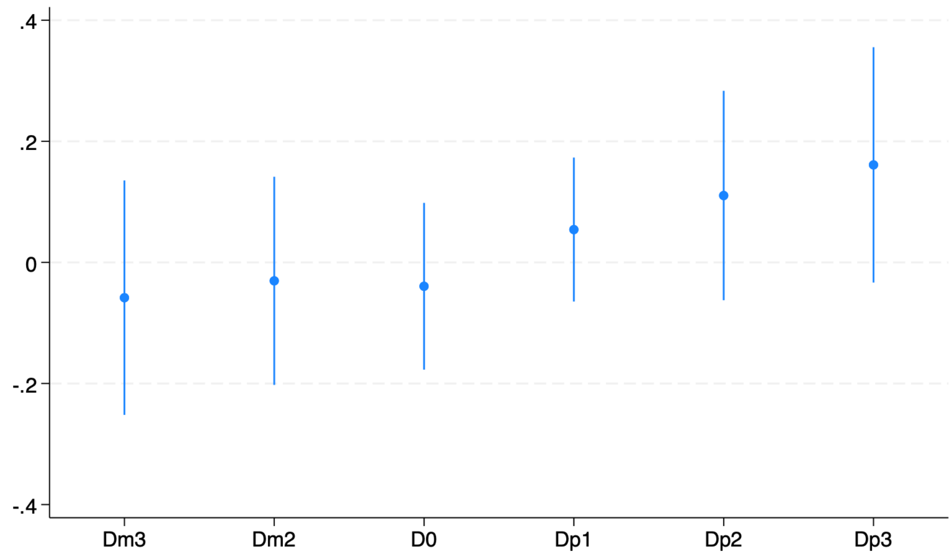

Figure 1: Parallel-Trend Validation Using Event-Study Specification

Figure 1 presents the estimated dynamic treatment effects using an event-study specification centered around the year of local newspaper closure. The omitted category is the year immediately preceding the closure ($t = -1$). The pre-treatment coefficients ($t = -3$ and $t = -2$) are small in magnitude and statistically indistinguishable from zero, providing support for the parallel trends assumption. Post-treatment coefficients increase gradually, with visible upward shifts starting from year +1 and statistical significance emerging by year +3. These findings suggest a delayed but persistent effect of newspaper closures on corporate greenwashing behavior.

Appendix A. Variable Definitions

Variable Name	Definition
Dependent Variable	
Greenwashing Ratio	The difference between symbolic and substantial CSR items, both measured at year t.
Variable of Interest	
Treatment	A binary variable equal to 1 if the firm is located in a county that experienced a local newspaper closure; equals 0 otherwise.
Firm-Level Characteristics	
Firm Size	Natural logarithm of total employees (`emp`)
ROA	Return on assets, calculated as operating income before depreciation (`oibdp`) as a fraction of average total assets (`at`) based on most recent two periods
Fixed Asset Ratio	Capital intensity, calculated as net property, plant, and equipment (`ppent`) divided by total assets (`at`)
R&D Intensity	Research and development expense(`xrd`) divided by total assets (`at`)
Debt Ratio	Total debt (`dt`) divided by total assets (`at`)
Capital Expenditures	Capital expenditures (`capx`) divided by total assets (`at`)
Risk Exposure	Inverse of solvency ratio, calculated as stockholders equity (`teq`) divided by total liabilities (`lt`)
Reputation	A binary variable equal to 1 if the firm is listed in *Fortune's 100 Best Companies to Work For in America* in a given year; 0 otherwise
County-Level Characteristics	
Ln Income Per Capita	Natural logarithm of county-level per capita income
Ln County Population	Natural logarithm of county population by year
% Population Change	Percentage change in population relative to the previous year

Appendix Table A1: Comparison of Treatment Timing Definitions

Alternative Treatment Definitions: Exclusion vs. Inclusion of Closure Year

This table compares regression results using two definitions of the treatment period. Column (1) uses the baseline definition, where *Treatment* equals one only if the observation year is strictly after the closure year ($t >$ ClosureYear). Column (2) defines *Treatment* as equal to one if the firm is located in a county that experienced a newspaper closure and the observation year is greater than or equal to the closure year ($t \geq$ ClosureYear). While both specifications include the same set of controls and fixed effects, the treatment effect is statistically significant only under the baseline definition, suggesting that greenwashing behavior adjusts with a delay rather than immediately upon closure. Robust standard errors are clustered at the county-by-year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

VARIABLES	(1) Baseline: Excludes Closure Year	(2) Treatment Includes Closure Year
Dependent variable: *GW2_std*		
Treatment	0.121**	0.067
	(0.045)	(0.057)
FirmSize	0.100**	0.101**
	(0.046)	(0.046)
ROA3	-0.095	-0.086
	(0.178)	(0.179)
FixedAsset	-0.601*	-0.597*
	(0.302)	(0.306)
RD	-1.178**	-1.152**
	(0.517)	(0.521)
DebtRatio	0.174	0.171
	(0.142)	(0.143)
ScaledCapitalExpenditure	0.110	0.096
	(0.725)	(0.734)
Risk	0.012	0.012
	(0.010)	(0.010)
Constant	-2.516***	-2.508***
	(0.140)	(0.142)
Fixed Effects	Firm, State-Year, Industry-Year	
Observations	7,207	7,207
Adj. R-squared	0.671	0.670