The effect of newspaper entry and exit on firm behavior

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Abstract: National newspapers' regional bureaus open and close over time, facilitating an examination of newspaper entry and exit into local markets. We find that after *Wall Street Journal*'s Boston bureau closure, local firms are more likely to report material misstatements, suggesting that the business press plays a monitoring role. We find that after *New York Times'* Nashville bureau opening, local firms are less likely to report immaterial misstatements, suggesting firms make cosmetic changes to placate the popular press without changing the substance of misreporting. Our collective evidence suggests that business and popular press play a distinctive role in shaping financial misreporting.

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1. Introduction

Financial reporters believe that monitoring companies to hold them accountable is the most important objective of journalism (Call, Emett, Maksymov and Sharp 2022). The national media is an effective monitor of financial misreporting (Miller 2006; Dyck, Morse, and Zingales 2010). Yet, we know relatively little about how the national media (e.g., *New York Times*, *Wall Street Journal*), which is headquartered in media markets such as New York City, monitors financial misreporting of firms dispersed across the U.S (Miller and Skinner 2015). Our study sheds initial light on one mechanism through which the national media exercises a local monitoring role. National newspapers operate regional bureaus across the U.S. that allow them to monitor firms. Approximately 50% of reporters working at large newspapers are assigned to regional bureaus outside of New York City (Ahern and Sosyura 2015). We propose that the *national* media, through its regional bureaus, can play a *local* monitoring, or watchdog, role for financial misreporting.

Regional bureaus benefit from the larger resource base of national media while maintaining a similar proximity to local information sources as local media (Shapira and Zingales 2017). In addition, national newspapers do not have the same reliance as local newspapers on local firms for advertising revenues (Gurun and Butler 2012) nor do they have as strong incentives to avoid upsetting the local reader base through critical coverage of local firms. As such, national newspapers have lower incentives to positively bias their coverage of local firms. Thus, national newspapers' regional bureaus are well-positioned to be effective monitors of local firms as evidenced by several anecdotal examples. Enron's questionable accounting practices were initially covered by a reporter from the Dallas bureau of the *Wall Street Journal* (Dyck and Zingales 2002) and reporters from the Boston bureau of the *Wall Street Journal* won a Pulitzer Prize for exposing stock option backdating by Massachusetts firms (e.g., Analog Devices) (Donelson, Kartapanis, and Yust 2021). While we know national newspapers serve an *ex-post* monitoring role in detecting corporate misconduct (Miller 2006; Dyck et al. 2010), there is little evidence on whether national newspapers can *ex-ante* deter corporate misconduct. We assess whether regional bureaus facilitate national newspapers' *ex-ante* monitoring role at the local level in deterring corporate misconduct.

We examine the impact of the 2009 closure of the *Wall Street Journal*'s Boston bureau and the 2020 opening of the *New York Times'* Nashville bureau on financial misreporting. These are the two largest national newspapers in the U.S. and managers believe these two media outlets have a significant impact on firm reputation (Flam, Sharp, Shafron, and Twedt 2024). We study whether managers decrease (increase) misreporting upon entry (exit) of the outlets in firms' local markets, in line with perceived misreporting costs varying with proximity (Kedia and Rajgopal 2011).

We first validate that regional bureau entry and exit impacts local firms' information environment. We find that the number of articles in the *Wall Street Journal* about treated firms in Massachusetts decrease after the 2009 bureau closure, relative to control firms located in other parts of the country. We find that the number of articles in the *New York Times* about treated firms in Tennessee (and adjacent states) increase after the 2020 bureau opening, relative to control firms located in other parts of the country.[1] This evidence suggests that regional bureaus impact the costs for national newspapers of supplying coverage of local firms. We also conduct falsification tests using firm-specific *New York Times* (*Wall Street Journal)* articles for the 2009 regional bureau closure (2020 regional bureau opening) event and demonstrate that national media coverage, more generally, of treated firms was not changing for reasons unrelated to our entry and exit events.

[1] For the *New York Times* Nashville bureau opening, the media outlet explicitly outlined the adjacent states that the bureau would be covering (i.e., Alabama, Arkansas, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, and Tennessee). For the *Wall Street Journal* Boston bureau closure, the media outlet did not explicitly outline the adjacent states that were covered. We take a conservative approach and only include Massachusetts. We find similar results if we include other New England states (i.e., Connecticut, Maine, New Hampshire, Rhode Island, and Vermont)

We further validate that regional bureau entry and exit impacts local information search activity (Dai, Engelberg and Gao 2011). We find that Google search volume for the "*Wall Street Journal*" in Massachusetts increases less after the 2009 bureau closure, relative to Google search volume for the "*Wall Street Journal*" in New York where the newspaper is headquartered. We find that the Google search volume for the "*New York Times*" in Tennessee (and adjacent states) increases more after the 2020 bureau closure, relative to the Google search volume for the "*New York Times*" in New York where the newspaper is headquartered.[2]This evidence suggests that regional bureaus impact the local demand for national newspapers (Miller and Shantikumar 2015).

Having validated the setting, we move on to our primary analysis by examining the impact of the 2009 closure of the *Wall Street Journal*'s Boston bureau on financial misreporting. Call et al. (2022) find that reporters believe readers are interested in misstatements. As the business press play an important watchdog role by engaging in original analysis and investigation of corporate misconduct (Miller 2006), we predict and find that financial misreporting among treated firms in Massachusetts increases after the exit of the *Wall Street Journal*, relative to control firms located in other parts of the country. We further find that our misstatement results are driven by "Big R" irregularities, which capture material financial misreporting (Hennis, Leone, and Miller 2008).

We next examine the impact of the 2020 opening of the *New York Times'* Nashville bureau on financial misreporting. The popular press plays a less impactful watchdog role as it mainly rebroadcasts information (Miller 2006).We find that financial misreporting among treated firms in Tennessee (and adjacent states) decreases after the entry of the *New York Times*, relative to control

[2] We collect the monthly internet search volume from Google Trends for the terms, "*Wall Street Journal"* and "*New York Times"*. State-level indices (SVIs) are not comparable when downloaded separately. Thus, following Kumar, Lei and Zhang (2022), we deflate the *SVI* for each state by the corresponding national *SVI* to ensure comparability in the cross-section and the time-series. This is why we benchmark focal states to New York state and not all other states.

firms located in other parts of the country. We find no evidence that our misstatement results are driven by "Big R" irregularities. Rather, we find that our misstatement results are driven by "little r" errors, which capture immaterial and benign revisions of financial reports (Hennis et al. 2008).

Managers can opportunistically use discretion to classify misstatements as "little r" errors that should be reported as "Big R" irregularities (McKenna 2013; Eaglesham 2019; Munter 2022). As errors are less costly to report than irregularities, this benefits managers (e.g., less compensation clawback), firms (e.g., muted negative market reaction), and audit firms (e.g., smaller reputation and litigation costs). To the extent that the *New York Times* plays a watchdog role, we expect our "little r" error results to be driven by material errors (Thompson 2023). We find no evidence that our "little r" errors results are driven by material errors. The evidence of reduced misstatements but not reduced "Big R" irregularities or reduced material errors is consistent with firms changing the form, but not substance, of their financial misreporting behavior to placate the popular press. Call et al. (2022) find reporters access 10-K/10-Q reports (where "little r" errors are reported) at similar frequency as 8-K reports (where "Big R" irregularities are reported). A reduction in misstatements is a visible metric that signals to reporters that financial misreporting is improved.

We conduct cross-sectional tests for our baseline evidence on the impact of the *Wall Street Journal's* Boston bureau closure and the *New York Times'* Nashville bureau opening on financial misreporting. We find evidence that both sets of results are concentrated among larger firms, consistent with visible firms being of interest to the national media (Miller 2006). We also find evidence that both sets of results are concentrated among firms with Big 4 auditors, consistent with national media's interest in larger audit firms (Ege, Wang and Xu 2025). While our setting involves regional bureaus, the media outlets are national, and these cross-sectional tests confirm that the *Wall Street Journal* and the *New York Times* direct their coverage towards larger, visible firms.

We attribute the differences in results (i.e., those relating to "Big R" irregularities versus "little r" errors) across the two events to the varying orientation of the newspapers (i.e., business vs. popular press). However, it is possible that the differences in results are driven by asymmetric effects (i.e., openings being less impactful than closings), an explanation for which prior research offers mixed evidence (Gentzkow et al. 2011; Gao, Lee, and Murphy 2020). It is also possible that the differences in results are driven by differing event years (i.e., 2009 vs. 2020). It could further be the case that financial misreporting in the Northeastern region differs from the Southeastern region in ways our research design does not account for, and these differences drive our findings. To address these possibilities, we exploit the 2008 closure of the *New York Times'* New Jersey bureaus in a falsification analysis. This closure event is proximate in calendar time and geographic region to the 2009 closure of the *Wall Street Journal's* Boston bureau. The two bureaus that were closed were in Newark and Trenton, New Jersey, only 20 and 70 miles away from the *New York Times* New York city headquarters.[3] It is thus likely that the *New York Times* could readily mitigate the loss of proximity to local information sources. We predict and find that financial misreporting among treated firms in New Jersey does not change after the 2008 exit of the *New York Times*.

The differences in results could also be driven by media outlets' orientation towards business, with the popular press being more anti-business (Cohen, Ding, Lesage, Stolowy 2017). However, this explanation would predict that given its more anti-business nature, the left-leaning outlet, *New York Times*, has a more impactful watchdog role. We observe the opposite - the more business-friendly, right-leaning outlet, *Wall Street Journal*, has a more impactful watchdog role.

[3] The *New York Times* Trenton bureau, 70 miles away from headquarters, was focused on political journalism in the state's capital city while the Newark bureau, 20 miles away from headquarters, was focused on financial journalism. The *Wall Street Journal's* Boston bureau was more than 200 miles away from headquarters in New York City. Baloria, Lo, and Shu (2025) use a 200 mile distance threshold to define proximity between media outlets and firms, finding that media coverage (of firm layoffs) decreases significantly beyond 200 miles (and linearly, even within 200 miles).

Given that national media outlets cover firms with opposing political ideologies differently (Baloria and Heese 2018; Rees and Twedt 2022; Goldman et al. 2024), we conduct cross-sectional tests for our baseline evidence based on the political ideology of firm managers. For the *Wall Street Journal* Boston bureau closure event, we find no evidence that the results are concentrated among left-leaning firms. For the *New York Times* Nashville bureau opening event, we find evidence that the results are concentrated among right-leaning firms. As the *New York Times* has a demonstrably stronger ideological slant in its news coverage than the *Wall Street Journal* (Groseclose and Milyo 2005), this evidence suggests that political discord (i.e. entry of a national newspaper with a strongly expressed opposing ideology) can shape managers' financial misreporting behavior.[4]

We conduct several robustness tests. First, the 2009 Boston bureau closure overlaps with the great recession while the 2020 Nashville bureau opening overlaps with the global pandemic. We find stronger effects in later years (e.g., 2012, 2022, 2023), mitigating concerns. Second, as misstatements take years to be uncovered and our sample period for the *New York Times* is 2017-2023, we focus on the year in which a misstatement is reported not the years in which misstatements occur. We find similar results when using years in which misstatements occur. Third, we find similar results when including only control states with existing regional bureaus. Fourth, the *New York Times* Nashville bureau's jurisdiction overlaps with pre-existing bureaus in Georgia and Louisiana. We find similar results when excluding these two states from our sample.

Our study makes several contributions to the literature. The *Wall Street Journal* and *New*

[4] Gentzkow and Shapiro (2010) categorize the *Wall Street Journal* as right-leaning and the *New York Times* as left-leaning and this forms the basis of assumptions about newspaper ideology in Goldman et al. (2024). Allsides provides a measure of media bias for several media outlets (Rees and Twedt 2022). It categorizes the *Wall Street Journal* news (opinion) section as center (right-leaning) and the *New York Times* news (opinion) section as left-leaning (left). Leung and Stumpf (2024) use machine learning to analyze 100,000 articles from these two national media outlets, noting "a discernable liberal bias in *New York Times* articles whereas the *Wall Street Journal* articles displayed more neutrality."

York Times are regarded as impactful media outlets yet little research exists on their regional bureaus. We leverage insights from the economics literature on newspaper entries and exits (Gentzkow et al. 2011) to demonstrate that regional bureau openings and closings by national media have localized effects. By highlighting the impact of national newspapers' regional bureaus, our study answers Miller and Skinner's (2015) call for research on the watchdog role of the media. This evidence is timely in light of the *Wall Street Journal*'s recent decision to move away from local economic news and to close U.S. regional bureaus, potentially exacerbating the significant political and corporate accountability issues that have emerged as a result of local newspaper closures (Gao et al. 2020; Heese et al. 2022; Kyung and Nam 2023; Wall Street Journal 2024).

We also contribute to literature that seeks to understand differences between business and popular press. Unraveling financial misreporting requires analysis of technical information by journalists. Relative to the popular press, the business press is more likely to have journalists with the requisite skillset and a reader base that values technical analysis (Miller 2006; Ahern and Sasouyra 2015). Our study suggests that firms internalize these differences by enacting substantive changes to financial misreporting in response to the business press but only cosmetic changes in response to the popular press (Core et al. 2008; Bednar 2012; Kuhnen and Niessen 2012). Thus, the effectiveness of the media's watchdog role relies not only on its resource base and independence (Besley and Prat 2006; You, Zhang, and Zhang 2018) but also on its orientation.

Finally, we contribute to the broader literature on the impact of geographical variation in scrutiny on financial misreporting. While extant research studies regulatory (Kedia and Rajgopal 2011; Weber, Xu, and Zhang 2024) and investor (Ayers, Ramalingegowda, and Yeung 2011) monitoring, Dyck et al. (2010) suggest that the media can play as important a monitoring role. We shed light on the impact of geographical variation in media monitoring on financial misreporting.

2. Institutional background and validation of the setting

Our empirical strategy is to examine changes in firm outcomes in states that experience an entry or exit of a national newspaper regional bureau relative to other firms in the same industry and year that do not. Newspaper entry and exit into a local market cause large, discrete changes in the local information environment and effects before and after the event are small compared to the effect of the event itself. Entry and exists thus provide an intuitive and powerful identification strategy (Gentzkow et al. 2011). The political economy literature finds symmetric effects while the financial economics literature finds asymmetric effects (i.e., closings have an impact, but openings do not). As noted by Gao et al. (2020), "During our sample period, the newspapers that open are fairly small compared to the newspapers that close, which had often been operating in the area for at least several decades. Thus, the asymmetric effect of newspaper openings versus closings on offering yield spreads is fairly unsurprising." We examine a regional bureau closure and opening at two media outlets that are viewed as being comparable (Kedia and Kim 2024).

National media outlets often have regional bureaus across the United States. Thompson, Olsen, and Dietrich (1987) were among the first to identify this institutional feature, noting that, over their sample period in 1983, the *Dow Jones* financial news organization had approximately 20 regional bureaus across the country. Dai, Pawarda, and Zhang (2015) similarly note that over their sample period of 2001-2012, the *Dow Jones* had approximately 8 regional bureaus across the country, consistent with an overall decline of resources dedicated to the media sector over time. Ahern and Sosyura (2015) note that, over their sample period of 2001-2011, 50 percent of reporters working for national newspapers (e.g., *Wall Street Journal* and *New York Times*) and large regional newspapers (e.g., *Boston Globe* and *Los Angelas Times*) are assigned to the New York City regional bureau, with 50 percent of reporters working in regional bureaus outside of New York.

On October 29, 2009, the managing editor of the *Wall Street Journal* issued both an internal memo and an external press release, noting, "The *Wall Street Journal* plans to close its news operation in Boston, eliminating nine positions. That there has been truly great reporting under the generalship of Gary Putka out of Boston for many, many years is not in doubt. But we remain in the midst of a profound downturn in advertising revenue and must think the unthinkable." (Wall Street Journal 2009). A *Wall Street Journal* spokesperson noted that the Boston regional bureau was a logical choice for closure in a challenging economic environment, given its geographical proximity to New York City and that the Boston market would be covered by offices in New York City after December 31, 2009, the effective date of the Boston regional bureau closure. Industry speculation indicated that the 2007 acquisition of the newspaper by News Corporation and the fact that an affiliated Dow Jones newswire regional bureau was present in Wellesley, Massachusetts played a role in the closure (New York Times 2009). At the time, the *Wall Street Journal* had 10 U.S. regional bureaus (see Figure 1). The other U.S. regional bureaus were in Atlanta (Georgia), Chicago (Illinois), Dallas (Texas), Detroit (Michigan), Los Angelas (California), Palo Alto (California), Pittsburgh (Pennsylvania), San Fransico (California), and Washington (D.C.).[5]

Our validation tests appear in Appendix A. We first validate that the 2009 closure of the *Wall Street Journal*'s Boston bureau impacts local firms' information environment. We define the pre-event period as 2006-2008, the event period as 2009, and the post-event period as 2010-2012. We remove observations from the event period as the closure was effective December 2009 and compare the pre-event period to the post-event period. We use RavenPack to capture the number of firm-specific *Wall Street Journal* articles.[6] We restrict our sample to S&P 500 firms as national

[5] This is based on a media directory as of 2007: https://www.mediacontactspro.com/list-of-all-us-newspapers/
[6] For practicality of gathering articles, Goldman et al. (2024) restrict the sample to the largest 100 firms and similar to our approach, Engelberg and Parsons (2011) restrict the sample to S&P 500 firms.

media outlets disproportionately cover larger firms (Engelberg and Parsons 2011). In column (1), we define our treatment firms as those headquartered in Massachusetts. In column (2), we define our treatment firms as those headquartered in any New England state (i.e., Connecticut, Massachusetts, Maine, New Hampshire, Rhode Island, and Vermont). In column (3), we include all New England states other than Connecticut, given its proximity to New York City, where the *Wall Street Journal* is headquartered. In Table A.1, Panel A, across all three columns, we find that the estimated coefficient on *Treat*Post* is negative and significant (at the 5 percent level). This suggests that after the 2009 Boston bureau closure, firm-specific coverage of local firms in the *Wall Street Journal* decreases, relative to control firms located in other parts of the country. In Table A.1, Panel B, we conduct the same analyses but use firm-specific *New York Times* articles. This falsification test ensures that national media coverage, more generally, of treatment firms was not changing around 2009 for reasons unrelated to our event. Across all three columns, we find that the estimated coefficient on *Treat*Post* is negative but insignificant (at the 10 percent level).

In addition to the regional bureau closure's impact on the supply of news about local firms, we also expect a decrease in demand for news among local readers (Gentzkow et al. 2011). We capture local demand using monthly Google search volume indices for the *Wall Street Journal* from Massachusetts (Dai et al. 2011). As the *Wall Street Journal* is headquartered in New York City, we benchmark changes in Massachusetts to New York state. Following Kumar et al. (2022), we deflate the *SVI* for each state by the corresponding national *SVI* to ensure comparability in the cross-section and the time-series. Figure A.1 demonstrates that, relative to 2006-2008, local demand for the *Wall Street Journal* increases in Massachusetts during 2009-2012 (the pre-post difference is 0.042). Figure A.2 demonstrates that, relative to 2006-2008, local demand for the *Wall Street Journal* increases in New York during 2009-2012 (the pre-post difference is 0.098).

This suggests that after the 2009 Boston bureau closure, local demand for the *Wall Street Journal* in Massachusetts increases by less than 50% percent of the local demand increase in New York.

On October 26, 2020, the managing editor of the *New York Times* issued an external press release, noting "Rick Rojas, our indefatigable Atlanta correspondent, is going to be opening a new bureau in Nashville, which will give us even wider breath across the South. In his initial months on the job, Rick has had dateline after dateline from Mississippi, Georgia, Louisiana, and numerous other states." (New York Times 2020). In 2022, Rick Rojas left to become bureau chief for the South, based out of Atlanta, and was replaced by Emily Cochrane, who noted, "My region of coverage includes Alabama, Arkansas, Georgia, Mississippi, Louisiana, the Carolinas and Tennessee" (New York Times 2022). Industry speculation indicated that the *New York Times* had long viewed the Southeastern United States, and Nashville specifically, as a growth area in terms of both culture and business (New York Times 2013; Nashville Business Journal 2020). At the time, the *New York Times* had 12 U.S. regional bureaus outside of New York state (see Figure 2). The other U.S. based regional bureaus were in Atlanta (Georgia), Boston (Massachusetts), Chicago (Illinois), Denver (Colorado), Houston (Texas), Los Angelas (California), Miami (Florida), New Orleans (Louisiana), San Francisco (California), Seattle (Washington) and Washington (D.C.).

We validate that the 2020 opening of the *New York Times'* Nashville bureau impacts local firms' information environment. We define the pre-event period as 2017-2019, the event period as 2020, and the post-event period as 2021-2023. We remove observations from the event period as the opening was announced in October 2020 and compare the pre-event period to the post-event period. As RavenPack's coverage of the *New York Times* stops in 2021, we hand-collect, from Factiva, the number of firm-specific *New York Times* articles. We restrict our sample to S&P 500 firms as national media outlets disproportionately cover larger firms. We define our treatment firms as those headquartered in states within the coverage area of the bureau - Alabama, Arkansas, Georgia, Mississippi, Louisiana, North Carolina, South Carolina, and Tennessee. In Table A.2, Panel A, we find that the estimated coefficient on *Treat*Post* is positive and significant (at the 5 percent level). This suggests that after the 2020 Nashville bureau opening, firm-specific coverage of local firms in the *New York Times* increases, relative to control firms located in other parts of the country. In Table A.2, Panel B, we conduct the same analyses but use firm-specific *Wall Street Journal* articles. This falsification test ensures that national media coverage, more generally, of treatment firms was not changing around 2020 for reasons unrelated to our bureau event. We find that the estimated coefficient on *Treat*Post* is positive but insignificant (at the 10 percent level).

In addition to the regional bureau opening's impact on the supply of news about local firms, we also expect an increase in demand for news among local readers. We capture local demand using monthly Google search volume indices for the *New York Times* from Tennessee. As the *New York Times* is headquartered in New York, we benchmark changes in Tennessee to New York state. Following Kumar et al. (2022), we deflate the *SVI* for each state by the corresponding national *SVI* to ensure comparability in the cross-section and the time-series. Figure A.3 demonstrates that, relative to 2017-2019, local demand for the *New York Times* increases in Tennessee during 2021-2023 (the pre-post difference is 0.182). Figure A.4 demonstrates that, relative to 2017-2019, local demand for the *New York Times* decreases in New York during 2021-2023 (the pre-post difference is -0.628). This suggests that after the 2020 Nashville bureau opening, local demand for the *New York Times* in Tennessee increases relative to the local demand increase in New York. We find similar evidence in other states affected by our bureau event.[7,8]

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[7] For brevity, we emphasize Tennessee. However, the pre-post increase in the other treatment states is also larger than in New York state: AL (0.230), AR (0.300), GA (-0.099), MS (0.175), LA (0.016), NC (0.234), and SC (0.347).
[8] We benchmark to New York and not all other states as state-level *SVI* needs to be deflated by national-level *SVI*.

3. Sample Selection and Research Design

3.1 Sample Selection

Table 1 presents our sample selection procedure. Our sample period for the *Wall Street Journal* event is from 2006 to 2012, three years before and after the bureau closure (Heese et al. 2022). In Table 1, Panel A, we start with 66,271 firm-year observations that are in Compustat from 2006 to 2012, excluding the event year 2009. We remove 25,230 firm-year observation for which we cannot obtain historical headquarter addresses using the "Augmented 10-X Header Data" provided by the Software Repository for Accounting and Finance and/or headquarter data from Jennings, Lee, and Matsumoto (2017). We remove another 2,092 firm-year observations from foreign firms. We also remove 12,075 firm-year observation with missing data for each control variable. Finally, we remove 459 firm-year observations that changed from treatment to control firms during the sample period. This results in a final sample of 26,415 firm-year observations.

Our sample period for the *New York Times* event is from 2017 to 2023, three years before and after the bureau opening. In Table 1, Panel B, we start with 71,186 firm-year observations that are in Compustat from 2017 to 2023, excluding the event year 2020. We remove 37,847 firm-year observation for which we cannot obtain historical headquarter addresses using the "Augmented 10-X Header Data" provided by the Software Repository for Accounting and Finance and/or headquarter data from Jennings et al. (2017). We remove another 2,029 firm-year observations from foreign firms. We also remove 8,948 firm-year observation with missing data for each control variable. Finally, we remove 250 firm-year observations that changed from treatment to control firms during the sample period. This results in a final sample of 22,212 firm-year observations.[9] Across the two samples, we observe relatively similar number of firm-year observations.

[9] We only have headquarters data up to 2022, so we make the assumption that headquarters do not change in 2023.

3.2 *Research Design*

We use a DID research design. We compare treatment firms that experience a regional bureau closure or opening event to control firms that do not experience a closure or opening event.

$$Misstatement\ Outcomes_{it} = b_1 Treat \times Post + \gamma' \mathrm{X}_{it} + \delta_{jt} + v_s + \varepsilon_{it}, \qquad (1)$$

where *i* denotes firm, *t* denotes fiscal year, *j* denotes industry, and s denotes state.

Misstatement Outcomes capture three outcomes. First, *Misstatement* is an indicator variable equal to one if the firm reported a restatement during the current fiscal year, and zero otherwise. Second, *Misstatement_BigR* is an indicator variable equal to one if the firm reported a restatement during the current fiscal year with an 8-K item 4.02 disclosure, and zero otherwise. Third, *Misstatement _Littler* is an indicator variable equal to one if the firm reported a restatement during the current fiscal year without an 8-K item 4.02 disclosure, and zero otherwise.

Misstatements that are material to prior period financial statements, “Big R” irregularities, must be restated through an 8-K filing within four days of discovery, warning investors not to rely on previously issued financial statements. The firm must also file amended 10-Q or 10-K forms for the affected quarters or years as a replacement for the original unreliable filings. The audit firm must also issue a revised audit opinion to disclose the misstatement and to highlight the financial statement footnote that describes the issue. “Big R” irregularities are thus viewed as consequential.

Misstatements that are immaterial to prior period financial statements, “little r” errors, do not require an 8-K filing and are disclosed in a footnote to the current 10-K or 10-Q filings. These occur when an error is discovered that was not material initially but accumulates over time to a material amount. Unlike “Big R” irregularities, “little r” errors do not require a Form 8-K filing within four days warning investors and further do not require the audit firm to amend the audit opinion. Relative to “Big R” irregularities, “little r” errors are thus viewed as less consequential.

Treat captures two different outcomes. First, *Treat_WSJ* is an indicator variable equal to one if the firm is headquartered in Massachusetts, and zero otherwise. Second, *Treat_NYT* is an indicator variable equal to one if the firm is headquartered in Alabama, Arkansas, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, or Tennessee, and zero otherwise.[10]

Post captures two different outcomes. First, *Post_WSJ* is an indicator variable equal to one if the firm's fiscal year is in or after 2010, and zero otherwise. Second, *Post_NYT* is an indicator variable equal to one if the firm's fiscal year is in or after 2021, and zero otherwise.[11]

The variable of interest is the interaction between *Treat* and *Post*, which compares financial misreporting between treatment versus control firms, before and after a closure or opening event.[12] The first difference is cross-sectional (treated vs. control) and the second time-series (pre vs. post).

We control for firm size (*Size*), leverage (*Leverage)*, book-to-market ratio (*BTM*), return on assets (*ROA*), property, plant, and equipment (*PPE*), capital expenditures (*CAPEX)*, research & development (*R&D*), cash flow from operations (*CFO*), and dividends (*Dividends)*. Lastly, we include industry-by-fiscal year as well as state fixed effects to control for time-varying industry shocks (at the two-digit SIC level) and time-invariant state characteristics (Baloria and Heese 2018).[13]Appendix B presents all variable definitions. Standard errors adjust for heteroscedasticity and are clustered by firm (Bertrand, Duflo, and Mullaninathan 2004). All continuous variables are winsorized at the 1st and 99th percentile to mitigate the potential influence of outlier observations.

[10] Shapira and Zingales (2017) find the local and national media provided little coverage of corporate misconduct by DuPont, headquartered in Delaware with operations in West Virginia. They attribute the lack of local media coverage to incentives to positively bias media coverage of local firms and the lack of national media coverage to costs of information acquisition. Neither the *Wall Street Journal* nor *New York Times* has regional bureaus adjacent to DuPont.
[11] The average lag between the end of the misstatement period and the restatement announcement is about two years.
[12] Call et al. (2022) survey financial reporters and find that 54% (49%) are very likely to use 8-K reports (10-K or 10-Q reports) in developing an article about a company. Journalists describe financial statements as "extremely valuable".
[13] We use OLS (i.e., a linear probability model) because it allows for a direct interpretation of average treatment effects and nonlinear models face potential complications when using granular fixed effects (e.g., the incidental parameters problem). In untabulated analysis, we also estimate our analyses using logistic regressions and find similar results.

4. Empirical Results: *Wall Street Journal* Regional Bureau Closure

4.1 Descriptive Statistics

Table 2 reports the full sample descriptive statistics. The *Misstatement* variable has a mean of 0.09, while the *Misstatment_BigR* variable has a mean of 0.05, suggesting that approximately 9% of firm-year observations report a misstatement, and approximately half of these misstatements are Big R irregularities, consistent with evidence in Cao, Myers, and Omer (2012). 4.8% of the firm-year observations in the sample belong to the treated group in Massachusetts. Firm-year observations after the regional bureau closure in 2009 represent 46% of the overall sample.

4.2 Main Analyses

In Table 3, we examine how the three misstatement variables (i.e., *Misstatement*, *Misstatement_BigR*, and *Misstatement_Littler*) change in response to the 2009 closure of the *Wall Street Journal*'s Boston regional bureau. In column (1), we examine *Misstatement*. We find that the estimated coefficient on *Treat*Post* is positive and significant (at the 5 percent level). In column (2), we examine *Misstatement_BigR*. We find that the estimated coefficient on *Treat*Post* is positive and significant (at the 5 percent level). In column (3), we examine *Misstatement_Littler*. We find that the estimated coefficient on *Treat*Post* is positive but insignificant (at the 10 percent level). Collectively, this evidence suggests that financial misreporting among treated firms in Massachusetts increases after the 2009 exit of the *Wall Street Journal*, relative to control firms located in other parts of the country. The misstatement results are driven by "Big R" irregularities, which represent a measure of material misreporting, rather than immaterial "little r" errors. The economic magnitudes of approximately 2% to 4% are quite comparable to magnitudes reported in prior studies that also examine misstatements over a similar sample period (Cao et al. 2012).

Control variables largely behave in line with expectations, with firm size (*Size*), leverage (*Leverage*), return on assets (*ROA*), property, plant & equipment (*PPE*), capital expenditures (*Capex*), research & development (*R&D*), and cash flow from operations (*CFO*) having significant (at the 10 percent level or better) estimated coefficients in columns (1) through (3). In the remainder of the analyses, for brevity, we examine the core misreporting proxy – *Misstatement*.

4.3 Cross-Sectional Analyses

Annual financial reports, including decisions about misstatements, reflect a joint decision-making process between audit and client firms (Thompson 2023). In Table 4, we examine whether our results are concentrated among (1) top tercile firms and (2) firms audited by Big 4 audit firms.

In Panel A, we focus on firm size (*MVE*), as captured by market value of equity, and split our sample intro terciles. While we do not expect small firms to be of interest to the national media, it is possible that regional bureaus of national media outlets provide coverage for medium size firms. It is also possible that regional bureaus of national media outlets provide coverage for larger firms.[14] We find that the estimated coefficient on *Treat*Post* is positive and significant (at the 5 percent level) only for larger firms, consistent with the national media monitoring visible firms.

In Panel B, we focus on audit firm size (*Big4*), as captured by whether or not the firm uses a Big 4 audit firm. Ege et al. (2025) conduct a descriptive analysis of media coverage of audit firms and note that the national media rarely provides coverage of non-Big 4 audit firms but can potentially impact the reputation of Big 4 audit firms in meaningful ways. We find that the estimated coefficient on *Treat*Post* is positive and significant (at the 10 percent level) only for firms with Big 4 audit firms, consistent with the national media monitoring larger audit firms.

[14] There is some ambiguity in the local newspaper closure literature with regards to whether large (Heese et al. 2022) or small (Kyung and Nam 2023) firms are more impacted by closures, further motivating the size-based tercile cuts.

4.4 Dynamic Analyses

The validity of a DID estimation depends on the parallel trends assumption: absent the bureau closure, treated firms' misreporting would have evolved in the same way as that of control firms. This assumption is untestable because we do not observe the treated firms in the absence of treatment. However, we can obtain suggestive evidence by examining pre-treatment trends. The bureau closed in December 2009.[15] In Figure 3, we employ an event-time specification where the key variables of interest are *Treat*Year [2007,2008, 2009]* with 2008 serving as the benchmark year and 2009 as the event year.[16] The estimated coefficients are not significant at conventional levels, suggesting that there is no significant difference in misstatements between treated versus control firms prior to the bureau closure. The post-treatment trends are consistent with changes to firms' financial misreporting occurring gradually in response to a shifting media environment.[17]

4.5 Robustness Analyses

In Table 5, Panel A, we extend our definition of *Treat* to include other New England states. In column (1), we define treatment firms as those headquartered in any New England state (i.e., Connecticut, Massachusetts, Maine, New Hampshire, Rhode Island, and Vermont). In column (2), we include all New England states other than Connecticut, given its geographical proximity to New York City, where the *Wall Street Journal* is headquartered.[18] Across both columns, we find that the estimated coefficient on *Treat*Post* is positive and significant (at the 5 percent level).

In Table 5, Panel B, we restrict our sample to include only firms headquartered in states

[15] *The New York Times* (2009) speculated on possible reasons for the regional bureau closure in Boston, noting, "Each of The Journal's bureaus around the country was built largely on covering the businesses based in its region, but in the last decade, many of New England's major corporations, were swallowed up by other companies based elsewhere."
[16] Our sample period is largely after the 2007 acquisition of the *Wall Street Journal* by News Corporation and before the restructuring in 2013 that separated publishing and broadcasting (Goldman et al. 2024; Kedia and Kim 2024).
[17] These are estimated with firm and fiscal year fixed effects. We find that our main results are robust to this structure.
[18] Most CT firms are headquartered within 100 miles of New York City (i.e., in Stamford, New Haven, or Hartford).

where either the *Wall Street Journal* is headquartered (i.e., New York) or to states with pre-existing regional bureaus. The media environment for firms headquartered in these states is likely more comparable to our treatment states, thereby resulting in a more appropriate control sample. We find that the estimated coefficient on *Treat*Post* is positive and significant (at the 5 percent level).

In Table 5, Panel C, rather than use the fiscal year in which the misstatement was reported, we use the fiscal years in which a misstatement occurred. For example, if a misstatement was reported in 2012, but related to fiscal years 2010, 2011, and 2012, our baseline definition would only include 2012, while this alternate definition would include all three years. In column (1), we examine *Misstatement*. We find that the estimated coefficient on *Treat*Post* is positive and significant (at the 10 percent level). In column (2), we examine *Misstatement_BigR*. We find that the estimated coefficient on *Treat*Post* is positive and significant (at the 5 percent level). In column (3), we examine *Misstatement_Littler*. We find that the estimated coefficient on *Treat*Post* is positive but insignificant (at the 10 percent level). These are in line with Table 3.

5. Empirical Results: *New York Times* Regional Bureau Opening

5.1 Descriptive Statistics

Table 6 reports the full sample descriptive statistics. The *Misstatement* variable has a mean of 0.06, while the *Misstatment_BigR* variable has a mean of 0.02, suggesting that approximately 6% of firm-year observations have a misstatement, and approximately a third of these misstatements are Big R irregularities, consistent with evidence in Thompson (2023). 6.7% of the firm-year observations in the sample belong to the treated group in Southeastern states. Firm-year observations after the regional bureau opening in 2020 represent 51% of the overall sample.

5.2 Main Analyses

In Table 7, we examine how the three misstatement variables (i.e., *Misstatement,*

Misstatement_BigR, and *Misstatement_Littler*) change in response to the 2020 opening of the *New York Times*' Nashville bureau. In column (1), we examine *Misstatement*. We find that the estimated coefficient on *Treat*Post* is negative and significant (at the 1 percent level). In column (2), we examine *Misstatement_BigR*. We find that the estimated coefficient on *Treat*Post* is positive but insignificant (at the 10 percent level). In column (3), we examine *Misstatement_Littler*. We find that the estimated coefficient on *Treat*Post* is negative and significant (at the 1 percent level). Collectively, this evidence suggests that financial misreporting among treated firms in Southeastern states decreases after the 2020 entry of the *New York Times*, relative to control firms located in other parts of the country. The misstatement results are driven by "little r" errors, which represent immaterial errors, as opposed to "Big R" irregularities, which represent material firm misreporting. The economic magnitudes of 4% are quite comparable to magnitudes reported in prior studies that also examine misstatements over a similar sample period (Thompson 2023).

Control variables largely behave in line with expectations, with firm size (*Size*), leverage (*Leverage*), book-to-market (*BTM*), property, plant & equipment (*PPE*), capital expenditures (*Capex*), research & development (*R&D*), and dividends (*Dividends*) having significant (at the 10 percent level or better) estimated coefficients in columns (1) through (3). In the remainder of the analyses, for brevity, we primarily examine the core misreporting proxy – *Misstatement*.

5.3 Material Errors Analyses

In Table 7, we observe decreases in errors, not irregularities, suggesting that firms make cosmetic changes to appease the popular press without changing the substance of their financial misreporting. This approach of decreasing errors but not irregularities can potentially be effective in signaling to the media that financial misreporting is improved. Call et al. (2022) find that reporters access 10-K/10-Q reports (where "little r" errors are reported) at similar frequency as 8-

K reports (where "Big R" irregularities are reported). As errors are less costly to report than irregularities, this approach benefits managers (e.g., less clawbacks), firms (e.g., muted negative market reaction to the disclosure), and audit firms (e.g., smaller reputation and litigation costs). The popular press is not as effective as the business press at *ex-post* detecting financial misreporting (Miller 2006), thereby rendering this "cosmetic" approach as potentially viable.

In recent years, the SEC and the business press (but not the popular press) have alleged that managers opportunistically use discretion to classify misstatements as "little r" errors that should otherwise be reported as "Big R" irregularities (McKenna 2013; Eaglesham 2019; Munter 2022). Thus, the results in Table 7 could still be consistent with a monitoring effect. To the extent that the *New York Times* Nashville bureau plays a watchdog role, we expect our "little r" error results to be driven by quantitatively or qualitatively material errors as defined by Thompson (2023). The quantitative criteria relate to 5% of the absolute value of net income while the qualitative criteria relate to the impact on loan covenants, earnings trend strings, meet or beat earnings relative to analyst expectations, change in the sign of earnings, and revenue-related issues.

In Table 8, we examine how the three variables related to the materiality of little r errors (*Misstatement_Littler_Quantitative,Misstatement_Littler_QualitativeMisstatement_Littler_Both*) change in response to the 2020 opening of the *New York Time*s Nashville bureau. Across all three columns, we find that the estimated coefficient on *Treat*Post* is negative but insignificant (at the 10 percent level). This evidence is inconsistent with the *New York Times* playing a watchdog role. That is, neither "Big R" irregularities nor material "little r" errors decrease upon bureau opening.

5.4 Cross-Sectional Analyses

Annual financial reports, including decisions about misstatements, reflect a joint decision

-making process between audit and client firms (Thompson 2023). In Table 9, we examine whether our results are concentrated among (1) top tercile firms and (2) firms audited by Big 4 audit firms.

In Panel A, we focus on firm size (*MVE*), as captured by market value of equity, and split our sample intro terciles. While we do not expect small firms to be of interest to the national media, it is possible that regional bureaus of national media outlets provide coverage for medium size firms. It is also possible that regional bureaus of national media outlets provide coverage for larger firms. We find that the estimated coefficient on *Treat*Post* is negative and significant (at the 5 percent level) only for larger firms, consistent with the national media monitoring visible firms.

In Panel B, we focus on audit firm size (*Big4*), as captured by whether or not the firm uses a Big 4 audit firm. Ege et al. (2025) conduct a descriptive analysis of media coverage of audit firms and note that the national media rarely provides coverage of non-Big 4 audit firms but can potentially impact the reputation of Big 4 audit firms in meaningful ways. We find that the estimated coefficient on *Treat*Post* is negative and significant (at the 5 percent level) only for firms with Big 4 audit firms, consistent with the national media monitoring larger audit firms.

5.5 Dynamic Analyses

The validity of a DID estimation depends on the parallel trends assumption: absent the bureau opening, treated firms' misreporting would have evolved in the same way as that of control firms. This assumption is untestable because we do not observe the treated firms in the absence of treatment. However, we can obtain suggestive evidence by examining pre-treatment trends. The bureau opened in October 2020.[19] In Figure 4, we employ an event-time specification where the key variables of interest are *Treat*Year [2018,2019,2020]* with 2019 serving as the benchmark

[19] The *Nashville Business Journal* (2020) noted the economic and cultural grounds for the opening in Nashville, nothing that the move is, "another sign of the city's growing economic and cultural importance in the Southeast."

year and 2020 as the event year.[20] The estimated coefficients are not significant at conventional levels, suggesting that there is no significant difference in misstatements between treated versus control firms prior to the bureau opening. The post-treatment trends are consistent with changes to firms' financial misreporting occurring gradually in response to a shifting media environment.[21]

5.6 Robustness Analyses

In Table 10, Panel A, we modify our definition of *Treat* to exclude Georgia and Louisiana. In 2020, The *New York Times* already had regional bureaus in Atlanta and New Orleans, potentially mitigating the impact of the Nashville bureau opening on local firms in these two states. We find that the estimated coefficient on *Treat*Post* is negative and significant (at the 5 percent level).

In Table 10, Panel B, we restrict our sample to include only firms headquartered in states where either the *New York Times* is headquartered (i.e., New York) or to states with pre-existing regional bureaus. The media environment for firms headquartered in these states is likely more comparable to our treatment states, thereby resulting in a more appropriate control sample. We find that the estimated coefficient on *Treat*Post* is negative and significant (at the 1 percent level).

In Table 10, Panel C, rather than use the fiscal year in which the misstatement was reported, we use the fiscal years in which a misstatement occurred. For example, if a misstatement was reported in 2023, but related to fiscal years 2021, 2022, and 2023, our baseline definition would only include 2023, while this alternate definition would include all three years. In column (1), we examine *Misstatement*. We find that the estimated coefficient on *Treat*Post* is negative and significant (at the 10 percent level). In column (2), we examine *Misstatement_BigR*. We find that

[20] Our sample period is after the *New York Times* expansion into local markets via home delivery in 1996-2000 (George and Waldfogel 2006). Over our sample period, it was a national newspaper, not a regional one (e.g., *Boston Globe*).
[21] These are estimated with firm and fiscal year fixed effects. We find that our main results are robust to this structure.

the estimated coefficient on *Treat*Post* is positive but insignificant (at the 10 percent level). In column (3), we examine *Misstatement_Littler*. We find that the estimated coefficient on *Treat*Post* is negative and significant (at the 5 percent level). These are in line with Table 7.

6. Empirical Results: Alternative Explanations

6.1 New York Times Regional Bureau Closure

Extant research either does not examine openings (Heese et al. 2022; Kyung and Nam 2022) or examines openings and closings that are very different from one another (Gao et al. 2020), finding asymmetric effects.[22] Our analyses examine a regional bureau closure and an opening at two media outlets that are relatively comparable (Kedia and Kim 2024). Nonetheless, there may be differences between our two events unrelated to our hypothesized channel (i.e., business vs. popular press). These differences relate to (1) sample periods (i.e., 2006-2012 vs. 2017-2023), (2) geographical region (i.e., Northeast vs. Southeast), and (3) openings being different than closings, although Gentzkow et al. (2011) examine this issue thoroughly and find no evidence of asymmetry.

To assess these possibilities, we conduct a falsification test where we try to replicate the features of the 2009 *Wall Street Journal* Boston bureau closure for the *New York Times*.[23] We do so by using the 2008 closure of the *New York Times'* New Jersey bureaus in Newark and Trenton.[24] Industry speculation indicated that the *New York Times* faced financial pressures and that these two bureaus were expendable given their proximity to New York City (New York Times 2017). This setting minimizes all three differences as it also occurs within the 2008-2010 period, occurs in the Northeastern region of the United States, and reflects a regional bureau closure as opposed

[22] George and Waldfogel (2006) study *New York Times* expansion into local markets via home delivery in 1996-2000. This is similar in spirit to the expansion into local markets by Fox News (DellaVigna and Kaplan 2007; Baloria and Heese 2018). Our focus is not on readership or viewership expansion, but rather local office expansion and contraction.
[23] The *Wall Street Journal* did not open a regional bureau in the Southeast around 2020, thereby precluding this test.
[24] A media directory as of 2007 reports these bureaus: https://www.mediacontactspro.com/list-of-all-us-newspapers/

to an opening. Importantly though, we do not expect significant effects arising from this closure as the *New York Times* is headquartered less than 20 miles from the Newark bureau and 70 miles from the Trent bureau. It is possible, and perhaps likely, that any loss of local information from the closure could be mitigated. Most New Jersey-based public firms are headquartered in the Northeast corner of the state, in close proximity to *New York Times'* New York City headquarters.

We define treatment firms as those headquartered in New Jersey, control firms as those headquartered in all other states, use a sample period of 2005-2011, and examine misstatements.[25]

In Table 11, Panel A we examine how the three misstatement variables (i.e., *Misstatement, Misstatement_BigR,* and *Misstatement_Littler*) change in response to the 2008 closure of *New York Time*s' New Jersey bureaus. In column (1), we examine *Misstatement*. We find that the estimated coefficient on *Treat*Post* is negative but insignificant (at the 10 percent level). In column (2), we examine *Misstatement_BigR*. We find that the estimated coefficient on *Treat*Post* is negative but insignificant (at the 10 percent level). In column (3), we examine *Misstatement_Littler*, we find that the estimated coefficient on *Treat*Post* is negative and significant (at the 10 percent level).

In Table 11, Panel B, rather than use the fiscal year in which the misstatement was reported, we use the fiscal years in which a misstatement occurred. Across all three columns, we find that the estimated coefficient on *Treat*Post* is positive but insignificant (at the 10 percent level). Collectively, we find little reliable evidence that the 2008 closure of the *New York Time*s' New Jersey bureaus impacted local firms' financial misreporting. This evidence suggests that differences relating to sample periods, geographical regions, and asymmetric effects of openings vs. closings are unlikely to be driving our findings for the 2009 and 2020 regional bureau events.

[25] The closure occurred during summer of 2008 (Koblin 2008). Consistent with our main analysis, we exclude 2008.

6.2 Ideological slant

The difference could also be driven by ideological slant. Gentzkow and Shapiro (2010) document that the *New York Times* is left-leaning while the *Wall Street Journal* is right-leaning. This explanation would predict that given its more anti-business nature, the left-leaning outlet, *New York Times,* has a more impactful watchdog role (Cohen, Ding, Lesage, Stoloway 2017). We observe the opposite - the more business-friendly, right-leaning outlet, *Wall Street Journal*, has a more impactful watchdog role, suggesting ideological slant is unlikely to be driving results.[26]

While ideological slant is unlikely to be the main driver behind our *on average* findings for the 2009 and 2020 regional bureau events, it is worthwhile to examine *cross-sectional* variation in ideological slant. Goldman et al. (2024) document that the *Wall Street Journal (New York Times)* provide more favorable coverage of right-leaning (left-leaning) firms' earnings announcements due to their ideological alignment. Firms are more responsive to media outlets with opposing political ideologies as these outlets negatively slant coverage and impact firm reputation (Baloria and Heese 2018). As the *New York Times* has a stronger ideological slant in its news coverage than the *Wall Street Journal* (Groseclose and Milyo 2005; Leung and Stumpf 2024), we expect the effect to be particularly important in explaining the cross-sectional variation for the 2020 event.

In Table 12, we restrict our sample to S&P 1500 firms for which we have data on executive campaign contributions that allow us to construct measures of firm ideology (Christensen, Dhaliwal, Boivie and Graffin 2015). In Panel A, we focus on the 2009 *Wall Street Journal* Boston bureau closure and split our sample into left-leaning firms and right-leaning firms.[27] We find that

[26] Inconsistent with the ideological slant explanation, Miller (2006) and Dyck et al. (2010) find that the *Wall Street Journal* has a more impactful watchdog role for *ex-post* detection of corporate misconduct than the *New York Times.*

[27] We define left-leaning (right-leaning) firms as those with CEOs that only contribute to the Democratic (Republican) party, resulting in sample sizes of 1,026 (2,389). We find similar results when using an indicator variable based on which party the CEO contributes more to, for which sample sizes are larger (2,279 left-leaning, 4,713 right-leaning).

the estimated coefficient on *Treat*Post* is positive but insignificant (at the 10 percent level) for both left-leaning and right-leaning firms, consistent with a smaller sample size and loss of power.[28]

In Panel B, we focus on the 2020 *New York Times* Nashville bureau opening and split our sample into left-leaning firms and right-leaning firms. We find that the estimated coefficient on *Treat*Post* is negative and significant (at the 5 percent level) only for right-leaning firms, consistent with right-leaning firms facing a higher threat of negatively slanted media coverage by the *New York Times* and in turn being more responsive to the regional bureau opening event.

This evidence suggests that ideological discord (i.e. entry or exit of a newspaper with a strongly expressed opposing ideology) shapes managers' financial misreporting behavior, in line with findings in Baloria and Heese (2018) that firms are responsive to the threat of media slant.

7. Conclusion

This study examines the effect of newspaper entry and exit of firm behavior. Closings and openings of national newspapers' regional bureaus represent a fundamental change to the local information environment. We find that after the 2009 closure of the *Wall Street Journal*'s Boston bureau, local firms are more likely to report financial misstatements. These effects are driven by irregularities (i.e., material misstatements), suggesting that the business press plays a critical watchdog role. We find that after the 2020 opening of *New York Times'* Nashville bureau, local firms are less likely to report financial misstatements. These effects are driven by errors, and not material errors that are potentially misclassified, suggesting firms make cosmetic changes to appease the popular press without changing the substance of their financial misreporting behavior.

[28] Another possibility is that right-leaning media outlets and firms are more opposed to financial misreporting as it does not align with their values relating to protection of individual economic interests (Hutton, Jiang and Kumar 2015). This explanation would predict that the *Wall Street Journal* plays a more impactful watchdog role with financial misstatements, and that these effects are concentrated among right-leaning firms, which we do not find evidence of.

We conduct cross-sectional tests to identify firms that are more likely to be impacted by closings and openings of national newspapers' regional bureaus. We find that changes in financial misstatements are concentrated among larger firms, consistent with visible firms being of interest to the national media. We also find that changes in financial misstatements are concentrated among firms with Big 4 auditors, consistent with large audit firms being of interest to the national media.

To mitigate the possibility that our results are driven by differences arising from sample periods, geographical regions of bureau events, or asymmetric effects of closings vs. openings, we conduct a falsification test using the 2008 closure of *New York Times'* Newark and Trenton, New Jersey bureaus. New Jersey headquartered firms are geographically adjacent to *New York Times'* New York City headquarters, potentially resulting in a minimal local information environment impact from the regional bureau closures. We find that after the 2008 closure of *New York Times'* New Jersey bureaus, local firms are not more or less likely to report financial misstatements.

Collectively, our results suggest that closings and openings of national newspapers' regional bureaus have localized effects on firm behavior. Our study sheds light on the mechanism (i.e., regional bureaus) through which the national media exercises a local monitoring role and deters corporate misconduct. Our findings have implications for the literature on the watchdog role the national media plays in financial markets, the importance of understanding differences between business and popular press, and the critical role of localized monitoring for financial misreporting.

Our findings are timely as Emma Tucker, Editor-in-Chief of the *Wall Street Journal*, has recently announced a restructuring plan, stating, "We are moving away from regional and local economic news. We are closing the East Coast, Mid-U.S., and West Coast regional bureaus." (Wall Street Journal 2024). Our evidence sheds light on the economic consequences of national media outlets' regional bureaus, an increasingly important issue given the decline of local journalism.

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Figure 1

In 2009, *Wall Street Journal* had regional bureaus in Atlanta (Georgia), Boston (Massachusetts), Chicago (Illinois), Dallas (Texas), Detroit (Michigan), Los Angelas (California), Palo Alto (California), Pittsburgh (Pennsylvania), San Fransico (California), and Washington (D.C.)

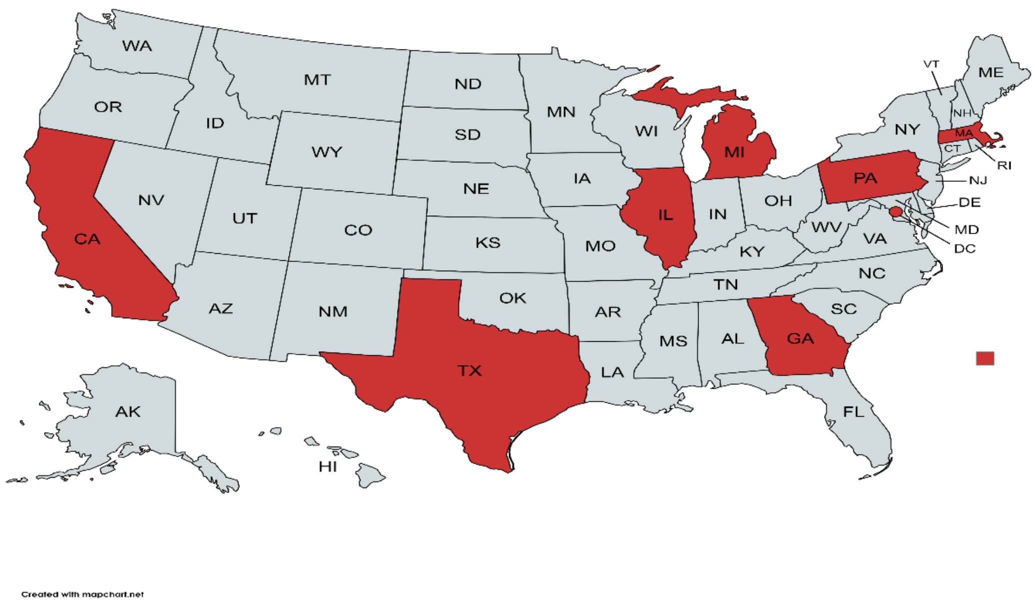


Figure 2

In 2020, *New York Times* had regional bureaus in Atlanta (Georgia), Boston (Massachusetts), Chicago (Illinois), Denver (Colorado), Houston (Texas), Los Angelas (California), Miami (Florida), Nashville (Tennessee), New Orleans (Louisiana), San Francisco (California), Seattle (Washington) and Washington (D.C.).

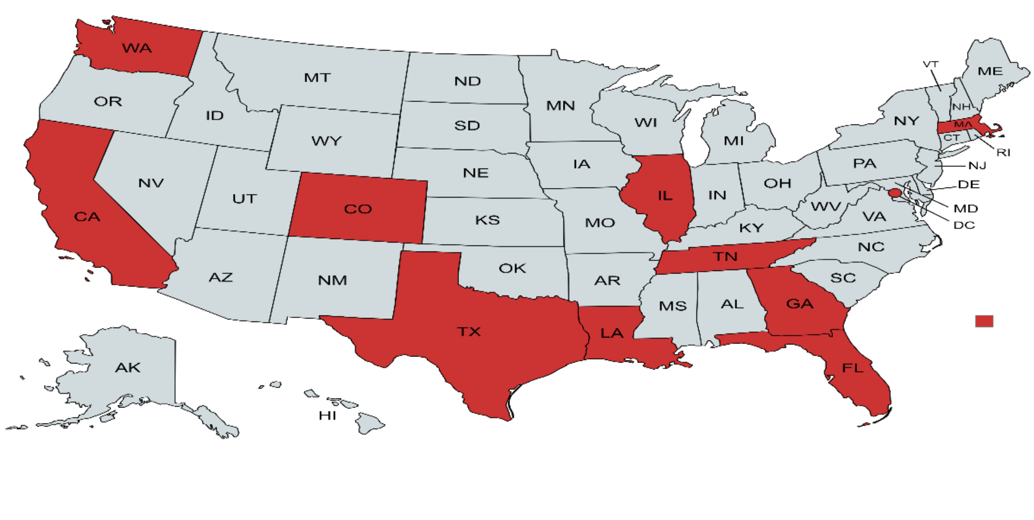


Figure 3
Time trend for misstatements for treatment versus control firms three years pre and post the 2009 closure of the Boston regional bureau of the *Wall Street Journal*.

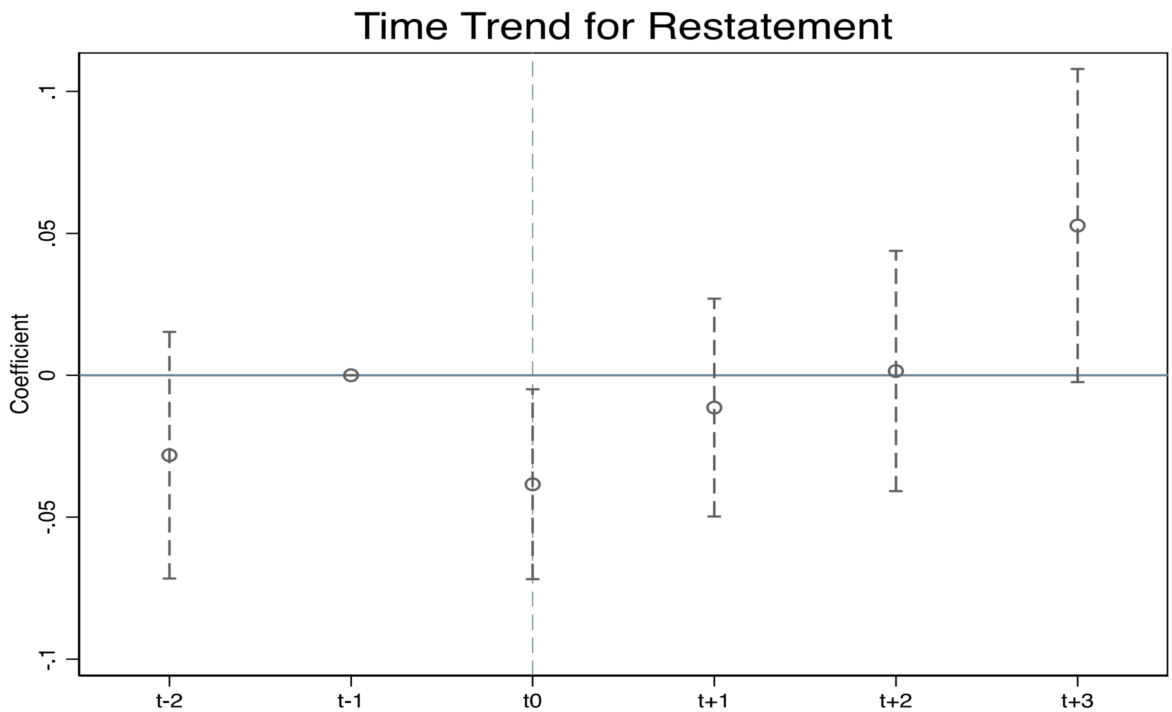
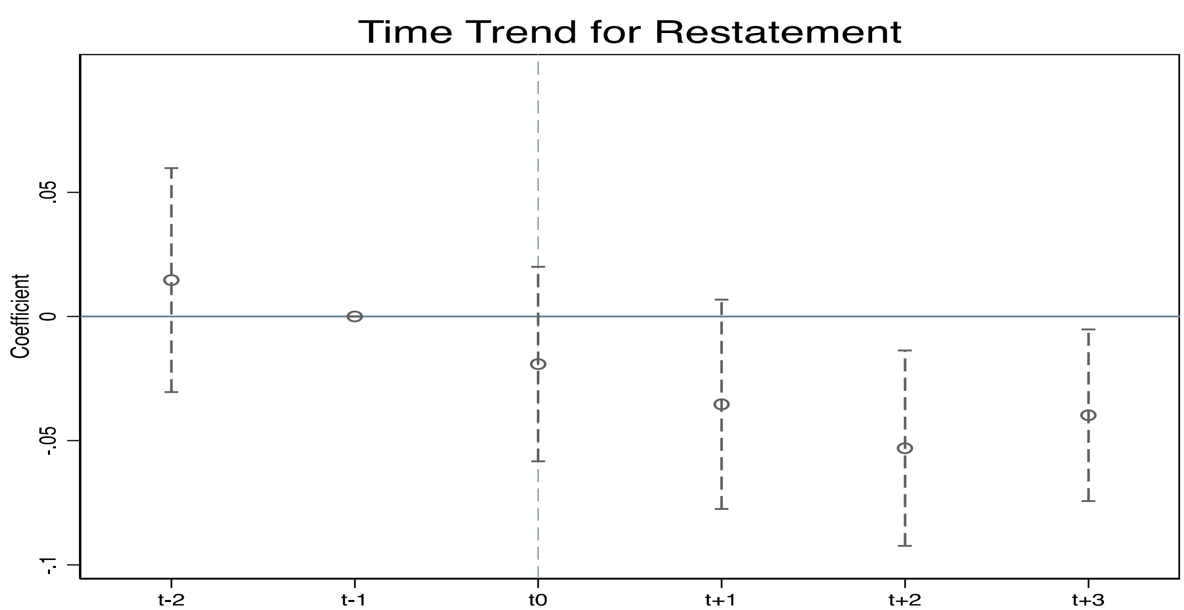


Figure 4
Time trend for misstatements for treatment versus control firms three years pre and post the 2020 opening of the Nashville regional bureau of the *New York Times*.



Appendix A

Validation analyses.

TABLE A.1 Validation Analyses-2009 *Wall Street Journal* Boston Regional Bureau Closure

Panel A: Impact of 2009 event on number of firm-specific articles in *Wall Street Journal*

	WSJ_Count		*WSJ_Count*		*WSJ_Count*	
*Treat_WSJ*Post_WSJ*	-0.492**	(-2.35)	-0.375**	(-2.40)	-0.498**	(-2.58)
Treat_WSJ	0.163	(0.97)	-0.001	(-0.07)	0.120	(0.58)
MVE	0.854***	(14.90)	0.851***	(15.05)	0.855***	(14.98)
ROA	-2.511***	(-3.52)	-2.486***	(-3.52)	-2.473***	(-3.48)
Loss	0.367***	(2.53)	0.376***	(2.60)	0.379***	(2.61)
Growth	-0.254	(-1.42)	-0.254	(-1.45)	-0.248	(-1.40)
Leverage	0.477*	(1.78)	0.529**	(2.02)	0.473*	(1.77)
PPE	-0.716**	(-2.08)	-0.680**	(-1.97)	-0.710**	(-2.04)
Merger	0.019	(0.28)	0.019	(0.28)	0.022	(0.32)
Foreign	-0.051	(-0.50)	-0.041	(-0.41)	-0.054	(-0.54)
Risk	0.481***	(7.45)	0.482***	(7.67)	0.473***	(7.36)
Returns	-0.262***	(-3.85)	-0.266***	(-3.99)	-0.263***	(-3.89)
Litigation Risk	0.086	(0.44)	0.117	(0.61)	0.090	(0.46)
Constant	-7.381***	(12.14)	-7.409***	(12.44)	-7.367***	(12.14)
Industry FE	Yes		Yes		Yes	
Year FE	Yes		Yes		Yes	
Observations	2,766		2,856		2,784	
Adjusted R-squared	0.465		0.461		0.465	

Panel B: Impact of 2009 event on number of firm-specific articles in *New York Times*

	NYT_Count		*NYT_Count*		*NYT_Count*	
*Treat_WSJ*Post_WSJ*	-0.036	(-0.18)	-0.121	(-0.85)	-0.148	(-0.80)
Treat_WSJ	-0.115	(0.67)	-0.048	(-0.41)	-0.086	(-0.54)
MVE	0.658***	(16.09)	0.655***	(16.63)	0.656***	(16.11)
ROA	-1.618***	(-2.66)	-1.570***	(-2.65)	-1.590***	(-2.62)
Loss	0.150	(1.34)	0.165	(1.50)	0.155	(1.38)
Growth	-0.423***	(-2.89)	-0.450***	(-3.15)	-0.436***	(-2.99)
Leverage	0.438**	(2.25)	0.450**	(2.39)	0.436**	(2.25)
PPE	-0.262	(-1.09)	-0.274	(-1.16)	-0.248	(-1.04)
Merger	0.077	(1.44)	0.091*	(1.73)	0.082	(1.53)
Foreign	0.028	(0.41)	0.035	(0.53)	0.036	(0.53)
Risk	0.226***	(5.28)	0.213***	(5.11)	0.224***	(5.27)
Returns	-0.250***	(-4.85)	-0.242***	(-4.76)	-0.246***	(-4.77)
Litigation Risk	0.223	(1.60)	0.217	(1.63)	0.228	(1.64)
Constant	-5.979***	(14.21)	-5.945***	(14.71)	-5.967***	(14.19)
Industry FE	Yes		Yes		Yes	
Year FE	Yes		Yes		Yes	
Observations	2,301		2,376		2,316	
Adjusted R-squared	0.489		0.489		0.488	

Notes: This table reports the results of testing for the impact of the 2009 *Wall Street Journal* Boston regional bureau closure on firm-specific articles in the *Wall Street Journal (*Panel A) and *New York Times* (Panel B). The sample is restricted to S&P 500 firms. Column (1) defines treatment firms as those headquartered in Massachusetts, Column (2) defines treatment firms as those headquartered in all New England states (Connecticut, Massachusetts, Maine, New Hampshire, Rhode Island, and Vermont), and Column (3) defines treatment firms as those headquartered in all New England states other than Connecticut. *T-statistics* appear in parentheses next to the coefficient estimates. *, **, and *** denote two-tailed statistical significance at the 10, 5, and 1 percent levels. Standard errors are clustered at the firm-level. Firm-specific articles are from RavenPack. *WSJ_Count* is the natural logarithm of the number of *Wall Street Journal* articles about firm *i* in fiscal year *t. NYT_Count* is the natural logarithm of the number of *New York Times* articles about firm *i* in fiscal year *t.* All other variables are defined in Appendix B or the notes to Table A.2.

TABLE A.2 Validation Analyses-2020 *New York Times* Nashville Regional Bureau Opening

Panel A: Impact of 2020 event on number of firm-specific articles in *New York Times*

	NYT_Count	
*Treat_NYT*Post_NYT*	0.191**	(1.97)
Treat_NYT	-0.088	(-0.68)
MVE	0.638***	(12.78)
ROA	-1.806***	(-3.57)
Loss	0.033	(0.33)
Growth	-0.088	(-0.74)
Leverage	-0.120	(-0.70)
PPE	-0.081	(-0.35)
Merger	-0.219***	(-3.84)
Foreign	-0.026	(-0.30)
Risk	0.400***	(6.04)
Returns	-0.187***	(-4.02)
Litigation Risk	0.252*	(1.90)
Constant	-6.197***	(11.76)
Industry FE	Yes	
Year FE	Yes	
Observations	2,455	
Adjusted R-squared	0.460	

Panel B: Impact of 2020 event on number of firm-specific articles in *Wall Street Journal*

	WSJ_Count	
*Treat_NYT*Post_NYT*	0.097	(1.57)
Treat_NYT	-0.112*	(-1.70)
MVE	0.441***	(15.14)
ROA	-0.863***	(-2.64)
Loss	0.081	(1.17)
Growth	-0.113	(-1.53)
Leverage	0.114	(1.08)
PPE	0.008	(0.05)
Merger	-0.058	(-1.65)
Foreign	-0.012	(-0.23)
Risk	0.265***	(7.19)
Returns	-0.186***	(-5.97)
Litigation Risk	0.024	(0.32)
Constant	-4.513***	(-14.20)
Industry FE	Yes	
Year FE	Yes	
Observations	2,455	
Adjusted R-squared	0.479	

Notes: This table reports the results of testing for the impact of the 2020 *New York Times* Nashville regional bureau opening on firm-specific articles in the *New York Times* (Panel A) and *Wall Street Journal (*Panel B). The sample is restricted to S&P 500 firms. Treatment firms are those headquartered in Alabama, Arkansas, Georgia, Mississippi, Louisiana, North Carolina, South Carolina, or Tennessee. *T-statistics* appear in parentheses next to the coefficient estimates. *, **, and *** denote two-tailed statistical significance at the 10, 5, and 1 percent levels. Standard errors are clustered at the firm-level. Firm-specific articles are hand-collected from Factiva (Panel A) or are from RavenPack (Panel B). *NYT_Count* is the natural logarithm of the number of *New York Times* articles about firm *i* in fiscal year *t. WSJ_Count* is the natural logarithm of the number of *Wall Street Journal* articles about firm *i* in fiscal year *t. Loss* equals 1 if *ROA* for firm *i* in fiscal year *t* is negative, and 0 otherwise. *Growth* is year-over-year sales growth for firm *i* in fiscal year *t. Merger* equals 1 if firm *i* reports the impact of a merger or acquisition on net income in fiscal year *t*, and zero otherwise. *Foreign* equals 1 if firm *i* reports non-zero pre-tax foreign income in fiscal year *t*, and zero otherwise. *Risk* equals stock return volatility for firm *i* in fiscal year *t. Returns* equals 12 month buy-and-hold returns for firm *i* in fiscal year *t*. All other variables are defined in Appendix B.

Figure A.1
Google Search Volume Index (SVI) for "*Wall Street Journal*" for Massachusetts from 2006-2012

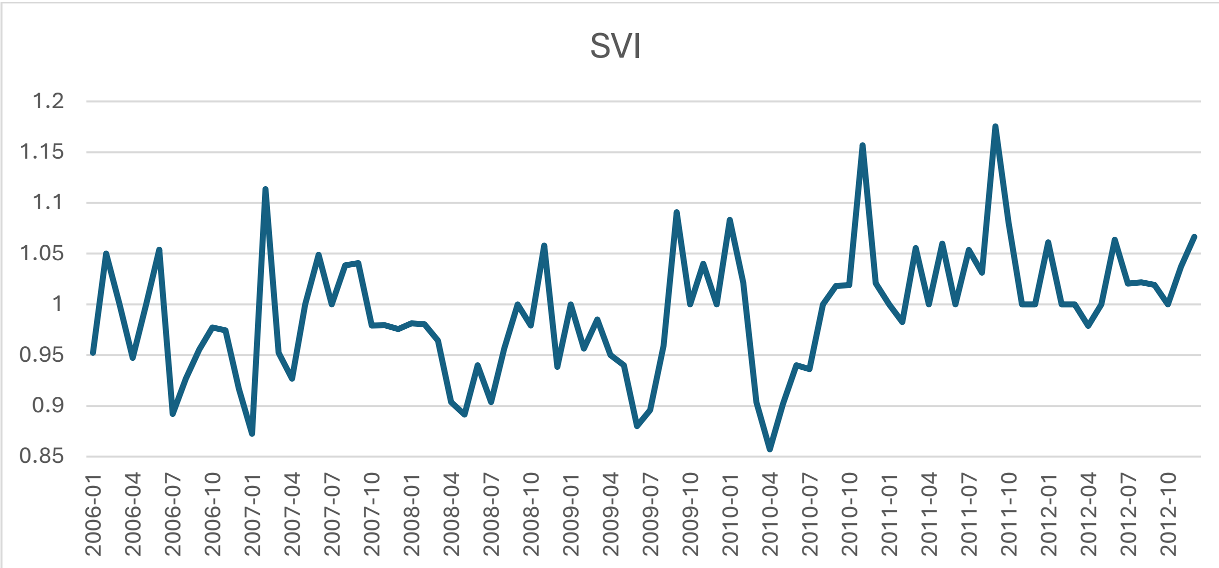


Figure A.2
Google Search Volume Index (SVI) for "*Wall Street Journal*" for New York from 2006-2012

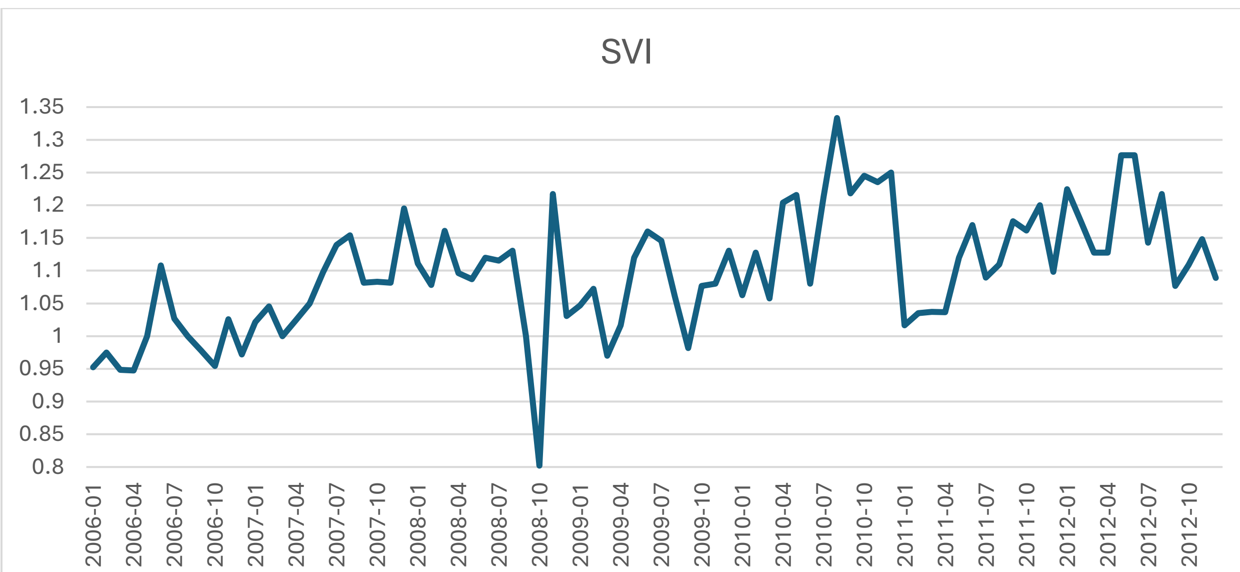


Figure A.3
Google Search Volume Index (SVI) for "*New York Times*" for Tennessee from 2017-2023

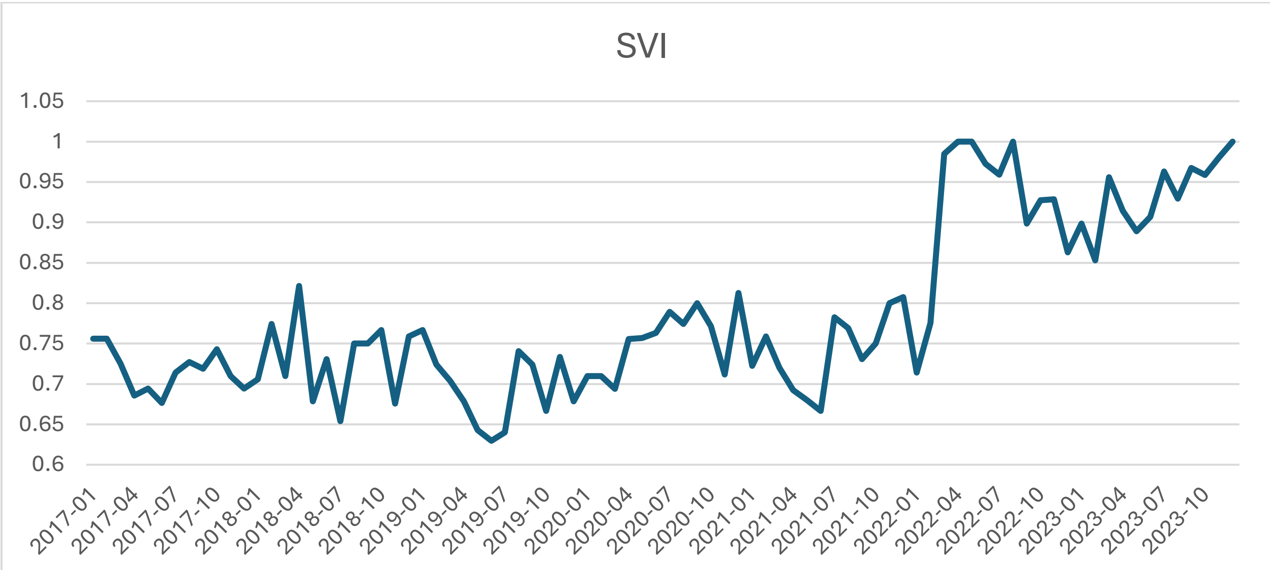
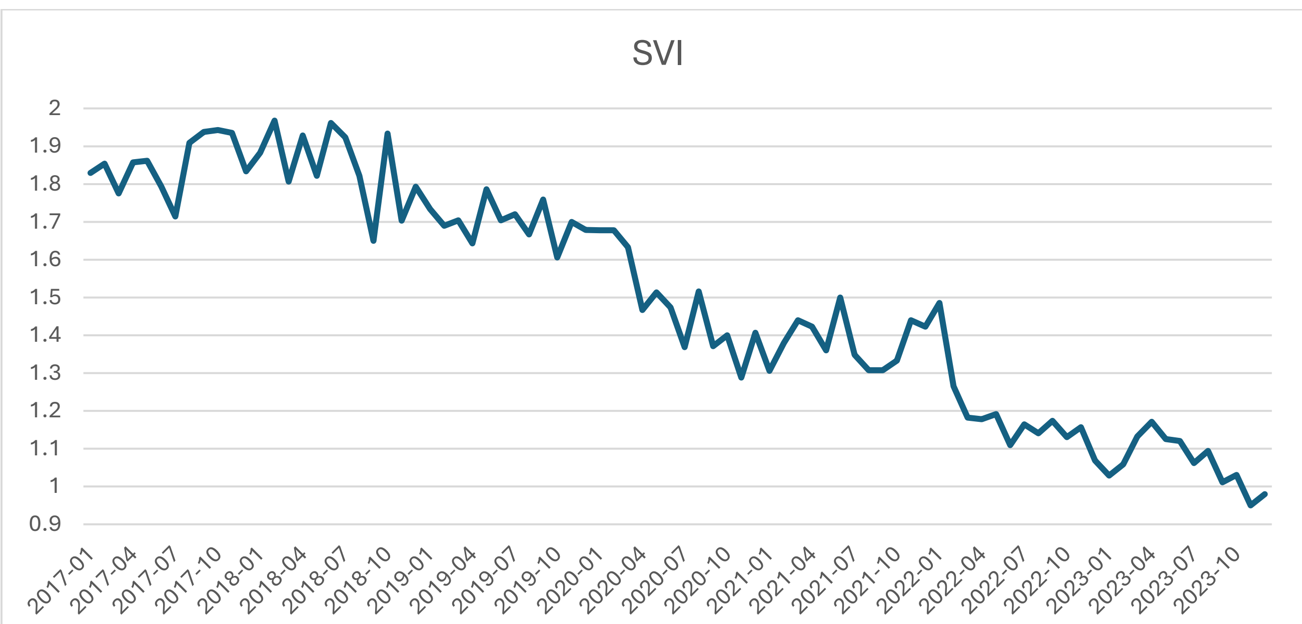


Figure A.4
Google Search Volume Index (SVI) for "*New York Times*" for New York from 2017-2023



We collect monthly internet search volume indices (SVI) from Google Trends for the terms, "*Wall Street Journal"* and "*New York Times"*. State-level indices are not comparable when downloaded separately. Following Kumar et al. (2022), we deflate the *SVI* for each state by the corresponding national *SVI* to ensure comparability in the cross-section and the time-series. We benchmark Massachusetts and Tennessee, where the regional bureaus closed and opened, to New York, where the two media outlets are headquartered

Appendix B

Variable descriptions.

Variable	Description (Compustat data items in parentheses)
Misstatement	Equals 1 if there is a restatement reported by firm *i* in fiscal year *t*, and 0 otherwise.
Misstatement_BigR	Equals 1 if there is a restatement reported using an 8-K item 4.02 disclosure by firm *i* in fiscal year *t*, and 0 otherwise.
Misstatement_Littler	Equals 1 if there is a restatement reported without using an 8-K item 4.02 disclosure by firm *i* in fiscal year *t*, and 0 otherwise.
Misstatement_Littler_Quantitative	Equals 1 if there is a restatement reported without using an 8-K item 4.02 disclosure by firm *i* in fiscal year *t* that meets the quantitative materiality thresholds as per Thompson (2023), and 0 otherwise.
Misstatement_Littler_Qualitative	Equals 1 if there is a restatement reported without using an 8-K item 4.02 disclosure by firm *i* in fiscal year *t* that meets the qualitative materiality thresholds as per Thompson (2023), and 0 otherwise.
Misstatement_Littler_Both	Equals 1 if there is a restatement reported without using an 8-K item 4.02 disclosure by firm *i* in fiscal year *t* that meets the qualitative or qualitative materiality thresholds as per Thompson (2023), and 0 otherwise.
Treat_WSJ	Equals 1 if the corporate headquarters is in Massachusetts for firm *i* in fiscal year *t*, and 0 otherwise.
Treat_NYT	Equals 1 if the corporate headquarters is in Alabama, Arkansas, Georgia, Mississippi, Louisiana, North Carolina, South Carolina, or Tennessee for firm *i* in fiscal year *t*, and 0 otherwise.
Treat_NYTF	Equals 1 if the corporate headquarters is in New Jersey for firm *i* in fiscal year *t*, and 0 otherwise.
Post_WSJ	Equals 1 for firm *i* in fiscal years 2010, 2011, 2012, and 0 for fiscal years 2006, 2007, and 2008.
Post_NYT	Equals 1 for firm *i* in fiscal years 2021, 2022, 2023, and 0 for fiscal years 2017, 2018, and 2019.
Post_NYTF	Equals 1 for firm *i* in fiscal years 2009, 2010, 2011, and 0 for fiscal years 2005, 2006, and 2007.
Size	Natural logarithm of the book value of assets (at) for firm *i* in fiscal year *t*.
Leverage	Book value of long-term debt (dltt) plus debt in current liabilities (dlc) divided by the book value of assets (at) for firm *i* in fiscal year *t*.
BTM	Book value of equity (ceq) divided by market value of equity (csho*prcc_f) for firm *i* in fiscal year *t*.

CAPEX	Capital expenditures (capx) divided by the book value of assets (at) for firm *i* in fiscal year *t*.
ROA	Net income (ni) divided by the book value of average assets (at) for firm *i* in fiscal year *t*.
PPE	Net property, plant, and equipment (ppegt) divided by the book value of assets (at) for firm *i* in fiscal year *t*.
R&D	Natural logarithm of one plus R&D expenditures (xrd) divided by book value of assets (at) for firm *i* in fiscal year *t*. *RD* equals 0 if its value is missing.
CFO	Cash flow from operating activities (oancf) divided by the book value of assets (at) for firm *i* in fiscal year *t*.
Dividends	Total cash dividends divided by the book value of assets (at) for firm *i* in fiscal year *t*.
MVE	Market value of assets (csho*prcc_f) for firm *i* in fiscal year *t*.
Big4	Equals 1 if the audit firm is one of Deloitte, E&Y, PwC, KMPG for firm *i* in fiscal year *t*, and 0 otherwise.
Left-Leaning	Equals 1 if the CEO of firm *i* in fiscal year *t* only contributes to the Democratic party, and zero otherwise.
Right-Leaning	Equals 1 if the CEO of firm *i* in fiscal year *t* only contributes to the Republican party, and zero otherwise.

TABLE 1 Sample Selection

Panel A: 2009 *Wall Street Journal* Boston Regional Bureau Closure		
Description		Number of Observations
Firms in Compustat from 2006-2012, excluding event year (2009)		66,271
Remove firms with missing headquarters data	(25,230)	41,041
Remove foreign firms	(2,092)	38,949
Remove firms with missing data for control variables	(12,075)	26,874
Remove firms that switched treatment status during sample period	(459)	26,415
Final sample		26,415

Panel B: 2020 *New York Times* Nashville Regional Bureau Opening		
Description		Number of Observations
Firms in Compustat from 2017-2023, excluding event year (2020)		71,286
Remove firms with missing headquarters data	(37,847)	33,339
Remove foreign firms	(2,029)	31,310
Remove firms with missing data for control variables	(8,948)	22,362
Remove firms that switched treatment status during sample period	(250)	22,212
Final sample		22,212

Notes: This table reports the sample selection procedure with the number of observations removed and retained in each step for both events. Panel A presents sample selection for the 2009 event and Panel B for the 2020 event.

TABLE 2 Descriptive Statistics – 2009 *Wall Street Journal* Boston Regional Bureau Closure

Variable	*N*	Mean	Std. dev.	Median	25th	75th
Treat_WSJ	26,415	0.048	0.213	0.000	0.000	0.000
Post_WSJ	26,415	0.457	0.498	0.000	0.000	1.000
Misstatement	26,415	0.095	0.292	0.000	0.000	0.000
Misstatment_BigR	26,415	0.050	0.217	0.000	0.000	0.000
Misstatement_Littler	26,415	0.045	0.207	0.000	0.000	0.000
Size	26,415	5.351	2.811	5.611	3.617	7.319
Leverage	26,415	0.437	1.317	0.174	0.008	0.378
BTM	26,415	0.295	1.876	0.436	0.189	0.771
ROA	26,415	-0.339	1.516	0.021	-0.123	0.073
PPE	26,415	0.472	0.447	0.331	0.125	0.720
CAPEX	26,415	0.048	0.069	0.026	0.009	0.057
R&D	26,415	0.081	0.218	0.000	0.000	0.061
CFO	26,415	-0.149	0.882	0.006	-0.039	0.119
Dividends	26,415	0.017	0.053	0.000	0.000	0.011

Notes: This table reports the descriptive statistics for the 2009 event. Variables are defined in Appendix B.

TABLE 3 Main Analyses - 2009 *Wall Street Journal* Boston Regional Bureau Closure

	Misstatement		*Misstatement_BigR*		*Misstatement_Littler*	
*Treat_WSJ*Post_WSJ*	0.039**	(2.49)	0.024**	(2.05)	0.015	(1.24)
Size	-0.003***	(-3.41)	-0.002***	(-3.28)	-0.001**	(-1.65)
Leverage	-0.002	(-0.96)	-0.004**	(-2.51)	0.002	(1.03)
BTM	-0.002	(-1.41)	-0.001	(-1.43)	-0.001	(-0.50)
ROA	-0.007**	(-2.52)	-0.004**	(-2.47)	-0.002	(-1.17)
PPE	-0.021***	(-3.84)	-0.013***	(-3.41)	-0.008**	(-1.96)
CAPEX	0.039	(0.97)	0.053**	(1.84)	-0.014	(-0.50)
R&D	-0.023*	(-1.95)	-0.003	(-0.30)	-0.020**	(-2.48)
CFO	-0.006	(-1.35)	0.001	(0.19)	-0.006*	(-1.91)
Dividends	0.009	(0.23)	-0.011	(-0.37)	0.020	(0.69)
Constant	0.118***	(18.28)	0.064***	(14.53)	0.054***	(11.39)
Industry-By-Year FE	Yes		Yes		Yes	
State FE	Yes		Yes		Yes	
Observations	26,415		26,415		26,415	
Adjusted R-squared	0.025		0.028		0.012	

Notes: This table reports the results of testing for the impact of the 2009 *Wall Street Journal* Boston regional bureau closure on misstatements. *T-statistics* appear in parentheses next to the coefficient estimates. *, **, and *** denote two-tailed statistical significance at the 10, 5, and 1 percent levels. Standard errors are clustered at the firm-level. Variables are defined in Appendix B.

TABLE 4 Cross-Sectional Analyses -
2009 *Wall Street Journal* Boston Regional Bureau Closure

Panel A: Firm Size

	(1)		(2)		(3)	
	Large		Medium		Small	
			Misstatement			
*Treat_WSJ*Post_WSJ*	0.060**	(2.11)	0.011	(0.42)	0.041	(1.53)
Controls and constant	Yes		Yes		Yes	
Industry-By-Year FE	Yes		Yes		Yes	
State FE	Yes		Yes		Yes	
Observations	8,805		8,805		8,805	
Adjusted R-squared	0.038		0.025		0.034	

Panel B: Auditor Size

	(1)		(2)	
	Big4		Non-Big4	
		Misstatement		
*Treat_WSJ*Post_WSJ*	0.036*	(1.90)	0.019	(0.72)
Controls and constant	Yes		Yes	
Industry-By-Year FE	Yes		Yes	
State FE	Yes		Yes	
Observations	15,891		10,524	
Adjusted R-squared	0.023		0.031	

Notes: This table reports the results of cross-sectional analyses for the 2009 event. Panel A splits the sample based on terciles of firm size. Panel B splits the sample based on whether the firm uses a Big 4 audit firm or not. *T-statistics* appear in parentheses next to the coefficient estimates. *, **, and *** denote two-tailed statistical significance at the 10, 5, and 1 percent levels. Standard errors are clustered at the firm-level. Variables are defined in Appendix B.

TABLE 5 Robustness Analyses – 2009 *Wall Street Journal* Boston Regional Bureau Closure

Panel A: New England States

	(1)		(2)	
	All New England States		Excluding Connecticut	
	Misstatement			
*Treat_WSJ*Post_WSJ*	0.027**	(2.11)	0.032**	(2.29)
Controls and constant	Yes		Yes	
Industry-By-Year FE	Yes		Yes	
State FE	Yes		Yes	
Observations	26,415		26,415	
Adjusted R-squared	0.025		0.023	

Panel B: Including only States with Regional Bureaus

	(1)	
	Misstatement	
*Treat_WSJ*Post_WSJ*	0.035**	(2.22)
Controls and constant	Yes	
Industry-By-Year FE	Yes	
State FE	Yes	
Observations	15,632	
Adjusted R-squared	0.023	

Panel C: Misstatement Occurrence Years

	(1)		(2)		(3)	
	Misstatement		*Misstatement_BigR*		*Misstatement_Littler*	
*Treat_WSJ*Post_WSJ*	0.021*	(1.90)	0.012**	(2.23)	0.009	(0.91)
Controls and constant	Yes		Yes		Yes	
Industry-By-Year FE	Yes		Yes		Yes	
State FE	Yes		Yes		Yes	
Observations	26,415		26,415		26,415	
Adjusted R-squared	0.012		0.007		0.010	

Notes: This table reports the results of robustness analyses for the 2009 event. Panel A includes additional treatment states. Panel B only includes control states where *Wall Street Journal* is headquartered (New York) or with regional bureaus. Panel C defines the misstatement variable based on the year of occurrence as opposed to the year of reporting. *T-statistics* appear in parentheses next to the coefficient estimates. *, **, and *** denote two-tailed statistical significance at the 10, 5, and 1 percent levels. Standard errors are clustered at the firm-level. Variables are defined in Appendix B.

TABLE 6 Descriptive Statistics – 2020 *New York Times* Nashville Regional Bureau Opening

Variable	*N*	Mean	Std. dev.	Median	25th	75th
Treat_NYT	22,112	0.067	0.250	0.000	0.000	0.000
Post_NYT	22,112	0.506	0.500	1.000	0.000	1.000
Misstatement	22,112	0.055	0.227	0.000	0.000	0.000
Misstatment_BigR	22,112	0.017	0.129	0.000	0.000	0.000
Misstatement_Littler	22,112	0.038	0.191	0.000	0.000	0.000
Size	22,112	6.144	2.825	6.460	4.449	8.045
Leverage	22,112	0.482	1.317	0.259	0.066	0.457
BTM	22,112	0.364	1.230	0.363	0.136	0.727
ROA	22,112	-0.375	1.643	0.006	-0.225	0.062
PPE	22,112	0.426	0.485	0.246	0.081	0.635
CAPEX	22,112	0.034	0.049	0.018	0.005	0.042
R&D	22,112	0.099	0.233	0.001	0.000	0.087
CFO	22,112	-0.174	0.841	0.041	-0.116	0.102
Dividends	22,112	0.015	0.038	0.000	0.000	0.014

Notes: This table reports the descriptive statistics for the 2020 event. Variables are defined in Appendix B.

TABLE 7 Main Analyses - 2020 *New York Times* Nashville Regional Bureau Opening

	Misstatement		*Misstatement_BigR*		*Misstatement_Littler*	
*Treat_NYT*Post_NYT*	-0.039***	(-2.63)	0.001	(0.07)	-0.039***	(-2.95)
Size	-0.002***	(-3.24)	-0.002***	(-5.76)	0.000	(0.02)
Leverage	-0.003*	(-1.72)	-0.004***	(-3.67)	0.000	(0.18)
BTM	-0.001	(-0.71)	-0.003**	(-2.37)	0.001	(1.15)
ROA	-0.002	(-1.02)	0.001	(0.13)	-0.002	(-1.31)
PPE	-0.013***	(-3.08)	-0.009***	(-3.72)	-0.004	(-1.10)
CAPEX	0.129***	(2.98)	0.049*	(1.84)	0.080**	(2.25)
R&D	-0.029***	(-3.45)	-0.005	(-1.22)	-0.024***	(-3.27)
CFO	-0.002	(-0.49)	0.001	(0.28)	-0.002	(-0.72)
Dividends	-0.107**	(-2.40)	-0.023	(-0.82)	-0.084***	(-2.59)
Constant	0.077***	(13.62)	0.037***	(10.58)	0.040***	(9.01)
Industry-By-Year FE	Yes		Yes		Yes	
State FE	Yes		Yes		Yes	
Observations	22,112		22,112		22,112	
Adjusted R-squared	0.035		0.054		0.021	

Notes: This table reports the results of testing for the impact of the 2020 *New York Times* Nashville regional bureau opening on misstatements. *T-statistics* appear in parentheses next to the coefficient estimates. *, **, and *** denote two-tailed statistical significance at the 10, 5, and 1 percent levels. Standard errors are clustered at the firm-level. Variables are defined in Appendix B.

TABLE 8 Material Errors Analyses –
2020 *New York Times* Nashville Regional Bureau Opening

	(1)		(2)		(3)	
	Misstatement_Littler_Quantitative		*Misstatement_Littler_Qualitative*		*Misstatement_Littler_Both*	
*Treat_NYT*Post_NYT*	-0.003	(-0.44)	-0.002	(0.46)	-0.010	(-1.15)
Controls and constant	Yes		Yes		Yes	
Industry-By-Year FE	Yes		Yes		Yes	
State FE	Yes		Yes		Yes	
Observations	22,112		22,112		22,112	
Adjusted R-squared	0.001		0.002		0.008	

Notes: This table reports the results of testing for the impact of the 2020 *New York Times* Nashville regional bureau opening on material errors. Materiality is defined based on (1) quantitative materiality thresholds as per Thompson (2023), (2) qualitative materiality thresholds as per Thompson (2023), or (3) either quantitative or qualitative materiality thresholds as per Thompson (2023). *T-statistics* appear in parentheses next to the coefficient estimates. *, **, and *** denote two-tailed statistical significance at the 10, 5, and 1 percent levels. Standard errors are clustered at the firm-level. Variables are defined in Appendix B.

TABLE 9 Cross-Sectional Analyses –
2020 *New York Times* Nashville Regional Bureau Opening

Panel A: Firm Size

	(1)		(2)		(3)	
	Large		Medium		Small	
			Misstatement			
*Treat_NYT*Post_NYT*	-0.042**	(-1.98)	-0.039	(-1.34)	-0.022	(-0.72)
Controls and constant	Yes		Yes		Yes	
Industry-By-Year FE	Yes		Yes		Yes	
State FE	Yes		Yes		Yes	
Observations	7,370		7,371		7,371	
Adjusted R-squared	0.041		0.093		0.031	

Panel B: Auditor Size

	(1)		(2)	
	Big4		Non-Big4	
		Misstatement		
*Treat_NYT*Post_NYT*	-0.045**	(-2.37)	-0.016	(-0.62)
Controls and constant	Yes		Yes	
Industry-By-Year FE	Yes		Yes	
State FE	Yes		Yes	
Observations	13,587		8,525	
Adjusted R-squared	0.038		0.052	

Notes: This table reports the results of cross-sectional analyses for the 2020 event. Panel A splits the sample based on terciles of firm size. Panel B splits the sample based on whether the firm uses a Big 4 audit firm or not. *T-statistics* appear in parentheses next to the coefficient estimates. *, **, and *** denote two-tailed statistical significance at the 10, 5, and 1 percent levels. Standard errors are clustered at the firm-level. Variables are defined in Appendix B.

TABLE 10 Robustness Analyses –
2020 *New York Times* Nashville Regional Bureau Opening

Panel A: Removing Treatment States with Pre-Existing Regional Bureaus

	(1)	
	Excluding Geogia and Louisiana	
	Misstatement	
*Treat_NYT*Post_NYT*	-0.040**	(-2.10)
Controls and constant	Yes	
Industry-By-Year FE	Yes	
State FE	Yes	
Observations	21,505	
Adjusted R-squared	0.035	

Panel B: Including only States with Regional Bureaus

	(1)	
	Misstatement	
*Treat_WSJ*Post_WSJ*	-0.043***	(2.74)
Controls and constant	Yes	
Industry-By-Year FE	Yes	
State FE	Yes	
Observations	14,936	
Adjusted R-squared	0.048	

Panel C: Misstatement Occurrence Years

	(1)		(2)		(3)	
	Misstatement		*Misstatement_BigR*		*Misstatement_Littler*	
*Treat_NYT*Post_NYT*	-0.021*	(-1.90)	0.004	(0.57)	-0.025**	(-2.51)
Controls and constant	Yes		Yes		Yes	
Industry-By-Year FE	Yes		Yes		Yes	
State FE	Yes		Yes		Yes	
Observations	22,112		22,112		22,112	
Adjusted R-squared	0.073		0.086		0.009	

Notes: This table reports the results of robustness analyses for the 2020 event. Panel A excludes treatment states (Georgia and Louisiana) that had pre-existing *New York Times* regional bureaus prior to 2020. Panel B only includes control states where *New York Times* is headquartered (New York) or with regional bureaus. Panel C defines the misstatement variable based on the year of occurrence as opposed to the year of reporting. *T-statistics* appear in parentheses next to the coefficient estimates. *, **, and *** denote two-tailed statistical significance at the 10, 5, and 1 percent levels. Standard errors are clustered at the firm-level. Variables are defined in Appendix B

TABLE 11 Falsification Analyses –
2008 *New York Times* New Jersey Regional Bureau Closures

Panel A: Misstatement Reporting Years

	(1)		(2)		(3)	
	Misstatement		*Misstatement_BigR*		*Misstatement_Littler*	
*Treat_NYTF*Post_NYTF*	-0.025	(-1.35)	-0.004	(-0.26)	-0.022*	(-1.78)
Controls and constant	Yes		Yes		Yes	
Industry-By-Year FE	Yes		Yes		Yes	
State FE	Yes		Yes		Yes	
Observations	27,199		27,199		27,199	
Adjusted R-squared	0.025		0.026		0.013	

Panel B: Misstatement Occurrence Years

	(1)		(2)		(3)	
	Misstatement		*Misstatement_BigR*		*Misstatement_Littler*	
*Treat_NYTF*Post_NYTF*	0.004	(0.38)	0.002	(0.29)	0.002	(0.23)
Controls and constant	Yes		Yes		Yes	
Industry-By-Year FE	Yes		Yes		Yes	
State FE	Yes		Yes		Yes	
Observations	27,199		27,199		27,199	
Adjusted R-squared	0.013		0.007		0.010	

Notes: This table reports the results of falsification analyses testing for the impact of the 2008 *New York Times* New Jersey (Newark and Trenton) regional bureau closures on misstatements. Panel A defines the misstatement variable based on the year of reporting. Panel B defines the misstatement variable based on the year of occurrence as opposed to the year of reporting. *T-statistics* appear in parentheses next to the coefficient estimates. *, **, and *** denote two-tailed statistical significance at the 10, 5, and 1 percent levels. Standard errors are clustered at the firm-level. Variables are defined in Appendix B.

TABLE 12 Ideological Slant – 2009 *Wall Street Journal* Boston Regional Bureau Closure & 2020 *New York Times* Nashville Regional Bureau Opening

Panel A: 2009 *Wall Street Journal* Boston Regional Bureau Closure

	(1)		(2)	
	Left-Leaning		Right-Leaning	
	Misstatement			
*Treat_WSJ*Post_WSJ*	0.0939	(1.24)	0.0182	(0.26)
Controls and constant	Yes		Yes	
Industry-By-Year FE	Yes		Yes	
State FE	Yes		Yes	
Observations	1,026		2,389	
Adjusted R-squared	0.035		0.048	

Panel B: 2020 *New York Times* Nashville Regional Bureau Opening

	(1)		(2)	
	Left-Leaning		Right-Leaning	
	Misstatement			
*Treat_NYT*Post_NYT*	0.2584	(1.14)	-0.1013**	(-2.36)
Controls and constant	Yes		Yes	
Industry-By-Year FE	Yes		Yes	
State FE	Yes		Yes	
Observations	953		1,484	
Adjusted R-squared	0.053		0.017	

Notes: This table reports the results of cross-sectional analyses for the 2009 and 2020 events. Panel A splits the 2009 sample based on whether firm CEOs are left-leaning or right-leaning. Panel B splits the 2020 sample based on whether firm CEOs are left-leaning or right-leaning *T-statistics* appear in parentheses next to the coefficient estimates. *, **, and *** denote two-tailed statistical significance at the 10, 5, and 1 percent levels. Standard errors are clustered at the firm-level. Variables are defined in Appendix B.