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

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Abstract

Ratings, scores and other forms of information disclosure are widely used to incentivize firms to improve their quality or performance. Increasingly, such policies are targeted to exclusively publicize firms whose performance is deemed particularly poor–i.e. through "shaming." Shaming may affect the behavior of publicized firms ("specific deterrence"), and more broadly it may affect the behavior of other firms that may seek to avoid their own publicity ("general deterrence"). This paper studies a targeted disclosure policy in which the Occupational Safety and Health Administration (OSHA) began issuing press releases about facilities found to be in violation of safety and health regulations if the penalties levied for those violations exceeded a cutoff. Using quasi-random variation induced by this cutoff, and the media sources to which these press releases were distributed, this paper finds that publicizing the violations of one facility leads geographically proximate facilities in the same sector to improve their compliance with safety and health regulations and to experience fewer occupational injuries. The effect sizes conservatively suggest that OSHA would have to conduct at least 40 additional inspections to achieve the same improvement in compliance as that achieved with a single press release. Finally, using geographic variation in the strength of labor unions, the paper provides evidence that employers improve safety following a press release to avoid costly responses from workers.

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1 Introduction

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; Delmas et al., 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 growing 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 seek to disclose information only about firms whose quality or performance is particularly 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] or the campaigns against companies that used sweatshop labor in the 1990s (Harrison and Scorse, 2010). 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

[1] Some examples are restaurant hygiene report cards (Jin and Leslie, 2003), disclosure of drinking-water quality (Bennear 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

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.

This paper overcomes these challenges. Specifically, I investigate a policy dubbed "regulation by shaming"[6] implemented by the Occupational Safety and Health Administration (OSHA). In 2009 OSHA, the regulatory agency charged with setting and enforcing workplace safety and health standards in the U.S., began issuing press releases about facilities[7] found to be violating safety and health 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 in a recent inspection of a facility and the financial penalties levied, 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 firms' behavior. First, OSHA used a cutoff rule whereby it issued a press release about a facility's violations if the financial penalties it levied at a recent inspection were above a certain 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 publicity of a press release. The policy was only announced internally within OSHA, and not made known to the general public; as a result it led to a sharp and unexpected increase in media coverage of OSHA violations,

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

[7]Hereafter, 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.

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 how facilities respond to press releases.

Understanding the extent to which publicity like this 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. Although workplace injury rates in the U.S. have declined in recent decades, they continue to have substantial welfare costs, with one recent study estimating that they cost the U.S. $250 billion per year (Leigh, 2011).

I find that press releases revealing OSHA non-compliance lead to substantial improvements in workplace safety and health. A press release about one facility leads to 1.7 fewer violations at other facilities in the same sector within a 5 kilometer radius ("peer facilities"), a decrease of 73 percent. To put the magnitude of this deterrence effect in perspective, an OSHA inspection has been estimated to lead to between 28 and 48 percent fewer violations at later inspections of the same facility (Ko et al., 2010). Thus, this paper's estimates imply publicizing violations committed by one facility leads other peer facilities to improve compliance by two to three times as much as if OSHA inspected each of these facilities instead. Given that inspections are relatively 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, this publicity appears to be a highly effective policy tool to improve workplace safety.

Furthermore, using the occurrence of OSHA inspections triggered by a fatal, or otherwise very 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.

The paper then tests for mechanisms through which OSHA's press releases lead facilities to improve their safety and health performance. One theory is that facilities 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 safer working conditions, or a larger compensating differential for job hazards, 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 measure workers' bargaining power using two proxies for 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 (regardless of their own unionization); those in areas where unions are relatively weak display no improvement. In other words, press releases lead to improvements in safety and health conditions 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 makes multiple contributions. First, its 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 or quality, 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 the realm of 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 builds on these literatures by exploring how shaming—and targeted information disclosure in general—affects firm behavior in a regulatory environment.

Second, this paper contributes to the literature on 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 as effects 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.[8] 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 and "shaming" have been overlooked as powerful forces governing firms' compliance decisions, at least for safety and health.

The remainder of this paper is organized as follows. Section 2 provides a brief conceptual framework for why media coverage about safety may affect compliance and other investments in safety. Section 3 provides institutional background on OSHA's press release policy and describes the data, and Section 4 develops the empirical methodology. Section 5 provides the empirical results of the effects of press releases on compliance, and Section 6 describes their effect on the occurrence of workplace injuries. Section 7 explores mechanisms underlying the results, and Section 8 concludes.

2 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.

[8] Shimshack and Ward (2005) find that EPA inspections resulting in a fine lead to a substantial reduction in the statewide violation rate, whereas inspections with no fine have no detectable effect, which the authors interpret as evidence that general deterrence operates through regulator reputation. Thornton et al. (2005) surveyed 233 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, although they (rightly) caution that the causality could run in the opposite direction.

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 stakeholders use this information to change their behavior in a way that is costly to the facility, 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.[9]

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 with OSHA

[9]Relatedly, 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).

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).

3 Institutional Background and Data

3.1 OSHA's Press Release Policy

OSHA has jurisdiction over 29 states, and the remaining 21 states have their own state-run occupational safety and health plans. Thus, all discussion of OSHA's policies, and all the analysis that follows, pertains only to the 29 states under federal jurisdiction. Figure A.I provides a map of which states are under OSHA's jurisdiction.

OSHA's primary tool for monitoring and enforcing compliance with its health and safety standards is workplace inspections. During these inspections, OSHA inspectors survey a workplace's operations and assess its compliance with standards. Inspections can be triggered by an event specific to the workplace, such as a complaint (by an employee or member of the public) alleging safety and health hazards at a facility; 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 a "catastrophe"). Other inspections are pre-planned, or "programmed" in OSHA parlance. Programmed inspections are often part of a broader agenda to focus on facilities in a particular industry or at risk of a particular safety hazard, and they are typically unrelated to events at the facility itself. If, during an inspection, the inspector finds the workplace to be out of compliance with any standards, she issues citations for each violation she observes. She then calculates the appropriate financial penalty for each violation, which is a function of the size of the employer, the number of employees exposed to the hazard, and her assessment of the likelihood the violation would lead to a severe accident (U.S. Occupational Safety and Health Administration, 2009). OSHA typically issues penalties a few weeks after an inspection takes place.

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.[10]

Since at least the beginning of the 2000s, OSHA's ten regional offices around the country would issue a press release detailing the violations found and penalties issued at 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 I 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 committed 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 across regions. 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.[11]. 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 vio-

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

[11]OSHA officials were unsure of the reasons for the difference in this cutoff across regions.

lators 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.[12] Additionally, OSHA hoped press releases would serve "educational and deterrent purposes for other companies in the same industry and geographic area." [13]

Figure II illustrates the effect of the 2009 policy change on the number of press releases issued by OSHA, and media coverage of OSHA violations, each year from 2002 to 2011. For media coverage, I use the number of articles found on newslibrary.com 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 early as 2002, and panel (b) plots for the later-adopting regions. Before 2009, there are consistently very few press releases issued outside of Regions 1 and 4. Beginning in 2009, there is a muted increase in the number of releases in Regions 1 and 4 and a drastic increase everywhere else. The almost one-to-one relationship between number of press releases and newspaper articles shows that the 2009 policy change significantly changed the frequency of media coverage about OSHA violations, albeit to a greater degree in some parts of the country than in others.

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 anyway 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 to be relevant, and in general the federal OSHA office did not enforce the cutoff rule with the regional offices.

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

[13] Comments 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

Furthermore, OSHA's 10 regions varied in their adherence to the policy. The empirical analysis that follows incorporates the fuzziness of this design.

3.2 Data

This paper's primary data source is OSHA's Integrated Management Information System (IMIS), which is a database that contains detailed information on each of OSHA's inspections.[14] 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 all addresses in the database using ArcGIS to get the latitude and longitude of each inspection. As for compliance measures, 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.[15]

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, and with penalties issued before December 2012, to provide sufficient post-inspection data through September 2013 (when the dataset ends). The press release policy does not cover the 21 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. Finally, I exclude inspections in the mining industry ($< 1\%$ of

[14]The data were downloaded from OSHA's website in February 2014. IMIS can be downloaded here: http://ogesdw.dol.gov/views/data_summary.php

[15]IMIS 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.

total inspections), as this industry is under the jurisdiction of the Mine Safety and Health Administration rather than OSHA's.

Table I 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 supports the idea that 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. The average facility in the subsample with penalties near the cutoff is slightly more likely to be unionized than the average facility in the whole sample.

Roughly 60 percent of inspections in the whole sample are programmed (i.e., planned ahead of time), and 34% are triggered by a complaint, referral or "catastrophe" (a fatality, or the hospitalization of three or more workers, due to workplace injury), with the remaining 3.1% classified otherwise.[16] The share of complaint, referral or catastrophe inspections rises to 53% in the sample near the cutoff.[17]

The final panel of Table I shows the distribution of inspections across sectors.[18] OSHA inspections are concentrated largely among construction and manufacturing workplaces, both in the whole sample and the subsample around the press release cutoff.

[16]The categories in "other" include monitoring, variance, and follow-up inspections.

[17]In the IMIS database, the average penalty issued from complaint or catastrophe inspection during this period was $4,690, whereas the average for all other inspections was $2,300.

[18]Sectors 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.

Because many of these variables are so skewed to the right, I topcode count variables at their respective 99th percentiles, and I take logs of continuous variables (penalties), to ensure that the analysis is not vulnerable to outliers.[19]

To determine the extent to which the cutoff rule for issuing press releases was followed in practice, I hand-linked the IMIS data to the set of archived press releases on OSHA's website to create an indicator for each inspection in IMIS equal to 1 if the inspection resulted in a press release.[20] Figure III uses the results of this linking to illustrate the discontinuity in the probability that a press release is issued at the cutoff. The figure makes clear the probability that an inspection results in a press release jumps significantly at the cutoff by 25 to 30 percentage points, highlighting the presence of the discontinuity but also the imperfect adherence to the policy by OSHA.

4 Empirical Strategy

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

Estimating how a policy that publicizes the most egregious violators of OSHA regulations affects facilities' compliance is fraught with empirical challenges. One such challenge 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/or workers to learn of the policy was to observe a press release directly (or to

[19]For logged variables, I add the first non-zero percentile of each variable before taking the log to account for zeros.

[20]The archive of OSHA's press releases since 2001 is available here: https://www.osha.gov/newsrelease.html.

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 more likely to be exposed to ensuing media coverage. Second, press releases were also typically sent to (and 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 adoption of new technology (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.

4.2 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), in general the occurrence of 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.[21]

However, we can address this concern. First, when measuring compliance conditional on inspection, we can focus on the subset of OSHA inspections not triggered by an accident, complaint, or other event at the facility, such as those that are pre-planned by OSHA and typically part of a broader industry- or region-wide focus. Because OSHA initiates such inspections for reasons exogenous to events at the facility, there is no reason to suspect any bias from comparing compliance conditional on inspection between treated and non-treated facilities.

4.3 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

[21] This effect is similar to the Conditional-on-Positive bias discussed in Angrist and Pischke (2008).

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 c, one can estimate the effects of these press releases on later outcomes 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. 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:

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

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 financial penalty, is identified from

variation on those penalties just below and just 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 measured compliance levied at an inspection of i. For the main results, I use number of violations, and the log of initial financial penalties, to measures compliance, and I restrict attention to inspections up to 36 months following the date of a press release ($\tau \in \{0, 36\}$).

Estimating the general deterrence, or spillover, effects of a press release on non-publicized facilities requires a slightly different specification. Suppose again that facility i is inspected at time t, and that we are interested in an outcome at facility j, within a particular vicinity v of i. We model an outcome Y at j as a function of whether i was the subject of a press release. Again, because penalties at i may have their own independent effect on outcomes at j, we control flexibly for this "focal penalty:"

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

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. If $Pen_{it} \geq c$, all facilities within vicinity v have been exposed to a press release, in an Intent-to-Treat (ITT) sense. We again measure compliance as the number of violations or the log of financial penalties and restrict attention to inspections up to 36 months following the focal date t.

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 at all, 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:

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

With j dropped from the notation, here $Y_{vit\tau}$ may be the number of fat/cat in-

spections among facilities in vicinity v of focal facility i between the focal date t and τ months following t.

Because press releases were covered in local newspapers and industry publications, the "vicinity" v has both a geographic and an industry component. As the main specification, I define a facility j to be in the vicinity of i if it is within a particular geographic radius of i (e.g., within a radius of 5 kilometers), and in the same sector (as defined as in Table I). 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 vicinity v of a particular focal inspection.

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. Robustness checks consider alternatives. Finally, 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.

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.

4.4 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 relevant cutoff for press releases

is as good as random. This assumption is valid if those involved have imperfect control over the exact penalty amount issued, and it can be jeopardized if there is room for manipulation. For example, if there are reputational costs associated with publicity about poor safety, the disutility from penalties is discontinuous at the cutoff c. If managers know the value of c they may prefer 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 to begin with. Furthermore, much evidence suggests that penalties levied by an OSHA inspector are a stochastic function of true noncompliance. 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). The stochastic element of the penalty function introduces an element of randomness from the facility's perspective, which would limit its ability to control the exact penalty given a level of true noncompliance.

On the other hand, in theory there is room for manipulation by the OSHA inspectors, since they issue violations and associated penalties themselves. For example, an inspector could tip a facility over the penalty cutoff if she thinks it deserves bad publicity, or in theory 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 IV 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 during the period May 2009 to 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 II shows the results of local linear regressions, estimating Equation 1, with $\tau = 0$ and $Y_{it\tau}$ equal to one of various baseline covariates measured at the time of the focal inspection, using a window of $10,000 around the cutoff c. The results show no evidence of a discontinuity in any covariates, providing further support an RD design will yield valid identification in this setting.[22]

5 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 tests for "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 briefly tests the "specific deterrence" effects of a press release on the compliance of the publicized facility.

5.1 General Deterrence Effects of OSHA Press Releases on Compliance

Figure V graphically tests whether facilities exposed to a press release have different levels of compliance with OSHA standards conditional on being inspected. Here, the

[22] An alternative way to check for smoothness in baseline covariates is to run a regression with D_{it} as the dependent variable, and to 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 in the paper, 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.

unit of analysis is the inspection, and the sample includes 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 log of 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 downward shift in non-compliance among facilities whose focal penalty is just to the right of the cutoff c, suggesting that facilities exposed to a press release improve their compliance with OSHA regulations.

Intent-to-treat (ITT) regression results for these effects are shown in Table III using a bandwidth of $10,000 around the cutoff and controlling linearly for the running variable. Columns (1)-(2) show the baseline results. Inspections of facilities within 5 km of and in the same sector as a facility with a focal penalty above the press release cutoff have -0.63 fewer violations and 33 percent lower financial penalties (exp(-0.40)-1) ($p < .01$ in both cases). Columns (3)-(4) restrict to programmed inspections (those that are by definition exogenous to events at the facility). The coefficients are essentially unchanged.

Columns (5)-(6) explore temporal effects of exposure to a press release, estimating the dynamic effects on compliance over time.[23] Effects show up immediately and remain through 36 months after the date the focal penalty is issued.

While the figures and ITT regressions each provide evidence that penalties above the press release cutoff lead to significantly 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

[23] These regressions utilize a a variant of Equation 2 of the form:

$$Y_{jit\tau} = \big(\sum_{k} \alpha_k + \gamma_k * D_{it} * \alpha_k\big) + f(Pen_{it} - c) + \epsilon_{jit\tau}$$

with: $k = \{\tau \in \{0-6\}, \tau \in \{7-12\}, \tau \in \{13-24\}, \tau \in \{25-36\}\}$ months.

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. To estimate the TOT effect, 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 those observations closer to the cutoff), rather than a uniform kernel, which the baseline ITT specification uses.

Table IV contains the estimates of the TOT effects. The first column is analogous to Column (1) of Table III, estimating the ITT (or "Reduced Form") 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 estimate, -0.45 ($p < .01$), is smaller than the ITT estimate using the uniform kernel and bandwidth of 10,000 (-0.63), but the difference is not statistically significant. The estimated first stage, in Column (2), is 0.26 ($p < .01$). Finally, the TOT estimate, essentially the ratio of Column (1) over Column (2), in Column (3) is -1.72 ($p < .01$). Since Controls (facilities with a focal inspection yielding penalties to the left of the cutoff) averaged 2.36 violations, the TOT estimate implies that a press release led to 73% fewer violations at later inspections of other facilities in the same sector within a 5 km radius.

To put the magnitude of the TOT effect in perspective, a useful benchmark is the estimated deterrence 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. The literature has estimated that one OSHA inspection leads to 28 to 48 percent fewer violations at later inspections (Ko et al., 2010). The results in Table IV suggest that a press release leads other facilities within a 5 km radius and the same sector to improve their compliance by two to three

times more than if OSHA inspected each of those facilities directly. Put another way, given that there are on average 20 inspections in each peer group in the sample used in the regressions reported in Table IV, OSHA would have to conduct at least 40 inspections to elicit the same level of deterrence as a single press release.[24]

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 very low potential for deterrence from inspections. On the other hand, the discussion in Section 2 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 inspections that may or may not take place.

Finally, Figure VI explores how the general deterrence effects of press releases change depending on how "vicinity" to the focal facility is defined. As discussed in Section 4, a press release about one facility is most likely to affect behavior at other facilities in the same sector and in close geographic proximity. Panel (a) of Figure VI plots the ITT point estimates and 95 percent confidence intervals from Equation 2 with "vicinity" still requiring a shared sector, for 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.

On the other hand, Panel (b) of Figure VI plots the ITT estimate of the general deterrence effect for facilities in sectors *different* from the focal facility. Interestingly, 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 prior work that has found evidence of knowledge spillovers

[24]Because this ignores any effects of press releases on uninspected facilities, and on facilities located further away than 5 km, it is likely a substantial underestimate of the number of inspections OSHA would need to conduct to achieve the deterrence of a press release.

within, but not across, industries (Bloom et al., 2017).

While these results suggest exposure to a press releases reduces the number of *total* violations of OSHA regulations, the IMIS data allow us to go one step further and estimate effects on various types of violations. For example, do press releases reduce the number of violations most likely to cause accidents? Or do they affect less serious violations that have little to no direct effect on safety and health? Table A.I reports estimates from variants on Equation 2, using different measures of compliance for the dependent variable. Column (1) considers the number of violations classified as "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). Column (2) considers violations of "gravity" 10: each violation is assigned a gravity on a scale of 1 to 10, with 10 being those 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 (as a percent of the corresponding sample means) monotonically increases across the columns, suggesting that observing a press release leads to an especially large decrease in especially high noncompliance.

5.2 Checks on Validity of Results

5.2.1 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 V shows the results of robustness checks on the baseline 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 (lying in a geographic boundary of multiple facilities with penalties near the press release cutoff). 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 observations in which a facility's focal penalty Pen_{it} is its maximum focal penalty 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 coefficient remains highly significant and is, remarkably, identical to the baseline result.

5.2.2 Placebo Tests

We 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 VI 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 A.II tests these predictions. Panel (a) estimates the first-stage effect of having a penalty just over $40,000 on the likelihood a press release is issued. The sample is restricted to inspections with penalties issued between 2002 and 2008. Column (1) shows that in Regions 1 and 4, the coefficient is 0.2 ($p < .01$), similar to the whole sample after the 2009 policy change, and Column (2) shows that the first-stage effect is essentially zero in other regions. Panel (b) estimates the general deterrence effects of a focal penalty exceeding the press release cutoff on the compliance of other facilities in the same sector and within 5 km. The coefficient for Regions 1 and 4 is -0.34 ($p = .012$), which is slightly smaller than the overall sample after 2009. Reassuringly, Column (2) shows that the coefficient is essentially zero and nowhere near statistically significant in other regions.

5.3 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. Indeed, one theory sug-

gests the specific and general deterrence effects of "shaming" may not behave the same way: if publicized facilities suffer a loss to their 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 (Gray and Jones, 1991; Ko et al., 2010), there may be less scope for additional deterrence effects from the publicity.

Empirically 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 inspection OSHA conducts of 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: for example, a manufacturing plant stays in one place, neatly fits the concept of an "establishment," 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 A.III displays ITT regression results, corresponding to Equation 1. Columns (1) and (2) include all types of inspections, and Columns (3) and (4) include only non-complaint, non-referral or non-accident inspections. The point estimates for compliance conditional on inspection are large and negative in all cases, and are statistically significant for violations ($p < .01$) and log penalties ($p < .10$) for the sample including all types of inspections. Thus, these estimates suggest that OSHA's press releases may have had large specific deterrence effects on the compliance of publicized facilities, though given the small sample size these results should be viewed as suggestive.

6 Does Publicizing OSHA Violations Lead to Fewer Occupational Injuries?

The previous section provided evidence that facilities most likely exposed to one of OSHA's press releases significantly improved their compliance with OSHA regulations. However, an arguably more important measure of the social benefit of press releases is whether they led to an improvement in workplace 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. If peer groups exposed to a press release experience fewer such inspections, it implies that they experienced fewer very serious workplace injuries.

Table VII shows ITT regression results, corresponding to the regression in Equation 3. In Column (1), the dependent variable is the number of fat/cat ("accident") inspections in the 36 months following the issuance date of a focal penalty, among facilities within 5 km and in the same sector as the focal facility. Because fat/cat inspections are rare, these regression models include a few additional controls to improve precision. Specifically, the models include the year of the focal inspection and the number of inspections in the focal facility's sector and county between 2005 and 2008.[25] In Column (1), a penalty above the press release cutoff is estimated to lead to 0.13 fewer accident inspections ($p = .041$) among other facilities within 5 km and in the same sector over the following 36 months, 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 VII estimates the effect on the number of

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

programmed inspections. Because these are exogenous to events at individual facilities, these results effectively serve as a placebo check. Reassuringly, the coefficient on *Focal penalty* $\geq c$ is tiny (equal to 1.5 percent of the mean) and nowhere near statistically significant.

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, slightly improving statistical significance ($p = .034$).

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.

7 Mechanisms: Why Do Press Releases Lead to Better Safety and Health Outcomes?

This section investigates the mechanism through which OSHA's press releases affect compliance with safety and health standards, by attempting to disentangle whether press releases improve compliance by affecting the facility's reputation or the regulator's reputation.

As described in Section 2, publicity about a facility caught violating safety regulations may lead managers at other facilities to improve their compliance so as to avoid their own future reputation-damaging news. If workers and other stakeholders who value workplace safety learn from press releases, and in turn take costly actions against a publicized facility, publicity imposes an additional cost to OSHA violations, above and beyond financial penalties levied by OSHA, that affects the marginal benefit of

non-compliance.

An alternative mechanism through which press releases could affect compliance, also described in Section 2, is by changing other managers' beliefs about the probability of their own OSHA enforcement. In other words, publicity raises the expected cost of OSHA violations by increasing the expected value of financial penalties levied by OSHA. One piece of evidence against this story stems from the specific deterrence results that publicized facilities improve their 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 OSHA enforcement.

A variation on this alternative story is a press release could change managers' beliefs regarding the *priorities* of enforcement: because press releases provide detailed descriptions of the specific violations found in an inspection, and the associated penalties, a press release could signal that OSHA is "cracking" down on violations of a particular set of 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 VIII 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 peer group (which, as in the baseline, includes inspections within 5 km and in the same sector). 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 (those described in the press release). 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.

But if it really is the *facility's* reputation—rather than the regulator's reputation—that a press release affects, a natural question is: which stakeholders care? As described in Section 2, 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 to no 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 that could arise following publicity.

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.[26] 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

[26]The correlation between these two measures is -.61.

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). According to Holmes (1998), RTW laws may also be correlated with other "pro-business" policies that disproportionately benefit employers relative to workers, potentially another channel through which RTW laws are associated with lower worker bargaining power.

Table IX tests whether these two measures of the strength of workers' bargaining power moderate the effects of press releases on facilities' compliance with OSHA regulations. 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.[27] 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 3.9 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 3.76 fewer violations among peer facilities in non-RTW states ($p < .01$).

Thus, the results in Table IX collectively provide strong evidence that press releases lead to a greater improvement in compliance when workers have more bargaining power, suggesting that one reason facilities improve compliance following press releases about a peer is that employers seek to avoid costly responses from workers.

[27]The sample median is 15 percent.

8 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 at OSHA that led to quasi-random variation in whether facilities' OSHA violations were covered 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 serious 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 and other stakeholders 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 job hazards and safety and health.

Second, this paper provides insight into how knowledge transfers across firms and workplaces. A central question in industrial organization and other fields in economics is how knowledge spills over across firms, and how firms observe and learn from one another (Jaffe et al., 1993). Because OSHA's policy change that vastly increased media coverage of violations was not announced publicly, this paper argued that firms learned about this new threat by observing media coverage, either 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 comparison suggests that media outlets and other sources can be a valuable and low-cost partner for regulatory agencies, many of which have seen their resources plateau or decline in recent decades. An interesting 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 typically much higher, such as at the Environmental Protection Agency (EPA).

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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:

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

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:

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

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.*

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Table I: Summary Statistics

	(1)	(2)	(3)	(4)
	All inspections		Inspections with Penalties within 10,000 of PR cutoff	
Panel A: Summary Statistics	Var. mean	SD	Var. mean	SD
Compliance measures				
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)
Facility characteristics				
union present	0.09	(0.29)	0.13	(0.34)
Panel B: Summary Indicators	Var. count	% of total	Var. count	% of total
Type of Inspection				
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%
Industry				
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 II: 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 inspections	(5) # prior violations
Penalty $\geq c$	0.19	0.039	0.044	0.20	-0.40
	(0.043)**	(0.060)	(0.041)	(0.24)	(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 III: Intent-to-Treat Estimates of the General Deterrence Effect of a Press Release on Compliance of Peer Facilities

	(1)	(2)	(3)	(4)	(5)	(6)
	All inspections		Programmed inspections		All inspections	
	# viols	ln(penalties)	# viols	ln(penalties)	# viols	ln(penalties)
Focal penalty$\geq c$	-0.63	-0.40	-0.65	-0.48		
	(0.18)**	(0.14)**	(0.22)**	(0.19)*		
Focal penalty$\geq c$, 0-6 months post					-0.55	-0.38
					(0.17)**	(0.13)**
Focal penalty$\geq c$, 6-12 months post					-0.74	-0.42
					(0.18)**	(0.14)**
Focal penalty$\geq c$, 12-24 months post					-0.51	-0.36
					(0.19)**	(0.14)**
Focal penalty$\geq c$, 24-36 months post					-0.79	-0.53
					(0.19)**	(0.16)**
Obs	21589	21589	12140	12140	21589	21589
obs Pen$\geq c$	5695	5695	3177	3177	5695	5695
obs Pen$< c$	15894	15894	8963	8963	15894	15894
# Peer groups	989	989	851	851	989	989
Control Mean	2.24	7.40	2.09	7.42	2.24	7.40

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 5 km radius and in the same sector ("peers"). 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 IV: Instrumental Variables (IV) Estimate of the General Deterrence Effect of a Press Release on Compliance of Peer Facilities

	(1) Reduced Form (DV= total viols)	(2) First stage (DV=Press Release Issued About Focal Facility)	(3) IV (DV = total viols)
Focal penalty $\geq c$	-0.45	0.26	
	(0.15)**	(0.065)**	
Press Release Issued About Focal Facility			-1.72
			(0.64)**
Robust p-value	0.0030	0.000054	0.0072
Obs	8488	8488	8488
Left Bandwidth	3597.4	3597.4	3597.4
Right Bandwidth	7915.9	7915.9	7915.9
Mean Dep Var	2.36	0.063	2.36

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 5 km 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 V: 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
Focal penalty$\geq c$	-0.63	-0.55	-0.66	-0.69	-0.63
	(0.18)**	(0.16)**	(0.21)**	(0.21)**	(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	21589	21589	9613	9144	7466
Control Mean	2.24	2.24	2.21	2.28	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 5 km radius and in the same sector ("peers"), 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 VI: Comparing Intent-to-Treat General Deterrence Effects Using the True Press Release Cutoff vs. Placebo Cutoffs

	(1)	(2)	(3)	(4)	(5)
			c=		
	25k	30k	PR cutoff	55k	65k
Dep Var=# total violations					
Focal penalty$\geq c$	0.029	-0.0081	-0.63	-0.0093	-0.083
	(0.098)	(0.12)	(0.18)**	(0.22)	(0.37)
Obs	113710	61433	21594	9377	4939
Control Mean	2.15	2.19	2.24	2.09	2.20
Dep Var=ln(penalties)					
Focal penalty$\geq c$	0.036	-0.0062	-0.40	0.092	-0.32
	(0.082)	(0.11)	(0.14)**	(0.21)	(0.30)
Obs	113710	61433	21594	9377	4939
Control Mean	7.41	7.43	7.40	7.30	7.40

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 5 km radius and in the same sector ("peers"). 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 actual 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 VII: Intent-to-Treat Estimates of the Effect of Exposure to a Press Release on the Number of Serious Workplace Injuries

	(1)	(2)	(3)
	Dep Var =		
	# Fat-Cat Insps	# Programmed Insps	# Fat-Cat Insps
Focal penalty $\geq c$	-0.13	0.21	-0.13
	(0.061)*	(2.16)	(0.060)*
# programmed insps following focal penalty			0.0087
			(0.0011)**
Obs	1204	1204	1204
Control Mean	0.26	14.0	0.26
Effect rel. to mean	-0.47	0.015	-0.48

The dependent variable in each column is the number of corresponding inspections in the 36 months following the date that a penalty is levied on one facility (the "focal penalty") among other facilities in the same sector and within a 5 km 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 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 VIII: Do Facilities Use Press Releases to Learn About the Priorities of OSHA Enforcement?

	(1)	(2)
	Split violation type by Relation to Focal Inspection	
	# focal viols	# non-focal viols
Focal penalty$\geq c$	-0.14	-0.54
	(0.14)	(0.15)**
Obs	21468	21468
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 5 km radius and in the same sector ("peers"). 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.

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

	(1)	(2)	(3)	(4)
	Share of pre-period inspected facilities unionized		State is Right-to-Work	
	Low	High	Yes	No
Press Release Issued About Focal Facility	0.31	-3.90	-0.049	-3.76
	(0.98)	(1.05)**	(0.40)	(1.38)**
Robust p-value	0.75	0.00022	0.90	0.0063
Obs	4112	4376	2590	5898
Left Bandwidth	3597.4	3597.4	3597.4	3597.4
Right Bandwidth	7915.9	7915.9	7915.9	7915.9
Mean Dep Var	2.28	2.42	1.88	2.55

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.

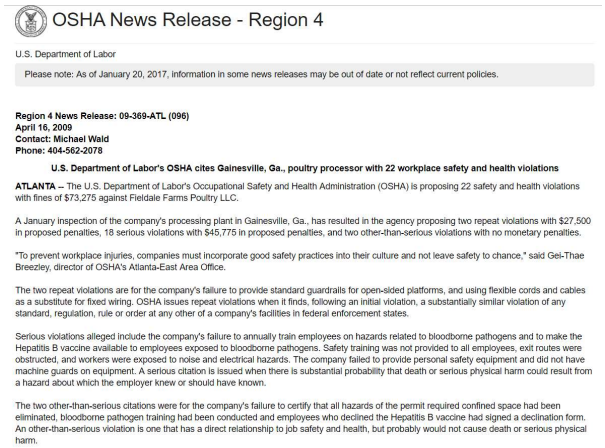
Columns (1) and (2) split the sample by whether percent of inspections between 2005 and 2008 in the peer group's focal facility's county that were of unionized workplaces is above or below the sample median. Columns (3) and (4) split the sample by whether a facility is located in a Right-to-Work state 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 topcoded at 99th percentiles

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



OSHA News Release - Region 4

U.S. Department of Labor

Please note: As of January 20, 2017, information in some news releases may be out of date or not reflect current policies.

Region 4 News Release: 09-369-ATL (096)
April 16, 2009
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U.S. Department of Labor's OSHA cites Gainesville, Ga., poultry processor with 22 workplace safety and health violations

ATLANTA – The U.S. Department of Labor's Occupational Safety and Health Administration (OSHA) is proposing 22 safety and health violations with fines of $73,275 against Fieldale Farms Poultry LLC.

A January inspection of the company's processing plant in Gainesville, Ga., has resulted in the agency proposing two repeat violations with $27,500 in proposed penalties, 18 serious violations with $45,775 in proposed penalties, and two other-than-serious violations with no monetary penalties.

"To prevent workplace injuries, companies must incorporate good safety practices into their culture and not leave safety to chance," said Gei-Thae Breezley, director of OSHA's Atlanta-East Area Office.

The two repeat violations are for the company's failure to provide standard guardrails for open-sided platforms, and using flexible cords and cables as a substitute for fixed wiring. OSHA issues repeat violations when it finds, following an initial violation, a substantially similar violation of any standard, regulation, rule or order at any other of a company's facilities in federal enforcement states.

Serious violations alleged include the company's failure to annually train employees on hazards related to bloodborne pathogens and to make the Hepatitis B vaccine available to employees exposed to bloodborne pathogens. Safety training was not provided to all employees, exit routes were obstructed, and workers were exposed to noise and electrical hazards. The company failed to provide personal safety equipment and did not have machine guards on equipment. A serious citation is issued when there is substantial probability that death or serious physical harm could result from a hazard about which the employer knew or should have known.

The two other-than-serious citations were for the company's failure to certify that all hazards of the permit required confined space had been eliminated, bloodborne pathogen training had been conducted and employees who declined the Hepatitis B vaccine had signed a declination form. An other-than-serious violation is one that has a direct relationship to job safety and health, but probably would not cause death or serious physical harm.

(a)

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OSHA cites Fieldale Farms for safety violations

By Harris Blackwood
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POSTED: April 16, 2009 10:58 p.m.

Federal officials are proposing a $73,275 fine against Fieldale Farms Corp. following a January inspection that turned up 22 violations.

The U.S. Department of Labor's Occupational Safety and Health Administration announced Thursday that the Hall County poultry processing company was being cited for two repeat violations and 18 new violations that were determined to be serious. There were two other violations that did not carry any monetary penalties.

"To prevent workplace injuries, companies must incorporate good safety practices into their culture and not leave safety to chance," Gei-Thae Breezley, director of OSHA's Atlanta-East Area Office, said in a statement.

Gus Arrendale, an executive vice president of Fieldale, said the company was working to correct any violations.

The two repeat violations are for the company's failure to provide standard guardrails for open-sided platforms, and using flexible cords and cables as a substitute for fixed wiring. The agency has proposed fines of $27,500 for the repeat offenses.

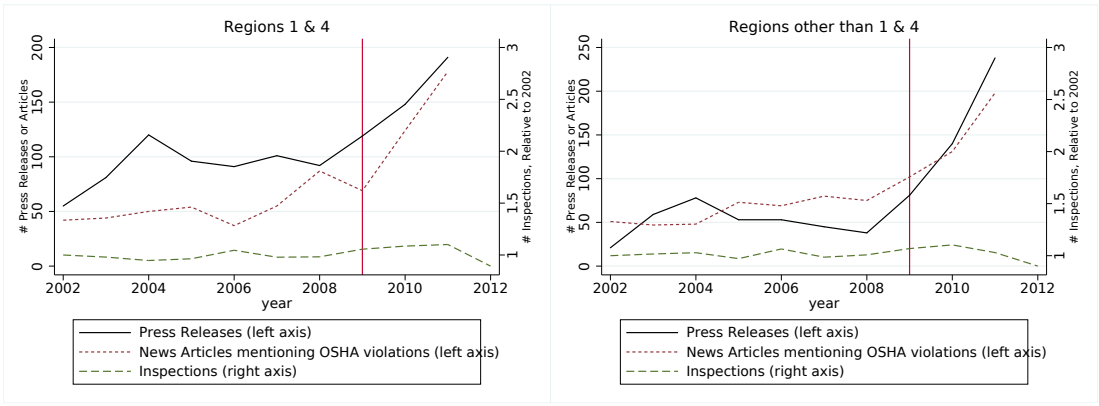
Serious violations alleged include the company's failure to annually train employees on hazards related to blood-borne pathogens and to make the Hepatitis B vaccine available to employees exposed to blood-borne pathogens.

Safety training was not provided to all employees, exit routes were obstructed, and workers were exposed to

(b)

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.

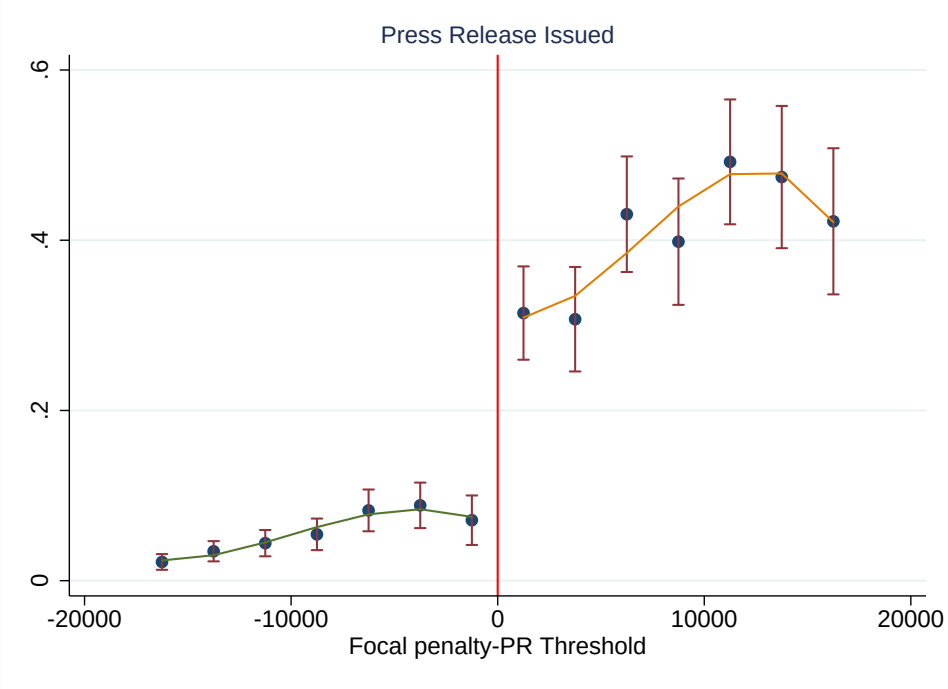
Figure II: Press Releases, Media Coverage, and Inspections by Year



(a) Regions 1 & 4 (b) Other regions

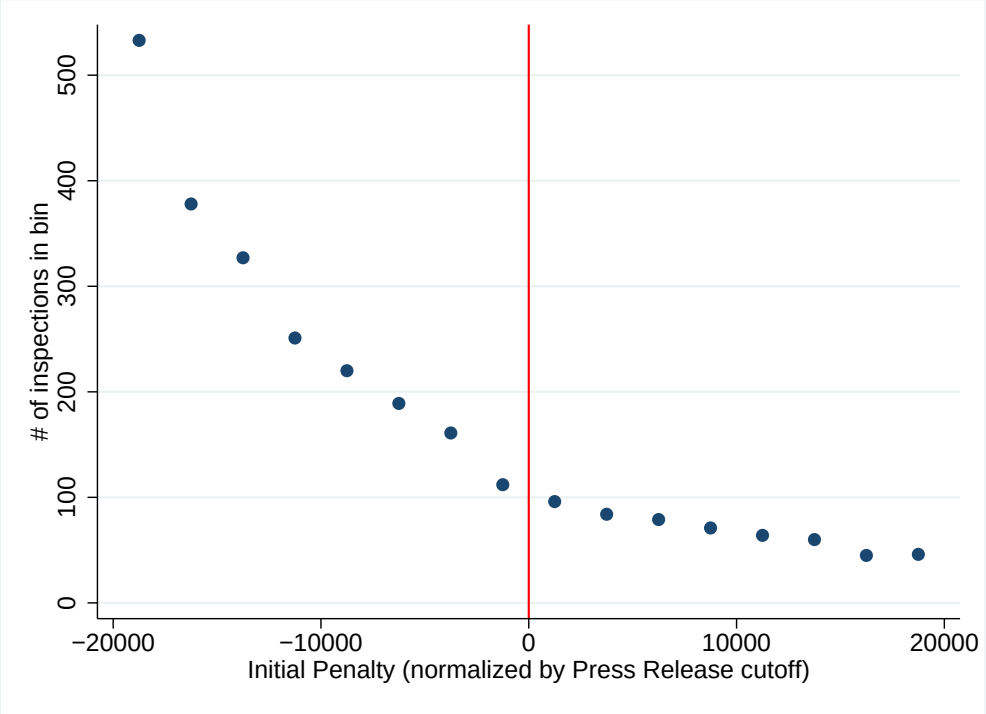
The figure gives the number of press releases on enforcement issued by OSHA each year, the number of newspaper articles in newslibrary.com mentioning "OSHA" in the title and "violations" anywhere in the text, and an index of the number of inspections, normalized by the number in 2002, each year during 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 III: Probability of a Press Release Jumps at the Cutoff by 25-30 Percentage Points



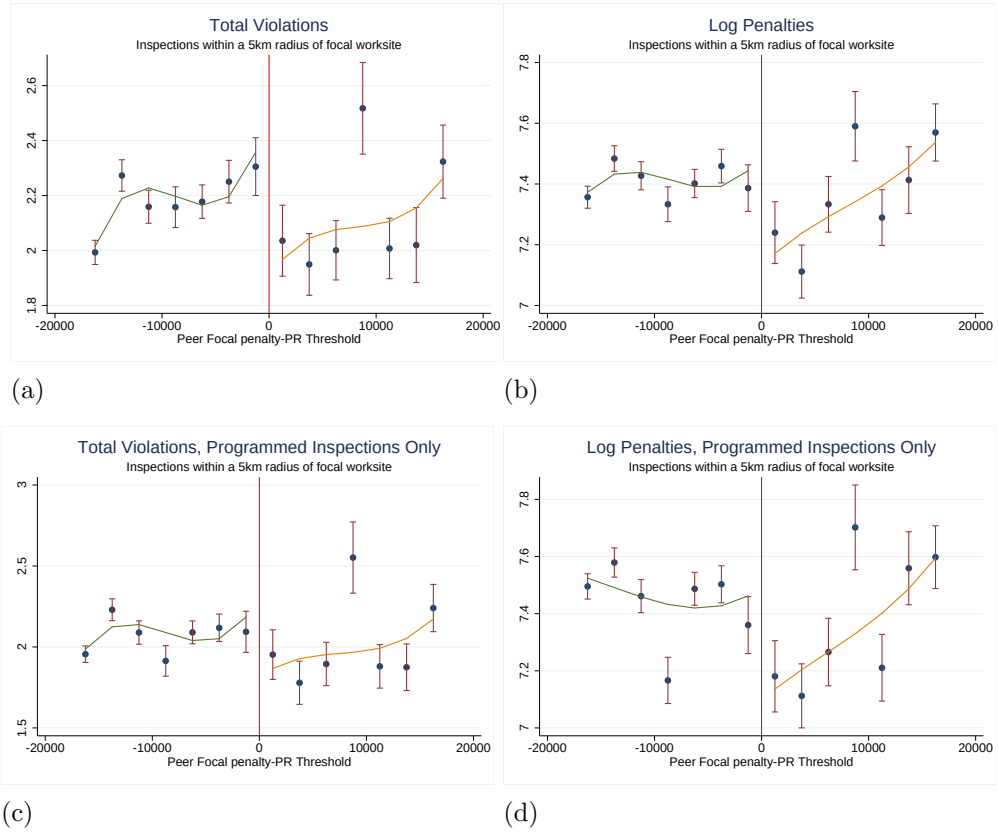
The figure shows the average of an indicator variable equal to 1 if an inspection resulted in a press release, ordered by the financial penalties levied at the inspection ("focal penalty"). Each dot corresponds to an average over a $2,500 bin. The continuous lines represent third-order polynomials fitted 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 federal OSHA jurisdiction.

Figure IV: Frequency of Inspections Around Penalty Cutoff for Issuing Press Releases



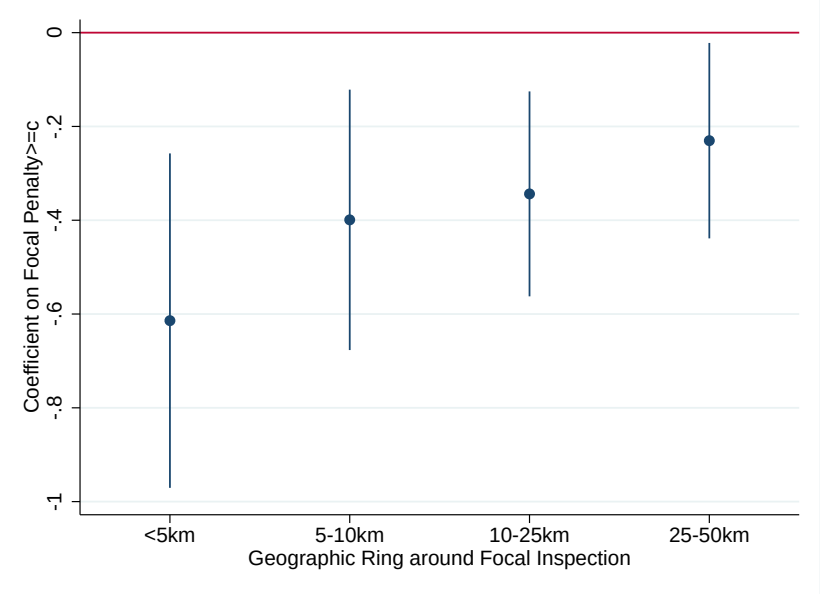
The figure shows the density of the number of inspections, by the financial penalties levied at the inspection. Each dot plots the number of inspections in a bin, where bins are defined by $2,500 non-overlapping intervals of penalty issued. The sample includes inspections with penalties issued from Aug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

Figure V: 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

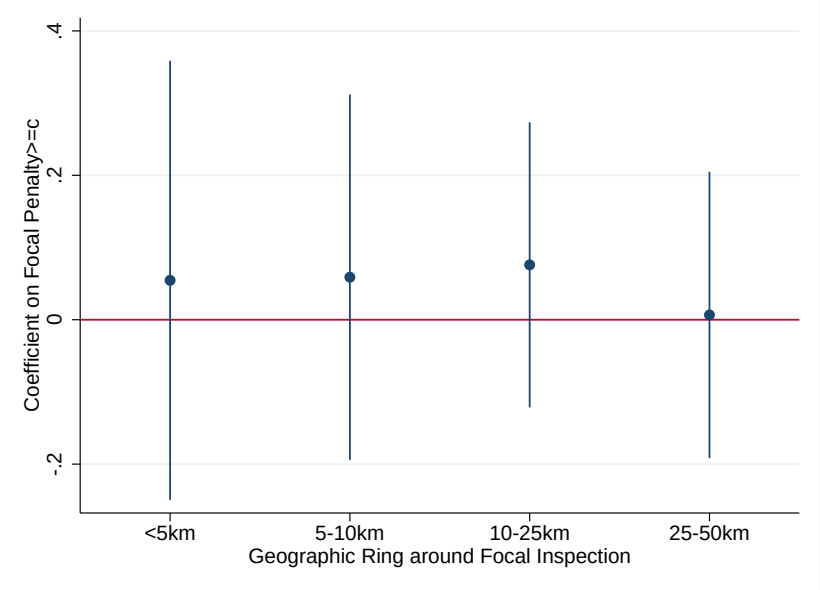


The panels show compliance for different measures of compliance, and different sample restrictions, among facilities in a 5 km radius and the same sector as an inspection with a "focal penalty" issued between Aug 2009 and Nov 2012. Each dot corresponds to an average over a $2,500 bandwidth of focal penalty, 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 VI: 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 radiii of, the focal facility. In panel (b), peer groups are defined as being in a different sector from, and within various geographic radiii of, the focal facility.

Table A.I: Intent-to-Treat Estimates of the General Deterrence Effect of a Press Release on Alternative Measures of Peers' Compliance

	(1) # repeat or willful viols	(2) # high gravity viols	(3)	(4) Total viols> 2	(5) 4
			0		
Focal penalty$\geq c$	-0.067	-0.21	-0.094	-0.089	-0.074
	(0.021)**	(0.076)**	(0.034)**	(0.029)**	(0.019)**
Obs	21589	21589	21589	21589	21589
obs Pen$\geq c$	5695	5695	5695	5695	5695
obs Pen$< c$	15894	15894	15894	15894	15894
# Peer groups	989	989	989	989	989
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 5 km radius and in the same sector ("peers"). 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 A.II: 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	0.013
	(0.069)**	(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	-0.039
	(0.13)*	(0.21)
Obs	12316	18658
Control Mean	2.26	2.06

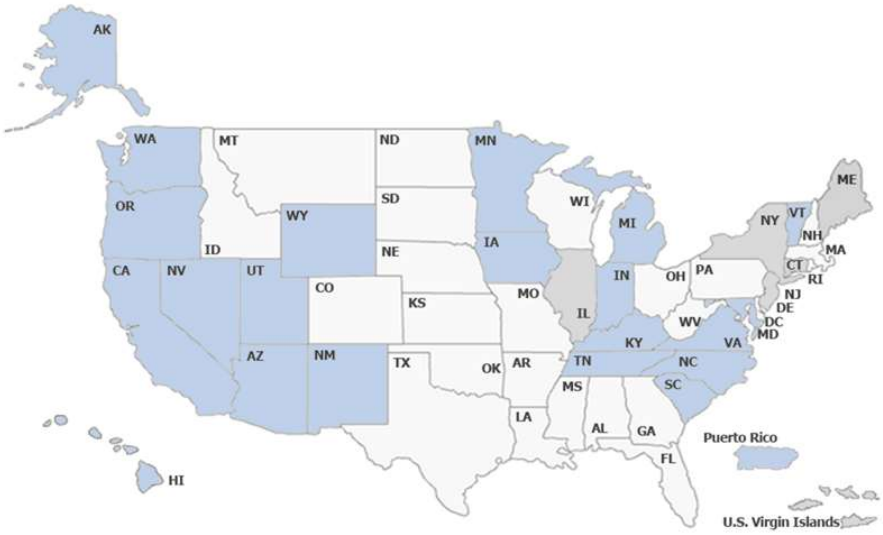
The sample period in all regressions includes focal penalties issued between 2002 to 2007.
In the top panel, the reported coefficient estimates the first-stage relationship between whether a press release is issued and whether the penalty is above 40,000.
In the bottom panel, the coefficient estimates 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 5 km radius and in the same sector ("peers").
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 A.III: Intent-to-Treat Estimates of the Specific Deterrence Effect of a Press Release on Future Compliance of the Publicized Facility

	(1)	(2)	(3)	(4)
	All inspections		Excl. complaint, or accident inspections	
	# violations	ln(Initial Penalties)	# violations	ln(Initial Penalties)
Focal penalty$\geq c$	-1.30	-0.75	-0.80	-0.069
	(0.49)**	(0.41)+	(0.63)	(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.

Figure A.I: Twenty-nine states are under OSHA's jurisdiction, and the remaining 21 states have their own state-run occupational safety and health plans



The states in white or grey are under federal OSHA jurisdiction. Source: https://www.osha.gov/dcsp/osp/