When the local newspaper leaves town: the effects of local newspaper closures on corporate misconduct

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Abstract

We examine whether the *local* press is an effective monitor of corporate misconduct. Specifically, we study the effects of local newspaper closures on violations by local facilities of publicly listed firms. After a local newspaper closure, local facilities increase violations by 1.1% and penalties by 15.2%, indicating that the closures reduce firm monitoring by the press. This effect is not driven by the underlying economic conditions, the underlying local fraud environment, or the underlying firm conditions. Taken together, our findings indicate that local newspapers are an important monitor of firms' misconduct.

Keywords: Corporate misconduct, Local newspapers, Media coverage.

JEL Classifications: M40; M41; M46.

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1. Introduction

In this study, we examine whether the *local* press is an effective monitor of corporate misconduct. Specifically, we investigate the effects of local newspaper closures on facility-level violations of publicly listed firms.[1] Examining this question is important for three reasons. First, there is no systematic evidence investigating the efficacy of the *local* press as a monitor of firms' misconduct. Although this question has not been examined systematically, prior studies suggest that the local press is ineffective in monitoring firm behavior (e.g., Gurun and Butler, 2012; Shapira and Zingales, 2017). Second, a few studies examine the efficacy of the press in general (as opposed to the *local* press) but find mixed evidence. Some show that the press disciplines firms' behavior (e.g., Dyck et al., 2008), while others find that it is ineffective in altering firms' conduct (e.g., Core et al., 2008). As a result, Miller and Skinner's (2015) review of the literature calls for more research on the monitoring role of the press. Third, over the last two decades, the circulation of local newspapers in the United States has decreased by nearly 50% (Pew Research Center, 2019). A concern is that less local news results in less local accountability and investigative reporting, and therefore increased local corruption and crime (Waldman, 2011). While prior studies show that areas with less local press have less informed voters (e.g., Gentzkow et al., 2011) and increased corruption by local politicians and borrowing costs for municipalities (Gao et al., 2020), little is known about whether the decline in local newspapers affects corporate misconduct.

From a theoretical standpoint, the effect of local newspaper closures on firms' legal violations is ambiguous. On the one hand, the local press could be an effective monitor of firms and hence affect corporate misconduct via investigations or dissemination (e.g., Miller, 2006; Dyck et al., 2008; Dyck et al., 2010).[2] For example, similar to the national press, the local press

[1] Facilities include regional offices, manufacturing plants, stores (e.g., convenience, department, and retail), distribution centers, refineries, mines, and shipyards, among others.

[2] Throughout the paper, we use the terms "corporate misconduct" and "violations" interchangeably. However, note that we can only observe *detected* violations, as firms' *undetected* misconduct is unobservable.

could undertake original investigations to detect corporate fraud (e.g., Miller, 2006). Local newspapers may be especially effective in discovering the misconduct of local firms because of their proximity to local sources such as employees and local suppliers (e.g., Engelberg and Parsons, 2011; Peress, 2014). The local press could also affect corporate misconduct by widely disseminating information about misbehavior (e.g., Dyck and Zingales, 2002; Dyck et al., 2008). Local newspapers play a central role in disseminating information because they provide the national press with credible information on local misconduct (Shapira and Zingales, 2017). Under this view, we would expect that the closure of a local newspaper weakens the monitoring of corporate misconduct, increasing facility-level violations.

Alternatively, local newspapers may be an ineffective monitor of firms' behavior for at least three reasons. First, local newspapers have incentives to avoid or slant reporting on local firms. Local newspapers rely on local firms for advertising income (e.g., Shapira and Zingales, 2017), which can lead the local press to positively slant news about local companies (Gurun and Butler, 2012). In addition, the press caters to its readers (e.g., Mullainathan and Shleifer, 2005). Thus, local newspapers may refrain from reporting critically about local companies, which are frequently large employers, to avoid the risk of upsetting their readers and possibly losing subscription income (Shapira and Zingales, 2017). Second, local newspapers often have constrained resources in terms of the number of reporters investigating local misbehavior. Third, local newspapers are typically limited in their reach, potentially reducing the impact of their reporting and hence rendering its monitoring ineffective. If local newspapers are ineffective at monitoring firms' behavior, then local newspaper closures should not affect violations by local facilities. Ultimately, whether and the extent to which local newspaper closures affect misconduct by local firms are empirical questions.

Our research strategy has three elements. First, we exploit the closure of local newspapers over time. These closures serve as a proxy for shocks to the strength of monitoring

by the local press because they cause large discrete reductions in local coverage of firms' activities (e.g., Gao et al., 2020). Second, we use a data set from Violation Tracker covering a wide range of federal violations and the resulting penalties issued by 44 agencies.[3] The data specify the geographical location of the facility in which the misconduct occurred, which enables us to identify the violations that occur in the coverage area of local newspapers. Our sample includes 26,450 violations perpetrated by 10,647 unique facilities of 1,383 Compustat firms, including approximately 80% of Fortune 100 and Fortune 500 firms, for the period 2000 to 2017. Third, we sharpen our identification of the effect by examining facility-level violations. In particular, the granularity of our analyses allows us to control for shocks at the firm level and local shocks in each facility's location.

We use a difference-in-differences methodology that exploits the staggered closure of local newspapers between 2003 and 2015. The first difference is either the change in the total number of violations or the dollar amount of penalties in each facility after the closure of a newspaper. The implicit control group at time *t* consists of facilities in locations not affected by a newspaper closure (this control group includes facilities of the same firm and facilities of other firms). The control group's change in violations and penalties is the second difference captured by our tests. The effect of newspaper closures on facility-level violations is estimated as the difference in those two differences.

[3] Violations in our sample relate to workplace safety and health, environmental violations, labor violations, government fraud, worker discrimination, and securities fraud, among many others (Heese and Pérez-Cavazos, 2020). This data set differs along three dimensions from other data sets of corporate misconduct used by prior literature (e.g., Karpoff et al., 2017; Parsons et al., 2018; Xu and Kim, 2021). First, the Violation Tracker data set includes a broad range of different types of corporate misconduct. In contrast, prior literature (e.g., Karpoff et al., 2017) typically only considers a specific type of misconduct (e.g., accounting-related misconduct). Second, the Violation Tracker data set only includes corporate misconduct that resulted in an *enforcement action* and *penalty*. Prior studies (e.g., Karpoff et al., 2017; Xu and Kim, 2021) also consider events such as restatements or toxic releases as corporate misconduct. However, many restatements do not involve penalties, and these restatements are often the result of mistakes rather than fraud (e.g., Hennes et al., 2008). Similarly, toxic releases do not per se constitute a violation of environmental laws (we present tests in Section 5.6. showing that local newspaper closures also result in higher toxic releases). Third, the Violation Tracker data set focuses on *corporate* misconduct, while prior literature (e.g., Parsons et al., 2018) also considers violations conducted by individuals, physicians, or politicians.

We also include a set of facility-, firm-, and county-level controls to ensure that our results are not driven by facility, firm, or macroeconomic conditions. In addition, we include facility fixed effects to control for time-invariant facility characteristics, and we include state-year fixed effects to control for time trends at the state level. While our identification strategy controls for firm-specific shocks, facility and state-year fixed effects control for omitted factors at the local, firm, or facility level over time.[4]

Our main results show that local newspaper closures lead to increased facilities' violations and penalties in the ensuing three years. Specifically, the number of violations increases by 1.1% and penalties increase by 15.2%. These magnitudes are economically significant. For the average facility, they represent approximately 0.95% of facility-level sales. When interpreting these magnitudes, it is important to note that our empirical tests rely on *detected* violations, as undetected violations are unobservable. As a result, these magnitudes are likely a lower-bound estimate. These findings indicate that local newspapers are an important monitor of corporate misconduct.

A potential concern with these results is that the closure of local newspapers and the increases in facility-level violations may be driven by changes in the underlying local economic conditions or the local fraud environment. We address this possibility in several ways. First, we conduct cross-sectional tests examining whether variation in the strength of monitoring by the local press alters the effect of local newspaper closures on facility-level misconduct. We focus on two aspects that likely affect the strength of monitoring by local newspapers: i) the newspapers' reporting incentives and ii) the availability of other local newspapers.

We explore whether differences in newspapers' reporting incentives moderate the effect of newspaper closures on facility-level misconduct. Prior research shows that the press

[4] In robustness tests, we also include industry-year and firm-year fixed effects to rule out the potential concern that our results may be driven by unobservable industry-year or firm-year effects, such as companywide policy introductions that coincide with local newspaper closures.

tends to focus its reporting on more visible firms and that these firms are subject to more scrutiny (e.g., Miller, 2006). Hence, we expect that the effect of newspaper closures on facility-level misconduct is stronger for facilities belonging to more visible firms, because such closures more strongly reduce monitoring for these firms. We categorize facilities that are part of firms with high local media coverage or part of large firms as more visible. As the facilities of more visible and less visible firms typically operate in the same county, these tests exploit the differential treatment effect of newspaper closures on facilities located within the same county. If declining economic conditions or changes in the local fraud environment drove both newspaper closures and changes in misconduct, we should not observe differential increases in misconduct across facilities of more or less visible firms. In contrast, we find that the effect of newspaper closures on facility-level misconduct is concentrated in facilities of more visible firms, suggesting that changes in local press monitoring drive the changes in firm behavior.

Next, we posit that newspaper closures are unlikely to substantially affect facility-level misconduct when there are other local newspapers, as these newspapers act as substitutes (e.g., Gentzkow et al., 2014; Gao et al., 2020). We interact our treatment variable with an indicator equal to 1 for counties with only one local newspaper and 0 for counties with more than one newspaper. Consistent with our conjecture, we find that the effect is concentrated in counties with one local newspaper.

As a second approach to mitigate the concern about changes in the underlying economics or fraud environment driving our results, we follow Gao et al. (2020) and use the expansion of Craigslist across the United States as an instrument for newspaper closures.[5] The expansion of Craigslist represents a plausibly exogenous shock to the probability of a newspaper closure without directly affecting facility-level misconduct. In the first-stage

[5] Craigslist is an online database of classified advertisements, and prior research has shown that advertising revenues for local newspapers were negatively affected by its expansion (e.g., Gurun and Butler, 2012; Kroft and Pope, 2014; Seamans and Zhu, 2014; Gurun et al., 2016; Gao et al., 2020).

regression, we find that Craigslist's entry increases the probability of a newspaper closure, meeting the instrument relevance condition. Consistent with the main results, the second-stage regression estimates show that Craigslist-induced newspaper closures increase facility-level misconduct. In sum, these tests mitigate the concern that changes in the underlying economics or fraud environment drive our results.

Our study makes three contributions to the literature on the role of the press in curtailing corporate misconduct. First, we provide the first systematic evidence showing that the *local* press is an effective monitor of corporate misconduct. This is an important finding, as prior studies suggest that local newspapers are ineffective monitors of local firms' behavior (e.g., Gurun and Butler, 2012; Shapira and Zingales, 2017). We show that local newspaper closures increase facility-level misconduct, highlighting the importance of local newspapers as monitors to curtail corporate misconduct.

Second, while a few studies suggest that the press is useful for exposing corporate fraud (e.g., Miller, 2006), empirical evidence on the monitoring role of the press is limited and mixed. Some studies find support for it (e.g., Dyck et al., 2008), while others do not (e.g., Core et al., 2008). As a result, in their review of the literature, Miller and Skinner (2015) call for more research on this topic. Our study contributes to this literature by documenting the monitoring role of local newspapers on a wide range of corporate violations that resulted in penalties, including securities law violations, environmental violations, consumer-protection violations, and workplace safety violations, among many others. Thus, our study provides a comprehensive analysis of the effect of local newspapers on firms' misconduct.

Finally, our findings improve our understanding of the consequences of reduced local newspaper coverage—an important topic in light of the decline in local newspapers in the United States (Pew Research Center, 2019). A major concern is that less local news leads to more local corruption and crime. Prior studies show that areas with less local press have higher

corruption by local politicians (e.g., Snyder and Strömberg, 2010; Gao et al., 2020). We contribute to this literature by providing evidence that reduced local newspaper coverage also affects firm behavior through increased misconduct.

2. Prior literature and hypotheses

2.1. Prior literature on the press as a monitor of corporate misconduct

Understanding whether the press is an effective monitor of corporate misconduct is an emerging research area. Miller (2006), for example, finds that the press helps expose accounting fraud by rebroadcasting information from other information intermediaries (analysts, auditors, and lawsuits) and by undertaking original investigations. Building on that study, a set of papers examine whether the press can identify and report on governance problems and whether firms, in turn, alter their behavior in response. Extant research examines this question using specific settings such as insider trading (e.g., Dai et al., 2015), corporate governance violations in Russia (e.g., Dyck et al., 2008), environmental violations (Dyck and Zingales, 2002), and excessive executive compensation (e.g., Core et al., 2008; Kuhnen and Niessen, 2012). The empirical evidence from these studies is mixed, with some showing that the press disciplines firm behavior (e.g., Dyck et al., 2008) and others finding that it is ineffective in altering firms' conduct (e.g., Core et al., 2008).

For example, Dyck et al. (2008) examine governance changes in Russian firms as a response to press coverage. They show that negative coverage by the Anglo-American press leads to improvements in governance and conclude that the broader dissemination of the governance problems pressures the firm and Russian regulators to change their behavior. In contrast, Core et al. (2008), who study whether the press monitors executive compensation, find that firms do not alter compensation in response to press coverage, which suggests that the media is not playing an effective monitoring role. In light of this limited and mixed evidence, Miller and Skinner (2015) call for more research on the monitoring role of the press.

Our study answers this call by examining whether the *local* press is an effective monitor of firms' corporate misconduct. While we focus specifically on the role of the *local* press (as systematic evidence is absent), we use a wide set of different types of legal violations to provide a comprehensive analysis of the monitoring role of the local press.

2.2. The local press as a monitor of corporate misconduct

The effect of local newspaper closures on firms' legal violations is unclear. On the one hand, the local press could be an effective monitor of firms and hence affect corporate misconduct via its investigations or dissemination (e.g., Miller, 2006; Dyck et al., 2008; Dyck et al., 2010). For example, Miller (2006) shows that the national press undertakes original investigations to detect corporate fraud and disseminates information about wrongdoing. Thus, similar to the national press, the local press could investigate firms to detect fraud. Similarly, Miller and Shanthikumar (2015) show that local newspapers provide greater coverage of local firms than national newspapers. Local newspapers could be especially effective in discovering misconduct of local firms and facilities because of their proximity to local sources such as employees and local suppliers (e.g., Engelberg and Parsons, 2011; Peress, 2014). The local press could also affect corporate misconduct by widely disseminating information about misbehavior (e.g., Dyck and Zingales, 2002; Dyck et al., 2008). Local newspapers play a central role in achieving wide dissemination of information because they provide the national press with credible information on local misconduct (Shapira and Zingales, 2017). Under this view, we would expect that the closure of a local newspaper weakens the monitoring of corporate misconduct, increasing facility-level violations.

Alternatively, prior work also provides arguments in support of the null hypothesis. Several studies find evidence that the local press is an ineffective monitor. Local newspapers generate approximately 60% of their revenues through advertising (Pew Research Center, 2019). Gurun and Butler (2012) show that advertising by local firms in local media creates a

conflict of interest, resulting in overly positive articles. Specifically, they find that when local newspapers report about local companies, they positively slant news about these firms. They argue that these findings suggest that the local press is susceptible to conflicts of interest from advertising, potentially undermining its effectiveness as a monitor. Similarly, Shapira and Zingales (2017) provide anecdotal evidence supporting Gurun and Butler's (2012) findings.[6] Beyond the conflict of interest arising from advertisement relationships, Shapira and Zingales (2017) also point to the local newspapers' incentive to cater to their consumers as a factor undermining local newspapers' monitoring ability. Prior research provides systematic evidence for the argument that the press caters to its readers (e.g., Mullainathan and Shleifer, 2005). Local newspapers are particularly dependent on local readers for subscription income. When local newspapers scrutinize local companies, which are often the biggest employers in town, they risk upsetting their readers (Shapira and Zingales, 2017). As a result, local newspapers may refrain from reporting critically about those firms. Furthermore, local newspapers often have only a small number of reporters on their payroll. Thus, these newspapers may not have sufficient resources to monitor local firms. Finally, local newspapers are typically limited in their reach, potentially reducing their reporting impact and rendering its monitoring ineffective. If local newspapers are ineffective at monitoring firms' behavior, local newspaper closures should not affect local facilities' violations. Ultimately, whether and the extent to which local newspaper closures affect misconduct by local firms are empirical questions.

3. Data

3.1. Violations and location of facilities

We obtain data on corporate misconduct from Violation Tracker, produced by the Corporate Research Project of Good Jobs First. The data are collected from more than 40

[6] Using DuPont's environmental scandal as a case study, Shapira and Zingales (2017) show that the local press was reluctant to criticize DuPont. They attribute this reluctance to DuPont's influence in terms of advertisement and financial support.

federal regulatory agencies and contain over 310,000 civil and criminal cases brought against firms since 2000, the beginning of the coverage period. Violation Tracker removes violations in which the penalty or settlement is lower than $5,000 (we do not observe the number of unique firms before Violation Tracker removes small violations). For completeness, Violation Tracker complements agency enforcement records with information collected on settlements announced in press releases. Joint ventures in which a parent company owns more than 50% are treated as owned facilities; otherwise, they are treated as independent companies. From the 310,000 violations, Violation Tracker links approximately 67,000 violations to 2,875 parent companies, representing close to 95% of the total penalty dollars.[7] From that database, we keep all cases in which the parent company is a publicly traded firm.[8] We drop violations by financial institutions. Violations in which the location of the misconduct is not available or is ambiguous are matched to a firm's headquarters location.[9] As we describe in more detail in the next section, we use a six-year window around the treatment date and drop all other years of treated facilities. In addition, we only include *treated* and *control* facilities if they are present in *both* the pre- and post-treatment periods, further reducing our sample. Our final data set consists of 26,450 violations perpetrated by 1,383 unique firms. Approximately 80% of Fortune 100 and Fortune 500 firms appear in our sample. We use a sample of 10,647 facilities with at least one violation belonging to these 1,383 firms for our main analyses. Table 1, Panel A describes our sample composition.

[7] Violation Tracker matches facilities to the *current* parent company, even if the facilities were part of a different corporate family at the time the penalty was imposed. We adjust for this choice by matching facilities to their *historical* parent over time (Raghunandan, 2021). While we ultimately cannot rule out false matches between facilities and Compustat firms in our sample, it is important to note that our primary tests are run at the facility level. Thus, our results are unlikely to be influenced by any potential false matches. For example, note that including firm-level control variables, which could be mismatched, has a minimal influence on the treatment effect (see Table 4, Columns 2 and 5).

[8] Violation Tracker matches facilities to the parent (Compustat) company based on information included in the enforcement actions, firms' disclosures, press releases, and firms' webpages. Violation Tracker also includes CIKs to facilitate matching to Compustat.

[9] The results are robust when excluding those violations (untabulated).

Note that the Violation Tracker database includes facilities with at least one violation during our sample period but not facilities with none. We access the Dun & Bradstreet Historical Duns Marketing Information (DMI) Files, which include annual establishment information, to obtain data on facilities' employees and sales.[10] To circumvent the concern that systematic differences between violation and non-violation facilities affect our results, we run our primary analyses using facilities with at least one violation at any point in the 18 years of our sample period. However, we run additional tests repeating our primary analyses using a sample that also includes non-violation facilities that report sales (see Table 10, Panel B). We obtain information on the location of these non-violation facilities from the Dun & Bradstreet DMI files.

Panel B provides summary statistics about the violations in the sample. The average firm engages in approximately 1.25 violations per year, with mean penalties amounting to $1,541,848. The average facility engages in approximately 0.16 violations per year, with mean penalties amounting to $197,851. In the average county, approximately 0.84 violations occur per year, with mean penalties of approximately $1 million. To mitigate the concern that outliers affect the estimates of the economic magnitudes, we winsorize our dependent variables at the 99th percentile throughout all tests.[11] Panel C provides a breakdown of the annual sum of the number of violations and penalties as well as the annual median penalty amount. Violations in our sample are scattered through time in terms of both the number of violations and penalties. The year with the highest number of violations is 2011 (9.8% of total), and the year with the

[10] Barnatchez et al. (2017) find that the D&B data can be unreliable for establishments with less than ten employees. In untabulated analyses, we rerun our primary tests and exclude all facilities with fewer than ten employees. Our results hold. We can find all facilities included in the Violation Tracker data set also in the Dun & Bradstreet DMI files.

[11] In untabulated tests, we find similar economic magnitudes to those in Table 4 when we (i) winsorize at the 97th percentile, (ii) winsorize at the 95th percentile, (iii) exclude facility-years with penalties larger than $10 million, or (iv) exclude facility-years with more than 100 violations per year. These results suggest that our estimates are largely unaffected by outliers in the distribution of our dependent variables.

highest amount of penalties is 2013 (14.3% of total).[12] Throughout our sample period, the median dollar penalty amount is approximately $15,000.

Our sample comprises a wide variety of offenses, including workplace safety violations, environmental violations, labor relations violations, employment discrimination violations, False Claims Act violations, securities violations, and consumer protection violations, among others. Panel D provides a breakdown of the number of violations and total penalties by offense type. In terms of the number of violations, workplace safety violations are the most prevalent, representing 56.7% of the total. However, in terms of the total penalty amount, this category represents only 0.7%. In contrast, False Claims Act violations account for only 0.5% of the number of violations but 24.4% of the total penalties.

– Insert Table 1 here –

3.2. Local Newspaper Closures

We obtain data on closures of U.S. daily newspapers from 2000 to 2017 from three data sources.[13] First, we collect data on daily newspapers for the years 2000 and 2004 from the United States Newspaper Panel constructed by Gentzkow et al. (2011). Second, we collect newspapers using UNC's Center for Innovation and Sustainability in Local Media's Database of Newspapers, which contains the name and geographical location of all daily and weekly newspapers in the U.S. in 2004, 2014, 2016, and 2019. We first collect daily newspapers that disappear across years and manually search for the year of the identified closures. Lastly, we complement the data by scraping content from the U.S. Newspaper Directory of Chronicling

[12] The total number of violations and penalties can differ across years due to changes in enforcement priorities or other factors. For example, the number of violations in our sample increases substantially from 2006 to 2007 due to more workplace safety violations. The increase in workplace safety violations appears to be the result of several strategic initiatives that OSHA started in the early 2000s to strengthen enforcement of workplace safety violations, such as Site Specific Targeting (SST), Local Emphasis Programs (LEPs), National Emphasis Programs (NEPs), and the Enhanced Enforcement Program (EEP). For an enforcement summary of OSHA in 2007, please see https://www.osha.gov/dep/enforcement/enforcement_results_07.html. As we discuss in more detail in Section 5.5, our results are not driven by any particular type of offense.

[13] We begin collecting data on newspaper closures in 2000 because Violation Tracker only provides data on corporate misconduct beginning in 2000.

America. Following Gentzkow et al. (2011), we match newspapers to counties based on the cities in which they are located using the 2010 U.S. Census county definition. The combined data set contains the name, city, and state of every daily newspaper in each year from 2000 to 2017. We match newspapers that are located on the border of two counties to both counties (Gao et al., 2020). Our sample consists of 1,637 newspapers serving 1,270 counties between 2000 and 2017.

We define years when a U.S. daily local newspaper closed as treatment years. We do not consider mergers, changes in frequency from daily to weekly, or changes to online only as treatment events because these events do not necessarily reduce local-news availability.[14] As we only include treated and control facilities if they are present in both the pre- and post-treatment periods, newspaper closures before 2003 and after 2015 are excluded from our analyses. During our sample period, we classify 33 newspaper closures as treatments. These closures affect 1,357 facilities in 49 counties and are not clustered geographically (see Fig. 1 for a map) or in time (see Table 2). We use a six-year window around the treatment date, i.e., treated facilities are included from three years before the treatment to three years after the treatment (but excluded for all other years). We use a six-year window because during this treatment window, it is plausible to expect that a newspaper closure would affect facility-level misconduct.[15]

– Insert Figure 1 and Table 2 here –

Anecdotal evidence suggests that local newspapers in our sample report about local facilities of publicly listed firms. For example, the *Rocky Mountain News*, a Denver newspaper that won four Pulitzer Prizes between 2000 and its closure in 2009, broke the story on

[14] We find consistent results when considering these additional events as treatments (see Table 10, Panel C), but the statistical as well as economic significance of the treatment effects is smaller.

[15] For example, there might not be an immediate effect of newspaper closures on facility-level misconduct, and after some years the effect might diminish. Regarding the former point, the tests reported in Table 10, Panel B show that the effect of newspaper closures on misconduct occurs in the years *t+1* and *t+2* after the closure, but we do not find a significant effect in year *t*.

fraudulent behavior at Qwest, a publicly listed company headquartered in Denver (Accola et al., 2003). One year later, Qwest was subject to an SEC enforcement action.[16] Similarly, the *Tampa Tribune*, which closed in 2016 after operating for 123 years, was known for its local investigative reporting and received numerous awards (Madigan, 2016).[17] A search of *Tampa Tribune* articles reveals that this newspaper covered local firms extensively. For example, in a series of articles, the *Tampa Tribune* reported about fraudulent transactions at Wachovia Bank in Tampa, which is part of Wells Fargo.[18]

Other local newspapers that are still operating are known for disclosing corporate scandals of publicly listed companies located in their vicinity. For example, the *Seattle Times* has often been the first news outlet providing information on Boeing's 737 MAX incident developments. Similarly, the *Los Angeles Times* disclosed the Wells Fargo scandal in 2013.

4. Empirical methodology and results

4.1. Empirical methodology

We examine the effect of newspaper closures on facility-level misconduct using a difference-in-differences methodology. The basic regression we estimate is as follows:[19]

$$Y_{ijlt} = \alpha_0 + \alpha_1 Treatment_{lt} + Controls + \alpha_j + \alpha_{st} + \epsilon_{ijlt}\,, \qquad (1)$$

where the dependent variable Y_{ijlt} is either the total dollar amount in penalties or the total number of violations that firm *i* incurred related to violations in its facility *j*, located in county *l*, during year *t*. The main explanatory variable $Treatment_{lt}$ takes the value of 1 for the three years following a newspaper closure in county *l*, and a value of 0 in the three years prior to a newspaper closure.[20]

[16] See https://www.sec.gov/news/press/2004-148.htm.
[17] For example, John Anthony Frasca of the Tampa Tribune won the Pulitzer Prize for local investigative special reporting in 1966.
[18] Note that the *Tampa Bay Times*, a local rival of the *Tampa Tribune*, originally reported about the bank fraud.
[19] Our results hold when using Poisson pseudo maximum likelihood regressions as alternative estimation technique (untabulated).
[20] Please note that although the standard formulation of a difference-in-differences includes two dummy variables and their interaction, this is not seen in Eq. 1 because of how *Treatment* is defined and because the facility and year fixed effects are suppressed.

This identification strategy allows us to employ a difference-in-differences methodology that exploits the staggered closure of newspapers over time. The first difference is the change in misconduct, measured either in terms of the total penalties or the total number of violations in each facility before and after the closure of a local newspaper. The implicit control group at time *t* consists of facilities located in areas without a newspaper closure. The change in misconduct within this control group is the second difference captured in our tests. The effect of newspaper closures on facility-level misconduct is estimated as the difference in those two differences.

Our tests include the following facility, firm, and county-level *Controls*. At the facility level, we control for size (measured as the natural logarithm of one plus facility-level revenues and one plus employees). We obtain information on the number of employees and sales volume at the facility level from Dun & Bradstreet. At the firm level, we control for size (measured as the natural logarithm of one plus total assets), profitability (measured as return on assets), and leverage. At the county level, we control for the size of the labor force and the unemployment rate to control for the macroeconomic conditions at the facilities' locations. All control variables are winsorized at the 1st and 99th percentiles. All variables are defined in the Appendix.

We employ facility and state-year fixed effects to control for omitted factors in our main analyses. Facility fixed effects control for time-invariant facility characteristics. The inclusion of state-year fixed effects implies that *Treatment* captures the effect of a newspaper closure in that county compared to other counties that experienced no newspaper closures within the same state and year. While our identification strategy controls for local and firm-specific shocks, facility fixed effects capture the baseline level of misconduct at each facility, and state-year fixed effects control for misconduct changes at the state level over time that are unrelated to the closure of newspapers. Standard errors are two-way clustered by state and

year.[21] Fig. 2 provides a graphical summary of our research design using the closure of the *Rocky Mountain News* as an example.

– Insert Figure 2 here –

Table 3 provides descriptive statistics on the variables included in our tests. Of the facility observations, 2.6% are subject to a newspaper closure. On average, facilities employ 564 employees and generate $3.2 million in sales. On average, the facilities are part of firms that have $27 billion in assets, a return on assets of 4.5%, and leverage of 35% and are located in areas with an average labor force of approximately 411,000 people and an unemployment rate of 6.2%.

– Insert Table 3 here –

4.2. Main results

To test our hypotheses, we conduct two analyses and report the results in Table 4. First, we examine whether dollar penalties from misconduct change at facilities after the closure of a local newspaper. To do so, we examine the natural logarithm of one plus the total dollar amount of penalties per facility and year (denoted *Penalties*) using Eq. (1). Second, we examine whether the number of violations changes at facilities after the closure of a local newspaper. To do so, we examine the natural logarithm of one plus the number of violations per facility and year (denoted *Number_Violations*) using Eq. (1). If the closure of a local newspaper affects misconduct at the facility level, we expect the dollar amount of penalties and the number of violations per facility to change following the closure.

Table 4 reports the results from estimating Eq. (1) using *Penalties* (Columns 1-3) and *Number_Violations* (Columns 4-6) as the dependent variables. The main difference across these models is the inclusion of different *Controls*. Specifically, Columns 1 and 4 do not include any *Controls;* Columns 2 and 5 include facility- and firm-specific *Controls;* and

[21] The results are robust to clustering at the facility or state level (see Table 10, Panel D).

Columns 3 and 6 also include facility-location *Controls*. All models include facility and state-year fixed effects. In all models, the coefficient associated with the treatment effect is positive and significant at $p<0.05$.

In terms of economic magnitude, the results indicate that the closure of a newspaper increases the dollar penalties and the number of violations in treated facilities by approximately 15.2% (based on Table 4, Column 3) and 1.1% (based on Table 4, Column 6), respectively. These findings indicate that newspaper closures increase misconduct in facilities, suggesting that local newspapers are, on average, effective monitors of firms. Moreover, comparing the magnitude of the *Treatment* coefficients across the models that use *Number_Violations* and *Penalties* as dependent variables suggests that the closure of local newspapers leads facilities to incur violations that carry higher penalties on average.

In terms of dollar magnitude, the increase in penalties for the average facility represents approximately 0.95% of facility-level sales. Given that the average newspaper closure affects 41.1 facilities in our sample, the closure of a local newspaper increases penalties by approximately $1,235,839 over three years. Three aspects of our research design should be considered when interpreting these figures. First, our tests focus on the facilities of publicly traded firms and ignore penalties incurred by private firms, which represent approximately 31% of total penalties. Second, our estimates rely on detected violations, as undetected misconduct is unobservable. Third, our research design and our inferences capture a partial equilibrium rather than a general equilibrium. For example, part of the effect could be driven by firms strategically shifting misconduct across their facilities in response to local newspaper closures.

– Insert Table 4 here –

4.3. Enhancing identification

In this section, we present several tests aimed at mitigating different types of identification concerns. We first present a set of tests to mitigate the concern that both the closure of local newspapers and the increases in facility-level violations are driven by changes in the underlying local economic conditions or local fraud environment (e.g., Glaeser et al., 1996; Parsons et al., 2018). We address this concern in three ways. Specifically, we conduct i) a set of cross-sectional tests, ii) an instrumental variable test, and iii) a falsification test. Finally, we run tests with alternative fixed-effects structures to mitigate the concern that our results can be explained by unobservable industry-year or firm-year effects. We describe all of these tests in more detail in the following sections.

4.3.1. Cross-sectional tests

In this section, we examine cross-sectional predictions examining whether variation in the strength of monitoring by the local press alters the effect of local newspaper closures on facility-level misconduct. We focus on two aspects that likely affect the strength of monitoring by local newspapers: i) the newspapers' reporting incentives and ii) the availability of other local newspapers.

Reporting incentives of newspapers

We explore whether differences in newspapers' reporting incentives moderate the effect of newspaper closures on facility-level misconduct. Prior research shows that the press tends to focus on visible firms in its reporting and that these firms are subject to more scrutiny (e.g., Miller, 2006). Hence, we expect that the effect of newspaper closures on facility-level misconduct is stronger for facilities of visible companies, because closures more strongly reduce the dissemination of information on misconduct by these facilities. We use local media coverage and firm size to identify visible facilities. We set *High_Local_Media_Coverage* to 1 if the facility belongs to a firm above the median level of local-newspaper coverage (measured by the number of articles written by local newspapers), and 0 otherwise. We obtain data on local newspaper articles from Ravenpack, and exclude articles published by national

newspapers (i.e., articles published in *Barrons*, the *Los Angeles Times*, newswire services, the *New York Times*, *USA Today*, the *Wall Street Journal*, the *Washington Post*, or the *Washington Times*). Similarly, we set *Large_Firm* to 1 if the facility belongs to a firm above the median firm asset size, and 0 otherwise. In these tests, we also control for social media coverage by including the natural logarithm of one plus the number of *Google_Searches* as an additional control variable, as prior studies highlight that social media can facilitate disseminating information about local firms (Baloria and Heese, 2017; Miller and Skinner, 2015).

Table 5, Panels A and B report the results. Column 1 uses penalties as the dependent variable, while Column 2 uses the number of violations as the dependent variable. We find that the coefficient on *Treatment x High_Local_Media_Coverage* is positive and significant at $p<0.10$. Similarly, we find that the coefficient on *Treatment x Large_Firm* is positive and significant at $p<0.10$. We also find that the coefficients on *Treatment* are statistically insignificant. These results suggest that facilities of more visible firms increase misconduct more strongly following the closure of a local newspaper. Note that the coefficient on *Google_Searches* is small in magnitude and statistically insignificant, suggesting that social media coverage does not affect firms' corporate misconduct during our sample period.

Availability of local information

We examine whether the availability of local information influences the effect of newspaper closures on facility-level misconduct. Following Gao et al. (2020), we focus on the number of local newspapers to capture the availability of local information. A newspaper closure is unlikely to have a strong effect on facility-level misconduct when there are many other local newspapers (e.g., Gentzkow et al., 2014; Gao et al., 2020). We interact *Treatment* with *Low_Number_Newspapers*, which equals 1 if the number of local newspapers is one, and 0 otherwise.

Table 5, Panel C reports the results. Column 1 uses penalties as the dependent variable, while Column 2 uses the number of violations as the dependent variable. We find that the coefficient on *Treatment x Low_Number_Newspapers* is positive and significant at $p<0.05$. We also find that the coefficients on *Treatment* are statistically insignificant. Consistent with our conjecture and prior research, these results indicate that the effect of newspaper closures on misconduct is concentrated in facilities located where there are fewer local newspapers. In sum, the results from these cross-sectional tests mitigate the concern that our results are explained by changes in the underlying local economics or the local fraud environment.

– Insert Table 5 here –

4.3.2. Craigslist

To further mitigate the concern that unobservable changes in the local economics or fraud environment explain our results, we use an instrumental variable approach similar to Gao et al. (2020). Specifically, we use the expansion of Craigslist across the United States as an instrument for newspaper closures. Craigslist is an online database of classified advertisements, and prior research has shown that advertising revenues for local newspapers were negatively affected by its expansion (e.g., Gurun and Butler, 2012; Kroft and Pope, 2014; Seamans and Zhu, 2014; Gurun et al., 2016; Gao et al., 2020). The expansion of Craigslist in the United States to a local area represents a plausibly exogenous shock to the probability of a newspaper closure without directly affecting local facility-level misconduct. We obtain the list of Craigslist sites and their entry years from www.craigslist.org.

We report the results of this test in Table 6. In the first-stage regression, we examine whether Craigslist's entry increases the probability of a newspaper closure. Specifically, we examine the likelihood of a local newspaper closure in the three years following Craigslist's entry into a specific county. In these tests, we exclude counties that do not experience Craigslist's entry during our sample period. As shown in Table 6, Column 1, we find that

Craigslist's entry is positively and significantly associated with the likelihood of local newspaper closure, meeting the instrument relevance condition. In the second-stage regression, we examine whether Craigslist-induced newspaper closures increase violations and penalties. To mimic the research design of our primary tests, we use the difference in the natural logarithm of the sum of one plus the dollar amount of penalties per facility over the subsequent three years minus the preceding three years, and the natural logarithm of the sum of one plus the number of violations per facility over the subsequent three years minus the preceding three years, as the dependent variables. As shown in Table 6, Columns 2 and 3, we find that Craigslist-induced newspaper closures increase violations and penalties, establishing a more robust connection between newspaper closures and facility-level violations.

– Insert Table 6 here –

4.3.3. Falsification test

Finally, we run a falsification test to provide further evidence that the results are unlikely to be driven by unobservable characteristics of the treated counties. Specifically, we randomize the year of the newspaper closure (i.e., we shift around the event for the treated facilities). If underlying characteristics in areas with newspaper closures drive our results, we would expect to find similar results using randomized treatment years.

Table 7 reports the results from this test. In the randomization test, we use a uniform distribution to generate 1,000 random placebo dates for the treated newspapers and report the average β_1 coefficient of estimating the model specified in Eq. (1). Across our two dependent variables, the coefficients based on the random data are close to zero and differ statistically and economically from the results using the actual data. Thus, these results provide additional

evidence ruling out the alternative explanation of unobservable county characteristics driving our results.[22]

– Insert Table 7 here –

4.3.4. Propensity score matching

We also examine the robustness of our main results using a propensity-score-matched sample to further mitigate the concern that facilities located in areas with and without local newspaper closures are systematically different and were nonrandomly assigned to their respective groups. For these tests, we use a propensity-score-matching approach to match each facility located within a county subject to a local newspaper closure to a facility located in an area without a local newspaper closure from the *same state* and *two-digit-SIC-code industry* and of similar characteristics along our *facility* and *firm* control variables. We match treated and control observations in the year before the newspaper closure using a predefined propensity score radius (or "caliper") of 0.05 without replacement. Of the 1,357 treated facilities in our sample, we find a matched pair for 1,034. Some counties have more than one newspaper closure. Hence, our final matched sample consists of 2,002 treated and 2,002 control observations. As reported in Table 8, Panel A, the mean comparisons of matched pairs indicate that the matching procedure successfully balances covariates. Consistent with our main results, Panel B shows that, after the treatment, treated facilities have significantly higher violations and penalties than control facilities. Using the matched sample, Figs. 3 and 4 plot the effect of newspaper closures in event time.

– Insert Table 8 and Figures 3 and 4 here –

4.3.5. Different fixed effects structures

[22] In untabulated tests, we also examine whether regulators respond to the closure of a local newspaper by conducting more investigations in the area affected by the newspaper closure. Using data on Occupational Safety and Health Administration (OSHA) inspections, we do not find a significant change in OSHA inspections after the closure of a local newspaper.

As described above, our primary analyses include facility and state-year fixed effects. We also examine whether our results are robust to alternative fixed-effects structures. First, we replace state-year fixed effects with industry-year fixed effects to rule out that unobservable industry-specific shifts over time drive our results. As shown in Columns 1 and 3 of Table 9, our results are robust to the inclusion of industry-year fixed effects.

Second, we replace state-year fixed effects with firm-year fixed effects to rule out that unobservable changes at the firm level that affect facility-level misconduct drive our results. Companywide changes, such as the introduction of new policies, can result in changes in facility-level misconduct. Although it is unlikely that companywide changes are correlated with local newspaper closures, this test helps us rule out this alternative explanation. As shown in Table 9, Columns 2 and 4, our results hold.

– Insert Table 9 here –

5. Additional tests

In this section, we present a set of tests that examine i) the dynamic effects around the closure of newspapers, ii) the effect of local newspapers on facility-level misconduct using a sample that also includes facilities without violations, iii) alternative treatments, iv) alternative clustering, and v) alternative dependent variables, such as toxic emissions.

5.1. Dynamic effects

In this subsection, we examine how the effect of local newspaper closures on facility-level misconduct evolves in the years surrounding the closure. Specifically, we estimate the main effect in the single-year treatment windows that range from one to three years before the newspaper closure to one to two years after the newspaper closure, using the year before the newspaper's closure as the baseline.

Table 10, Panel A shows the results. We find that the coefficients on $Treatment_{t-3}$ and $Treatment_{t-2}$ are statistically insignificant, indicating that treated and control facilities are

indistinguishable from each other before the newspaper closure. This finding further mitigates the concern about correlated omitted variables driving newspaper closures and facility-level misconduct. We also find a positive and significant coefficient on $Treatment_t$ to $Treatment_{t+2}$, indicating that the effect of newspaper closures on facility-level misconduct occurs after the newspaper closure.

5.2. Large sample of facilities with and without violations

As described above, our primary analyses focus on facilities with *violations* but do not include facilities without violations. We use data from Dun & Bradstreet to complement our sample, and identify 25,905 non-violation facilities that report sales at least once during our sample period.

We estimate our primary model using the sample of violation and non-violation facilities, resulting in a panel of 411,421 observations. Note that with sales of approximately $273,000, the average non-violation facility is more than ten times smaller than the average facility in the violation sample (see Table 3), indicating that violation and non-violation facilities are different. We repeat our main analyses using penalties (Column 1) and the number of violations (Column 2) as the dependent variables. As shown in Table 10, Panel B, we find a positive and significant coefficient on *Treatment* in both models. In terms of economic magnitude, the results indicate that the closure of a newspaper increases the dollar penalties and the number of violations in treated facilities by approximately 15.8% and 1.04%, respectively. These magnitudes are similar to those reported in Table 4.

5.3. Alternative treatments

As described above, our primary tests only define years when a U.S. daily local newspaper closed as treatment years. The advantage of this approach is that it captures events that sharply reduce local-news availability. However, we also rerun our primary tests considering newspaper mergers (57 events), changes in frequency from daily to weekly (54

events), and changes to online only (10 events) as additional treatment events. As shown in Table 10, Panel C, we find consistent results. Possibly because the reduction in information availability from these alternative treatment events is smaller, the economic and statistical significance of the treatment effect is smaller.

5.4. Alternative clustering

In our primary tests, we cluster the standard errors by state and year. We also rerun our primary tests clustering by state or facility. As shown in Table 9, Panel D, we find consistent results using these alternative clustering approaches.

5.5. Alternative dependent variables

We examine the robustness of our primary results to excluding each major violation category one at a time to investigate whether a specific violation category drives our results. In these tests, we exclude either workplace safety violations (the category with the largest number of violations), environmental violations, False Claims Act violations, or violations related to the promotion of unapproved medical products (the three categories with the largest penalties). As shown in Table 10, Panel E, the results are not driven by any particular category.

5.6. Toxic releases

While our primary tests focus on corporate misconduct that resulted in regulatory penalties, we also examine whether local newspaper closures result in higher toxic releases (e.g., Xu and Kim, 2021). We obtain Toxics Release Inventory (TRI) data from the Environmental Protection Agency (EPA) for the period 2000-2017. We focus on the facilities in our violation sample that also appear in the TRI data set. Our sample for these tests consists of 862 facilities belonging to 236 unique firms from 2000-2017. Consistent with our primary results, we find that local newspaper closures result in significantly higher toxic releases following a local newspaper closure (see Table 10, Panel F). More specifically, we find that

local newspaper closures increase toxic releases by approximately 18.3%, an effect that is similar in magnitude to our main results.

– Insert Table 10 here –

6. Conclusions

This paper examines the effect of local newspaper closures on facility-level misconduct. We find that local newspaper closures increase penalties by 15.2% and violations by 1.1% at the facility level. These results, robust to identification concerns, provide evidence that local newspapers are an important monitor of firms' misconduct.

Our study makes three contributions to the literature on the role of the press in curtailing corporate misconduct. First, our results provide the first systematic evidence showing that the *local* press is an effective monitor of corporate misconduct. Second, our findings contribute to the literature on the press in general as a monitor of corporate behavior, which has found mixed evidence on the monitoring role of the press. Finally, our findings also increase our understanding of the consequences of reduced local newspaper coverage, an important topic in light of the decline in local newspapers in the U.S. (Pew Research Center, 2019).

Appendix

Variable Definitions.

The following variables are constructed using data from Violation Tracker's data set of corporate misconduct [VT], data on facilities from Dun and Bradstreet [D&B], data on google searches per firm from Google Trends [GOOGLE], data on newspaper closures from the United States Newspaper Panel constructed by Gentzkow et al. (2011) [PANEL], UNC's Center for Innovation and Sustainability in Local Media's Database of Newspapers [UNC] and the U.S. Newspaper Directory of Chronicling America [DIR], data on newspaper articles from Ravenpack [RP], Compustat [C], data on county characteristics from the Bureau of Labor Statistics [BLS], data on Craigslist from www.craigslist.org [CRAIGSLIST], and data on toxic releases from the EPA's Toxic Release Inventory data set [TRI].

A. Variables of Interest	
Penalties	The natural logarithm of one plus total penalties for misconduct per facility and year winsorized at the 99th percentile. [VT]
Number_Violations	The natural logarithm of one plus the number of violations per facility and year winsorized at the 99th percentile. [VT]
Treatment	Indicator variable that is set to 1 in the three years following the closure of a local newspaper, and 0 in the three years prior to the closure. [PANEL + UNC + DIR]
High_Local_Media_Coverage	Indicator variable that is set to 1 if the number of articles published in local newspapers about the firm is larger than the median, and 0 otherwise. We obtain data on articles from Ravenpack, and exclude articles published by the following national newspapers: *Barrons*, the *Los Angeles Times*, newswire services, the *New York Times*, *USA Today*, the *Wall Street Journal*, the *Washington Post*, and the *Washington Times*. [RP]
Large_Firm	Indicator variable that is set to 1 if the facility is part of a firm larger than the median asset size, and 0 otherwise. [C]
Low_ Number_Newspapers	Indicator variable that is set to 1 if the number of local newspapers per county is three or smaller than three, 0 otherwise. [PANEL + UNC + DIR]
Craigslist_Entry	Indicator variable that is set to 1 in the three years following Craigslist's entry into a specific county. We obtain the list of Craigslist sites and their entry years from www.craigslist.org. [CRAIGSLIST]
Toxic_Releases	The natural logarithm of one plus the toxic releases in tons per facility and year. [TRI]
B. Controls	
Employees_Facility	The natural logarithm of one plus the number of employees per facility. [D&B]
Sales_Facility	The natural logarithm of one plus sales per facility (in thousands of dollars). [D&B]
Size	The natural logarithm of one plus the firm's market value (in millions of dollars). [C]
Leverage	The ratio of total liabilities to total equity. [C]
ROA	Net income scaled by total assets. [C]
Labor_Force	The natural logarithm of one plus the labor force per county. [BLS]
Unemployment_Rate	The unemployment rate per county adjusted for the average state unemployment rate. [BLS]
Google_Searches	The natural logarithm of one plus the number of google searches per firm and year [GOOGLE].

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Fig. 1

Map of newspaper closures.

This map shows the geographic distribution of the 45 counties affected by the 33 local newspaper closures used in our analyses across the United States.

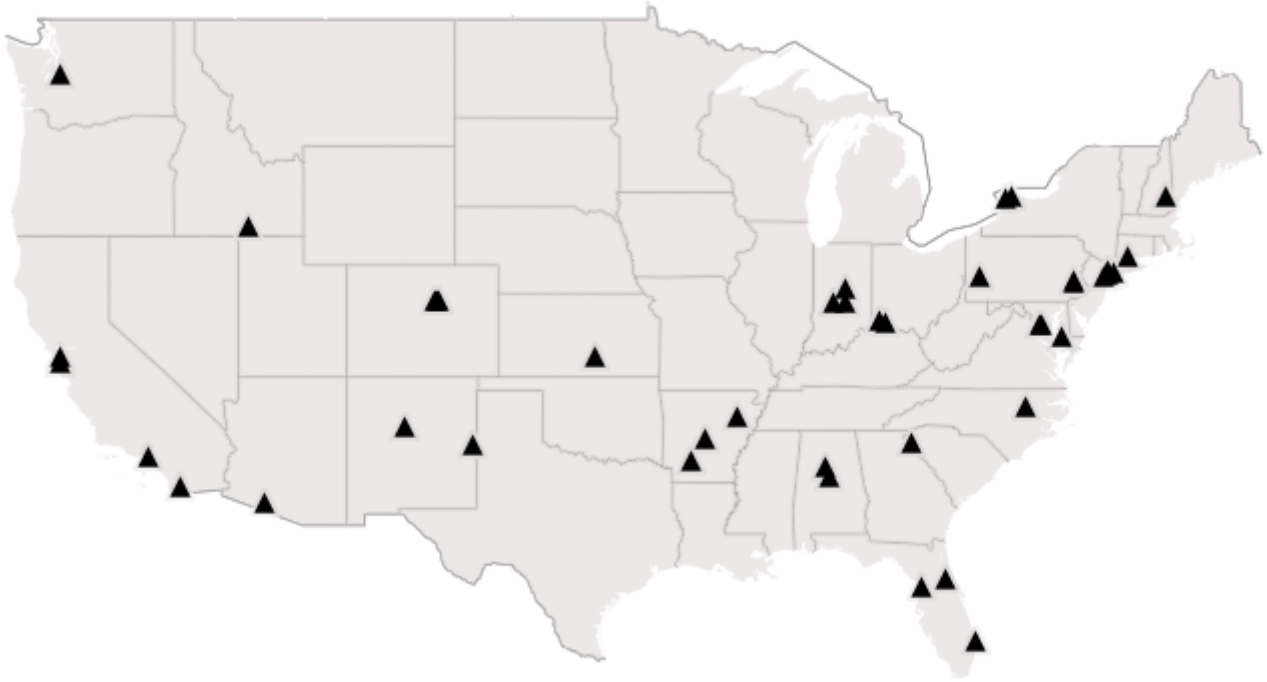

Fig. 2

Research design.

This figure provides an example from our sample to better illustrate how we code the *Treatment* variable for our analyses. Consider a facility located in Denver County, Colorado. In 2009, the *Rocky Mountain News* ceased publication (therefore 2009 is the first treatment year). We use a six-year window around the treatment date, meaning treated facilities are included from three years before the treatment to three years after the treatment. We only include treated and control facilities if they are present in both the pre- and post-treatment periods. Other facilities of the same firm or some other firm located elsewhere form the control group. The closure of local newspapers occurs at different points in time for our sample firms, affecting the time series of the *Treatment* variable. Each 0/1 coded cell (emphasized in bold) represents a facility-year observation included in our analysis.

Facility	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017
Facility located in Denver County							**0**	**0**	**0**	**1**	**1**	**1**						
Facility of the same firm ***not*** located in Denver County	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**
Facility of other firm ***not*** located in San Diego's North County	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**

Fig. 3

Treatment effects for penalties.

This graph plots the coefficient on *Treatment* (and the 95% confidence intervals) using the natural logarithm of one plus the dollar amount of penalties as the dependent variable around the newspaper closure for the propensity-score-matched sample.

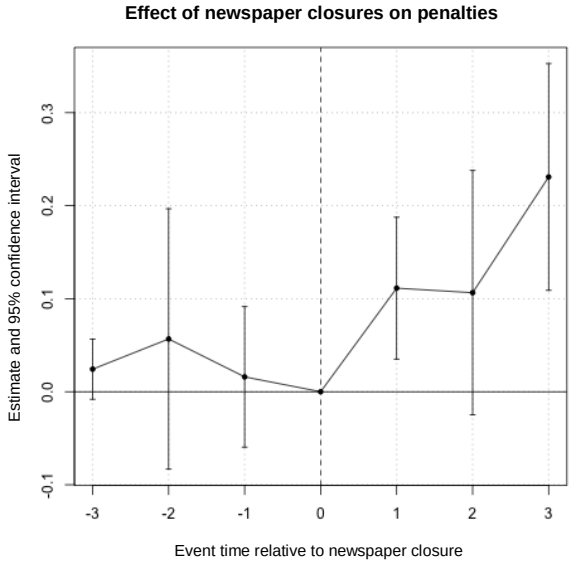

Fig. 4

Treatment effects for violations.

This graph plots the coefficient on *Treatment* (and the 95% confidence intervals) using the natural logarithm of one plus the number of violations as the dependent variable around the newspaper closure for the propensity-score-matched sample.

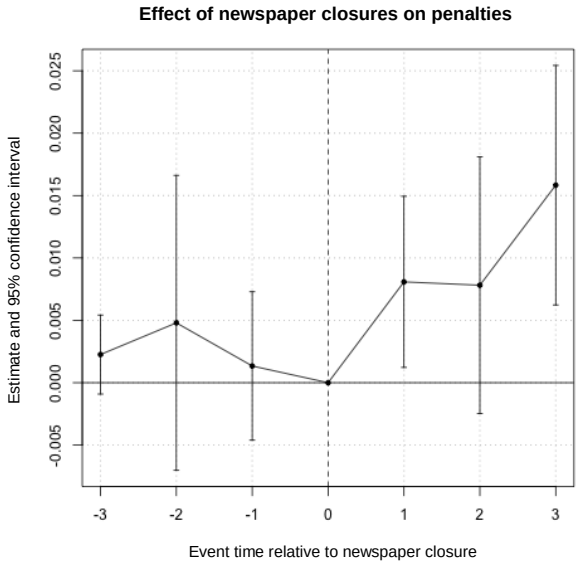

Table 1

Sample.

This table presents information on the sample composition for the period 2000-2017. Panel A presents the sample composition for the period 2000-2017. Panel B presents the summary statistics on the number of violations and penalties by facilities, firms, and counties for the period 2000-2017. Panel C presents the distribution of the sum of violations and penalties as well as the median penalty amount in our sample for the period 2000-2017 by year. Panel D presents the sample composition for the period 2000-2017 by offense type.

Panel A: Sample composition

	Number of Violations (1)	Number of Firms (2)	Number of Facilities (3)
All violations	310,000		
Less: Not linked by Violation Tracker [a]	(243,000)		
Violation Tracker sample	67,000	2,875	
Less: Private companies	(23,637)	(1,362)	
Less: Financial industry	(5,231)	(130)	
Less: Treated firms, untreated periods	(2,134)	(0)	
Less: Missing control variables and incomplete data	(9,548)	(0)	
Final sample	26,450	1,383	10,647

[a] We do not observe the number of unique firms before Violation Tracker removes small violations.

Panel B: Summary statistics violations by facilities, firms, and counties

Facility-Years Sample (N=164,128)											
	Mean	Std.	Min.	5th	10th	25th	Median	75th	90th	95th	Max.
Number of Violations	0.161	1.989	0	0	0	0	0	0	0	1	243
Penalties (in $)	197,851	11,882,840	0	0	0	0	0	0	0	10,000	2,610,000,000
Firm-Years (N=19,341)											
	Mean	Std.	Min.	5th	10th	25th	Median	75th	90th	95th	Max.
Number of Violations	1.250	10.414	0	0	0	0	0	1	2	3	741
Penalties (in $)	1,541,848	34,181,340	0	0	0	0	0	10,001	91,612	468,373	2,612,400,000
County-Years (N=28,326)											
	Mean	Std.	Min.	5th	10th	25th	Median	75th	90th	95th	Max.
Number of Violations	0.836	5.367	0	0	0	0	0	0	1	3	273
Penalties (in $)	1,016,743	28,081,260	0	0	0	0	0	0	31,094	125,334	2,610,000,000

Panel C: Sample composition by year

Year	Number of Violations	% of Total	Penalties ($m)	% of Total	Median Penalty ($)
2000	706	2.7%	1,278	3.9%	10,963
2001	750	2.8%	171	0.5%	12,155
2002	703	2.7%	152	0.5%	12,600
2003	721	2.7%	2,655	8.2%	15,000
2004	892	3.4%	1,200	3.7%	15,875
2005	912	3.4%	1,208	3.7%	16,500
2006	970	3.7%	1,951	6.0%	13,200
2007	1,638	6.2%	621	1.9%	18,000
2008	1,651	6.2%	440	1.4%	16,895
2009	1,974	7.5%	5,093	15.7%	15,000
2010	2,403	9.1%	1,789	5.5%	16,450
2011	2,587	9.8%	1,761	5.4%	16,834
2012	2,373	9.0%	3,476	10.7%	17,942
2013	1,511	5.7%	4,648	14.3%	14,850
2014	1,670	6.3%	1,224	3.8%	17,050
2015	1,585	6.0%	1,331	4.1%	14,700
2016	1,855	7.0%	1,937	6.0%	16,550
2017	1,549	5.9%	1,538	4.7%	15,229
Total	26,450	100%	32,473	100%	15,375

Panel D: Sample composition by offense type

Offense Type	Number of Violations	% of Total	Penalties ($m)	% of Total
Workplace safety or health violation	15,006	56.7%	238.5	0.7%
Environmental violation	2,924	11.1%	6,432.9	19.8%
Railroad safety violation	2,244	8.5%	23.1	0.1%
Labor relations violation	1,467	5.5%	291.0	0.9%
Aviation safety violation	1,084	4.1%	133.0	0.4%
Employment discrimination	537	2.0%	662.2	2.0%
Wage and hour violation	483	1.8%	2,029.0	6.2%
Motor vehicle safety violation	327	1.2%	9.5	0.0%
False Claims Act	135	0.5%	7,924.6	24.4%
Benefit plan administrator violation	87	0.3%	784.1	2.4%
Export control violation	84	0.3%	181.3	0.6%
Securities violation	37	0.1%	1,478.5	4.6%
Consumer protection violation	28	0.1%	665.0	2.0%
Unapproved promotion of medical products	11	0.0%	7,795.6	24.0%
Other	1,996	7.5%	3,824.8	11.8%
Total	26,450	100%	32,473	100%

Table 2
Summary statistics newspaper closures.

This table presents the distribution of newspaper closures in our sample for the period 2000-2017 by year. We only include treated and control facilities if they are present in both the pre- and post-treatment periods. As our sample spans the period 2000-2017, this research-design choice implies that newspaper closures before 2004 and after 2015 are excluded from our analyses to ensure that the facilities are present both in the pre- and post-treatment windows.

Year	Number of Newspaper Closures	% of Total
2000	-	-
2001	-	-
2002	-	-
2003	1	3.0%
2004	0	0.0%
2005	2	6.1%
2006	0	0.0%
2007	3	9.1%
2008	5	15.2%
2009	6	18.2%
2010	0	0.0%
2011	1	3.0%
2012	1	3.0%
2013	2	6.1%
2014	7	21.2%
2015	5	15.2%
2016	-	-
2017	-	-
Total	33	100%

Table 3
Summary statistics facilities.

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

	Facility-Years Sample (N = 164,128)				
Variable	Mean	Std.	Min.	Median	Max.
Treatment	0.026	0.158	0	0	1
Employees_Facility	564	2,498	1	150	100,505
Sales_Facility (in thousands)	3,169	14,993	0.11	60.8	274,485
Size (in millions)	26,738	57,842	128	7,041	402,672
Leverage	0.348	0.449	0	0.261	3.269
ROA	0.045	0.067	–0.246	0.048	0.214
Labor_Force	411,066	701,327	3,131	168,789	5,054,938
Unemployment_Rate (in %)	6.22	2.50	1.10	5.60	28.90

Table 4
Newspaper closures and facility-level misconduct.

This table reports the estimation results from linear regressions of the following form:

$$Y_{ijlt} = \alpha_0 + \alpha_1 \, Treatment_{lt} + \alpha_j + \alpha_{st} + \phi \, Controls + \varepsilon_{ijlt}$$

Y is either the natural logarithm of one plus the dollar amount of penalties per facility and year (Columns 1-3) or the natural logarithm of one plus the number of violations per facility and year (Columns 4-6). Columns 1 and 4 report results without *Controls*. Columns 2 and 5 report results with facility-level and firm-level *Controls*. Columns 3 and 6 report results with facility-level, firm-level, and county-level *Controls*. Our main explanatory variable is *Treatment*, which takes the value of 1 for the three years after the closure of a local newspaper, and 0 in the three years prior to the closure of a local newspaper. All variables are defined in the Appendix, and the sample spans the period 2000-2017. Standard errors are two-way clustered by year and state. Standard errors are reported below the coefficients. *, **, and *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable		Penalties			Number_Violations		
Variables	Pred.	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	+	0.1568**	0.1537**	0.1519**	0.0110**	0.0107**	0.0106**
		(0.0619)	(0.0618)	(0.0657)	(0.0046)	(0.0046)	(0.0048)
Employees_Facility			0.0559***	0.0559***		0.0043***	0.0044***
			(0.0115)	(0.0126)		(0.0009)	(0.0009)
Sales_Facility			0.0027	0.0027		0.0003	0.0003
			(0.0048)	(0.0052)		(0.0004)	(0.0004)
Size			0.2535***	0.2516***		0.0210***	0.0209***
			(0.0421)	(0.0765)		(0.0039)	(0.0051)
Leverage			0.0627	0.0590		0.0030	0.0027
			(0.0593)	(0.0695)		(0.0043)	(0.0043)
ROA			0.3193**	0.3080**		0.0023**	0.0022**
			(0.1165)	(0.1217)		(0.0092)	(0.0090)
Labor_Force				0.4135***			0.0283***
				(0.0363)			(0.0035)
Unemployment_Rate				–0.0505**			–0.0046**
				(0.0230)			(0.0017)
Facility FE		Yes	Yes	Yes	Yes	Yes	Yes
Year FE x State FE		Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-square		0.073	0.076	0.076	0.104	0.107	0.107
Observations		164,128	164,128	164,128	164,128	164,128	164,128

Table 5

Cross-sectional tests.

This table analyzes cross-sectional variation in the results of Table 4. Panel A shows results from cross-sectional tests based on local media coverage. *High_Local_Media_Coverage* equals 1 if the number of articles published in local newspapers about the firm is above the median, and 0 otherwise. Panel B shows results from cross-sectional tests based on firm size. *Large_Firm* equals 1 if the firm asset size is above the median, and 0 otherwise. Panel C shows results from cross-sectional tests based on the number of local newspapers. *Low_Number_Newspapers* equals 1 if the number of local newspapers is one, and 0 otherwise. The dependent variable is either the natural logarithm of one plus the dollar amount of penalties per facility and year (i.e., Column 1) or the natural logarithm of one plus the number of violations (i.e., Column 2). Our main explanatory variable is *Treatment*, which takes the value of 1 for the three years after the closure of a local newspaper, and 0 in the three years prior to the closure of a local newspaper. We use an F-test to test whether the sum of the coefficients ($\alpha 1 + \alpha 3$) is greater than 0 and report the p-values in square brackets. *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. All variables are defined in the Appendix, and the sample spans the period 2000-2017. Standard errors are two-way clustered by state and year. Standard errors are reported below the coefficients. *, **, and *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Panel A: Local media coverage

Dependent Variables		Penalties	Number_Violations
Variables		(1)	(2)
Treatment x High_Local_Media_Coverage	α_3	0.1658*	0.0125**
		(0.0855)	(0.0058)
Treatment	α_1	0.0175	0.0005
		(0.0979)	(0.0069)
High_Local_Media_Coverage		–0.1093***	–0.0089***
		(0.0377)	(0.0026)
F-Test: $\alpha_1 + \alpha_3 > 0$		0.1833***	0.0130**
		[0.008]	[0.012]
Google_Searches		–0.0063	–0.0005
		(0.0053)	(0.0004)
Controls		Yes	Yes
Facility FE		Yes	Yes
Year x State FE		Yes	Yes
Adj. R-square		0.076	0.107
Observations		164,128	164,128

Panel B: Firm size

Dependent Variables		Penalties	Number_Violations
Variables		(1)	(2)
Treatment x Large_Firm	α_3	0.2242**	0.0160*
		(0.1043)	(0.0091)
Treatment	α_1	0.0136	0.0008
		(0.1018)	(0.0077)
Large_Firm		–0.0005	–0.0003
		(0.0488)	(0.0037)
F-Test: $\alpha_1 + \alpha_3 > 0$		0.2378***	0.0168**
		[0.006]	[0.012]
Google_Searches		0.0022	–0.0000
		(0.0090)	(0.0008)
Controls		Yes	Yes
Facility FE		Yes	Yes
Year x State FE		Yes	Yes
Adj. R-square		0.076	0.107
Observations		164,128	164,128

Panel C: Number of local newspapers

Dependent Variables		Penalties	Number_Violations
Variables		(1)	(2)
Treatment x Low_Number_Newspapers	α_3	0.3597**	0.0260**
		(0.1565)	(0.0110)
Treatment	α_1	0.0961	0.0067
		(0.0691)	(0.0046)
Low_Number_Newspapers		–0.1021***	–0.0083***
		(0.0226)	(0.0016)
F-Test: $\alpha_1 + \alpha_3 > 0$		0.4558***	0.0327***
		[0.002]	[0.004]
Controls		Yes	Yes
Facility FE		Yes	Yes
Year x State FE		Yes	Yes
Adj. R-square		0.076	0.107
Observations		164,128	164,128

Table 6

Craigslist introduction – instrumental variable approach

This table uses an instrumental variable approach to examine the effect of newspaper closures on facility-level misconduct. Column 1 presents the results from a first-stage regression of the newspaper closure event, denoted *Newspaper_Closure*, on *Craigslist_Entry*, which is set to 1 in the three years following Craigslist's entry into a specific county. Counties that do not experience Craigslist's entry are excluded. Columns 2 and 3 present the results from second-stage regressions of dollar penalties and number of violations on the predicted value of newspaper closure, denoted *Predicted_Closure*, from the first-stage regression in Column 1. *Penalties_Diff* is the difference in the sum of one plus the dollar amount of penalties per facility over the subsequent three years minus the preceding three years. *Number_Violations_Diff* is the difference in the sum of one plus the number of violations per facility over the subsequent three years minus the preceding three years. *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. All variables are defined in the Appendix, and the sample spans the period 2000-2017. Standard errors are clustered are two-way clustered by state and year. Standard errors are reported below the coefficients. *, **, and *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Newspaper Closure (1st Stage)	Penalties_Diff (2nd Stage)	Number_Violations_Diff (2nd Stage)
Variables	(1)	(2)	(3)
Predicted_Closure		27.6758**	2.0332**
		(11.7687)	(0.8331)
Craigslist_Entry	0.0046*		
	(0.0025)		
Controls	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Adj. R-square	0.125	0.068	0.068
Observations	67,027	67,027	67,027

Table 7
Falsification test.

This table presents falsification tests on the number of facility-level violations, and penalties in response to the closure of local newspapers. The dependent variable is the penalty amount or the number of violations. Our main explanatory variable is *Treatment*, which takes the value of 1 for the three years after a local newspaper closure, and 0 in the three years prior to the closure. The randomization procedure is as follows: we use a uniform distribution to randomize the year in which each newspaper closes. The randomization procedure takes 1,000 random draws of the randomized element. Standard errors are reported below the coefficients. *p*-values (in brackets) reflect the probability that the coefficient estimated using the randomized data (β_1) is greater than the coefficient estimated using the actual data based on Table 4, Column 3 ($\widehat{\beta_{1,penalties}} = 0.1519$) and Column 6 ($\widehat{\beta_{1,violations}} = 0.0106$). *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. Variables are defined in the Appendix.

Dependent variable	$\widehat{\beta_1}$ *Actual data*	β_1 *Random data*	H_o: $\beta_1 > \widehat{\beta_1}$ [*p*-value]
(1) Penalties	0.1519 (0.0657)	–0.1083 (0.0850)	[<0.001]
(2) Violations	0.0106 (0.0048)	–0.0098 (0.0072)	[<0.001]

Table 8

Propensity score matching.

Panel A presents the propensity score estimation (Column 1) and covariate balance between the matched pairs of facilities located in areas with and without local newspaper closures (Columns 2-4). The dependent variable in Column 1 is an indicator set to 1 in the year prior to the newspaper closure, and 0 otherwise. The likelihood of newspaper closure is predicted based on the firm- and facility-level covariates used in the models presented in Table 4 using Probit regression estimation. The model spans the period 2000-2017 and the tests are run within state and two-digit-SIC code industries. Facilities located in areas without local newspaper closures are matched to facilities with local newspaper closures within the same year, industry, state, and all other covariates, using the propensity scores obtained from the Probit regression without replacement. The matching procedure uses a caliper of 0.05. Columns 2-4 report the average values of the variables used in the matching procedure after matching and the average difference in these variables across treated and non-treated matched facilities. Standard errors are clustered are two-way clustered by state and year. Standard errors are reported in parentheses below the coefficients. *P*-values are displayed in brackets below the mean differences in coefficient estimates. Panel B reports the average treatment effect of local newspaper closures on the natural logarithm of one plus the dollar amount of penalties (Column 1) or the natural logarithm of one plus the number of violations (Column 2).*, **, and *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. Variables are defined in the Appendix.

Panel A: First stage regression and covariate balance

Variables	Newspaper Closure (1)	Mean Treated Firm (2)	Mean Matched Firm (3)	Mean Difference (2) – (3) (4)
Employees_Facility	0.0084	1.6775	1.5826	0.0949
	(0.0119)			[0.254]
Sales_Facility	0.0552***	0.9314	0.8664	0.0650
	(0.0100)			[0.509]
Size	–0.0144	9.1500	9.1464	0.0036
	(0.0161)			[0.947]
ROA	–0.1676	0.0444	0.0449	–0.0005
	(0.4340)			[0.794]
Leverage	0.3714***	0.2725	0.2805	–0.0080
	(0.1364)			[0.156]
Pseudo R-squared	0.007			
Area under ROC curve	0.541			
Observations	6,498	2,002	2,002	

Panel B: Average treatment effects

Dependent Variables	Penalties	Number_Violations
Variables	(1)	(2)
Mean Treated Firm	0.7050	0.0513
Mean Matched Firm	0.6055	0.0450
Mean Difference	0.0995***	0.0063**
	[0.005]	[0.014]
Observations	2,002	2,002

Table 9

Different fixed effects structures.

This table examines the robustness of our primary results tabulated in Table 4 to different fixed effects structures. Columns 1 and 3 report results with year fixed effects interacted with industry fixed effects. Columns 2 and 4 report results with year fixed effects interacted with firm fixed effects. Our main explanatory variable is *Treatment*, which takes the value of 1 for the three years after the closure of a local newspaper, and 0 in the three years prior to the closure of a local newspaper. The dependent variable is either the natural logarithm of one plus the dollar amount of penalties per facility and year (i.e., Columns 1-2) or the natural logarithm of one plus the number of violations (i.e., Columns 3-4). *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. All variables are defined in the Appendix, and the sample spans the period 2000-2017. Standard errors are two-way clustered by state and year. Standard errors are reported below the coefficients. *, **, and *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Penalties		Number_Violations	
Variables	(1)	(2)	(3)	(4)
Treatment	0.1747**	0.1570*	0.0115**	0.0098*
	(0.0693)	(0.0817)	(0.0052)	(0.0054)
Controls	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes
Industry FE x Year FE	Yes	No	Yes	No
Firm FE x Year FE	No	Yes	No	Yes
Adj. R-square	0.080	0.097	0.113	0.072
Observations	164,128	164,128	164,128	164,128

Table 10

Additional tests.

Panel A reports results from a dynamic-effects model. The main explanatory variables are single-year treatment windows that range from 1 to 3 years before the newspaper closure to 1 to 2 years after the newspaper closure. These treatment windows are benchmarked against the year *t-1* before the closure of the newspaper. Panel B reports results using a sample that also includes facilities without violations that report sales during our sample period. Panel C examines the robustness of our primary results tabulated in Table 4 considering mergers, reduced frequency, and moving the newspaper to an online-only issue as additional treatments. Our main explanatory variable is *Treatment*, which takes the value of 1 for the three years after the closure of a local newspaper, newspaper mergers, reducing the frequency of a local newspaper, or moving to an online-only issue, and 0 in the three years prior to these events. Panel D examines the robustness of our primary results tabulated in Table 4 to different clustering of standard errors. Columns 1 and 3 report results with standard errors clustered by facility. Columns 2 and 4 report results with standard errors clustered by state. Panel E examines the robustness of our primary results tabulated in Table 4 to different definitions of the dependent variables. In Columns 1 and 2, we exclude workplace safety violations. In Columns 3 and 4, we exclude environmental violations. In Columns 5 and 6, we exclude False Claims Act violations. In Columns 7 and 8, we exclude violations related to the promotion of unapproved medical products. Panel F reports the estimation results from linear regressions of the effect of local newspaper closures on facility-level toxic releases. The dependent variable is the natural logarithm of one plus the toxic releases per facility and year. Column 1 reports results without *Controls*. Column 2 reports results with facility-level and firm-level *Controls*. Column 3 reports results with facility-level, firm-level, and county-level *Controls*. The dependent variable is either the natural logarithm of one plus the dollar amount of penalties per facility and year (Column 1) or the natural logarithm of one plus the number of violations per facility and year (Column 2), if not defined otherwise. Our main explanatory variable is *Treatment*, which takes the value of 1 for the three years after the closure of a local newspaper, and 0 in the three years prior to the closure of a local newspaper, if not defined otherwise. *Controls* includes *Employees_Facility*, *Sales_Facility*, *Size*, *Leverage*, *ROA*, *Labor_Force*, and *Unemployment_Rate*. All variables are defined in the Appendix, and the sample spans the period 2000-2017. Standard errors are two-way clustered by state and year. Standard errors are reported below the coefficients. *, **, and *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Panel A: Dynamic effects

Dependent Variable	Penalties	Number_Violations
Variables	(1)	(2)
$Treatment_{t-3}$	0.0217	0.0041
	(0.0145)	(0.0073)
$Treatment_{t-2}$	0.0523	0.0052
	(0.1045)	(0.0087)
$Treatment_{t}$	0.1118*	0.0086**
	(0.0559)	(0.0033)
$Treatment_{t+1}$	0.1278*	0.0109*
	(0.0729)	(0.0053)
$Treatment_{t+2}$	0.2133*	0.0174**
	(0.1017)	(0.0074)
Controls	Yes	Yes
Facility FE	Yes	Yes
Year FE x State FE	Yes	Yes
Adj. R-square	0.076	0.107
Observations	164,128	164,128

Panel B: Newspaper closures and facility-level misconduct – Large sample

Dependent Variable		Penalties	Number_Violations
Variables	Pred.	(1)	(2)
Treatment	+	0.1583*	0.0104*
		(0.0808)	(0.0059)
Employees_Facility		0.0016	0.0001
		(0.0023)	(0.0002)
Sales_Facility		–0.0001	–0.0000
		(0.0010)	(0.0001)
Size		0.0728***	0.0049***
		(0.0205)	(0.0013)
Leverage		–0.0446	–0.0034*
		(0.0285)	(0.0019)
ROA		0.0600	0.0036
		(0.0421)	(0.0027)
Labor_Force		0.0996*	0.0079**
		(0.0501)	(0.0037)
Unemployment_Rate		–0.0212*	–0.0014*
		(0.0101)	(0.0007)
Facility FE		Yes	Yes
Year FE x State FE		Yes	Yes
Adj. R-square		0.082	0.072
Observations		411,421	411,421

Panel C: Alternative treatment

Dependent Variable	Penalties	Number_Violations
Variables	(1)	(2)
Treatment	0.1123**	0.0080*
	(0.0419)	(0.0045)
Controls	Yes	Yes
Facility FE	Yes	Yes
Year FE x State FE	Yes	Yes
Adj. R-square	0.091	0.228
Observations	153,587	153,587

Panel D: Alternative clustering

Dependent Variable	Penalties		Number_Violations	
Variables	(1)	(2)	(3)	(4)
Treatment	0.1519**	0.1519***	0.0106**	0.0106***
	(0.0696)	(0.0428)	(0.0052)	(0.0033)
Controls	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes
Year x State FE	Yes	Yes	Yes	Yes
Clustered by	Facility	State	Facility	State
Adj. R-square	0.076	0.076	0.107	0.107
Observations	164,128	164,128	164,128	164,128

Panel E: Alternative dependent variables

Dependent Variable	Penalties	Number_Violations	Penalties	Number_Violations	Penalties	Number_Violations	Penalties	Number_Violations
Violations	Without Workplace Safety Violations		Without Environmental Violations		Without False Claims Act Violations		Without Medical Products Violations	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.1301*	0.0103*	0.1100*	0.0096**	0.1370*	0.0116**	0.1560**	0.0129**
	(0.0643)	(0.0057)	(0.0574)	(0.0044)	(0.0699)	(0.0050)	(0.0663)	(0.0047)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-square	0.154	0.301	0.081	0.215	0.084	0.220	0.089	0.222
Observations	164,128	164,128	164,128	164,128	164,128	164,128	164,128	164,128

Panel F: Newspaper closures and facility-level toxic releases

Dependent Variable		Toxic_Releases		
Variables	Pred.	(1)	(2)	(3)
Treatment	+	0.2002*	0.1859*	0.1834*
		(0.0986)	(0.0951)	(0.0899)
Employees_Facility			0.0084	0.0085
			(0.0092)	(0.0091)
Sales_Facility			0.0015	0.0010
			(0.0089)	(0.0086)
Size			0.0455	0.0437
			(0.0658)	(0.0640)
Leverage			0.0173	0.0193
			(0.0277)	(0.0276)
ROA			–0.0716	–0.0637
			(0.2394)	(0.2389)
Labor_Force				0.2391
				(0.2829)
Unemployment_Rate				–0.0432*
				(0.0248)
Facility FE		Yes	Yes	Yes
Year FE x State FE		Yes	Yes	Yes
Adj. R-square		0.911	0.911	0.911
Observations		8,112	8,112	8,112