

Regulation by Shaming: Deterrence Effects of Publicizing Violations of Workplace Safety and Health Laws *

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October 28, 2018

Abstract

Publicizing firms' socially undesirable actions may alleviate information frictions that distort incentives to avoid such actions. In 2009, the Occupational Safety and Health Administration (OSHA) began issuing press releases about facilities found to be violating safety and health regulations. Using quasi-random variation arising from a cutoff rule OSHA followed, and the sources that publicized these releases, I find that publicizing a facility's violations led other facilities to substantially improve their compliance and experience fewer occupational injuries. The estimates imply that OSHA would need to conduct 110 additional inspections to achieve the same improvement in compliance as achieved with a single press release. Evidence suggests that employers improve compliance to avoid costly responses from workers.

JEL: J28, J81, L51, I18

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Ratings, scores, disclosure, and other means of informing a firm’s stakeholders about an aspect of its quality or performance have proliferated in recent years (Dranove and Jin, 2010). Such policies are guided by the basic economic insight that, when quality is imperfectly observed, providing information mitigates a moral hazard problem that distorts firms’ incentives to invest in quality. Indeed, a rich empirical literature has found that providing information about quality to the public leads rated, scored, or otherwise disclosed firms to improve the quality of the attributes under scrutiny.¹

Many sources, though, seek to disclose information only about firms whose quality or performance is low: that is, “shaming.” For example, non-governmental organizations and media outlets compile lists of firms that fail in some dimension according to objective data sources, such as “Least Green Companies in America.”² Increasingly, technology and social media have enabled customers, former workers, and other stakeholders to expose companies’ actions ranging from tax avoidance,³ high medical drug prices,⁴ and sexual harassment of employees.⁵ While one intent of such tactics is to pressure the entity being targeted to improve its behavior (“specific deterrence”), a broader and perhaps more important intent is to encourage improvements in quality at other entities who wish to avoid being the target of their own future negative publicity (“general deterrence”). Despite the growing prevalence of these policies, little is known about how firms respond to such information disclosure policies targeted only at the worst performers. Estimating their effects poses substantial empirical challenges due (1) to the purposely nonrandom selection of entities that are publicized, (2) to the difficulty in knowing which other entities are the most likely respond to general deterrence, and (3) to a dearth in data on outcomes typically under scrutiny.

¹Some examples are restaurant hygiene report cards (Jin and Leslie, 2003), disclosure of drinking-water quality (Benjamin and Olmstead, 2008), and environmental ratings (Chatterji and Toffel, 2010). See Dranove and Jin (2010) for an overview of the literature.

²*Newsweek*, “Least Green Companies in America: Photos,” October 16, 2011. <http://www.newsweek.com/least-green-companies-america-photos-68107>

³*BBC*, “Google, Amazon, Starbucks: The Rise of ‘Tax Shaming’,” May 21, 2013. <http://www.bbc.com/news/magazine-20560359>

⁴“Social Media Shaming: Can Outrage Be Effective?” Knowledge@Wharton, November 20, 2015, accessed September 15, 2016. <http://knowledge.wharton.upenn.edu/article/social-media-shaming-can-outrage-be-effective/>

⁵*NPR*, “Uber Orders Investigation Into Sexual Harassment Claims,” February 20, 2017. <http://www.npr.org/2017/02/20/516292319/uber-orders-investigation-into-sexual-harassment-claims>

This paper overcomes these challenges. I investigate a policy implemented by the Occupational Safety and Health Administration (OSHA), the regulatory agency charged with setting and enforcing workplace safety and health standards in the U.S., in which it began issuing press releases about employers found to be violating its standards in a recent inspection. The policy was intended to expose egregious violators to public scrutiny and to publicize OSHA’s enforcement actions. These press releases described the violations found and financial penalties levied in the inspection of an employer’s facility⁶, and they implied that the employer was exposing its workers to substantial safety and health hazards.

The initiation of OSHA’s press release policy provides an ideal setting to understand the scale, scope and persistence with which publicizing poor performance affects employers’ behavior. First, OSHA used a cutoff rule whereby it issued a press release if it fined the employer an amount above a threshold. This rule provides quasi-random variation in publicity among otherwise similar facilities that lends itself to a Regression Discontinuity (RD) design. Second, OSHA distributed these press releases to local newspapers and industry trade publications, meaning that other facilities in close geographic proximity and in the same industry were most likely to be exposed to ensuing publicity. The policy was not made known to the general public; as a result it led to a sharp and unexpected increase in media coverage of OSHA violations, and it meant that a well-defined set of facilities were made aware of this new threat of media coverage. Third, OSHA routinely inspects a broad set of workplaces to detect health and safety violations and collects the results in an internal database, providing a timely and systematic data source to measure facilities’ health and safety outcomes.

Understanding the extent to which such publicity affects workplace safety and health is useful not only to understand how firms respond to targeted information disclosure, but it is also an important question for public policy. Workplace injury rates have substantial welfare costs, with one study estimating that they cost the U.S. \$250 billion per year (Leigh, 2011). At the same time, it is not obvious based on economic theory that publicizing facilities’ safety and health record would be an effective means to improve it: in a competitive labor market in which workers are fully informed about job hazards, the level of safety is an efficient outcome in

⁶Hereafter, the term “facility” is used to signify an establishment, or—for the construction sector, in which the concept of an “establishment” is ill-defined—the location of a construction work site.

which workers are appropriately compensated for hazards (Rosen, 1986), meaning there is no market imperfection that publicity would alleviate.

I find that press releases revealing OSHA non-compliance lead to substantial improvements in workplace safety and health. A press release leads to 2.1 fewer violations at other facilities in the same sector within a 5 kilometer radius (“peer facilities”), a decrease of 88 percent. Improvements in compliance persist for facilities located up to 50 kilometers away. To put this magnitude in perspective, one recent study found that a typical OSHA inspection leads to 48 percent fewer violations at later inspections of the same facility (Ko et al., 2010). Thus, this paper’s estimates imply that publicizing violations of one facility leads neighboring facilities in the same sector to improve compliance by twice as much as if OSHA inspected each of these facilities instead. Given that inspections are costly and that OSHA’s budget constraints—like those in many other regulatory agencies—dictate that it can inspect only a small subset of regulated workplaces, publicity appears to be a highly effective policy tool to improve workplace safety.

Furthermore, using the occurrence of OSHA inspections triggered by a serious workplace injury, I find that press releases lead not only to improved compliance with OSHA regulations, but also to fewer injuries. An inspection with penalties just above the press release cutoff leads to significantly fewer inspections triggered by a serious accident among other peer facilities. The magnitude of the effect, as with compliance, is substantial.

I then test for mechanisms through which press releases lead to improvements in safety and health. Facilities may improve compliance to avoid costly responses from stakeholders, especially workers. Workers who have more bargaining power may have more scope to leverage a press release to demand better working conditions from an employer. Drawing from literature on how the presence of labor unions affects workers’ bargaining power (both at unionized and non-unionized workplaces), I proxy for workers’ bargaining power using two measures of the strength of labor unions: whether a facility is located in a Right-to-Work state, and a facility’s county’s baseline unionization rate. Using either measure, facilities in areas where unions are strong improve compliance by a substantial amount following a press release about a peer; those in areas where unions are relatively weak display no improvement. In other words, press releases lead to improvements in safety and health only when workers are most likely to be able to use

information about an employer’s safety record to respond in a costly way.

This paper’s findings provide a novel contribution to a literature on the disciplinary effects of information provision. While a growing body of work (such as those papers cited in Footnote 1) has investigated the extent to, and conditions under, which information disclosure leads firms to improve their performance, this paper is one of the first to identify how providing information about some targeted firms can have broader effects on the behavior of other firms. Other papers have explored the effect of “shaming” in other domains, such as public release of criminal records (Luca, 2011) and tax delinquency (Perez-Truglia and Troiano, 2015). In politics, media coverage has been shown to affect politicians’ incentives to engage in malfeasant behavior (Snyder Jr and Strömberg, 2010; Larreguy et al., 2014). This paper’s findings on the general deterrence effects of information provision complement work by Freedman et al. (2012) who find recalls on one set of consumer products has spillover effects on how consumers behave with respect to other products. This paper builds on these literatures by exploring how shaming—and targeted information disclosure—affects firm behavior in a regulatory environment.

Second, this paper provides new insight into the determinants of regulatory compliance in firms. Many prior studies have investigated the specific deterrence effects of OSHA inspections on future compliance of inspected facilities (Gray and Jones, 1991; Weil, 1996; Ko et al., 2010), as well in other regulatory domains such as by the Environmental Protection Agency (see Alm et al. (2014) for an overview). A smaller body of literature has sought to estimate the general deterrence effects of enforcement on other facilities.⁷ At least in the environmental domain, the consensus in this literature seems to be that “rigorous monitoring and enforcement remains the number one motivator for many facilities’ environmental compliance decisions” (Gray and Shimshack, 2011, pp.1). The findings of this paper suggest the media has been overlooked as powerful forces governing firms’ compliance decisions, at least for safety and health.

⁷For example, Shimshack and Ward (2005) find that EPA inspections resulting in a fine lead to a substantial reduction in the statewide violation rate. Thornton et al. (2005) surveyed manufacturing firms and found that the number of examples of enforcement actions at other firms that respondents could recall was significantly and positively associated with whether the respondent reported having taken action to improve environmental performance.

1 Conceptual Framework

This section briefly discusses why a policy publicizing facilities caught violating workplace safety and health regulations might affect managers' decisions to comply and make other investments in safety and health.

First, such publicity could provide new information to stakeholders who value facilities' commitments to workplace safety and/or regulatory performance more broadly. Unless compliance with OSHA regulations is perfectly observable to stakeholders, publicity revealing that a facility is violating OSHA regulations signals that these commitments are low (i.e., the facility is uncommitted to workplace safety and/or regulatory performance). If consumers or other stakeholders use this information to update their belief about facilities' quality and change their behavior in a way that is costly (Freedman et al., 2012), then not-yet publicized facilities face incentives to invest in their own compliance to avoid being the object of future reputation-damaging news (Board and Meyer-ter Vehn, 2013).

One set of stakeholders that certainly values facilities' OSHA compliance and safety performance is workers. While textbook labor economics theory says the level of workplace safety and health is an efficient equilibrium outcome based on workers' preferences and employers' costs (Rosen, 1986), there is evidence that workers are not fully informed about job hazards. For example, Viscusi and O'Connor (1984) found that giving workers information about the hazards associated with their job increased their reservation wage and probability of quitting. This evidence suggests that workers begin their jobs with imperfect information about hazards and, as they learn over time, quit if their updated beliefs make the position sufficiently unattractive. Thus, publicity about OSHA violations could mitigate a market imperfection, and it could lead current workers to update their beliefs about the risks they face and in turn to quit, or lead potential new workers to be more informed at the outset of a job and in turn demand higher wages.⁸

Publicity about OSHA violations could lead other stakeholders that value OSHA compliance and workplace safety to update their beliefs as well. For example, consumers or downstream trading partners may infer that non-compliance

⁸Relatedly, there is evidence that employers lack full private incentive to provide safety and health in the workplace. OSHA inspections are relatively rare and the financial penalties low, so the threat of enforcement may be ineffective in incentivizing compliance. Additionally, imperfections in workers' compensation mean that employers only partially internalize the costs of workplace injuries and illnesses (Leigh and Marcin, 2012).

with OSHA standards indicates labor unrest, which has been shown to worsen product quality (Mas, 2008). Consumers might respond for other reasons: in the weeks following the widely publicized 2010 British Petroleum (BP) oil spill, which killed 11 workers and released millions of gallons of oil into marine waters, BP margins and volumes declined significantly (Barrage et al., 2014). Publicity about violating safety standards thus may impose an additional cost on noncompliance, above and beyond enforcement penalties, insurance premiums, and other existing costs on noncompliance.

A second way that publicity detailing violations found at a recent OSHA inspection could affect compliance is by changing managers’ beliefs about the probability of future OSHA enforcement (i.e., affecting the *regulator’s* reputation). While neoclassical models of compliance view agents as choosing compliance based on all present and future expected benefits and costs, in reality these decisions are made in the presence of imperfect information. There are hundreds of safety and health regulatory standards in the U.S., and given this regulatory complexity even the most well-intentioned firm may not be perfectly compliant (Malloy, 2003). A press release could affect managers’ beliefs about the *probability* and *severity* of enforcement: because OSHA inspects only a small subset of operating workplaces each year, many managers may be unaware of its inspection and enforcement activities. Publicity could also change managers’ beliefs about *priorities* of enforcement: because press releases provide detailed descriptions of the specific violations found in an inspection, and the penalty associated with each violation, a press release could signal that OSHA is cracking down on a particular set of standards.

More generally, press releases could exert a behavioral effect simply by making safety standards more salient to managers. Reminders that make the cost of an agent’s actions more salient have been shown to affect behavior in energy use (Gilbert and Zivin, 2014) and individual saving (Karlan et al., 2016).

2 Institutional Background and Data

2.1 Background on OSHA

OSHA, created in 1970, is the federal regulatory agency charged with assuring “safe and healthful working conditions” in the U.S. by establishing and enforcing

standards.⁹ Many employers are required to comply with hundreds of OSHA standards, which range from maintenance of specific capital equipment to more general restrictions that workers not be exposed to particular hazards. Organizationally, OSHA partitions the country into 10 regions, each with its own regional office, and 138 Area Offices that oversee the implementation of inspections and enforcement. OSHA has jurisdiction over 28 states; the remaining 22 states have received federal approval to operate their own state-run safety and health programs. Figure A.I provides a map of which states are under OSHA's jurisdiction, and a map of how OSHA partitions the country into 10 regions.

Inspections are OSHA's primary tool for monitoring compliance with health and safety standards. Among non-construction industries, an inspection is typically conducted at the level of the establishment. For construction industries, inspections typically take place at a work site; if multiple companies are working on the same site, the inspector may conduct a separate inspection of each company. During inspections, OSHA inspectors review paperwork and tour a facility's operations to assess their hazards and compliance with relevant standards. When inspectors find facilities to be out of compliance with any standards, they issue citations for each violation they observe. Once inspections are completed, inspectors consult with the Director of the OSHA Area Office to calculate the financial penalty for each violation, which is a function of the size of the employer, the number of workers exposed to the hazard, and the likelihood the violation would lead to a severe accident (U.S. Occupational Safety and Health Administration, 2009).

OSHA inspections can be initiated for two broad categories. "Programmed" inspections focus on particular industries or hazards that OSHA prioritizes, and typically constitute roughly 60% of annual inspections. These inspections are pursuant to National Emphasis Programs (NEPs), which focus on nationwide priorities, or Local Emphasis Programs (LEPs), which focus on regional priorities.¹⁰ Because programmed inspections target facilities only based on their being in a particular industry or possessing a specific hazard, no other facility-specific

⁹OSHA, About OSHA, <https://www.osha.gov/about.html>, accessed Aug. 22, 2016.

¹⁰For example, one industry-specific NEP from 2016 focused on facilities that stored highly hazardous chemicals. An example of a hazard-specific NEP is one from 2008 that focused on reducing occupational exposure to lead, which targeted industries where such exposure was most likely. See "OSHA's Active National and Special Emphasis Program Index," available at <https://www.osha.gov/dep/neps/nep-programs.html>, accessed September 2017.

factors (such as recent injuries) influence which plants receive a programmed inspection.¹¹ Furthermore, conditional on the criteria on which NEPs or LEPs are based (e.g. industry or region), OSHA typically randomly assigns inspections among plants that meet those criteria.¹²

The remainder of OSHA’s inspections are triggered by an event specific to the facility, such as a complaint (by an employee or member of the public) alleging safety and health hazards; a “referral” (an allegation of hazards made by an inspector, government agency or media); or a serious accident (worker fatality or hospitalization of three or more workers, or what OSHA calls a “catastrophe”).

While inspections are central to OSHA’s monitoring efforts, in practice budget constraints dictate that OSHA inspect only a tiny subset of regulated establishments. OSHA and its state counterparts conducted 75,000 inspections in 2016, which covered less than one percent of the 8 million workplaces required to comply with OSHA regulations.¹³

2.2 OSHA’s Press Release Policy

Since at least the beginning of the 2000s, OSHA’s ten regional offices around the country would issue a press release detailing the results on an inspection if the regional office’s Office of Public Affairs (OPA) deemed one appropriate. The regional office would then send the press release to local media and industry trade press. Figure 1 gives an example of such news coverage: OSHA inspected a poultry processing plant in Gainesville, Georgia in January 2009, and the inspector issued \$73,275 in penalties on April 16, 2009. OSHA immediately issued a press release about the inspection, which begins by suggesting the plant was not com-

¹¹One exception to this rule is OSHA’s Site-Specific Targeting program, running from 1999 to 2010, which focused inspections on plants that recently experienced high injury rates. However, this program is largely irrelevant to this paper, as SST excluded construction (which makes up a majority of the sample in this paper’s analysis), and the bulk of the sample period used in this paper’s analysis is after the SST program ended.

¹²As one example, the 2008 NEP on Lead stated that, once OSHA officials determined the set of industries deemed to have high potential for lead exposure, the list of plants in those industries was distributed to each of OSHA’s Area Offices around the country. Each plant in an Area Office’s list was then assigned a random number. Once the Office determined how many plants on the list it could reasonably inspect, it selected those whose random number was below that number for inspection.

¹³United States Department of Labor, Occupational Health and Safety Administration, Commonly Used Statistics, <https://www.osha.gov/oshstats/commonstats.html>, accessed February 2017.

mitted to protecting its workers and had not made safety part of its culture. The article then describes in detail the specific violations found during the inspection, citing the plant’s lack of “standard guardrails” and “us[e of] flexible cords instead of fixed wiring,” among others. The same day that OSHA issued its press release, the story appeared in the plant’s local newspaper, *The Gainesville Times*.

Before 2009, the criteria used for determining whether to issue a press release were largely left to OSHA’s ten regional offices. These criteria varied substantially. Some regions used a cutoff rule: Regions 1 and 4 (covering New England and the Southeast, respectively) issued press releases for inspections resulting in penalties of at least \$40,000, and Region 5 (in the Midwest) used \$100,000 as a cutoff. Some regions effectively issued no press releases at all.

However, in May 2009 OSHA’s national headquarters in Washington, D.C. standardized these criteria across regions. As a result, Regions 1-4, 6, 9, and 10 instituted a common cutoff of \$40,000, Regions 5, 7 and 8 instituted a cutoff of \$45,000.¹⁴ OSHA did not announce these cutoffs publicly, and only communicated them internally, a detail important to support the validity of the empirical design that follows. Statements by OSHA officials reveal the policy was intended both to reveal exceptionally high violators to the general public, and to publicize OSHA’s enforcement activity. Dr. David Michaels, then the Assistant Secretary of Labor and Director of OSHA, called press releases “regulation by shaming,” suggesting the intent that press releases impose a cost on publicized employers and add a disincentive to violate OSHA regulations.¹⁵ Additionally, OSHA hoped press releases would serve “educational and deterrent purposes for other companies in the same industry and geographic area.”¹⁶

Figure 2 illustrates the effect of the 2009 policy change on the number of press releases issued by OSHA and media coverage of OSHA violations. For media coverage, I use the number of articles found on newslibrary.com¹⁷ that contain “OSHA” in the title and “violations” anywhere in the text. Panel (a) plots these series for Regions 1 and 4, which were using the \$40,000 cutoff rule at least as

¹⁴OSHA officials were unsure of the reasons for the difference in this cutoff across regions.

¹⁵*Michaels, David. OSHA at Forty: New Challenges and New Directions. July 19, 2010. Available here: https://www.osha.gov/as/opa/Michaels_vision.html*

¹⁶Comments from Patrick Kapust, deputy director of OSHA Directorate of Enforcement Programs, in a December 1, 2012 interview: <http://www.safetyandhealthmagazine.com/articles/examining-the-top-10-2?page=2>

¹⁷newslibrary.com is a compendium of roughly 4,000 U.S. newspapers and other news outlets.

early as 2002, and panel (b) plots for the later-adopting regions. A few take-aways emerge from these figures. First, there is a marked increase in the number of press releases issued across all OSHA regions coinciding with the policy change in 2009, which is more dramatic outside of Regions 1 and 4. Second, the number of newspaper articles about OSHA violations exhibits a roughly one-to-one relationship with the number of press releases written, illustrating that the 2009 policy change significantly changed the frequency of media coverage about OSHA violations. This second point is important because information about OSHA inspections is publicly available on its website and, in theory, journalists could cover large OSHA penalties (and other stakeholders could learn about them) even in the absence of OSHA-issued press releases. However, this information is not easily accessible¹⁸, and these figures reveal that press releases relaxed a constraint that otherwise limited the media’s coverage of OSHA violations.

While this policy change made the probability of a press release a discontinuous function of penalties, in practice the cutoff rule was not a sharp one. Some inspections with penalties below the cutoff resulted in a press release if, for example, the inspector found violations that posed a new and little-publicized kind of hazard. Some inspections above the cutoff did not get a press release if the inspector did not send the necessary information to the regional OPA in time. The federal OSHA office also did not enforce the cutoff rule with the regional offices. The empirical analysis that follows incorporates the fuzziness of this design.

2.3 Data

This paper’s primary data source is OSHA’s Integrated Management Information System (IMIS), which is a database that contains detailed information on ever OSHA inspection.¹⁹ Key variables it includes are the date the inspection is opened, the reason the inspection was initiated (complaint, referral, accident, programmed, other), and facility characteristics (name, address, industry, number of employees present, whether the employees are represented by a union, etc.). I geocoded addresses using ArcGIS to get the latitude and longitude of each

¹⁸Users would have to know to go to OSHA’s website at <https://www.osha.gov/oshstats/> to search for information about recent inspections, where they can view recent inspection regards for a specific establishment or OSHA area office.

¹⁹The data were downloaded from OSHA’s website in February 2014. IMIS can be downloaded here: <http://ogesdw.dol.gov/views/data.summary.php>

inspection. IMIS includes a detailed report of each violation found (if any), with the OSHA standard that was violated, its corresponding financial penalty, and the date the violations were issued. I collapse the data to the facility-inspection level by summing each type of violation and all penalties levied at each inspection. Since facilities are inspected at varying frequency—with some inspected multiple times, others inspected only once—the data constitute an unbalanced panel.²⁰

For most of the analysis, I restrict attention to inspections with penalties issued in May 2009 and after, since this is when OSHA made its press release policy relatively uniform. The dataset ends in December 2013. The press release policy did not cover the 22 states with state-run OSHA offices, so I exclude inspections in these states. I also exclude Regions 2 and 3 (covering primarily New York and New Jersey), as the data suggest that these regions did not adhere to the cutoff rule for issuing press releases. I also exclude inspections in the mining industry ($< 1\%$ of total inspections), as this industry is under the Mine Safety and Health Administration’s jurisdiction, rather than OSHA’s, and is thus not technically eligible for OSHA inspections.

Table 1 provides summary statistics separately for the entire sample of inspections initiated between January 2009 and December 2013, and for the subset of inspections with penalties between August 2009 and November 2012 and within \$10,000 of the press release cutoff for its corresponding region (within \$30,000 and \$50,000 for Regions 1, 4, 6, 9, and 10, and within \$35,000 and \$55,000 for Regions 5, 7 and 8). Most inspections result in relatively small penalties: of the roughly 150,000 inspections during this period, the average inspection results in just over \$4,600 in penalties (but is highly skewed) and just 1 percent result in penalties above the corresponding press release cutoff. That the press release cutoff is at the 99th percentile of the penalty distribution reflects how OSHA intended press releases to expose the highest violators. The average inspection finds 2 violations, while the average inspection in the subset around the press release cutoff finds over 8 violations.

Roughly 60 percent of inspections in the whole sample are programmed, and 34% are triggered by a complaint, referral or “fat/cat” (fatality or catastrophe)

²⁰IMIS does not keep a unique facility identifier to track the same facility over time. Thus, various “fuzzy matching” techniques were used to link records of the same facility over time. I thank Melissa Ouellet for help with this endeavor.

with the remaining 3.1% classified otherwise.²¹ The share of complaint, referral or catastrophe inspections rises to 53% in the sample near the cutoff.

The final panel of Table 1 shows the distribution of inspections across sectors.²² Inspections are concentrated in construction and manufacturing, both in the whole sample and the subsample around the press release cutoff.

Because many of these variables are skewed to the right, I topcode count variables at their 99th percentiles and take logs of continuous variables to ensure that the analysis is not vulnerable to outliers. I add the first non-zero percentile of each variable before taking the log to account for zeros.²³

To determine the extent to which the cutoff rule for issuing press releases was followed in practice, I²⁴ hand-linked the set of archived press releases on OSHA’s website to the IMIS dataset to create an indicator for each inspection if the inspection resulted in a press release.²⁵ Figure 3 uses the results to illustrate that the probability that an inspection results in a press release jumps at the cutoff by 20 to 25 percentage points, highlighting the discontinuity but also the imperfect adherence to the policy by OSHA. I²⁶ also linked the archived press releases to newslibrary.com to identify if a press release was covered by by a newspaper.²⁷

3 Empirical Strategy

3.1 Estimating General Deterrence Effects of Publicizing Facilities Caught Violating OSHA Regulations

Estimating how a policy that publicizes egregious violators of OSHA regulations affects facilities’ compliance is fraught with empirical challenges. One such chal-

²¹The categories in “other” include monitoring, variance, and follow-up inspections.

²²Sectors are roughly 2-digit NAICS codes, except that codes 31-33 are pooled for Manufacturing, 44 and 45 are pooled for Retail Trade, 48 and 49 are pooled for Transportation and Warehousing, and 1-digit 5-9 are pooled for Services.

²³I do this rather than the more common approach of adding 1 before taking the log because of how financial penalties are distributed. In my sample, conditional on any penalties being levied, almost no inspections have less than \$100 in penalties, and the first percentile is \$1,000.

²⁴By “I,” I mean a dutiful research assistant to whom I am very grateful.

²⁵The archive of OSHA’s press releases is available: <https://www.osha.gov/newsrelease.html>. I was unable to match about 3% of press releases to an inspection in IMIS.

²⁶See Footnote 24.

²⁷For each press release, a research assistant searched newslibrary.com for any news articles published within a 2-week window of the date the press release was issued that contained a salient feature of the company name, as well as “OSHA,” anywhere in the text.

lenge is identifying a set of “treated” facilities in which managers and/or workers become aware of the policy that violators will be publicized and a set of “control” facilities in which managers and workers remain unaware of the policy and the corresponding risk of publicity.

Fortunately, the introduction of OSHA’s press release policy offers a unique setting to overcome this challenge. Because OSHA’s policy change to begin publicizing egregious violators was not made known to the general public, the only way for managers and workers to learn of the policy was to observe a press release directly (or to interact with someone who had). The media outlets through which OSHA distributed its press releases provide natural boundaries for who would be exposed to a particular press release. First, OSHA typically sent its press releases to local (and not national) media outlets, meaning that facilities near the publicized facility were likely to be exposed to ensuing media coverage. Second, press releases were also typically covered by industry trade publications. As a result, a press release was most likely seen by managers or workers in facilities that were geographically proximate to and in the same industry as the publicized facility.

Other aspects reinforce the idea that managers in facilities sharing the same region and industry would view publicity about one another’s OSHA compliance. Corporate networks have a significant geographic component (Davis and Greve, 1997), suggesting managers of facilities located near each other have more contact than they do with those further away. In other domains, knowledge spillovers have been shown to decline with geographic distance (e.g., from patents (Jaffe et al., 1993; Belenzon and Schankerman, 2013)) and technology adoption (Agha and Molitor, 2015) and to be stronger for firms in the same industry (e.g., management practices (Bloom et al., 2017)). Furthermore, the set of standards that OSHA checks for in an inspection, as well as the likelihood that OSHA will inspect a particular facility at all, varies widely by industry (Weil, 1996); thus the description of the inspection results in a press release is likely more relevant to other facilities in the same industry.

While the media distribution of OSHA’s press releases provides a natural way to characterize how managers and workers most likely became aware of the resulting new threat of publicity, an additional empirical challenge arises in measuring compliance with OSHA regulations, which is addressed in the next section.

3.2 Estimating the Effects of Press Releases on Compliance When Inspections are Endogenous

Estimating the deterrence effects of publicizing violators of OSHA regulations requires measuring facilities' compliance with these regulations. However, a facility's compliance is only observed conditional on being inspected, based on the assessment of the inspector. Comparing compliance at future inspections of facilities that are or are not exposed to a press release could be biased if exposure to a press release changes the types of facilities that get inspected. Because many OSHA inspections are triggered by an event at the facility (e.g., an accident, complaint, or referral), the occurrence of such an inspection itself is endogenous. If press releases affect the probability that such events occur, then the underlying types of facilities that get inspected after observing a press release may be different from the types inspected without having observed a press release. If present, such an effect can bias an estimate of the effect of press releases on compliance. This issue is described more formally in Appendix A.

However, we can address this concern. First, when measuring compliance conditional on inspection, we can focus on programmed inspections, which as described above are not influenced by facility-specific events. Because OSHA initiates such inspections for reasons exogenous to the facility (conditional on industry and other criteria), there is little scope for bias from comparing compliance conditional on such inspections between treated and non-treated facilities.

3.3 Regression Discontinuity (RD) Method

A final empirical challenge to estimating the deterrence effects of publicizing OSHA violations is that press releases are not randomly assigned. By construction, press releases are written about the most egregious violators only, and as a result facilities subjected to a press release are systematically different from those that are not. Such differences may bias not only estimates of the specific deterrence effects of press releases but also the general deterrence effect on other facilities exposed to the publicity, for example if there is spatial correlation in rates of OSHA non-compliance.

Fortunately, OSHA's procedures to issue press releases provide a set of inspected facilities that did and did not become the subject of a press release, but

that were otherwise very similar. Specifically, because OSHA used a rule to issue a press release about violations only if the financial penalties were above a cutoff, one can estimate effects of these press releases using a regression discontinuity (RD) design—provided certain identification assumptions are met.

Suppose we are interested in the effect of a press release on some outcome for a facility that is publicized in a press release (“specific deterrence”). Whether the facility is the subject of a press release is a function of the penalty issued at an OSHA inspection—or the “running” variable in RD terminology. Because penalties may also have their own direct effect on later outcomes, such as later OSHA compliance, it is important to control flexibly for the running variable itself to isolate the effects of the press release.

Suppose facility i inspected and receives penalties levied at date t amounting to Pen_{it} , and we are interested in an outcome for i observed at a date τ months relative to t . It is most natural to re-orient a facility’s inspection history around the “focal” penalty levied on date t the following way:

$$Y_{it\tau} = \alpha + \gamma D_{it} + f(Pen_{it} - c) + \epsilon_{it\tau} \quad (1)$$

Where

$$\begin{aligned} Pen_{it} &= \text{penalty levied at } i \text{ at time } t \\ D_{it} &= \mathbb{1}\{Pen_{it} \geq c\} \end{aligned}$$

with $f(\cdot)$ a functional form to be determined, and γ the treatment effect of a press release which, since Equation 1 controls flexibly for Pen_{it} , is identified from variation on those penalties just below and above the cutoff c .

To estimate the effects of a press release on compliance conditional on a future inspection, $Y_{it\tau}$ is a function of assessed compliance at an inspection of i initiated at a date after t . For the main results, I measure compliance as the number of violations and the initial financial penalties,²⁸ and I restrict attention to inspections up to 36 months following the focal date ($\tau \in \{0, 36\}$).

Estimating the general deterrence, or spillover, effects of a press release on non-publicized facilities requires a slightly different specification. Suppose again

²⁸“Initial” penalties are those initially levied by the inspector. Facilities have the right to contest penalties, and the final penalty amounts often get decreased after a period of negotiation between OSHA and the facility.

that facility i is inspected at time t and is issued penalties Pen_{it} , and that we are interested in an outcome at “peer” facility j , within a particular vicinity v of i . I create a new expanded dataset in which all facilities in vicinity v of focal facility i are re-oriented around i ’s focal penalty date t , such that the unit of observation is now an inspection-focal date (jit). I model an outcome Y at peer facility j as a function of whether its focal penalty Pen_{it} was above the press release threshold. Again, because penalties at i may have their own independent effect on outcomes at j , we control flexibly for the focal penalty:

$$Y_{jvit\tau} = \alpha + \gamma D_{it} + f(Pen_{it} - c) + \epsilon_{jvit\tau} \quad (2)$$

Now, the running variable for all facilities within a vicinity v of the “focal” facility i is the focal penalty, Pen_{it} , assessed at i at time t .²⁹ If $Pen_{it} \geq c$, all facilities within vicinity v have been exposed to a press release, in an Intent-to-Treat (ITT) sense. Compliance is again measured as the number of violations or the financial penalties and $\tau \in \{0, 36\}$.

We may also be interested not in compliance conditional on later inspection, but in whether exposure to a press release affects the likelihood that certain types of inspections take place such as those triggered by a serious injury (fat/cat inspections). To investigate effects of exposure to a press release on these outcomes, I modify Equation 2 as follows:

$$Y_{vit\tau} = \alpha + \gamma D_{it} + f(Pen_{it} - c) + \epsilon_{vit\tau} \quad (3)$$

With j dropped from the notation, here $Y_{vit\tau}$ may be the number of fat/cat inspections among facilities in vicinity v of focal facility i between the focal date t and τ months following t .

Since press releases were covered in local newspapers and industry publica-

²⁹Two points about this data construction are worth making. First, it is possible that a given facility could be both a peer (j) and focal (i) facility. However, because I restrict attention to focal penalties in a neighborhood around the press release cutoff, and the cutoff is at the upper right tail of the penalty distribution, this occurs very infrequently. For example, in my main regressions below, roughly 1% of inspections among peer facilities result in penalties that would make them eligible to also be a focal facility. Second, a given facility j could be in the vicinity of two different focal facilities i and i' , in which case the same inspection of j will show up in this expanded dataset once as a peer of i and again as a peer of i' . In robustness checks, I restrict attention to a facility’s earliest focal penalty, or its maximum focal penalty, so that an inspection can only be considered a peer of a single focal facility i .

tions, the vicinity v has both a geographic and an industry component. As the baseline specification, I define a facility j to be in the vicinity of i if it is within a 5 km geographic radius of i and in the same sector (as defined as in Table 1); while 5 km is an arbitrary numeration, it is intended to group facilities that are very close to each other and thus especially like to communicate with and observe publicity about one another. I use expanded geographic rings in follow-on analysis. Because there may be correlation in OSHA compliance between facilities in close proximity to each other, the regressions cluster standard errors to allow arbitrary correlation in ϵ among all facilities in the same peer group.³⁰

In baseline specifications I use a uniform kernel around the running variable (i.e., observations just at the cutoff and those farther from the cutoff get equal weight), and I include focal penalties in a bandwidth within \$10,000 of the cutoff c . Various strategies exist to approximate functional form of $f(\cdot)$. However, Hahn et al. (2001) show that local linear regression—that is, estimating a standard linear regression restricted to a narrow bandwidth around the cutoff point c —is a non-parametric way to obtain an unbiased estimate of the treatment effect τ . To implement the local linear regression, I estimate Equation 1 locally around the cutoff c specifying $f(\cdot)$ as a linear function but allowing for different slopes on each side of the penalty cutoff c . In other specifications, I use the approach developed by Calonico et al. (2016) to select the optimal RD kernel, bandwidth, and polynomial. Because the construction industry has its own set of OSHA standards not applicable to other industries, and because inspections of construction sites are conducted differently than inspections of establishments in other industries (Weil, 2001), I also include a dummy variable for construction in all regressions to improve precision.

3.4 Checking the Validity of the RD Design

The RD design rests on the assumption that whether the running variable (here, OSHA financial penalties) ends up just above or just below the cutoff for press releases is random. This assumption is valid if those involved have imperfect control over the exact penalty issued, and it can be jeopardized if there is room

³⁰Because the way I constructed the data means that one facility may show up as a peer of two different focal facilities, the standard errors should technically allow for two-way clustering by peer group and by facility. However, standard errors are essentially identical under these two approaches, so to ease exposition I only report standard errors clustered by peer group.

for manipulation. For example, if there are reputational costs from publicity about poor safety, the disutility from penalties is discontinuous at the cutoff c . If managers know the value of c they may attempt to bunch just below it.

However, it is *ex ante* unlikely that managers have the potential to manipulate whether they are just above or just below the cutoff. First, the cutoff rule was not announced publicly, so managers were likely unaware of the cutoff. Furthermore, penalties levied by an OSHA inspector are a stochastic function of true non-compliance. For example, different OSHA inspectors may have varying degrees of “toughness,” not every OSHA standard is checked at every inspection, and standards have been refined or eliminated over time (Weil, 1996). This stochastic nature of penalties introduces an element of randomness from the facility’s perspective, which would limit its ability to control the exact penalty given its level of noncompliance.

On the other hand, in theory there is room for manipulation by the OSHA inspectors, since they issue violations and associated penalties. An inspector could tip a facility over the penalty cutoff if she thinks it deserves bad publicity, or she could accept a bribe to leave penalties just below. OSHA officials have confirmed that the method inspectors use to determine penalties is mechanical and pre-determined, and that any notion of whether the facility is above or below the press release cutoff never enters into the equation. However, it is still necessary to determine whether this lack of manipulation appears true quantitatively.

One test of the validity is whether the density of penalties associated with inspections is smooth around the cutoff c . If there is a discontinuity in the density at the cutoff, then one may suspect that either managers or inspectors are manipulating penalty amounts to be on one side or the other. Figure 4 illustrates the density around the cutoff. Penalty amounts are normalized by the corresponding regional cutoff c and are placed in equally sized bins of \$2,500 (ensuring all bins are on only one side of each cutoff), and frequencies are calculated for each bin. The sample includes inspections with penalties issued between May 2009 and November 2012. The density appears overall quite smooth, and implementing the test proposed by McCrary (2008) confirms that there is no statistically significant change in the density at the cutoff.

A second test of the validity of the “imprecise control” assumption is whether relevant baseline characteristics are smooth around the cutoff. Table B.1 shows

results from estimating Equation 1, with $\tau = 0$. In Column (1), the dependent variable is whether a press release is issued in the inspection; the coefficient indicates that inspections with penalties just above the press release cutoff are 19 percentage points more likely to be the subject of a press release ($p < .01$), which roughly corresponds to the graphical discontinuity in Figure 3. In the remaining columns the outcome variable is equal to one of various baseline covariates measured at the time of the focal inspection. The results show no evidence of a discontinuity in any covariates, providing further support an RD design will yield valid identification in this setting.³¹

4 The Effects of Publicizing OSHA Violations on Future Compliance

This section investigates the extent to which press releases about OSHA violations affected facilities’ compliance. It first estimates “general deterrence” effects, or how a press release about one facility affects the compliance of other facilities likely exposed to it. After verifying the robustness of the results, it then estimates the “specific deterrence” effects of a press release on the compliance of the publicized facility.

4.1 General Deterrence Effects on Compliance

Figure 5 graphically tests whether facilities exposed to a press release have different levels of compliance with OSHA standards. The sample includes inspections of facilities within 5 km and in the same sector as a facility with a focal penalty within \$15,000 of the relevant press release cutoff. Each facility is placed into a bin based on its focal penalty. In panels (a) and (b), the dependent variables are number of violations and total financial penalties, respectively. In panels (c) and (d), the dependent variables are the same, but the sample is restricted to programmed inspections. Each of the four graphs depicts a clear discontinuous

³¹An alternative way to check for smoothness in baseline covariates is to run a regression with D_{it} as the dependent variable, include each baseline covariate as a right-hand side variable, and to conduct an F-test that coefficients on all baseline covariates are equal to zero. The results of this specification, not shown, yield an F-stat of 0.96 and p-value of 0.44, providing further evidence that baseline covariates show no discontinuities around the cutoff.

downward shift in non-compliance among facilities whose focal penalty is just to the right of the cutoff c .³²

Intent-to-treat (ITT) regression results for these effects are shown in Table 2 using a bandwidth of \$10,000 around the cutoff and controlling linearly for the running variable. Columns (1)-(2) show the baseline results. Inspections of facilities within 5 km of and in the same sector as a facility with a focal penalty above the press release cutoff have -0.63 fewer violations and 19 percent lower financial penalties ($\exp(-0.21)-1$) ($p < .01$ in both cases).³³ Columns (3)-(4) restrict to programmed inspections (those that are by definition exogenous to events at the facility). The coefficients are, if anything, slightly larger.

Columns (5)-(6) examine inspections of peer facilities that happen in the 36 months *before* the date the focal penalty is issued: because outcomes in such inspections should not be affected by future events, these regressions serve as a placebo check on the main results. Reassuringly, in both cases the estimated coefficients are small and statistically insignificant.

While the figures and regressions provide evidence that penalties above the press release cutoff lead to higher compliance in later inspections of peer facilities, the true magnitude of interest is the effect of a press release on future compliance, which is not the same thing due to the fuzziness of the cutoff rule. The standard way to compute this Treatment-on-the-Treated (TOT) effect is to divide the ITT estimate by the “first stage” effect of the increase in the probability that the focal inspection was the subject of a press release at the cutoff. This approach is akin to using an Instrumental Variables (IV) strategy to instrument whether a press release was issued in the focal inspection with whether the focal penalty was above the cutoff c . I employ the procedure detailed in Calonico et al. (2016) to optimally select the bandwidth for a fuzzy RD design that allows for clustering (by peer group) and covariate adjustment (a dummy for construction). This procedure also uses a triangular kernel around the cutoff (giving more weight to observations closer to the cutoff), rather than a uniform kernel.

Table 3 contains the TOT estimates. The first column is analogous to Column

³²The anomalous high average for the fourth bin to the right of the cutoff arises because it happens to contain a disproportionately high number of inspections in Connecticut, and the average number of violations among inspections in Connecticut is significantly higher than the average among inspections in all other states in the sample.

³³If I instead use the *level* of initial penalties as the dependent variable, instead of its log, I get a coefficient of -1155 ($p = .011$), which is 20% of the sample mean of \$5842.

(1) of Table 2, estimating the ITT effect of a focal penalty above the press release cutoff on compliance of peer facilities in a 5 km radius and the same sector. The bandwidth that minimizes mean squared error is 3,256 to the left and 9,590 to the right of the cutoff; this bandwidth is smaller than the \$10,000 bandwidth used in the baseline estimates, which leads to a smaller sample. The ITT point estimate, -0.49 ($p < .01$), is slightly smaller than the ITT estimate using the uniform kernel and bandwidth of 10,000 (-0.63). The estimated first stage, in Column (2), is 0.23 ($p < .01$). Finally, the TOT estimate, essentially the ratio of Column (1) over Column (2), in Column (3) is -2.10 ($p < .01$). Since facilities with a focal penalty to the left of the cutoff averaged 2.38 violations, the TOT estimate implies that a press release led to 88% fewer violations at later inspections of other facilities in the same sector within a 5 km radius.

It is plausible that press releases would affect the behavior of facilities located further away than 5 km. Figure 6 explores how the general deterrence effects of press releases change depending on how “vicinity” to the focal facility is defined. Panel (a) of Figure 6 plots the ITT point estimates and 95 percent confidence intervals from Equation 2 with “vicinity” requiring shared sector, for non-overlapping radii around the focal facility of 5 km, 6 to 10 km, 11 to 25 km, and 26 to 50 km. The point estimate decreases roughly linearly with distance, but none of the point estimates are statistically significantly different from each other, and the effect remains significant for all vicinities. Table B.2 displays the ITT, first stage, and TOT estimates for the shared-sector peer groups of various geographic vicinities, but using overlapping groups (i.e. up to 5 km, up to 10 km, up to 25 km, and up to 50 km). The estimates imply that the typical press release leads to 35% fewer violations among facilities located up to 50 km away ($\beta = -0.78$, relative to control mean of 2.26).

On the other hand, Panel (b) of Figure 6 plots the ITT estimate of the general deterrence effect for facilities in sectors *different* from the focal facility. The point estimate is essentially zero for *any* geographic radii, suggesting that press releases do not affect behavior at facilities in other sectors at any geographic distance. This is consistent with work that has found evidence of knowledge spillovers within, but not across, industries in other contexts (Bloom et al., 2017).

To put the magnitude of the TOT estimate in perspective, a useful benchmark is the effect of inspections themselves on compliance. Like many regulatory

agencies, inspections are, and have historically been, OSHA’s primary tool to monitor, enforce, and promote compliance. One study by Ko et al. (2010), which examined inspections of manufacturing plants between 1996 and 2006, estimated that one OSHA inspection led to 48 percent fewer violations at later inspections. Another study by Weil (2001), which examined inspections of large construction firms between 1987 and 1993, found that inspections led to a more modest improvement in compliance. Taking the estimate in Ko et al. (2010) as an upper bound of the specific deterrence effects of inspections, the results in Table 3 suggest that a press release led the typical facility within a 5 km radius and the same sector to improve compliance by twice as much as if OSHA inspected each of those facilities directly, and the typical facility within a 50 km radius to improve by 73 percent as much. Put another way, given that there are on average 311 inspections in the sample period among peers in the same sector and a 50 km radius of focal facilities, OSHA would have to conduct 109 inspections to elicit the same level of deterrence as a single press release. Because this estimate ignores any effects on uninspected facilities, as well as on facilities located further away than 50 km, and the results from Weil (2001) suggest the deterrence effects of inspections of construction firms (which make up a large portion of this analysis’s sample) is smaller than the estimate of 48% from Ko et al. (2010), it is likely an underestimate of the number of inspections OSHA would need to conduct to achieve the deterrence of a press release.

While this magnitude is strikingly large, it is not unbelievable. OSHA has historically been statutorily limited in its ability to issue fines, and the likelihood that OSHA will repeatedly inspect a given facility is quite low. As a result, a standard model of crime (Becker, 1968) would predict low potential for deterrence from inspections. On the other hand, the discussion in Section 1 illustrates several substantial potential costs of publicity about OSHA violations, and a manager may be much more incentivized to improve compliance to avoid such publicity than to avoid more fines at any future potential inspections.

These results suggest exposure to a press releases reduces the number of *total* violations of OSHA regulations, but the IMIS data allow us to go one step further and estimate effects on various types of violations. Do press releases reduce the violations most likely to cause accidents? Or do they affect less serious violations that have little direct effect on safety and health? Table B.3 reports estimates,

based on Equation 2, using different measures of compliance for the dependent variable. Column (1) considers the number of “willful” (in which an employer has demonstrated either intentional disregard for requirements of the OSHA Act or a plain indifference to employee safety and health) or “repeat” (if the employer has previously been cited for the same condition or hazard) violations. Column (2) considers violations of “gravity” 10: violations are assigned a gravity on a scale of 1 to 10, with 10 being most likely to result in severe incident, and thus OSHA considers violations with a gravity of 10 to be the most hazardous. The ITT estimates reveal that exposure to a press release leads to significantly fewer of both of these, and the effect sizes are large in percentage terms. Columns (3) through (5) assess the effects on the distribution of violations by using a dependent variable equal to 1 if total violations exceed 0, 2 and 4, respectively. The magnitude of the effects in percentage terms monotonically increases across the columns, suggesting that press releases lead to an especially large decrease in especially high noncompliance.

4.2 Checks on Validity of Results

4.2.1 Robustness checks

Next, I conduct several tests to ensure the validity of the baseline results. First, I conduct tests to ensure the estimates are robust to alternative specifications and are not driven by spurious relationships.

Table B.4 shows the results of robustness checks on the baseline ITT specification on the effect of exposure to a press release on number of violations. Column (1) reproduces the baseline result. Column (2) includes a few controls, including region fixed effects, fixed effects for the year the focal penalty was issued, the number of inspections between 2005-2008 in the county and sector of the focal inspection, and the 75th percentile of penalties issued between 2005 and 2008 in the county and sector of the focal inspection. While these covariates should be uncorrelated with treatment for the RD design to be valid, including them as controls may improve efficiency. The assumption appears to be met, as the point estimate changes by a small magnitude only, and the standard error also shrinks.

One potential concern with the baseline specification is that one facility may fall within the radius of multiple “focal” inspections. As a result, we could be

classifying a facility as “treated” and “not treated” at the same time. To test how this possibility confounds the baseline results, Column (3) restricts to a facility’s earliest focal penalty (above \$25,000) over the sample period, and Column (4) restricts to a facility’s maximum focal penalty Pen_{it} over the sample period. In both cases, the coefficients are slightly larger than that from the baseline specification, which would be expected if the use of repeated observations in the baseline specification mutes the effect of “treatment.”

Column (5) defines facilities in the “vicinity” of a focal inspection as those in the same zip code rather than with a geographic radius. The sample size shrinks, but the point estimate remains highly significant and is, remarkably, identical to the baseline result.

4.2.2 Placebo Tests

I run two placebo tests to validate the causal interpretation of the above results. First, I re-run the regressions corresponding to Equation 2 but replacing the true cutoff c with a series of placebo meaningless cutoffs. If we were to find a significant coefficient using any of these meaningless cutoffs, one would worry that the above significant estimates are spurious. Table 4 displays the results. Using all cutoffs other than the true press release cutoff, the estimated coefficient is tiny and statistically indistinguishable from zero, whether the dependent variable is number of violations or log of penalties.

Second, I run a placebo test to ensure that the results are not driven by some other factor that “switches on” at penalty amounts exceeding \$40,000 or \$45,000. Recall that Regions 1 and 4 adopted the \$40,000 cutoff several years before 2009 but that all other regions had been using either a significantly higher cutoff or none at all. If we run the regression corresponding to Equation 2 but oriented around inspections with penalties levied before May 2009, and specifying c as \$40,000, one should expect a significant coefficient on D_{it} for Regions 1 and 4, but zero for all others.

Table B.5 tests these predictions. Panel (a) estimates the first-stage effect of having a penalty just over \$40,000 on the likelihood a press release is issued among inspections with penalties issued between 2002 and 2008. Column (1) shows that in Regions 1 and 4, the coefficient is 0.2 ($p < .01$), similar to the whole sample after the 2009 policy change, and Column (2) shows that the first-stage effect

is essentially zero in other regions. Panel (b) estimates the general deterrence effects on the compliance of other facilities in the same sector and within 5 km. The coefficient for Regions 1 and 4 is -0.34 ($p = .012$); reassuringly, the coefficient is essentially zero and nowhere near statistically significant in other regions.

4.3 Specific Deterrence Effects on Compliance of Publicized Facilities

The above results provide evidence that, when OSHA began publicizing facilities caught violating OSHA standards, other facilities that were most likely exposed to the publicity substantially improved their compliance. A separate question is how such publicity affected the subsequent compliance of the *publicized* facility. One theory suggests the specific and general deterrence effects of “shaming” may differ: if publicized facilities suffer a loss to reputation, and subsequently have few opportunities to signal improvements in their compliance to stakeholders, they may face weak incentives to improve compliance (Board and Meyer-ter Vehn, 2013). Additionally, incentives aside, because inspections have their own independent effect on improving compliance (Ko et al., 2010), there may be less scope for additional deterrence effects from publicity.

Estimating the specific deterrence effect of publicity on publicized facilities is complicated by the fact that, in construction, the concept of a “facility” is ill-defined. If OSHA issues penalties to a construction contractor at one work site, the next time OSHA inspects that contractor may be at a completely different site, making it both conceptually and practically challenging to create a facility identifier for inspections in this industry. The task is more straightforward for non-construction: a manufacturing plant stays in one place and is relatively easy to track across repeat inspections. Thus, for this analysis, I define a “facility” as inspections sharing the same sector and an identical latitude and longitude. Furthermore, to increase the sample size as much as possible, I combine focal penalties issued between August 2009 and September 2012 (the baseline sample) with focal penalties in Regions 1 and 4 issued between 2002 and 2008 (which, recall, were using the \$40,000 cutoff since at least 2002).

Table B.6 displays ITT regression results, corresponding to Equation 1. Columns (1) and (2) include all inspections, and Columns (3) and (4) include only non-complaint, non-referral or non-accident inspections. The point estimates for com-

pliance conditional on inspection are large and negative in all cases, and are statistically significant for violations ($p < .01$) and penalties ($p < .10$) for the sample including all inspections. Thus, these estimates suggest that press releases may have had large specific deterrence effects on the compliance of publicized facilities, though given the small sample size they should be viewed as suggestive.

5 Does Publicizing OSHA Violations Lead to Fewer Occupational Injuries?

The previous section provided evidence that facilities most likely exposed to OSHA’s press releases improved their compliance with OSHA regulations. However, a more direct measure of the social benefit is whether press releases led to improved health and safety *outcomes*. This section investigates this question.

To measure safety and health outcomes, I use the occurrence of OSHA “fat/cat” inspections: those triggered by a fatal injury or by the hospitalization of three or more workers resulting from injury. I calculate the number of such inspections that occur in a peer group over the 36 months following the date of the focal penalty, where peer groups are again defined as facilities in the same sector as and within 5 km of the focal facility.

Table 5 shows ITT regression results, corresponding to Equation 3. In Column (1), the dependent variable is the number of fat/cat inspections among peer facilities in the same sector and 5 km radius in the 36 months following the date of a focal penalty. Because fat/cat inspections are rare, these regression models include additional controls (the year of the focal inspection and the number of inspections in the focal facility’s sector and county between 2005 and 2008) to improve precision.³⁴ In Column (1), a penalty above the press release cutoff is estimated to lead to 0.12 fewer accident inspections ($p = .036$), which is 47 percent of the mean among controls.

One concern with these results is that press releases may affect the rate of OSHA inspections overall, not just those inspections triggered by a serious accident. To address this concern, Column (2) of Table 5 estimates the effect on the number of programmed inspections. Because these are exogenous to events at individual facilities, these results effectively serve as a placebo check. Reas-

³⁴Omitting these controls turns out to have essentially no effect on the results that follow.

surprisingly, the coefficient is tiny (equal to 1.8 percent of the mean) and nowhere near statistically significant. Figure A.II further bolsters the conclusion that a focal penalty above the press release threshold does not affect the number of programmed inspections of peer facilities: the figure plots the density of the number of programmed inspections of peer facilities in the 36 months following the date the focal penalty is issued. The density is smooth around the cutoff and indicates no discontinuous change in the frequency of programmed inspections.

Given that the number of programmed inspections should not be an endogenous outcome (and is not, according to Column 2), but may improve precision if included as a control when estimating the effect on the number of fat/cat inspections, Column (3) re-runs the model corresponding to Column (1) but includes the number of programmed inspections following the focal penalty as a control variable. The coefficient on *Focal penalty* $\geq c$ remains unchanged from Column (1), but the standard error decreases by a small amount.

Overall, the results in this section provide evidence that OSHA’s press releases led facilities not only to improve their compliance with OSHA regulations, but also to experience fewer serious workplace injuries and illnesses.

6 Why Do Press Releases Lead to Better Safety and Health Outcomes?

As described in Section 1, publicity about a facility caught violating safety regulations could either affect the *facility’s* reputation, which could lead stakeholders to take costly actions against publicized facilities, but it could also affect the *regulator’s* reputation, by changing other managers’ beliefs about the probability of their own OSHA enforcement. In other words, publicity may raise the expected cost of OSHA violations by increasing the expected value of financial penalties levied by OSHA. One piece of evidence against this latter story stems from the specific deterrence results that publicized facilities improve compliance following a press release. If managers use press releases to learn about OSHA enforcement, those at publicized facilities—already subject to the enforcement—learn nothing new. Because we see publicized facilities improve compliance relative to facilities that were also inspected and fined nearly identical penalties, it suggests managers and/or workers exposed to press releases are changing their behavior for reasons

other than learning about the magnitude of OSHA enforcement.

A variation on this alternative story is a press release could change managers' beliefs regarding the *priorities* of enforcement: because press releases describe the violations found in an inspection, and the associated penalties, a press release could signal that OSHA is “cracking” down on violations of particular standards. Under this scenario, peers of a publicized facility would improve compliance with the standards violated in the focal inspection, relative to other OSHA standards.

Table B.7 tests this prediction. For each focal inspection, I identify the set of OSHA standards violated, which I call the “focal” violations, and I calculate the number of focal and non-focal violations for each subsequent inspection in the same-sector and 5 km radius peer group. If managers use press releases to learn about the priorities of OSHA enforcement, one would expect the observed improvement in compliance to be driven by focal violations. However, if anything, there is a larger drop in *non-focal violations* (Column 2) relative to focal violations (Column 1), providing no evidence that general deterrence effects operate through updating beliefs about the priorities of enforcement.

If, then, it is the *facility's*—rather than the regulator's—reputation that a press release affects, a natural question is: which stakeholders care? As described in Section 1, one set of stakeholders for which information about violations of safety and health standards is especially relevant is workers. Potential new workers may choose to work elsewhere or demand higher wages upon learning an employer is unsafe, and existing workers may update their beliefs about risks and in turn demand better working conditions or quit. While plausible, there is substantial variation in the extent to which workers can demand better working conditions. If workers have limited bargaining power, then they may have little scope to leverage a press release to demand better working conditions from an employer. In other words, a press release is less likely to lead to a costly response when workers' bargaining power is low. Thus, if press releases lead to a greater improvement in OSHA compliance when workers have more bargaining power, it would suggest employers are seeking to avoid costly demands from workers.

To test this idea, I examine whether two measures of workers' bargaining power moderate the effect of press releases on compliance. One key proxy for workers' bargaining power is the strength of labor unions. A longstanding theory says that the presence of unions leads nonunion employers to improve working

conditions to forestall unionization, often called the union “threat effect” (Freeman and Medoff, 1984). Indeed, prior work has found that an increase in local unionization leads to higher nonunion wages (Neumark and Wachter, 1995). I measure the strength of labor unions in two ways. The first measure is the percent of OSHA inspections in a peer group’s focal facility’s county between 2005 and 2008 in which a union was present (*baseline unionization rate*). The second measure is whether a facility is located in a Right-to-Work (RTW) state.³⁵ RTW laws allow workers to decline to pay union dues even if they are covered by a collective bargaining agreement. Because RTW laws lead unions to face free-rider problems, they have been shown to decrease union membership and to limit the bargaining strength of existing unions (Ichniowski and Zax, 1991). RTW laws may also be correlated with other “pro-business” policies that disproportionately benefit employers relative to workers (Holmes, 1998), potentially another channel through which RTW laws are associated with lower worker bargaining power.

Table 6 tests whether these two measures of the strength of workers’ bargaining power moderate the effects of press releases on facilities’ compliance with OSHA regulations, using the shared-sector 5 km radius peer groupings. Because these tests split the sample into different geographic areas, and since the first-stage relationship (the extent to which penalties above the cutoff increased the probability of a press release) differed across OSHA states and regions, differences in the ITT effect across states may be misleading. Thus, this table shows the TOT effects for each subgroup to account for any differences in the first-stage, again using the procedure from Calonico et al. (2016).

Columns 1 and 2 split the sample by whether the peer group’s *baseline unionization rate* is above or below the sample median.³⁶ Among peer groups with relatively low baseline unionization, the TOT effect is insignificant and slightly *positive*, but among those with relatively high baseline unionization a press release is estimated to lead to 4.9 fewer violations ($p < .01$).

Columns 3 and 4 split the sample by RTW and non-RTW states. The TOT effect is very close to zero and statistically insignificant for facilities in RTW states. On the other hand, press releases lead to 5.11 fewer violations among peer facilities in non-RTW states ($p < .01$).

Of course, there could be many differences between areas with high and low

³⁵The correlation between these two measures is -.61.

³⁶The sample median is 15 percent.

union presence, other than worker bargaining power, that could lead to differential responses by employers to OSHA’s press releases. While it is not feasible to isolate the exact role of bargaining power, I briefly consider two alternative mechanisms by which union presence could moderate responsiveness. First, another stakeholder that could punish firms for poor safety and health performance is consumers, both retail consumers and also large construction firms contracting jobs out to smaller construction contractors. The extent to which consumers would be prone to punish firms for this information would plausibly be correlated with their views on OSHA and other government regulation; if a consumer has an unfavorable view of regulation, then she should be less likely to punish a company for violating OSHA regulations. To measure attitudes about government regulation, I use responses to the 2014 Pew Survey on Political Polarization in the American Public (Dimock et al., 2014), which asked respondents about their views on government regulation in general, and on environmental regulation in particular.³⁷ I construct each state’s average view as the percent of respondents who report an unfavorable view of regulation. Among the states under federal OSHA’s jurisdiction and used in the regressions report in Table 6, the average of the percent of a state’s respondents with a negative view of government regulation is 50% and 48% in low and high *baseline unionization rate* counties, respectively, and 51% and 46% in RTW and non-RTW states, respectively.³⁸ Thus, there are indeed slight differences in views on regulation across areas with high and low union presence, but the differences are not large enough to plausibly lead to large differences in consumer responses to OSHA’s press releases.

A second alternative mechanisms by which union presence could moderate responsiveness to press releases could be differences in the likelihood that media covers them. If newspaper readers are less likely to care about OSHA violations in areas where unions are weak, and newspaper coverage is one way that employers observe publicity following a press release, then the larger response in areas with high union presence could be simply because there is more media coverage of press releases. To test for this alternative, I calculate the percent of press releases

³⁷Specifically, respondents were which statement more closely aligned with their views: “Government regulation of business usually does more harm than good” vs “Government regulation of business is necessary to protect the public interest” and “Stricter environmental laws and regulations cost too many jobs and hurt the economy” vs. “Stricter environmental laws and regulations are worth the cost.”

³⁸Results are very similar for views on environmental regulation.

in each state between May 2009 and December 2012 that were covered in a newspaper, as measured by whether I found a newspaper article about the press release on newslibrary.com. 65% and 68% of press releases were covered in a newspaper in low and high *baseline unionization rate* counties, respectively; 54% and 69% of press releases were covered in a newspaper in RTW and non-RTW states, respectively. Thus, there is again some differences in media distribution of press releases across areas with high and low union presence, but the differences are not plausibly large enough to explain the differential responsiveness.

Collectively, the results in this section provide evidence that press releases led to a greater improvement in compliance when workers have more bargaining power, suggesting that one reason facilities comply more following press releases about a peer is that employers seek to avoid costly responses from workers.

7 Conclusion

Increasingly, customers, former workers, Non-Governmental Organizations, and other actors are using various platforms to “shame” companies for actions perceived as wrongdoing. Such tactics are essentially a form of targeted information disclosure seeking to mitigate an information asymmetry between a firm and its stakeholders who might value the action under scrutiny. Despite a large literature assessing how broadly-applied information disclosure affects the behavior of those firms whose actions or attributes are disclosed, there is no empirical evidence to date of how targeted information disclosure, and in particular how the *threat* of such disclosure, affects firms’ behavior.

This paper investigated the effects of a targeted disclosure policy in which a government agency, via media outlets, publicized employers found to be egregiously violating workplace safety and health regulations. Leveraging a policy change that led to quasi-random variation in whether facilities’ OSHA violations were publicized in a press release and subsequent media outlets, this paper found publicizing a facility’s violations led other peer facilities to substantially improve their compliance with OSHA regulations and experience fewer workplace injuries.

This paper’s findings have several broad implications. First, they shed light on how workplace safety—a key non-wage job attribute—is provided in the labor market. While classical labor economic theory is ambivalent about the need for

information disclosure in this domain, there is evidence that workers lack full information about job hazards and firms' safety and health performance. Such imperfect information, if present, leads to moral hazard that distorts firms' incentives to invest in safety, in which case job hazards and the rate of injuries and illnesses would be inefficiently high. The analysis suggests that facilities improve compliance following a press release to avoid their own future negative publicity. Furthermore, by comparing improvements in areas where workers are more or less likely to have stronger bargaining power, the paper provides suggestive evidence that employers' improvements are driven in part to avoid costly responses from workers. These effects would be hard to explain if the labor market were characterized by perfect information about safety and health.

Second, this paper sheds light on how knowledge transfers across firms. A central question in industrial organization and other fields in economics is how firms observe and learn from one another (Jaffe et al., 1993). Because OSHA's policy change that increased media coverage of violations was not announced publicly, this paper argued that firms learned about this new threat by observing media coverage about themselves or a peer. The results imply that facilities in close proximity to each other and in the same sector are keenly aware of publicity about one another's OSHA violations; if they were not, a press release would have no effect on safety and health outcomes among other facilities. In contrast, there is no evidence that a press release led to improvements among facilities in different sectors, suggesting (at least in this domain) that knowledge transfers within, but not across, sectors.

Third, this paper has implications for regulatory agencies. As with other regulators, OSHA has traditionally relied on inspections and fines as a primary tool for enforcing standards and promoting safety and health. This paper's estimates imply that publicizing violations from one inspection leads to a far greater decrease in non-compliance and injuries than do inspections themselves. This is not to suggest that inspections are ineffective; prior studies have found OSHA inspections lead to greater compliance (Weil, 1996; Ko et al., 2010) and fewer injuries (Haviland et al., 2012; Levine et al., 2012). But since OSHA is limited by statute in the level of fines it can levy, and limited by resources in the number of inspections it can conduct, it is plausible that the threat of publicity and media coverage of violations could have a stronger deterrent effect than inspections

themselves. This suggests that media outlets and other sources can be a valuable partner for regulatory agencies, many of which have seen their resources plateau or decline in recent decades. A question for future research is whether publicity and media coverage would also have a stronger effect than traditional enforcement in regulatory domains in which inspections are more frequent and financial penalties are higher, such as at the Environmental Protection Agency.

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Table 1: Summary Statistics

	(1) All inspections	(2)	(3) Inspections with Penalties within 10,000 of PR cutoff	(4)
Panel A: Summary Statistics	Var. <u>mean</u>	<u>SD</u>	Var. <u>mean</u>	<u>SD</u>
<i>Compliance measures</i>				
number of violations	2.01	(2.65)	8.13	(4.30)
initial penalties	4629.85	(7792.07)	37379.37	(8268.28)
Initial penalties \geq Press Release cutoff	0.01	(0.11)	0.28	(0.45)
<i>Facility characteristics</i>				
union present	0.09	(0.29)	0.13	(0.34)
Panel B: Summary Indicators	Var. <u>count</u>	% of <u>total</u>	Var. <u>count</u>	% of <u>total</u>
<i>Type of Inspection</i>				
programmed inspection	89922	59.7%	536	41.9%
complaint inspection	32018	21.3%	364	28.4%
Referral inspection	16220	10.8%	237	18.5%
fatality or catastrophe inspection	3209	2.1%	76	5.9%
Related or Other inspection	4693	3.1%	50	3.9%
<i>Industry</i>				
Ag, forestry, fishing	1009	0.7%	9	0.7%
Utilities	571	0.4%	10	0.8%
Construction	86506	57.4%	421	32.9%
Manufacturing	34280	22.8%	603	47.1%
Wholesale Trade	5287	3.5%	68	5.3%
Retail Trade	3798	2.5%	26	2.0%
Transportation, Warehousing	4647	3.1%	49	3.8%
Services	14572	9.7%	94	7.3%
Number of inspections	150670		1280	

The sample in Columns (1) and (2) includes all inspections opened Jan 2009 to Dec 2012 in states under the jurisdiction of federal OSHA. The subsample in Columns (3) and (4) consists of all inspections for which penalties were issued within the given bandwidth of the relevant press release cutoff, and excludes Regions 2 and 3. Inspections classified as Other include referral, monitoring, variance, follow-up, and other.

For OSHA regions 5, 7 and 8, the relevant press release cutoff is 45,000, and for all others it is 40,000.

Table 2: Intent-to-Treat Estimates of the General Deterrence Effect of a Press Release on Compliance of Other Facilities Within a 5 Kilometer Radius and in the Same Sector (Peers)

	(1) All inspections	(2) All inspections	(3) Programmed inspections	(4) Programmed inspections	(5) All inspections Sample period = prior to focal penalty (Placebo test)	(6) All inspections Sample period = prior to focal penalty (Placebo test)
	# viols	ln(pen- alties)	# viols	ln(pen- alties)	# viols	ln(pen- alties)
Focal penalty $\geq c$	-0.63 (0.18)**	-0.21 (0.070)**	-0.65 (0.22)**	-0.24 (0.097)*	0.057 (0.16)	-0.015 (0.085)
Obs	21593	21593	12142	12142	26735	26735
obs Pen $\geq c$	5699	5699	3179	3179	7511	7511
obs Pen $< c$	15894	15894	8963	8963	19224	19224
# Peer groups	992	992	853	853	1016	1016
Control Mean	2.24	8.26	2.09	8.24	2.36	7.75

The table shows the effects of a penalty levied on one facility (the *focal penalty*) that is above the press release cutoff c on compliance assessed in later inspections of other facilities within a 5km radius and in the same sector (). The sample includes inspections occurring in the 36 months following the date the focal penalty was issued, that were opened from Aug 2009 through Dec 2013, and for which the focal penalty was issued between Aug 2009 and Nov 2012. Columns (3) and (4) restrict the sample to programmed (pre-planned) inspections. All regressions exclude the facility responsible for the focal penalty. OSHA Regions 2 and 3, and states not under federal OSHA jurisdiction, are not included.

Each coefficient is estimated in a separate regression which controls linearly for the focal penalty with different slopes on each side of the cutoff. All regressions use a bandwidth around the press release cutoff of 10,000 and include a construction dummy. Robust standard errors clustered by peer group +P<.1, *P<.05, **P<.01.

For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000. Count variables topcoded at 99th percentiles, logged variables add the first non-zero percentile to accommodate zeros.

Table 3: Instrumental Variables (IV) Estimate of the General Deterrence Effect of a Press Release on Compliance of Other Facilities Within a 5 Kilometer Radius and in the Same Sector

	(1) Reduced Form (DV= # viols)	(2) First stage (DV=Press Release in focal inspection)	(3) IV (DV = total viols)
Focal penalty $\geq c$	-0.49 (0.14)**	0.23 (0.058)**	
Press Release in Focal Inspection			-2.10 (0.71)**
Robust p-value	< .01	< .01	< .01
# Obs	8652	8652	8652
# peer groups	449	449	449
Left Bandwidth	3256.5	3256.5	3256.5
Right Bandwidth	9590.3	9590.3	9590.3
Control Mean Dep Var	2.38	0.071	2.38

Regression estimates computed using the Stata package -rdrobust- based on the approach detailed in Calonico, Cattaneo, Farrell and Titiunik (2016). Peers of a focal facility (the facility responsible for the focal penalty) are defined as other facilities within a 5km radius and in the same sector. The running variable is the focal penalty. All regressions exclude the facility responsible for the focal penalty. The sample includes inspections occurring in the 36 months following the date the focal penalty was issued, that were opened from Aug 2009 through Dec 2013, and for which the focal penalty was issued between Aug 2009 and Nov 2012. Regions 2 and 3, and states not under federal OSHA jurisdiction, are not included.

The optimal bandwidth is chosen using the approach detailed in Calonico, Cattaneo, Farrell and Titiunik (2016). Regressions include a construction dummy. Robust standard errors clustered by peer group +P<.1, *P<.05, **P<.01.

For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000.

Table 4: Comparing Intent-to-Treat General Deterrence Effects Using the True Press Release Cutoff vs. Placebo Cutoffs

	(1)	(2)	(3)	(4)	(5)
			c= PR cutoff		
	25k	30k		55k	65k
Dep Var=# total violations					
Post inspection of peer with $\text{Pen} \geq c$	0.029 (0.098)	-0.0089 (0.12)	-0.63 (0.18)**	-0.040 (0.23)	-0.083 (0.37)
Obs	113657	61441	21603	9149	4940
Control Mean	2.15	2.20	2.24	2.11	2.20
Dep Var=ln(penalties)					
Post inspection of peer with $\text{Pen} \geq c$	0.019 (0.042)	0.0077 (0.054)	-0.21 (0.070)**	0.0033 (0.11)	-0.21 (0.17)
Obs	113657	61441	21603	9149	4940
Control Mean	8.26	8.27	8.26	8.18	8.15

The table shows the effects of a penalty levied on one facility (the *focal penalty*) that is above various cutoffs on compliance assessed in later inspections of other facilities within a 5km radius and in the same sector (). The sample includes inspections occurring in the 36 months following the date the focal penalty was issued, that were opened from Aug 2009 through Dec 2013, and for which the focal penalty was issued between Aug 2009 and Nov 2012. Columns (3) and (4) restrict the sample to programmed (pre-planned) inspections. All regressions exclude the facility responsible for the focal penalty. OSHA Regions 2 and 3, and states not under federal OSHA jurisdiction, are not included.

Each coefficient is estimated in a separate regression which controls linearly for the focal penalty with different slopes on each side of the corresponding cutoff. All regressions use a bandwidth around the corresponding cutoff of 10,000 and include a construction dummy. Robust standard errors clustered by peer group +P<.1, *P<.05, **P<.01.

For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000. Count variables topcoded at 99th percentiles; logged variables add the first non-zero percentile to accommodate zeros.

Table 5: Intent-to-treat Estimates of the Effect of Exposure to a Press release on the Number of Serious Workplace Injuries

	(1)	(2) Dep Var =	(3)
	# Fat- Cat Insp	# Pro- grammed Insp	# Fat- Cat Insp
Focal penalty $\geq c$	-0.12 (0.060)*	0.23 (1.90)	-0.12 (0.059)*
# programmed insps following focal penalty			0.0095 (0.0015)**
Obs	1204	1204	1204
Control Mean	0.26	12.7	0.26
Effect rel. to mean	-0.47	0.018	-0.47

The dependent variable in each column is the number of corresponding inspections in the 36 months following a focal penalty among other facilities in the same sector and within a 5km radius of the focal facility. Fat/Cat inspections are triggered by a fatal injury or hospitalization of 3 or more workers, and programmed inspections are pre-planned by OSHA and typically unrelated to events at the facility. All regressions use a bandwidth around the press release cutoff of 10,000. Regressions include a construction dummy, the year the focal penalty was issued, and the number of inspections in the focal facility's sector and county between 2005 and 2008. The running variable is the penalty issued at the focal inspection. The dependent variables all exclude inspections of the facility responsible for the focal penalty and include inspections up to December 2013. The sample includes focal penalties issued between August 2009 and November 2012. OSHA Regions 2 and 3, and states not under federal OSHA jurisdiction, are not included. Each coefficient is estimated in a separate regression which controls linearly for the focal penalty with different slopes on each side of the cutoff. Robust standard errors in parentheses +P<.1, *P<.05, **P<.01.

For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000. All dependent variables are topcoded at 99th percentiles.

Table 6: Do Press Releases Have a Stronger Effect on Compliance When Workers Have More Bargaining Power?

	(1) Share of pre-period inspected facilities unionized	(2) Share of pre-period inspected facilities unionized	(3) State is Right-to-Work	(4) State is Right-to-Work
	Low	High	Yes	No
Focal penalty $\geq c$	0.36 (0.92)	-4.90 (1.42)**	0.17 (0.41)	-5.11 (1.74)**
Robust p-value	0.69	0.00058	0.68	0.0033
Obs	4331	4321	2771	5881
Left Bandwidth	3256.5	3256.5	3256.5	3256.5
Right Bandwidth	9590.3	9590.3	9590.3	9590.3
Mean Dep Var	2.28	2.47	1.89	2.59

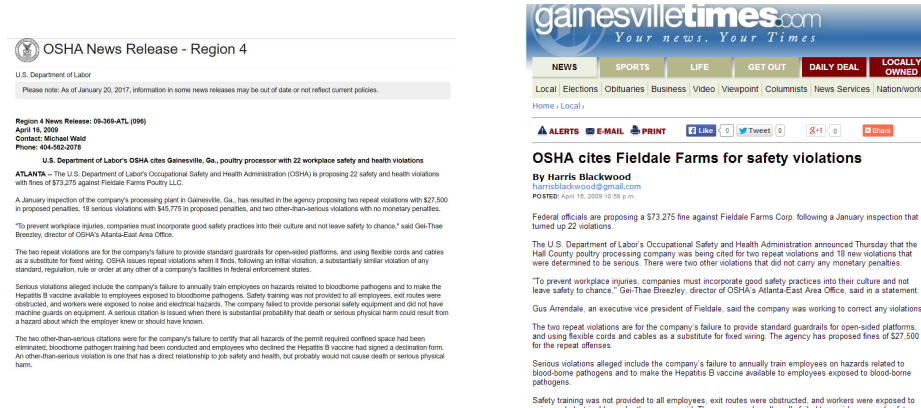
The table shows IV estimates of the effects of a press release about one facility on compliance of peers, using the approach detailed in Calonico, Cattaneo, Farrell and Titiunik (2016). Peers of a focal facility are defined as other facilities within a 5km radius and in the same sector. Columns (1) and (2) split the sample by whether the percent of inspections between 2005 and 2008 in the peer group's focal facility's county that were of unionized workplaces is above or below the sample median. Columns (3) and (4) split the sample by whether a facility is located in a Right-to-Work state or not.

The running variable is the focal penalty. All regressions exclude the facility responsible for the focal penalty. The sample includes inspections occurring in the 36 months following the date the focal penalty was issued, that were opened from Aug 2009 through Dec 2013, and for which the focal penalty was issued between Aug 2009 and Nov 2012. Regions 2 and 3, and states not under federal OSHA jurisdiction, are not included.

All regressions include a construction dummy. Robust standard errors clustered by peer group +P<.1, *P<.05, **P<.01.

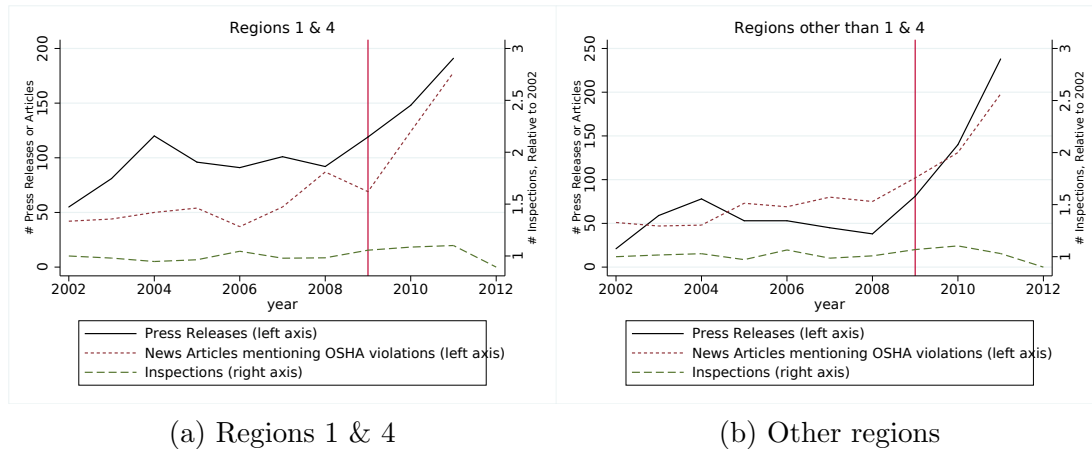
For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000. Count variables are top-coded at 99th percentiles

Figure 1: Example of an OSHA Press Release and Subsequent Media Coverage



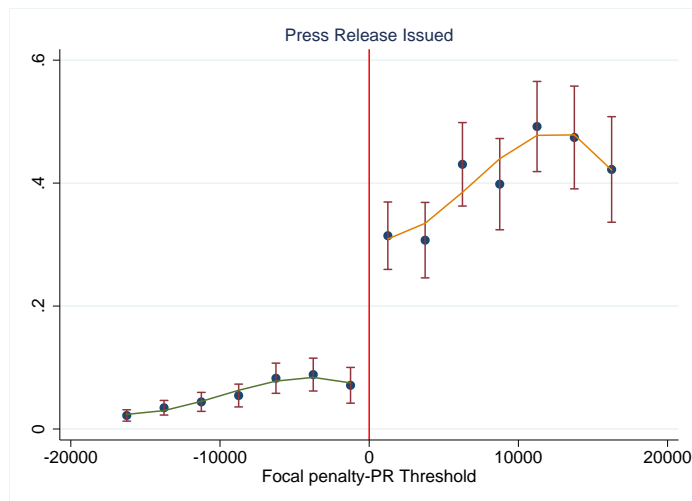
The source for panel (a) is downloaded from OSHA's archive of news releases, available at <https://www.osha.gov/news/newsreleases/region4/04162009>. The source for panel (b) is: *The Gainesville Times*, accessed March 2014.

Figure 2: Press Releases, Media Coverage, and Inspections by Year



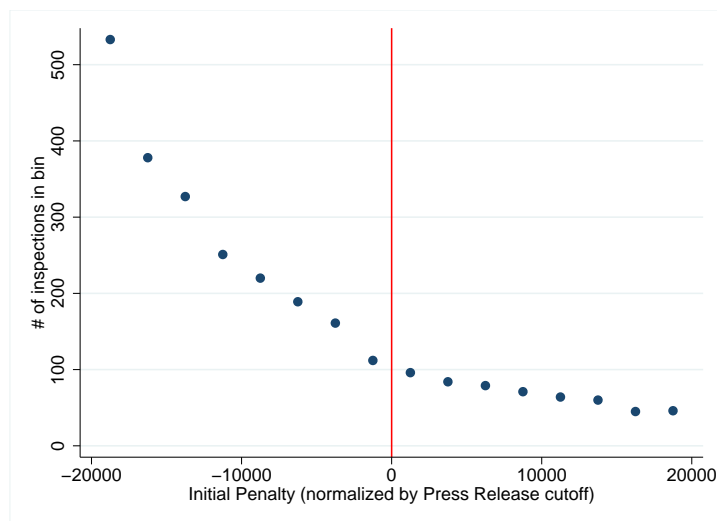
The figure gives the number of press releases on enforcement issued by OSHA each year, the number of newspaper articles in newslibrary.com mentioning "OSHA" in the title and "violations" anywhere in the text, and an index of the number of inspections, normalized by the number in 2002, each year during the period 2002-2011. Panel (a) does so for Regions 1 and 4, which used a cutoff of \$40,000 to issue press releases for the entire sample period. Panel (b) does so for all other regions, which adopted the \$40,000 cutoff rule for issuing press releases in 2009).

Figure 3: Probability of a Press Release Jumps at the Cutoff by 20-25 Percentage Points



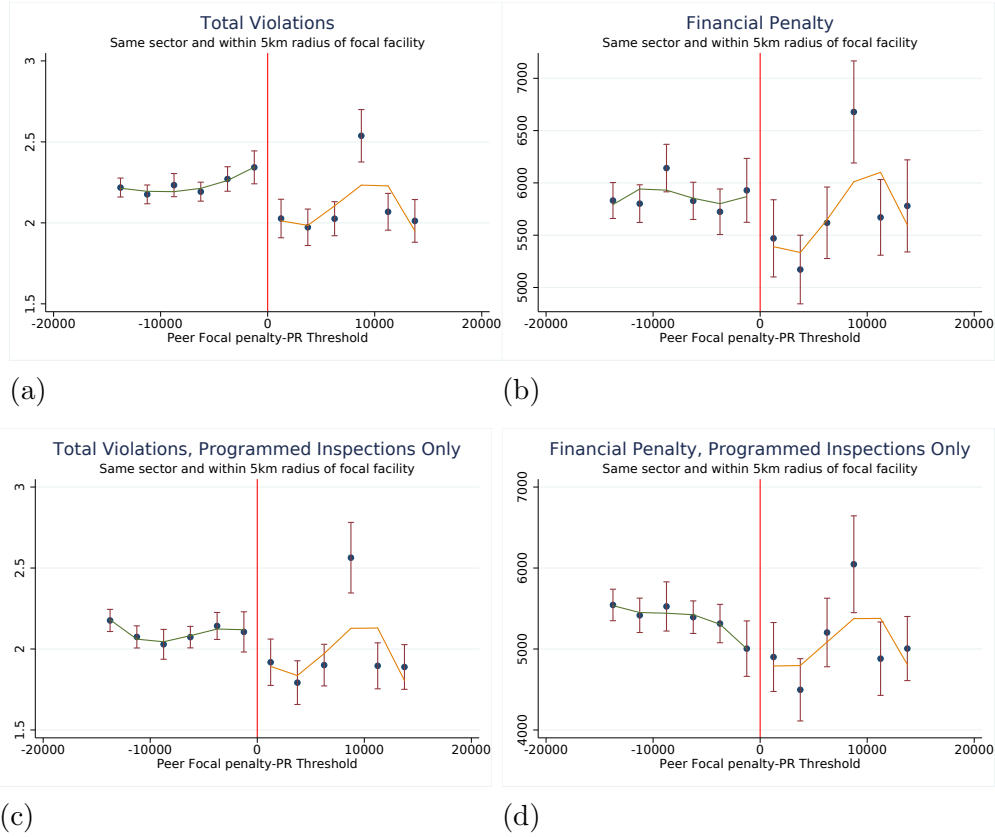
The figure shows the average of an indicator variable if an inspection resulted in a press release, ordered by the financial penalties levied at the inspection (“focal penalty”). Each dot corresponds to an average over a \$2,500 bin. The continuous lines represent third-order polynomials fitted separately on each side of the cutoff. The sample includes inspections with penalties issued from Aug 2009 through Nov 2012 and excludes Regions 2 and 3 and states not in OSHA jurisdiction.

Figure 4: Frequency of Inspections Around Press Release Cutoff



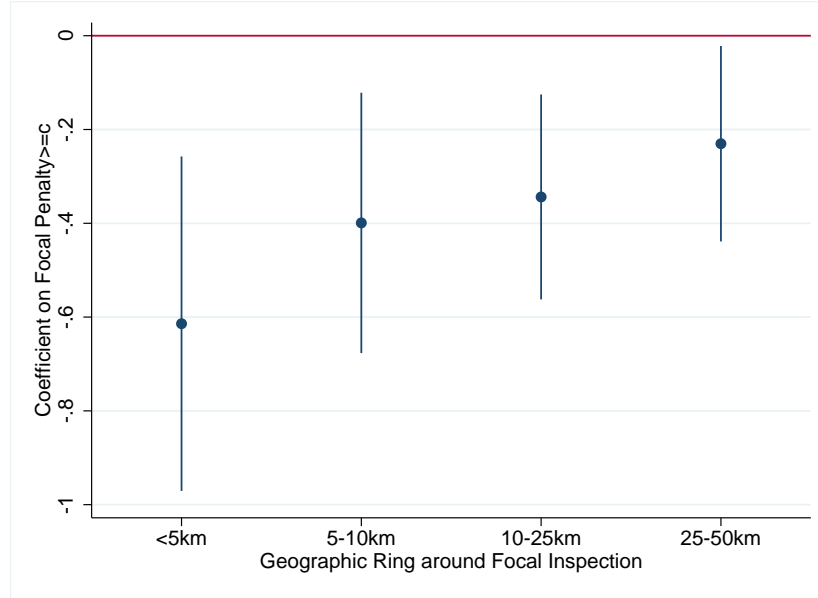
The figure shows the density of the number of inspections, by the financial penalties levied at the inspection. Each dot plots the number of inspections in a bin, where bins are defined by \$2,500 non-overlapping intervals of penalty issued. The sample includes inspections with penalties issued from Aug 2009 through Nov 2012, and excludes Regions 2 and 3 and states not in federal OSHA jurisdiction.

Figure 5: Intent-to-treat (ITT) General Deterrence Effect of a Press Release on Subsequent Compliance of Other Facilities in a 5 km Radius and in the Same Sector

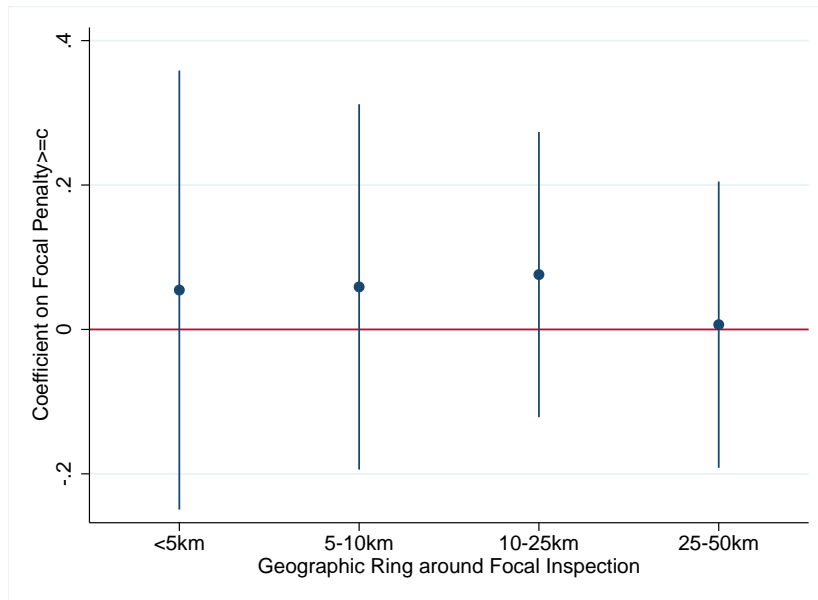


The panels show compliance for different measures of compliance, and different sample restrictions, among facilities in a 5 km radius and the same sector as an inspection with a “focal penalty” issued between Aug 2009 and Nov 2012. Each dot corresponds to an average over a \$2,500 bandwidth of focal penalty, with 90% confidence intervals included. The continuous lines represent third-order polynomials fitted separately on each side of the cutoff. The sample includes inspections occurring in the 36 months following the date the focal penalty was issued, that were opened from Aug 2009 through Dec 2013, and for which the focal penalty was issued between Aug 2009 and Nov 2012. Regions 2 and 3, and states not under federal OSHA jurisdiction, are not included.

Figure 6: Intent-to-treat (ITT) General Deterrence Effects of Press Releases as a Function of Geographic and Sectoral Distance



(a) Facilities in same sector as focal facility



(b) Facilities in different sectors from focal facility

The figures plot the point estimate and 95 percent confidence interval of the coefficient on Focal penalty $\geq c$ for facilities in the peer group of the focal facility, for different definitions of peer group. In panel (a), peer groups are defined as being in the same sector as, and within various geographic radii of, the focal facility. In panel (b), peer groups are defined as being in a different sector from, and within various geographic radii of, the focal facility.

A Appendix: Estimating the Effects of Press Releases on Compliance When Inspections are Endogenous: Formal Illustration

This appendix formally illustrates the issue that arises when a facility's compliance with OSHA regulations is only observed conditional on being inspected, and the likelihood of being inspected is potentially endogenous to being exposed to a press release. Suppose we are interested in using the number of violations of OSHA standards V_i as a metric of facility i 's compliance, but the econometrician only observes violations conditional on an inspection being opened, $V_i|I_i = 1$. Denote D_i as a dummy equal to 1 if facility i has been exposed to a press release (Treatment), and equal to 0 otherwise (Control), and suppose that exposure to a press release is randomly assigned. Using the potential outcomes framework, denote V_i^1 as violations if i is treated, and V_i^0 as violations if i is a control.

If we could measure compliance for everyone, then by random assignment of D_i comparing violations at Treatments and Controls estimates the Average Treatment Effect of press releases on the Treatment Group:

$$E(V_i^1|D_i = 1) - E(V_i^0|D_i = 0) = E(V_i^1 - V_i^0|D_i = 1) \quad (\text{A.1})$$

However, because we do not observe V_i for non-inspected facilities, we cannot directly estimate Equation A.1. A possible alternative is to estimate the treatment effect on the number of violations cited by OSHA, which captures both the effect on underlying compliance, and the likelihood that an inspection is opened:

$$\begin{aligned} &= E[V_i^1|D_i = 1, I_i = 1]Pr(I_i = 1|D_i = 1) - E[V_i^0|D_i = 0, I_i = 1]Pr(I_i = 1|D_i = 0) \\ &= \underbrace{[Pr(I_i = 1|D_i = 1) - Pr(I_i = 1|D_i = 0)]}_{\text{participation effect}} * (E[V_i^1|I_i = 1, D_i = 1]) \\ &\quad - \underbrace{(E[V_i^1|I_i = 1, D_i = 1] - E[V_i^0|I_i = 1, D_i = 0])}_{\text{Conditional on Inspection (COI) effect}} * Pr(I_i = 1|D_i = 0) \end{aligned} \quad (\text{A.2})$$

The difference in the number of violations found between those who have and have not observed a press release has two components: the first term of Equation A.2 which gives the difference in the probability an inspection is initiated (“participation” effect), and the difference in mean violations conditional on inspection (“Conditional on Inspection” (COI) effect).

These effects can be estimated separately. The COI effect is akin to comparing the number of violations found at future inspections of Treatment and Control facilities. However, the COI effect may be plagued by selection bias if treatment affects the types of facilities that get inspected—in other words, if the participation effect is not zero. To see this, we can further decompose the COI effect into two parts:

$$\begin{aligned}
& E[V_i^1 | I_i = 1, D_i = 1] - E[V_i^0 | I_i = 1, D_i = 0] \\
&= \underbrace{E(V_i^1 - V_i^0 | D_i = 1, I_i = 1)}_{\text{causal effect}} + \underbrace{E(V_i^0 | D_i = 1, I_i = 1) - E(V_i^0 | D_i = 0, I_i = 1)}_{\text{selection bias}}
\end{aligned}$$

The first term of the COI is a causal effect on Treatment facilities that get inspected. However, the second term is a form of selection bias: the difference in V_i^0 (i 's compliance in the absence of treatment) between Treatment and Control facilities that are inspected. For example, if observing a press releases causes extremely dangerous facilities (with the highest V_i^0) to improve safety hazards, thus reducing the likelihood of an inspection triggered by an accident, then Treatment facilities with the highest V_i^0 are not inspected, making the second term negative. In other words, if treatment changes the *composition* of who gets inspected, the COI effect does not have a causal interpretation—even if observing a press release is randomly assigned.

B Appendix Tables and Figures

Table B.1: Smoothness of Predetermined Variables Around Press Release Cutoff

	(1) Press Release (First stage)	(2) Complaint, referral, or fat/cat insp	(3) union present	(4) # prior inspec- tions	(5) # prior viol- ations
Penalty $\geq c$	0.19 (0.043)**	0.039 (0.060)	0.044 (0.041)	0.20 (0.24)	-0.40 (0.93)
Obs	1204	1204	1204	1204	1204
Obs Pen $\geq c$	375	375	375	375	375
Obs Pen $< c$	829	829	829	829	829
Control Mean	0.12	0.52	0.13	0.85	3.36

The sample is restricted to inspections with penalties issued between from Aug 2009 to Nov 2012. Regions 2 and 3, and states not under federal OSHA jurisdiction, are not included.

The coefficients estimate the magnitude of the change in the dependent variable for inspections with penalties at the press release cutoff. Each coefficient is estimated in a separate regression which controls linearly for penalty with different slopes on each side of the cutoff. All regressions use a bandwidth around the press release cutoff of 10,000 and include a construction dummy. Robust standard errors in parentheses. +P<.1, *P<.05, **P<.01.

For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000.

Count variables are topcoded at 99th percentiles.

Table B.2: Instrumental Variables Estimates of the Effect of a Press Release on Compliance of Facilities in the Same Sector and at Varying Geographic Distances

	(1) Reduced Form (DV= total viols)	(2) First stage (DV=Press Release in focal inspection)	(3) IV (DV = total viols)
5 km radius			
Focal penalty $\geq c$	-0.49 (0.14)**	0.23 (0.058)**	-2.10 (0.71)**
Robust p-value	0.00046	0.000065	0.0032
Obs	8652	8652	8652
# peer groups	449	449	449
Control Mean Dep Var	2.38	0.071	2.38
10 km radius			
Focal penalty $\geq c$	-0.34 (0.14)*	0.29 (0.057)**	-1.17 (0.52)*
Robust p-value	0.016	0.00000042	0.023
Obs	23268	23268	23268
# peer groups	483	483	483
Control Mean Dep Var	2.29	0.050	2.29
25 km radius			
Focal penalty $\geq c$	-0.28 (0.098)**	0.37 (0.062)**	-0.75 (0.29)**
Robust p-value	0.0046	2.1e-09	0.0091
Obs	80366	80366	80366
# peer groups	527	527	527
Control Mean Dep Var	2.28	0.055	2.28
50 km radius			
Focal penalty $\geq c$	-0.28 (0.10)**	0.36 (0.060)**	-0.78 (0.30)**
Robust p-value	0.0075	2.0e-09	0.0094
Obs	168110	168110	168110
# peer groups	540	540	540
Control Mean Dep Var	2.26	0.046	2.26

Estimates computed using the approach detailed in Calonico, Cattaneo, Farrell and Titiunik (2016). Peers of a focal facility are defined as other facilities in the same sector and within the specified radius around the focal facility. The running variable is the focal penalty. All regressions exclude the facility responsible for the focal penalty. The sample includes inspections in the 36 months following the date the focal penalty was issued, that were opened from Aug 2009 through Dec 2013, and for which the focal penalty was issued between Aug 2009 and Nov 2012. Regions 2 and 3, and states not under federal OSHA jurisdiction, are not included.

The optimal bandwidth (3,256 to the left and 9,590 to the right of the cutoff) is chosen for the 5 km radius group and is used for all other rings. Regressions include a construction dummy. Robust standard errors clustered by peer group +P<.1, *P<.05, **P<.01.

Table B.3: Intent-to-Treat Estimates of the General Deterrence Effect of a Press Release on Alternative Measures of Compliance

	(1) # repeat or willful viols	(2) # high gravity viols	(3) 0	(4) Total viols > 2	(5) 4
Focal penalty $\geq c$	-0.067 (0.021)**	-0.21 (0.076)**	-0.094 (0.034)**	-0.089 (0.029)**	-0.074 (0.019)**
Obs	21593	21593	21593	21593	21593
obs Pen $\geq c$	5699	5699	5699	5699	5699
obs Pen $< c$	15894	15894	15894	15894	15894
# Peer groups	992	992	992	992	992
Control Mean	0.11	0.69	0.70	0.32	0.15

The table shows the effects of a penalty levied on one facility (the *focal penalty*) that is above the press release cutoff c on different measures of compliance assessed in later inspections of other facilities within a 5km radius and in the same sector (). The sample includes inspections occurring in the 36 months following the date the focal penalty was issued, that were opened from Aug 2009 through Dec 2013, and for which the focal penalty was issued between Aug 2009 and Nov 2012. All regressions exclude the facility responsible for the focal penalty. OSHA Regions 2 and 3, and states not under federal OSHA jurisdiction, are not included.

The dependent variable in Column (1) is the number of violations classified as repeat or willful, and that in Column (2) is the number of violations with gravity (a measure of inspector's assessment of the likelihood that the violation will lead to a serious hazard) of 10, the highest possible score.

Each coefficient is estimated in a separate regression which controls linearly for the focal penalty with different slopes on each side of the cutoff. All regressions use a bandwidth around the press release cutoff of 10,000 and include a construction dummy. Robust standard errors clustered by peer group +P<.1, *P<.05, **P<.01. For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000. Count variables are topcoded at 99th percentiles.

Table B.4: Robustness Checks on Intent-to-Treat General Deterrence Regressions

	(1) Base- line	(2) Include Baseline controls	(3) restrict to a facility's: first focal penalty	(4) max focal penalty	(5) Peers= shared sector and zip code
Post inspection of peer with $\text{Pen} \geq c$	-0.63 (0.18)**	-0.55 (0.16)**	-0.66 (0.21)**	-0.79 (0.20)**	-0.63 (0.21)**
# inspections in focal county/sector, 2005-2008 (00s)		0.012 (0.0064)+			
75th percentile of penalties in focal county/sector, 2005-2008		0.095 (0.018)**			
Obs	21593	21593	9694	9326	7466
Control Mean	2.24	2.24	2.22	2.26	2.39
Region and Focal Year FE	N	Y	N	N	N

The dependent variable is the total number of violations resulting from an inspection. The table tests the robustness of the estimated effect of a penalty levied on one facility (the *focal penalty*) that is above the press release cutoff c on compliance assessed in later inspections of other facilities within a 5km radius and in the same sector (), except for column (5) which defines peers differently. The sample includes inspections occurring in the 36 months following the date the focal penalty was issued, that were opened from Aug 2009 through Dec 2013, and for which the focal penalty was issued between Aug 2009 and Nov 2012. Column (3) makes the restriction that, if a facility is in the radius of multiple focal penalties, only the maximum focal penalty is included in the sample. Column (4) makes a similar restriction, but only uses the first focal penalty exceeding 20,000. Column (5) defines peer groups as facilities in the same zip code and sector as the focal inspection.

The running variable is the penalty issued at the focal inspection. All regressions exclude the facility responsible for the focal penalty. Regions 2 and 3, and states not under federal OSHA jurisdiction, are not included.

Each coefficient is estimated in a separate regression which controls linearly for the focal penalty with different slopes on each side of the cutoff. All regressions use a bandwidth around the press release cutoff of 10,000 and include a construction dummy. Robust standard errors clustered by peer group +P<.1, *P<.05, **P<.01.

For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000. Count variables are topcoded at 99th percentiles.

Table B.5: The Effect of Receiving Penalties Above 40,000 Prior to 2009 on the Probability that a Press Release is Issued, and the Intent-to-Treat Effect on Future Compliance of Peer Facilities.

	(1) Regions 1, 4 (PR policy in place 2002)	(2) Regions NOT 1, 4 (PR policy begins 2009)
Dep var = Press Release Issued		
Focal penalty $\geq c$	0.20 (0.069)**	0.013 (0.023)
Obs	541	814
Control Mean	0.057	0.013
Dep Var = Violations at later inspections within 5km radius and same sector		
Focal penalty $\geq c$	-0.34 (0.13)*	-0.039 (0.21)
Obs	12316	18658
Control Mean	2.26	2.06

The sample period in all regressions includes focal penalties issued between 2002 to 2007. In the top panel, the reported coefficient estimates the first-stage relationship between whether a press release is issued and whether the penalty is above 40,000.

In the bottom panel, the coefficient estimates the effect of a penalty levied on one facility (the *focal penalty*) that is just above 40,000 on the subsequent compliance of other facilities within a 5km radius and in the same sector.

All regressions use a bandwidth around the cutoff of 10,000 and include a construction dummy and a linear term for the focal year. Regions 2 and 3, and states not under federal OSHA jurisdiction, are not included.

Each coefficient is estimated in a separate regression which controls linearly for the focal penalty with different slopes on each side of the cutoff. Robust standard errors clustered by facility in the top panel, and by peer group in the bottom panel +P<.1, *P<.05, **P<.01.

Count variables are topcoded at 99th percentiles.

Table B.6: Intent-to-Treat Estimates of the Specific Deterrence Effect of a Press Release on Future Compliance of the Publicized Facility

	(1) All inspections	(2) All inspections	(3) Excl. complaint, or accident inspections	(4) Excl. complaint, or accident inspections
	# viol- ations	ln(Initial Penalties)	# viol- ations	ln(Initial Penalties)
Post inspection with Penalty $\geq c$	-1.30 (0.49)**	-0.75 (0.41)+	-0.80 (0.63)	-0.069 (0.52)
Obs	631	631	375	375
Obs Pen $\geq c$	214	214	133	133
Obs Pen $< c$	417	417	242	242
Control Mean	2.32	7.20	1.95	6.81

The table shows the effects of a penalty levied on one facility (the *focal penalty*) that is above the press release cutoff c on compliance assessed in later inspections of that facility.

For columns (1)-(2), the sample includes inspections of a) facilities in Regions 1 and 4 that received a focal penalty in a prior inspection issued between 2002 and 2008, and b) facilities in all regions that received a focal penalty in a prior inspection issued between Aug 2009 and Nov 2012. The sample includes inspections within 36 months following the date penalties are issued in the focal inspection and through December 2013, and to focal penalties within 10,000 of the press release cutoff. In Columns (3) and (4), the sample is restricted to programmed and follow-up inspections.

Each coefficient is estimated in a separate regression which controls linearly for the focal penalty with different slopes on each side of the cutoff. Each regression includes a construction dummy, a dummy equal to 1 if the penalty was issued after May 2009, and a dummy for regions 1 and 4 (those using the policy prior to 2009). Robust standard errors clustered by facility +P<.1, *P<.05, **P<.01.

For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000.

Count variables are topcoded at 99th percentiles.

Table B.7: Do Facilities Use Press Releases to Learn About the Priorities of OSHA Enforcement?

	(1) Split violation type by relation to Focal Inspection	(2)
	# focal viols	# non-focal viols
Focal penalty $\geq c$	-0.14 (0.14)	-0.54 (0.15)**
Obs	21472	21472
Control Mean	0.87	1.40

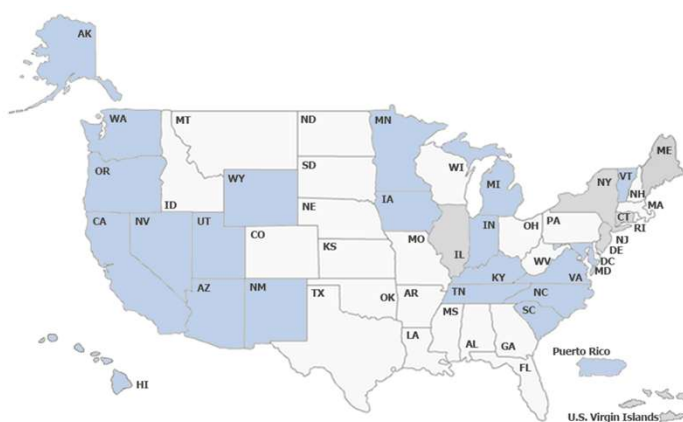
This table classifies violations by whether they are of an OSHA standard that was also violated in the inspection responsible for the peer group's focal penalty.

The table shows the effects of a penalty levied on one facility (the *focal penalty*) that is above the press release cutoff c on the number of each type of violation assessed in later inspections of other facilities within a 5km radius and in the same sector (). The sample includes inspections occurring in the 36 months following the date the focal penalty was issued, that were opened from Aug 2009 through Dec 2013, and for which the focal penalty was issued between Aug 2009 and Nov 2012. Columns (3) and (4) restrict the sample to programmed (pre-planned) inspections. All regressions exclude the facility responsible for the focal penalty. OSHA Regions 2 and 3, and states not under federal OSHA jurisdiction, are not included.

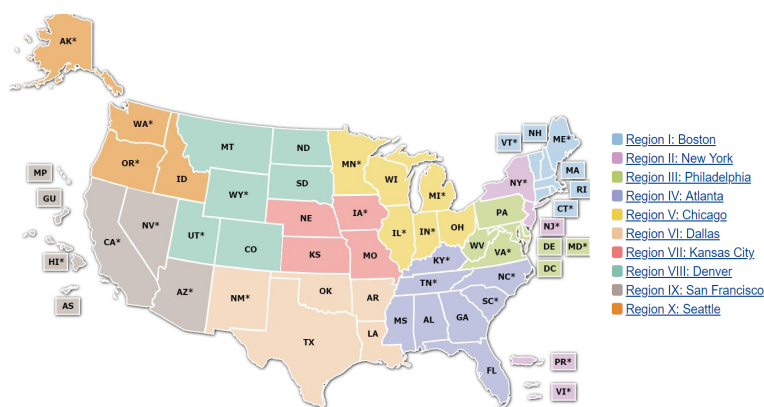
Each coefficient is estimated in a separate regression which controls linearly for the focal penalty with different slopes on each side of the cutoff. All regressions use a bandwidth around the press release cutoff of 10,000 and include a construction dummy. Robust standard errors clustered by peer group +P<.1, *P<.05, **P<.01.

For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000. Count variables are topcoded at 99th percentiles

Figure A.I: The Twenty-eight States Under OSHA's Jurisdiction, and the Location of OSHA's 10 Regions



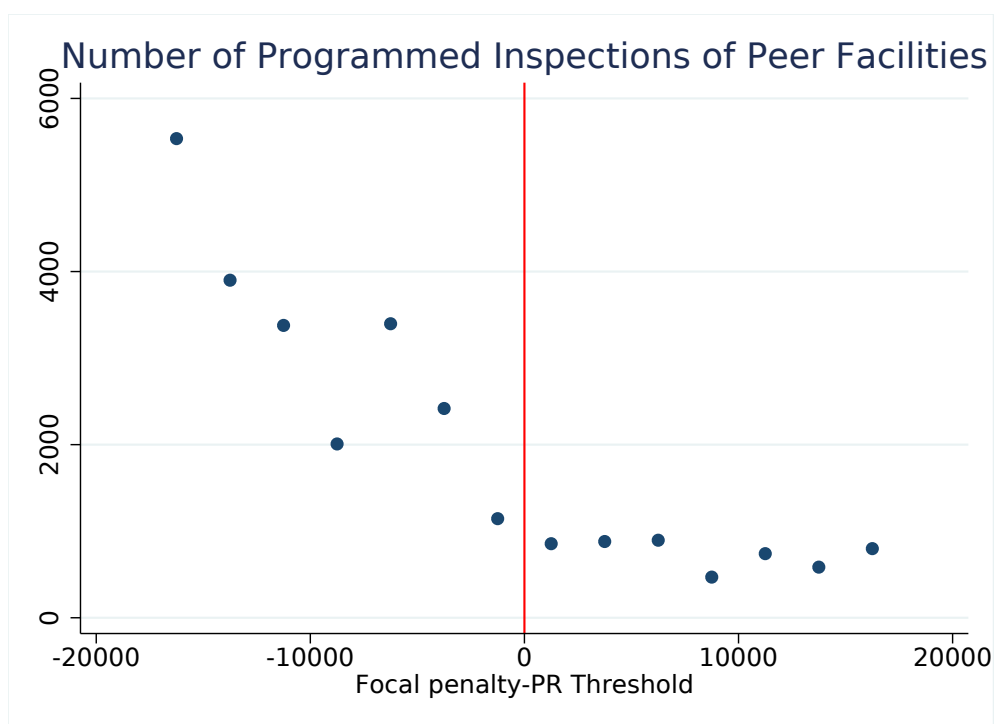
(a)



(b)

In panel (a), the states in white or grey are under federal OSHA jurisdiction. Panel (b) denotes how OSHA partitions the United States into 10 distinct regions. Source: <https://www.osha.gov/dcsp/osp/>

Figure A.II: Frequency of Programmed Inspections of Peer Facilities Following the Date a Focal Penalty is Issued



The figure shows the density of the number of programmed inspections of facilities in the same sector and within a 5 km radius of a facility with a focal penalty, by the focal penalty. Each dot plots the number of inspections in a bin, where bins are defined by \$2,500 non-overlapping intervals of penalty issued. The sample includes focal inspections with penalties issued from Aug 2009 through Nov 2012, peer inspections in the 36 months following the focal date and up to December 2013, and excludes Regions 2 and 3 and states not in federal OSHA jurisdiction.