Regulation by Shaming: Direct and Spillover Effects of Publicizing Violations of Workplace Safety and Health Laws

Matthew S. Johnson *
Boston University

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Abstract

Ratings, scores and other forms of information provision are increasingly seen as a tool to incentivize firms to improve their quality or attributes, but less is understood about policies which exclusively publicize firms discovered to have the lowest quality attributes–i.e. "shaming." Shaming may affect the decisions of publicized firms, and perhaps of greater policy relevance is its deterrent effect on not-yet publicized firms. After a 2009 policy change, the Occupational Safety and Health Administration (OSHA) began issuing press releases about workplaces 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, I estimate the effect of publicizing violations by one workplace on its own subsequent compliance with OSHA regulations, and also the spillover effect onto the compliance of "peer" workplaces most likely exposed to the publicity. Workplaces with a peer whose violations were publicized significantly improve compliance with safety and health regulations. These spillover effects decrease with geographic and industry "distance" to the publicized workplace, and the response appears to be driven by employers acting to avoid their own future publicity, rather than by updating beliefs over the probability of future regulatory enforcement. Finally, the magnitude of spillover effects associated with a press release diminish in the years after the policy's introduction, suggesting the information content relevant to employers was strongest for the earliest press releases.
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1 Introduction

Information provision has increasingly become a tool regulators and other agencies use to encourage firms to improve their quality or performance (Delmas et al 2010). These policies are guided by the basic economic insight that markets function better when consumers have more information about quality: providing information about firms' quality can reduce information asymmetries between firms and their stakeholders, leveraging pressure from those stakeholders that incentivize firms to upgrade their quality. Indeed, a growing academic literature has generally found information provision policies lead publicized, rated, or otherwise *revealed* firms to improve the quality of the attributes under scrutiny.[1] However, in practice, many disclosure regimes are far from complete: some regimes only seek to provide information about those at the extreme ends of the performance distribution, such as eBay's "top rated seller" certification, or 24/7 Wall St's annual "Worst Companies to Work For" list. Apart from affecting the performance of revealed firms, such regimes may have spillover, or deterrence, effects onto the behavior of other *unrevealed* firms. This paper estimates how publicizing the performance of poorly performing firms–i.e. "shaming"–affects future performance by other, non-publicized firms.

Spillover effects from information provision could arise for several reasons. For example, unrevealed firms may anticipate having their quality revealed in the future, or they may learn from the information provided about revealed firms. Furthermore, the magnitude, direction and reach of these spillover effects has enormous implications for targeting decisions by agencies designing information disclosure regimes. Indeed, as in any program evaluation, failing to account for a policy's spillover effects on a non-treated group can lead to biased estimates of the program's impact and biased policy recommendations. However, the empirical challenges to identifying spillover effects of information provision can be severe. Along with issues like omitted variables that can bias a comparison between firms who are and are not rated poorly, additional challenges arise when attempting to identify spillover effects. For example, given that information provided about revealed firms is typically publicly available by design, finding a set of peers indirectly affected by the disclosed information, and a comparison group unaffected, can be difficult.

This paper overcomes these challenges to estimate the spillover effects of information provision using an empirical approach that provides quasi-random variation in poorly performing employers which did or did not their performance publicly revealed, and a setting which provides a natural way to define employers which were and were

[1] Some examples are restaurant hygiene (Jin and Leslie 2003), to drinking water quality (Bennear and Olmstead 2008), to environmental ratings (Chatterji and Toffel 2010).

not exposed to this publicity. In 2009, the Occupational Safety and Health Administration (OSHA)–the regulatory agency charged with setting and enforcing workplace safety and health regulations in the U.S.– began issuing press releases about employers found to be violating OSHA regulations in a recent inspection, a policy referred to as "regulation by shaming" by the Assistant Secretary of Labor. [2] These press releases were almost always covered by local newspapers and industry trade publications, and they describe the violations found, the penalties issued, and often use language suggesting the employer was not committed to protecting its workers from safety hazards. Specifically, OSHA began issuing press releases about employers that were cited with financial penalties above a particular cutoff in a recent enforcement inspection.

The initiation of this policy offers a unique opportunity to estimate the extent to which publicizing information about one employer's regulatory performance affects the behavior of non-publicized employers. Firstly, OSHA's press releases significantly increased publicity about workplace safety violations from a very low baseline, and thus the 2009 policy change was akin to increasing the *threat* of publicity from the viewpoint of employers. Second, because the policy change instituting the cutoff rule was only announced internally at OSHA and not to the broader community, the only way for employers to become aware of the increased threat of publicity induced by the policy was to observe *realized* publicity. Given the local and industry-specific media distribution of OSHA's press releases, media coverage following a press release about an employer was most likely observed by geographically proximate employers in the same industry. Thus, estimating how a press release affects the compliance of neighboring employers of the publicized employer provides an opportunity to cleanly test the spillover effects of the "regulation by shaming" policy.

Guided both by the cutoff rule OSHA used to issue press releases, and the local and industry-specific media pickup of these press releases, this paper estimates the effects of a press release about one employer on the subsequent compliance of other employers most likely exposed to the press release ("peers"). Using a Regression Discontinuity (RD) design, the estimation strategy compares the compliance of employers whose peer was recently cited with OSHA penalties just above the cutoff, to that of employers with a peer recently cited with penalties just below the cutoff. Furthermore, the paper investigates the extent to which spillover effects are a function of peers' proximity to the publicized employer, both geographic and technological (proxied by industry), by varying the narrowness with which peer groups are defined.

The analysis finds that a press release revealing an employer to be violating health and safety standards has significant and very large spillover effects. Using shared zip

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

code and sector as a baseline definition of peers, employers with a peer cited with penalties just above the press release cutoff have on average 43 percent fewer violations and 39% lower financial penalties than employers with a peer inspected with penalties just below the cutoff. These spillover effects shrink in magnitude–but remain statistically significant–when we broaden peer groups to have a wider geographic range (county), or omit the industry criterion, supporting the importance of proximity.

One potential concern with this analysis is compliance with OSHA regulations is only observed conditional on an inspection being opened. If being exposed to a press release affects the probability an inspection happens, the estimated effects on compliance could be biased. Explicit tests on the effect of exposure to press releases on inspections occurring, and restricting the compliance regressions to inspections whose occurrence are exogenous to events at the workplace, show the results are unaffected by this concern in practice. Additionally, results are robust to a wide range of alternative specifications, and placebo checks using different cutoffs, and the same cutoff in states without the press release policy, strongly support the validity of the results.

The paper then considers mechanisms that could be driving the spillover effects of press releases. One potential mechanism is that publicity about safety violations damages an employer's reputation, and employers improve their compliance following a press release about a peer as a preemptive investment to avoid their own negative publicity ("reputational deterrence). Indeed, since press releases were often covered by local newspapers and industry trade publications, a press release about an employer could be seen by its consumers, potential new workers, up/downstream firms, and competitors, and each of these stakeholders may value attention to workplace safety for a variety of reasons. An alternative mechanism that could explain the spillover effects is reading a press release about an inspection of one's peer could change other employers' beliefs about the probability of OSHA enforcement ("enforcement deterrence); because OSHA only inspects a small subset of operating workplaces every year, many employers may be unaware of OSHA's inspection and enforcement activities, and a press release about large penalties at a recent inspection could lead employers to update the penalties they expect to receive from OSHA for a given level of noncompliance.

Several pieces of evidence suggest that "reputational deterrence" is driving the results. Firstly, several years after the press release policy has been in place, there is a striking shift in the distribution of OSHA penalties just around the press release cutoff. While the density of penalties remains smooth around the cutoff (which is essential for the RD strategy to remain valid), there is a leftward shift in mass from just around the cutoff suggesting employers improve compliance just enough to surely avoid a press release. At the same time, there appears to be very little, if any, change in compliance at the bottom of the distribution (at zero) and the right tail. Secondly,

the spillover effects of press releases are not moderated by employers' experience with OSHA enforcement prior to 2009; if some employers update their beliefs about OSHA enforcement upon reading a press release, updating should be strongest for those which have been subject to low rates of enforcement in the past. Overall, these results suggest the observed responsiveness of compliance to press releases is due to employers acting to preserve harm to their reputation.

This paper's findings provide a novel contribution to a literature on the disciplinary effects of information provision and the media. While the papers cited in Footnote 1 investigate how firms respond to being rated, publicized or scored, this paper is one of, if not the, first in this literature to estimate the deterrent effects of information provision onto non-publicized firms. Other papers have investigated the deterrent effects of information provision in domains other than firm performance: Lee (2013) finds the introduction of public access to criminal records has a large deterrence effect on first-time crime, but the opposite effect on criminal recidivism. In the realm of politics, Snyder and Stromberg (2010) show U.S. Congressmen in districts with less local political coverage perform worse for their constituents, and Larreguy, Marshall and Snyder (2014) provide evidence that press coverage leads voters to punish malfeasant local elected officials.

This paper also contributes to the literature on the deterrence effects of regulatory enforcement. A large literature has investigated the effects of enforcement inspections on the future compliance of an inspected ("focal") facility,[3] and a smaller literature has tried to estimate the spillover effects of enforcement onto other (non-focal) facilities.[4] 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). The findings of this paper suggest the media and "shaming" are have been overlooked as powerful forces governing employers' compliance decisions, at least for safety and health.

The remainder of this paper is organized as follows. Section 2 provides a brief conceptual framework of why media coverage about safety may affect compliance and other investments in safety. Section 3 provides institutional background of OSHA's press release policy and describes the data, and Section 4 develops the empirical methodology.

[3] As just two examples in a very large literature, see Weil (1996) for OSHA inspections and Hanna and Oliva (2010) for EPA inspections.

[4] Shimshack and Ward (2005) find that EPA inspections resulting in a fine result in 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) conducted a survey among 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, though they (rightly) caution the causality could run in the opposite direction.

Section 5 provides the results of the empirical analysis, and Section 6 provides robustness and placebo checks to test the validity of the results. Section 7 investigates the mechanism behind the main result and where the effects of press releases are the strongest. Finally, Section 8 concludes.

2 Conceptual Framework

This section briefly discusses why a policy publicizing employers who violate workplace safety and health regulations would affect employers' future compliance with these regulations and other investments in health and safety.

Firstly, publicity suggesting an employer is uncommitted to protecting its workers could change the market's beliefs about an employer's compliance with safety and health regulations. An employer's compliance with safety and health regulations could provide a signal of different dimensions of its "quality" for different types of stakeholders. For example, consumers or downstream trading partners may infer that non-compliance with safety standards is indicative of labor unrest, which has been shown to lead to lower product quality (Mas 2008). More directly, potential new workers may view non-compliance as a signal of poor workplace safety culture with higher risk of injury, and as a result demand higher wages (Rosen 1986). However, if these stakeholders cannot directly observe an employer's compliance with safety regulations, the market's beliefs about its quality can be thought of as an employer's reputation for quality.[5] Publicizing an employer's compliance performance provides information about its quality, and this increase in transparency can alleviate information asymmetries which otherwise enable employers with poor safety compliance to remain in the market. Publicity about violating safety standards thus imposes an additional cost to non-compliance, above and beyond enforcement penalties, insurance premiums, and other existing costs to noncompliance.

A policy that publicizes only the worst non-compliers, as in the setting considered in this paper–would provide a very particular incentive scheme with respect to firms' compliance decisions. When consumers and other stakeholders can only learn about a firm's quality through "bad news", such as only learning of a firm's compliance when it is very low (and violations are high), Board and Meyer-ter-Vehn (2013) show firms' incentives to invest in quality improvements is increasing in their current reputation. If news revealing poor compliance damages a firm's reputation, then not-yet publicized employers (whose reputation will be high, on average) invest to increase their compliance to avoid being the object of future reputation-damaaging news. On the other

[5]The phrase "reputation for quality" is taken directly from Board and Meyer-ter-Vehn (2013).

hand, the compliance incentives for already-publicized employers (whose reputation has been damaged) are less clear: if being publicized damages an employer's reputation enough, publicized employers' best response may actually be to (weakly) *decrease* their investments in compliance; in the absence of a mechanism for stakeholders to learn about improvements in quality, an employer's reputation may never be able to recover after bad publicity about safety violations, in which case incentives to invest in quality improvements are low. [6]

An alternative way publicity detailing violations found at a recent OSHA inspection could affect compliance is by changing *employers'* beliefs about the probability of future OSHA enforcement. While neoclassical models of compliance view agents as choosing compliance based on all present and future expected benefits and costs, in reality these decisions may be made in the presence of imperfect information. There are hundreds of safety and health regulatory standards, and given this regulatory complexity even the most well-intentioned firm may not be perfectly compliant (Malloy 2003). A press release could affect employers' beliefs about the *probability* of enforcement: because OSHA only inspects a small subset of operating workplaces every year, many employers may be unaware of OSHA's inspection and enforcement activities. Media coverage detailing violations at a recent inspection of one employer could lead other employers to update their beliefs over the expected costs of noncompliance imposed by regulatory enforcement, either through beliefs about the probability of inspection or the expected fine conditional on inspection (Becker (1958)). More generally, press releases could have a behavioral effect simply by making safety standards more salient to managers, or by reminding them that "OSHA is out there." Reminders which 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 2010). Notably, under this story, press releases should have no effect on compliance decisions of the publicized employer, as it learns no new information from the information in media coverage about itself.

An additional way a press release could change employers' beliefs about OSHA enforcement is by beliefs over the *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 regulations.

[6] Of course, in reality the effects of publicity on firms' reputations and incentives to invest in compliance will be influenced by other factors. For example, the extent to which stakeholders view compliance with safety and health regulations as a signal of "quality" will vary widely across different industries and employers.

3 Institutional Background and Data

3.1 OSHA's Press Release Policy

OSHA's primary tool to enforce its health and safety regulations is inspections of workplaces. During these inspections OSHA inspectors survey a workplace's operations and assess its compliance with standards. Inspections can be triggered by a complaint (by an employee or member of the public) alleging safety and health hazards at a worksite, a "referral' (an allegation of hazards made by an inspector, government agency or media), what is called a catastrophe (worker fatality or hospitalization of three or more workers), or otherwise pre-planned ("programmed"), for example as part of a national emphasis program for a certain industry or type of safety hazard. If, during the inspection, the inspector finds the workplace out of compliance with any standards, she issues violations for each regulation not being followed. The inspector then calculates the appropriate financial penalty for each violation, which are a function of the size of the employer, number of employees exposed to the hazard, and the likelihood the violation would lead to a severe accident. Penalties are typically issued a few months after an inspection takes place.

For at least the past decade, 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 deemed one appropriate. The regional office would then send the press release to local media, and industry trade press, at least one of which nearly always takes up the story. Figure 1 gives an example of such news coverage: an inspection of a poultry processing plant in Gainesville, GA. was begun 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 proceeds to describe the specific violations found during the inspection in detail, citing the plant's lack of "standard guardrails" and "using 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 to determine whether to issue a press release was 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. Some regions effectively issued no press releases at all.

However, in May 2009 OSHA's national headquarters in Washington D.C. decided

to standardize these criteria across regions. As a result, a common cutoff of $40,000 was instituted for Regions 1-4, 6, 9, and 10, and a cutoff of $45,000 for Regions 5, 7 and 8.[7]. These cutoffs were not announced publicly, and were only communicated internally, an important detail to support the validity of the empirical design that follows. Statements by OSHA officials reveal the policy was intended both to reveal exceptionally high violators to the general public, and also to provide publicity about OSHA's enforcement activity. David Michaels, the Assistant Secretary of Labor and Director of OSHA, has called press releases "regulation by shaming," suggesting the intent that press releases impose a cost on publicized employers and add a disincentive to violate OSHAS regulations.[8] Additionally, press releases are meant to serve "educational and deterrent purposes for other companies in the same industry and geographic area."[9]

Figure 2 illustrates the effect of the 2009 policy change on the number of press releases issued by OSHA, and media coverage of OSHA violations, each year 2002-2011. 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 other later-adopting regions. Prior to 2009, there are consistently very few press releases issued outside of Regions 1 and 4. Starting in 2009, there is a muted increase in the number of releases in Regions 1 and 4, and a much more drastic increase everywhere else. The almost one-to-one relationship between number of press releases and newspaper articles shows 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 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, novel violations were found that pose a new and little-publicized kind of hazard. Furthermore, some inspections above the cutoff would not get press releases if the inspector does not send the necessary information to the regional office in time to be relevant. Furthermore, OSHA's 10 regions varied in their adherence to the policy. The "fuzziness" of this design is incorporated into the empirical analysis.

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

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

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

3.2 Data

The primary data source used in the analysis is OSHA's Integrated Management Information System (IMIS), which contains detailed information on each of OSHA's inspections started between January 2001 and December 2012. Key variables included are the date the inspection is opened, the type of inspection (complaint, accident, programmed, other), workplace characteristics (address, industry, number of employees present, whether the employees are represented by a union, etc). As for compliance measures, a detailed report of each violation found (if any) is included with the type of each violation, its corresponding financial penalty, and the date the violations are issued (typically a few months after the date the inspection is opened). I collapse the data to the establishment-inspection level by summing each type of violation and all penalties levied at each inspection. Since many establishments are inspected multiple times throughout the sample period, but at varying rates, the data constitute an unbalanced panel.[10]

For most of the analysis, I restrict attention to inspections with penalties issued May 2009 and after, since this is when OSHA made its press release policy relatively uniform, and with penalties issued before December 2011, to provide sufficient post-inspection data through December 2012 (when the dataset ends). The press release policy does not cover the 21 states with state-run OSHA offices, so inspections in these states are excluded, except for a placebo check on the main results. I also exclude Regions 2 and 3 (covering primarily New York and New Jersey), as the data suggest these regions did not adhere to the cutoff rule. Finally, I exclude inspections in the mining industry ($< 1\% of total inspections$), as this industry is under the jurisdiction of the Mine Safety and Health Administration, rather than OSHA's.

Summary statistics are provided in Table 1 separately for the entire sample of inspections, and for the subset of inspections with penalties 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: out of the nearly 95,000 inspections during this period, the average inspection results in just over \$6,000 in penalties (but is highly skewed) and just 2 percent result in penalties above the corresponding press release cutoff. That the press release cutoff is at the 98th percentile of the penalty distribution supports the idea the policy was intended to expose the highest violators. The average inspection finds 2 violation, while the average inspection in the subset around the press release cutoff finds nearly 9 violations.

[10] IMIS does not keep a unique establishment identifier to track the same establishment over time. Thus, various "fuzzy matching" techniques were used to link records of the same establishment over time. I thanks Melissa Ouellet for help with this endeavor.

Roughly 60 percent of inspections in the whole sample are programmed (i.e. planned ahead of time) and 22% are triggered by a complaint or "catastrophe," with the remaining 19% classified otherwise.[11] However, the share of complaint or catastrophe inspections rises to 33% in the "near cutoff" sample, which makes sense as these types of inspections are more likely to result in violations.[12] The average workplace in the "near cutoff" subsample is nearly twice as large (in terms of employment) and 50 percent more likely to be unionized than the average workplace in the whole sample.

The final panel of Table 1 shows the distribution of inspections across sectors. [13] OSHA inspections are concentrated largely among construction and manufacturing workplaces, both in the whole sample as well as the subsample around the press release cutoff.

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, # employees), to ensure the analysis is not vulnerable to outliers.[14]

To determine the extent to which the cutoff rule for issuing press releases was followed in practice, I 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.[15] Figure 3 uses the results of this linking to illustrate the discontinuity at the cutoff. The Figure makes clear the probability an inspection results in a press release jumps significantly at the cutoff by 25-30 percentage points, highlighting the presence of the discontinuity but also the imperfect adherence to the policy by OSHA.

4 Empirical Strategy

4.0.1 Identifying Spillover Effects of Press Releases on non-publicized employers

The primary goal of this paper is to understand how publicizing the performance of employers egregiously violating health and safety regulations affects the subsequent compliance decisions of other, non-publicized employers. However, an empirical chal-

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

[12] In the IMIS database, the average penalty issued from complaint or catastrophe inspection during this period was $4690, whereas the average for all other inspections was $2,300.

[13] Sectors are roughly 2-digit NAICS codes, except 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

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

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

lenge to estimating these spillover effects is identifying a set of "treated" employers exposed to (but not necessarily the subject of) such publicity and a set of "control" employers not exposed to (and, presumably, unaware of) any publicity about OSHA violations.

Fortunately, the introduction of OSHA's press release policy offers a unique setting to identify these spillover effects. Because OSHA's policy change to begin publicizing egregious violators was not made known to the general public, arguably the only way for employers to be made aware of this new policy was to observe a press release directly. The media outlets through which OSHA distributed its press releases provides plausible classifications of the types of employers likely exposed to a particular press release. Firstly, OSHA typically sent its press releases to local (and not national) media outlets, meaning that employers located nearby the publicized employers would be more likely to be exposed to ensuing media coverage. Secondly, press releases were also typically sent to (and covered by) industry trade publications. As a result, a press release would be most likely seen by other employers in the same region and industry as the publicized employer.

Other details reinforce the idea that employers sharing the same region and industry would view publicity about one another's OSHA compliance. For one, the set of standards OSHA checks for in an inspection, as well as the likelihood a particular employer will be inspected at all, varies widely by industry (Weil 1996), and as a result the description of the violations in a press release are likely to be more relevant to other employers in the same industry. Furthermore, network ties between firms has been shown to have a significant geographic component (Jaffe, Trajtenberg and Henderson 1993), suggesting employers located near each other are in greater contact than with those further away.

Based on these factors, in my baseline specification I group workplaces into "peer groups" if they share the same zip code and industry (with industry classifications corresponding to those at the bottom of Table 1).

4.1 Measuring Compliance (and the effect of publicizing it)

As discussed in Section 2, there are several reasons to believe the introduction of OSHA press releases would increase employers' subsequent compliance with safety and health standards. However, estimating the causal effects of press releases on compliance is complicated by the inconvenient fact that the true state of a workplace's OSHA compliance is unobservable. The IMIS data provide a measure of compliance conditional on being inspected based on the assessment of the inspector. One approach would be to compare measured compliance at future inspections of workplaces who have and have

not been exposed to a press release. However, this comparison could be biased if observing a press release changes the types of workplaces who get inspected. Because such inspections are irregular, and are often a response to an event (accident, complaint, etc), 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 workplaces who get inspected after observing a press release may be different than 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 conditional on inspection.[16]

To illustrate this argument more formally, suppose we are interested in using the number of violations of OSHA standards V_i as a metric of workplace 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 workplace 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:

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

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

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

The difference in the number of violations found between those who have and have not observed a press release has two components: the first term of Equation 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).

[16] This effect is similar to the Conditional-on-Positive bias discussed in Angrist and Pischke (2009).

These effects can be estimated separately. The participation effect can be estimated directly by the difference in inspection rates between Treatment and Control workplaces. However, *a priori* the direction of the participation effect is ambiguous. On the one hand, if the publicity from a press release causes a workplace to improve its true state of compliance, then a press release may reduce the likelihood of an accident, complaint, or other event leading to an endogenous OSHA inspection. On the other hand, the effect could go the other way, for example if reading press releases empowers employees to complain to OSHA when they otherwise would not, leading to *more* endogenous OSHA inspections.. For pre-planned programmed inspections, the onset of these inspections are essentially exogenous to the workplace's activity, and thus the participation effect for these types of inspections should unambiguously be zero.

The COI effect can be estimated by comparing the number of violations found at future inspections of Treatment and Control workplaces. At first blush, this difference may appear to be a valid estimate of the effects of Treatment on our outcome Y. However, the COI effect may be plagued by selection bias if treatment affects the types of workplaces which get inspected–in other words, if the participation effect is not zero. To see this, the COI effect can be further decomposed 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 Treatments who 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 workplaces who are inspected. For example, if observing a press releases causes extremely dangerous workplaces (with the highest V_i^0) to improve safety hazards, thus reducing the likelihood of a catastrophe inspection, then Treatment workplaces with the highest V_i^0 are not inspected, making the second term negative. Other stories could posit a positive bias. 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.*

This discussion is not to say all hope is lost identifying the effects of press releases on workplaces' compliance: there are two reasons the COI may yield a valid estimate of the causal effect of interest. Firstly, we can estimate the COI effect restricting attention to programmed inspections, which are part of broader emphasis programs and pre-plannned by OSHA, and thus very unlikely to be endogenous to observing a

press release. Secondly, if the participation effect is estimated to be zero, we can be confident the selection bias term of the COI effect with no sample restrictions is not a practical concern.

4.2 RD Method

OSHA'S policy to issue a press release about the violations found in an inspection if it results in penalties above a cutoff c, where $c = \$45,000$ for Regions 5,7,8 and $\$40,000$ for all other regions, provides an opportunity to estimate the effects of these press releases using 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 publicized workplace. Whether the workplace is the subject of a press release is a function of the penalty issued at an OSHA inspection–or the "running" variable in RD jargon. 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 to isolate the effects of the press release.

Suppose workplace i has an inspection with penalties levied at date t amounting to Pen_{it}, and we are interested in an outcome observed at a date τ. It is most natural to "re-orient" a workplace's inspection history around the "focal" inspection at date t the following way:

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

Where

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

with $f(\cdot)$ a functional form to be determined, and γ the treatment effect of a press release which, since we are controlling flexibly for financial penalty, is identified from variation on those penalties just below and just above the cutoff c.

To estimate the "participation effect" (effect on the probability a future inspection is initiated), we can drop the τ subscript and let Y_{it} be the number of any, complaint, or catastrophe inspections of i in the 36 months following date t. To estimate the effects of a press release on compliance conditional on a future inspection, $Y_{it\tau}$ is a function of measured compliance (such as violations or penalties) levied at an inspection of i opened at time τ, where $\tau > t$.

Estimating the spillover effects of a press release on non-publicized employers re-

quires a slightly more nuanced specification. In this setting, the running variable for a workplace i, which determines whether i is exposed to a press release, may correspond to a penalty levied at an inspection of i, or at another workplace in i's peer group. If there are several workplaces in the same peer group inspected in close succession, the choice of which inspection to use as the running variable can quickly become subjective. For the estimation, I adapt Equation 3 the following way:

$$Y_{ijt} = \alpha + D_{jt}\gamma + f(P^{max}_{jt\tau} - c) + \epsilon_{ijt\tau} \tag{4}$$

Where $Y_{ij\tau}$ is an outcome for workplace i in peer group j at time τ, and

$$P^{max}_{jt\tau} = \max_{\substack{i' \in j \\ t \in [May2009, \tau)}} \{\text{penalty cited against i', levied at time t}\}$$
$$D_{jt} = \mathbb{1}\{P_{jt\tau} \geq c\}$$

In words, $P^{max}_{jt\tau}$ is the largest penalty issued at any workplace in a peer group prior to the current date, and D_{jt} switches to 1 as soon as one workplace in group j has penalties issued exceeding the threshold c and remains at 1 for the remainder of the sample period. γ is is treatment effect of being exposed to a press release, either about oneself or about one's peer. To estimate the effect of press releases on the probability of future OSHA inspections, we omit the i and τ subscripts and let Y_{jt} be the number of inspections of workplaces in group j after date t, as long as $P^{max}_{jt\tau}$ remains unchanged. To estimate the effect on compliance conditional on inspection, $Y_{ijt\tau}$ is a function of violations found or penalties assessed from an inspection of workplace i in group j at time τ. Robust standard errors are clustered at the peer group j level.[17]

A note about this specification of peer groups is it makes no distinction between the publicized workplace and the non-publicized workplace in the same peer group; $P^{max}_{jt\tau}$ is the same for all members of a peer group. Such a specification implicitly assumes no difference between the effects of observing a press release and being the subject of one.

[17] An alternative specification to Equation 4 could be, for a given inspection at any member of peer group j at time t, to re-orient the inspection history of all workplaces in group j around this "focal" inspection:

$$Y_{ijt\tau} = \alpha + \gamma D_{jt} + f(P_{jt} - c) + \epsilon_{ijt\tau} \tag{5}$$

Where $Y_{ijt\tau}$ is an outcome for workplace i in peer group j at date $\tau > t$, and

$$P_{jt} = \text{ penalty levied at an inspection of any} i \in j \text{ at time } t$$
$$D_{jt} = \mathbb{1}\{P_{jt} \geq c\}$$

However, one drawback of this specification is that, for peer groups with multiple inspections in close succession, the number of focal inspections can get arbitrarily large and observations will be repeated. For this reason, Equation 4 is used as the baseline specification, though this model is used as a robustness check.

This lack of distinction is at odds with the discussion in Section 2 arguing that publicity about OSHA violations would result in different incentives (under the reputational cost mechanism) or different amounts of information (in the salience mechanism) for the publicized workplace and its non-publicized peers. One reason for this specification is the relative infrequency with which workplaces are inspected multiple times, though some specifications remove the publicized workplace to explicitly model the spillover effects of press releases onto non-publicized workplaces.

For simplicity, all specifications use a uniform kernel around the forcing variable (i.e. placing equal weight to observations just at the cutoff, to those further away from the cutoff), though robustness checks consider alternatives. Finally, because OSHA regions varied in their adherence to the press release cutoff rule, and because the construction industry has its own set of OSHA standards not applicable to other industries, we include a dummy variable for each OSHA region and a dummy variable for construction in all regressions.

Various strategies exist to approximate the ex ante unknown 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, we will estimate Equation 3 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.

4.3 Checking the Validity of RD Design

The RD design rests on the assumption that whether inspected workplaces end up just above or just below the relevant cutoff for press releases is 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 to publicity about poor safety, the disutility from penalties is discontinuous at the cutoff c, and if managers know the value of c they may prefer to "bunch up" 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 and foremost, the cutoff rule was not announced publicly, so managers are likely unaware of the cutoff to begin with. Furthermore, much evidence suggests penalties levied by an OSHA inspector are a stochastic function of true noncompliance. For example, different OSHA inspectors may have varying degrees of "toughness," and not every OSHA standard is checked at every inspection, and very often standards have been refined or eliminated over time

(Weil 1996). The stochastic element of the penalty function introduces an element of randomness from the workplace's perspective, which would limit its ability to control the exact penalty given a level of true noncompliance.

On the other hand, there is entirely room for manipulation by the OSHA inspectors, since they issue violations and associated penalties themselves. For example, one may worry that if an inspector thinks a certain employer is poorly run and deserves bad publicity from a press release, she may tip the employer over the penalty cutoff, which would be a clear violation of the "imprecise control" assumption. OSHA officials have confirmed that the method inspectors use to determine penalties is very mechanical, and that any notion of whether the employer 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 aggregate density at the cutoff, then one may suspect either workplaces or inspectors are manipulating penalty amounts to be on one side or the other. Figure 4 examines the density around the cutoff visually. Penalty amounts are normalized by the corresponding regional cutoff c and are placed in equally sized bins of \$2500 (with care to ensure all bins are on only one side of each cutoff), and frequencies are calculated for each bin. The sample is restricted to inspections during May 2009-Dec 2011. The density appears overall quite smooth, and implementing the test proposed by McCrary (2008) confirms 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 2 shows the results of local linear regressions, estimating Equation 3, 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 that the assumptions needed for identification using the RD design are met. [18]

[18] An alternative way to check for smoothness in baseline covariates is to run a regression with D_{it} as the dependent variable, and include each baseline covariate as a righthand side variable, and 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 0.23 and p-value of 0.97, further evidence that baseline covariates show no discotninuties around the cutoff.

5 The Effects of Publicizing Violations on Future Compliance

5.1 Direct Effect on Publicized Employers

While the primary goal of this paper is to estimate the effects of publicizing OSHA violations on the future compliance decisions of *non-publicized* workplaces, we first briefly consider the effects of OSHA press releases on the compliance of the publicized workplace ("specific deterrence"). One complication to this exercise is that measuring specific deterrence is not a completely straightforward task in this setting, particularly for construction. The concept of a "workplace" is ill-defined in construction: if OSHA issues penalties to a construction contractor at one work site, the next time OSHA inspects that contractor may be at a completely different worksite, making it both conceptually and practically challenging to create a workplace identifier for inspections in this industry. The task is more straightforward for non-construction: for example, a manufacturing plant stays in one place, and neatly fits the concept of an "establishment" and is relatively easy to track across repeat inspections. However, because construction makes up over half the sample of inspections near the cutoff, the difficulties surrounding construction will reduce power in this analysis.

As described in Section 4, we first evaluate the effect of press releases on the probability of future inspection and then turn to their effect on compliance conditional on future inspection. Figure A.1 display graphical results for the number of later inspections: it again places workplaces into equally sized bins based on the penalty in the focal inspection, and plots the number of later inspections for each workplace. Panel (a) plots number of later complaint, referral or catastrophe inspections (more endogeneous to conditions at the workplace), and panel (b) plots number of programmed or follow-up inspections (less endogeneous to conditions at the workplace). Panel (a) appears to show a drop in the number of future inspections triggered by a complaint, referral or accident for workplaces with a penalty just to the right of the cutoff, while Panel (b) shows no sign of any difference in rates of future other inspections.

Columns (1)-(2) of Table 3 display corresponding regression results. These regressions estimate Equation 3 with Y_{it} equal to the number of inspections at i following its inspection with penalties levied at time t over the next 36 months. All regressions are Intent to Treat (ITT) analysis, in which we compare outcomes of those above and below the press release cutoff, irrespective of whether a press release is actually issued. Column 1 shows that workplaces just above the press release cutoff are estimated to have 0.085 fewer complaint, referral or accident, over 60 percent of the sample mean, though the result is not statistically significant (p=.15). The estimate for other inspections is

tiny and nowhere near significant.

We next turn to effects of press releases on compliance conditional on inspection. Because the results above show Treatments may have fewer endogeneous inspections, these results restrict to compliance conditional on later non-complaint, -referral or -accident inspections. Figure A.2 displays the plots for violations and penalties levied at later inspections and show no sign of a discontinuous jump in either one at the cutoff. Columns (4)-(5) of Table 3 display the regression results for compliance conditional on future inspection, implementing Equation 3, for violation counts and for log of financial penalties. The point estimates are quite noisy and are drawn from quite a small sample, thus are relatively uninformative.

5.2 Spillover Effects on Other Employers

This section investigates the deterrence effects of a press release on the compliance of all other employers in the publicized employer's peer group. Figure A.3 plots the effect on whether a press release affects the number of future inspections at other employers in the publicized employer's peer group. Each peer group is placed into a bin, where now the running variable corresponds to the *peer group* focal penalty $P_{jt\tau}^{max}$, and average values of each dependent variable are calculated for each bin, where the dependent variable is the number of inspections in the three years following the issuance date of the focal penalty. Each graph shows no sign of a discontinuity in the number of inspections in a peer group at the cutoff.

Intent-to-treat (ITT) regression results for these deterrent effects are shown in Table 4 using a bandwidth of $10,000 around the cutoff and controlling linearly for the running variable. Columns (1)-(3) show that peer groups in which one member's inspection yielded penalties just above the press release cutoff are not inspected at a significantly different rate than groups with a member yielding penalties just below the cutoff. While the imprecision of the estimates means large effects cannot be ruled out, the combination of the graphical results and lack of statistical significance suggests selection bias in the estimates of the effect conditional on future inspection is not a first order concern.

Graphical representation of the deterrence effects of press releases on compliance conditional on inspection is shown in Figure 5. Each inspection is placed into a bin, where now the running variable corresponds to the *peer group* focal penalty $P_{jt\tau}^{max}$, and average values of each dependent variable are calculated for each bin. Panels (a) and (b) consider number of total violations and value of initial penalties, respectively, and Panels (c) and (d) restrict to programmed inspections. Each of the four graphs depicts a clear downward shift in violations or penalties among employers in a peer group just

to the right of the cutoff c, suggesting employers exposed to a press release about a peer significantly improve their compliance with OSHA regulations.

Columns (4)-(5) of Table 4 show the baseline regression results for compliance conditional on inspection. Inspections of employers in peer groups in which the largest penalty received by one of its member is above the press release cutoff have significantly fewer violations and lower financial penalties than those groups in which the largest penalty is below the cutoff. Having observed a press release (in an ITT sense) leads to 43% fewer violations (-1.13/2.64=43%) , and 39% lower penalties (exp(-0.5)-1).

Columns (6) and (7) explore temporal effects of press releases, estimating the effects on compliance change over time. [19] Effects show up immediately and appear to increase in magnitude through three years after a press release is issued.

Table 5 explores how the spillover effects change as a function of distance to the publicized employer. The columns vary the geographic grouping of peer groups from county (wider) to 5-digit zip (narrower), and the rows vary the industry grouping from no industry distinction, NAICS sector, and 3-digit NAICS. Using zipcode as the geographic group, moving from the baseline grouping of 2-digit NAICS sector to narrower 3 digit industries yields essentially identical results, while removing the industry grouping altogether shrinks the magnitude by almost half. Interestingly, moving from 5-digit zip to county also shrinks the magnitude by about half, suggesting that both geographic and industrial proximity matter for the spillover effects of press releases.

6 Checks on Validity of Spillover Effect Results

6.1 Robustness checks

Next, we do several robustness checks on the primary specification. Table 6 illustrates some of these checks.

While Columns 1-3 of Table 4 showed no evidence that press releases significantly affect the composition of who gets inspected, we may still be worried about the selection bias on the Conditional on Inspection (COI) effects discussed in Section 4. Column 1 of Table 6 restricts the sample to programmed inspections. Since these inspections are pre-planned by OSHA and typically part of larger emphasis programs, there is little scope for them to be endogenous to media coverage. The point estimate reduces slightly in magnitude but the effect size relative to the Control Mean is almost identical to the baseline result.

Column (2) includes a few potentially relevant baseline controls, which RD assump-

[19] These regressions utilize a a variant of Equation 4 of the form $Y_{ijt\tau} = \left(\sum_k \alpha_k + D_{jtk}\tau_k\right) + f(P^{max}_{jt\tau} - c) + \epsilon_{ijt\tau}, \quad k = \{\tau - t \in \{0-6\}, \tau - t \in \{7-12\}, \tau - t \in \{13-24\}, \tau - t \in \{25-36\}\}$ months.

tions require to be uncorrelated with treatment, but whose inclusion may improve efficiency. The assumption appears to be met, as the point estimates are nearly identical to the baseline.

Columns (3)-(6) use variants of a slightly different specification for the running variable, described in Footnote 17. Column 3 restricts attention to running variables P_{jt} which are a peer group j's *first* penalty levied at any member of j in the sample period above \$20,000, and Column 4 restrictions attention to running variables which are the *maximum* penalty levied in a peer group in the sample period, and Column 5 makes all penalties eligible. Results are remarkably unchanged from the baseline specification.

Two pieces of evidence provide assurance that the results are not sensitive to my specification choice, including window around the cutoff c and linear polynomial in the running variable P_{jt}. Firstly, I implement the procedure outlined in Calonico, Cattaneo, and Titiunik (2014) to optimally select the bandwidth, polynomial order, and confidence intervals for robust inference in an RD design, using the "rdrobust" Stata package made available by the authors. Results are shown in Table A.1. Based on the procedure in Calonico, Cattaneo, and Titiunik (2014), a bandwidth of \$11,603 is chosen around the cutoff c when the dependent variable is Violation Count, and \$12,686 when the dependent variable is log(penalties), both of which happens to be very close to the bandwidth of \$10,000 used in the baseline results, and a triangular kernel around c is used, which places more weight to observations closer to c, instead of a uniform kernel. The magnitudes of the *treated* coefficient decrease slightly in magnitude compared to baseline but remain highly significant.

Secondly, Table A.3 investigates the sensitivity of the results to varying the window around the cutoff c and the functional form of the running variable. The baseline results are remarkably consistent across the different specifications considered.

Overall, the robustness tests provide credibility to the baseline results.

6.2 Placebo Tests

We run two placebo tests to validate the causal interpretation of the above results. Firstly, we re-run the regressions corresponding to Equation 4 but replacing the true cutoff c with a series of placebo meaningless cutoffs. If we found a significant coefficient using any of these meaningless cutoffs, we would worry the above significant estimates are spurious. The results are shown in Table 7. Using all cutoffs other than the true press release cutoff, the estimated coefficient is tiny and statistically indistinguishable from zero.

We run a second placebo test to ensure the results are not driven by some other

factor that "switches on" at penalty amounts exceeding $40,000 or $45,000. Recall that while Regions 1 and 4 had adopted the $40,000 cutoff several years before 2009, all other regions had been using either a significantly higher cutoff or none at all. If we run the regression corresponding to Equation 4 but oriented around inspections with penalties levied before May 2009, we should expect a significant γ for Regions 1 and 4, but zero for all others. Furthermore, recall the press release policy was only implemented in the states under the jurisdiction of OSHA's federal office, and not in the 21 states with approved state plans. Running Equation 4 for state plan states should yield a $\gamma = 0$ for both the pre- and post-2009 periods.

Panel 1 of Table 8 reports the results of these regressions. Columns (1), (3) and (5) report the first stage results (using Equation 3, restricted to penalties issued Jan 2002-Dec 2008) and confirm that, in the years preceding 2009, Pr(press release) significantly jumped at the cutoff of $40,000 in Regions 1 and 4, but not other regions and not in state-plan states. Columns (2), (4) and (6) estimate the spillover regression among peer groups whose running variable $P_{jt\tau}^{max}$ is restricted to be between Jan 2002-Dec 2008. Effects in Regions 1 and 4 are quite similar to the baseline results for all regions in the post-2009 period and, reassuringly, no effect is found in other regions nor in state-plan states.

Panel 2 reports results from the same regressions but for the post-2009 period considered in the baseline regressions. Pr(Press Release) now significantly jumps at the cutoff for all federally-run regions but, interestingly, the effect on compliance vanishes for Regions 1 and 4. One explanation for this could be that, as the cumulative number of press releases written up in an area increases, the *marginal* effect of an additional press releases on compliance diminishes. For example, if the primary mechanism through which press releases affect compliance is their effect on a firm's reputation, then press releases issued at the onset of the policy may significantly increase the perceived *threat* of negative media exposure, but after awhile an additional press release does little to change the perceived threat. Alternatively, if press releases affect compliance by making safety concerns or OSHA enforcement more salient to managers, a press release's marginal effect on salience may also decline over time. Again, reassuringly, there is no effect in the state-plan states.

7 Deterrence Effects: Reputation or Salience?

This section considers mechanisms that could be driving the observed deterrence effects of press releases on the compliance of peer employers.

As described in Section 2, one potential mechanism is that publicity about safety violations damages an employer's reputation, and employers improve their compliance

following a press release about a peer as a preemptive investment to avoid their own negative publicity. In other words, publicity imposes an additional cost to OSHA violations, above and beyond financial penalties levied by OSHA. Indeed, since press releases were often covered by local newspapers and industry trade publications, a press release about an employer could be seen by its consumers, potential new workers, up/downstream firms, and competitors, and each of these stakeholders may value attention to workplace safety for a variety of reasons (as described in Section 2). In this story, press releases incentivize employers to improve compliance for the sole purpose of avoiding their own press release in the future. As a result, issuing press releases about violations should only affect compliance decisions when expected penalties lie in a neighborhood around the press release cutoff, and should have no effect on compliance at the tails of the penalty distribution. This argument is fleshed out below.

Consider an employer who learns OSHA has begun publicizing employers found with many violations and cited with large penalties. The employer may not know the exact value of the cutoff rule used by OSHA for issuing press releases, but may infer a rough sense it, for example by viewing other press releases OSHA has already issued. If the employer already compliance for which expected penalties are sufficiently below the cutoff, the chance of receiving penalties resulting in a press release are low, and thus the marginal benefit of perfectly improving compliance is very small, as it relates to the probability of bad publicity. If improving compliance is costly, we would not expect this employer to change its state of compliance.

Consider another employer at the other tail of the compliance distribution egregiously out of compliance with expected penalties well above the cutoff, which will face penalties far higher than the (perceived) press release cutoff if inspected. Such an employer would have to dramatically improve compliance to even moderately reduce the chance of having its violations publicized, and if the cost of remediating violations is costly, we would also not expect this employer to change its state of compliance.

Finally, consider an employer whose level of non-compliance is in a neighborhood around the (perceived) press release cutoff. For such an employer, small improvements in compliance can have large benefits in expectation by decreasing the chance of a press release. As a result, we would expect employers to actively avoid receiving penalties in a neighborhood around the cutoff and improve compliance enough so that expected penalties is sufficiently to the left of the press release cutoff. Note we would not expect this employer to necessarily improve to perfect compliance, but rather just enough to avoid the chance of a press release. Of course, if employers knew the precise value of the press release cutoff (and the cutoff was a sharp one), and employers could perfectly control the penalties they would receive in an inspection, we would expect employers to bunch just to the left of the cutoff, but reasons already discussed in Section 4 suggest

this situation is very unlikely.

Thus, the "reputation deterrence" story yields a clear empirical prediction: The introduction of the press release policy should result in little to no improvements in compliance in the tails of the distribution of OSHA penalties, but should lead to a leftward shift in mass from a neighborhood around the press release cutoff. Figure 6 tests this prediction. It plots the distribution of penalties for all inspections opened in 2008 in states under federal OSHA jurisdiction, separately for Regions 1 and 4 (which had been using a $40,000 cutoff to issue press releases since at least 2000) and Regions 5,6,7 and 8, which did not begin using a $40,000 (or $45,000) cutoff until 2009, using a kernel density estimation. The top panel of the figure plots the part of the distribution with penalties between $0 and $10,000, encompassing the left tail of the distribution nowhere near the $40,000 cutoff being used in Regions 1 and 4 during this period, and the bottom panel plots the part of the distribution between $20,000 and $100,000, encompassing the area surrounding the $40,000 cutoff and the right tail (the mass beyond $100,000 is minuscule and thus omitted). At the left end of the distribution, there is no evidence that employers in Regions 1 and 4 improved their compliance relative to those in other regions: if anything, the distribution is shifted toward slightly *worse* compliance. On the other hand, the right panel depicts a clear leftward shift in the mass away from the area just surrounding the cutoff, while showing no change in the right tail.

While the changes in the distribution of penalties supports the "reputation deterrence" story, it does not rule out alternative mechanisms. The primary alternative mechanism that could explain the spillover effects, described in Section 2, is reading a press release about an inspection of one's peer could change other employers' beliefs about the probability of OSHA enforcement ("enforcement deterrence) in two ways. One way is updating beliefs about the *probability* of enforcement, either through the probability of inspection or the expected fine conditional on inspection. If press releases were informing employers OSHA was active in their region and sector, then we would expect a larger effect in areas with low OSHA activity in the years prior to the introduction of the press release policy.

A second way a press release could change employers' beliefs about OSHA enforcement is by beliefs over the *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 regulations. Under this story, we would expect peers of a publicized employer to improve compliance with the regulations violated by the publicized employer, relative to other OSHA regulations.

These predictions of the "enforcement deterrence story are tested in Table 9. The

first four columns investigate how the spillover effects of press releases differ by baseline OSHA enforcement. For each peer group, I calculate the number of OSHA inspections occurring between 2005 and 2008, and the median penalty conditional on inspections between 2005 and 2008. For each measure, I split the sample into "high baseline enforcement and "low baseline enforcement" peer groups based on whether a peer group lies above or below the median. For both measures of enforcement, the results are essentially identical for "low" and "high" enforcement groups, providing no evidence that spillover effects operate through updating beliefs about the probability of enforcement.

Columns 5-6 investigate whether the spillover effects are driven by updating beliefs over the priorities of OSHA enforcement. For each peer group, I identify the set of OSHA regulations violated in the focal inspection responsible for the running variable P^{max}, and I calculate the number of "focal" and "non-focal" violations. If anything, there is a larger drop in non-focal violations, providing no evidence that spillover effects operate through updating beliefs about the priorities of enforcement.

Overall, these results suggest the observed responsiveness of compliance to press releases is due to employers acting to preserve harm to their reputation, rather than updating their beliefs regarding OSHA enforcement.

8 Conclusion

While a growing literature has found evidence that providing information about firms' quality leads to quality improvements, less is understood about the deterrence effects of information provision on non-publicized firms. This paper provided robust, quasi-experimental evidence that publicizing employers which violate safety and health regulations leads to significantly higher compliance with those regulations among other employers most likely exposed to the publicity. Furthermore, the evidence overwhelmingly suggests the observed responses are due to employers making defensive investments to avoid their own publicity, rather than using the information to update their beliefs about the probability and severity of regulatory enforcement. On the other hand, the paper did not find that "shaming" led publicized employers to improve their compliance: while these results may suggest that shaming has asymmetric direct and deterrent effects, the imprecision of the estimated direct effects due to the small sample size makes it difficult to credibly interpret the direct effects. This imprecision is one of the limitations of the paper, and is an important direction for future research.

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Table 1: Summary Statistics

	(1)	(2)	(3)	(4)
	All inspections		Penalties within 10,000 of PR cutoff	
Panel A: Summary Stastics	Var. mean	SD	Var. mean	SD
Compliance measures				
number of violations	2.58	(3.56)	10.59	(6.46)
initial penalties	4497.60	(7644.17)	36613.27	(7889.61)
Initial penalties $\geq$ Press Release cutoff	0.01	(0.12)	0.28	(0.45)
Workplace characteristics				
Number of employees	60.71	(190.67)	127.36	(278.38)
union present	0.10	(0.30)	0.15	(0.35)
Panel B: Summary Indicators	Var. count	% of total	Var. count	% of total
Type of Inspection				
complaint inspection	18248	19.8%	261	27.1%
Referral inspection	10499	11.4%	174	18.1%
fatality or catastrophe inspection	2011	2.2%	60	6.2%
programmed inspection	55821	60.5%	413	42.9%
Other inspections	5791	6.3%	60	6.2%
Industry				
Ag, forestry, fishing	549	0.6%	6	0.6%
Utilities	377	0.4%	6	0.6%
Construction	54834	59.5%	324	33.7%
Manufacturing	20351	22.1%	446	46.4%
Wholesale Trade	2893	3.1%	50	5.2%
Retail Trade	2142	2.3%	19	2.0%
Transportation, Warehousing	2715	2.9%	30	3.1%
Services	8374	9.1%	81	8.4%
Number of inspections	92235		962	

The sample in Columns (1) and (2) includes all inspections opened Jan 2009-Dec 2011 in states under the juristiction of federal OSHA. The subsample in Columns (3) and (4) consists of all inspections which have penalties issued within the given bandwidth of the relevant press release cutoff, and exclude Regions 2 or 3.
Inspections classified as Other include follow-up, monitoring, variance, and other.
For OSHA regions 5, 7 and 8, the relevant press release cutoff is 45,000, and for all others it is 40,000.

Table 2: Smoothness of covariates around press release cutoff

	(1) Press Release issued	(2) Complaint Insp	(3) Referral Insp	(4) Fat-Cat Insp	(5) ln (emp)	(6) union present	(7) # prior inspections	(8) # prior violations
Penalty$\geq c$	0.19	0.023	-0.037	0.0070	0.11	0.0055	0.13	-0.25
	(0.052)**	(0.062)	(0.048)	(0.039)	(0.20)	(0.045)	(0.21)	(1.31)
Obs	921	921	921	921	921	921	921	921
Obs Pen$\geq c$	291	291	291	291	291	291	291	291
Obs Pen$< c$	630	630	630	630	630	630	630	630
Control Mean	0.065	0.28	0.15	0.067	3.40	0.13	0.65	4.23

The sample is restricted to inspections with penalties issued between May 2009-Dec 2011. All specifications use a window around the PR cutoff of 10,000.
The coefficients estimate the magnitude of the change in the dependent variable for inspections with penalties issued 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. Robust standard errors, in parentehses, clustered at initial penalty. +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 3: The Effect of a Press Release on Future Inspections and Compliance of the Publicized Employer

	(1)	(2)	(3)	(4)
	probability of inspection		compliance conditional on later (non-complaint, -referral, or -accident) inspection	
	# complaint, referral, or accident insps	# other insps	ln(Initial Penalties)	# Total violations
Post inspection with Penalty$\geq c$	-0.085	0.033	-0.61	-1.93
	(0.059)	(0.049)	(0.94)	(1.67)
Obs	921	921	148	148
Obs Pen$\geq c$	291	291	45	45
Obs Pen$< c$	630	630	103	103
Control Mean	0.14	0.15	6.29	2.01

For columns (1)-(2), the sample includes all workplaces with (focal) inspections with penalties issued between May 2009-Dec 2011 resulting in penalties within 10,000 of the press release cutoff. The dependent variables are equal to the number of subsequent inspections of the corresponding type of the workplace within 3 years of the focal date through June 2013.
For the remaining columns, the sample includes all programmed and follow-up inspections within 3 years following the date penalties are issued in the focal inspection, and through June 2013.
The coefficients estimate the magnitude of the change in the dependent variable when penalties from the focal inspection just exceed the press release cutoff. Each coefficient is estimated in a separate regression which controls linearly for penalty at focal inspection 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 region fixed effects. Robust standard errors clustered by workplace +P<.1, *P<.05, **P<.01.
Inspections in Column 1 include Complaint and referrals, those triggered by an allegation made to OSHA of safety or health hazards at an employer by an employee, and inspector, media report, or government agency, respectively.
Inspections in Column 2 are all other types of inspections: programmed inspections, follow-up inspections, and a small percentage of other types of inspections. Programmed inspections are initiated based on a broad OSHA emphasis program and are independent of activity at the workplace, and follow-up inspections occur to assess an employer's compliance with violations cited at a previous inspection.
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 4: The effect of a press release on compliance of all other employers in the publicized employers's peer group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pr(inspection) (unit of analysis =peer group)			conditional on inspection (unit of analysis =inspection)			
	# total inspections	# complaint, referral, or accident insps	# other insps	# viols	ln(penalties)	# viols	ln(penalties)
Post inspection of peer with penalty$\geq c$	0.54	-0.30	0.85	-1.13	-0.50		
	(1.01)	(0.45)	(0.77)	(0.32)**	(0.22)*		
0-6 months post penalty$\geq c$						-0.93	-0.53
						(0.35)**	(0.24)*
6-12 months post penalty$\geq c$						-0.96	-0.50
						(0.40)*	(0.24)*
12-24 months post penalty$\geq c$						-1.35	-0.58
						(0.35)**	(0.23)*
24-36 months post penalty$\geq c$						-1.38	-0.77
						(0.42)**	(0.32)*
Obs	753	753	753	3456	3456	3456	3456
obs Pen$\geq c$	249	249	249	1234	1234	1234	1234
obs Pen$< c$	504	504	504	2222	2222	2222	2222
Control Mean	4.41	1.49	2.91	2.64	7.43	2.64	7.43

Peer groups are defined as all workplaces sharing the same industry and zip code. All regressions use a bandwidth around the press release cutoff of 10,000 and include focal year, region and industry fixed effects. The running variable is the largest penalty issued in a peer group between May 2009 and the previous month. All regressions exclude the employer responsible for the focal penalty. The sample period is restricted to June 2009-Dec 2012 and to peer groups whose running variable is between May 2009 and Dec 2011. Regions 2 and 3 not included. Each coefficient is estimated in a separate regression which controls linearly for P(max) with different slopes on each side of the cutoff. 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 acccomodate zeroes.

Table 5: Spillover effects of a press release attenuate with distance to publiczed workplace

	(1)	(2)
	Geographic Group	
	county	zip code
Industry Group		
All industries	-0.29	-0.62
	(0.38)	(0.24)*
Obs	4954	6852
Control Mean	2.56	2.53
2-digit NAICS sector	-0.53	-1.12
	(0.27)*	(0.32)**
Obs	6705	3456
Control Mean	2.76	2.64
3-digit NAICS	-0.43	-0.92
	(0.27)	(0.36)*
Obs	4761	1575
Control Mean	2.61	2.19

The dependent variable is the number of violations in an inspection. All regressions use a bandwidth around the press release cutoff of 10,000 and include focal year, region and industry fixed effects. The running variable is the largest penalty issued in a peer group between May 2009 and the previous month. The sample period is restricted to June 2009-Dec 2012 and to peer groups whose running variable is between May 2009 and Dec 2011. Regions 2 and 3 not included.

Each coefficient is estimated in a separate regression which controls linearly for P(max) with different slopes on each side of the cutoff. 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 6: Robustness Checks: General deterrence regressions

	(1)	(2)	(3)	(4)	(5)
			different running vars		
	Programmed only	Baseline controls	first P(jt)	max P(jt)	all P(jt)
Post inspection of peer with Pen$\geq c$	-0.95	-1.12	-1.05	-1.01	-0.84
	(0.36)**	(0.32)**	(0.28)**	(0.26)**	(0.23)**
Obs	2138	3423	3231	3474	4877
Control Mean	2.38	2.62	2.21	2.24	2.29

The Programmed Only specification restricts the sample to programmed (pre-planned) inspections. Column 2 uses the whole sample but includes controls for the peer group's pre-2009 share of inspections wiht a union present, number employees present at the median inspection, penalty at the median inspection, and number of inspections.

Columns 3-5 use a slightly different model specification that re-orients all inspections of a peer group around a focal inspection in the group. In Column 3 the focal inspection is the first inspection in the peer group with penalties exceeding 20,000. In Column 4 the focal inspection in the inspection in the group yielding the highest penalties in the sample period. In Column 5, all inspections are eligible to be focal inspections. See footnote 17 for details.

Peer groups are defined as all workplaces sharing the same industry and zip code. All regressions use a bandwidth around the press release cutoff of 10,000 and include focal year, region and industry fixed effects. The running variable is the largest penalty issued in a peer group between May 2009 and the previous month. The sample period is restricted to June 2009-Dec 2012 and to peer groups whose running variable is between May 2009 and Dec 2011. Regions 2 and 3 not included.

Each coefficient is estimated in a separate regression which controls linearly for P(max) with different slopes on each side of the cutoff. 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 7: Comparing general deterrence effects using the true Press Release threshold relative to placebo cutoffs

	(1)	(2)	(3)	(4)
			c=	
	20k	30k	PR cutoff	55k
Dep Var=# total violations				
Post inspection of peer with penalty$\geq c$	0.15	0.054	-1.13	-0.0032
	(0.12)	(0.23)	(0.32)**	(0.33)
Obs	20184	8712	3456	1974
Control Mean	2.30	2.35	2.64	2.38
Dep Var=ln(penalties)				
Post inspection of peer with penalty$\geq c$	0.066	0.029	-0.50	-0.22
	(0.085)	(0.19)	(0.22)*	(0.27)
Obs	20184	8712	3456	1974
Control Mean	7.24	7.33	7.43	7.29

Peer groups are defined as all workplaces sharing the same industry and zip code. All regressions use a bandwidth around the cutoff corresponding to the respective column of 10,000 and include focal year, region and industry fixed effects. The running variable is the largest penalty issued in a peer group between May 2009 and the previous month. The sample period is restricted to June 2009-Dec 2012 and to peer groups whose running variable is between May 2009 and Dec 2011. Regions 2 and 3 not included.
Each coefficient is estimated in a separate regression which controls linearly for P(max) with different slopes on each side of the cutoff. 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 8: Pre- vs. post-2009 regressions comparing a) regions using 40,000 threshold pre-2009, b) regions who began May 2009, and c) state-plan states never with a cutoff rule

	(1)	(2)	(3)	(4)	(5)	(6)
	Regions 1, 4 PR policy in place 2002		Regions NOT 1, 4 PR policy begins 2009		State Plan States PR policy never implemented	
	DV= Press Release Issued	DV= # Viols (peer group)	DV= Press Release Issued	DV= # Viols (peer group)	DV= Press Release Issued	DV= # Viols (peer group)
Panel A: Running var includes penalties issued 2002-2008, c=40,000						
Penalty $\geq c$	0.22	-1.33	0.027	0.16	0.023	-0.15
	(0.071)**	(0.51)**	(0.025)	(0.31)	(0.015)	(0.27)
Obs	528	2321	788	4485	350	9839
Control Mean	0.058	3.11	0.014	2.62	0	2.41
Panel B: Running var includes penalties issued 2009-2011, c=PR cutoff						
Penalty $\geq c$	0.13	-0.13	0.20	-1.74	0.024	0.26
	(0.069)+	(0.53)	(0.071)**	(0.48)**	(0.045)	(0.44)
Obs	446	1428	506	1681	203	2172
Control Mean	0.037	2.45	0.043	2.87	0	2.31

Columns (1), (3) and (5) report the coefficient on a dummy for *Penalty*$\geq c$, where c is designated in the respective panel, from the regression specification corresponding to Equation 3, capturing the first stage relationship between whether a press release is issued and whether penalties are just above the cutoff.

Columns (2), (4) and (6) report the coefficient on a dummy for *P(max)*$\geq c$, where c is designated in the respective panel, from the regression specification corresponding to Equation 4, estimating the spillover effects of press releases on the compliance of peer employers.

Peer groups are defined as all workplaces sharing the same industry and zip code. All regressions use a bandwidth around the cutoff corresponding to the respective column of 10,000 and include focal year, region and industry fixed effects. In panel A, the running variable is the largest penalty issued in a peer group between January 2002 and the previous month, and in Panel B, the running variable is the largest penalty issued between May 2009 and the previous month. Regressions restricted to a window of 36 months after the focal date. Regions 2 and 3 not included.

Each coefficient is estimated in a separate regression which controls linearly for P(max) with different slopes on each side of the cutoff. Robust standard errors clustered by peer group +P<.1, *P<.05, **P<.01.

In Panel B, 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 9: Testing if enforcement deterrence is driving spillover effects of press releases

	(1)	(2)	(3)	(4)	(5)	(6)
	Split sample by peer group's pre-period				Split violation type by Relation to Focal Inspection	
	# inspections		Median penalty			
	High	Low	High	Low	# focal viols	# non-focal viols
Post inspection of peer with penalty$\geq c$	-1.46	-0.78	-0.77	-1.10	-0.23	-0.77
	(0.50)**	(0.42)+	(0.46)+	(0.45)*	(0.24)	(0.27)**
Obs	1715	1741	1489	1967	3456	3456
Control Mean	3.04	2.25	2.44	2.78	1.23	1.70

Columns 1-2 take the number of OSHA inspections in a peer group between 2005-2008, and split the sample into peer groups which are above or below the sample median number of inspections. Columns 3-4 take the median penalty levied over all OSHA inspections in a peer group between 2005-2008, and split the sample into peer groups which are above or below the sample median.

Columns 5-6 use the full sample, but classify violations by whether they where also violated in a peer group's focal inspection.

Peer groups are defined as all workplaces sharing the same industry and zip code. All regressions use a bandwidth around the press release cutoff of 10,000 and include focal year, region and industry fixed effects. The running variable is the largest penalty issued in a peer group between May 2009 and the previous month. The sample period is restricted to June 2009-Dec 2012 and to peer groups whose running variable is between May 2009 and Dec 2011. Regions 2 and 3 not included.

Each coefficient is estimated in a separate regression which controls linearly for P(max) with different slopes on each side of the cutoff. 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.

Figure 1: Example of OSHA press release picked up by local media

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OSHA cites Fieldale Farms for safety violations

By Harris Blackwood
harrisblackwood@gmail.com
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.

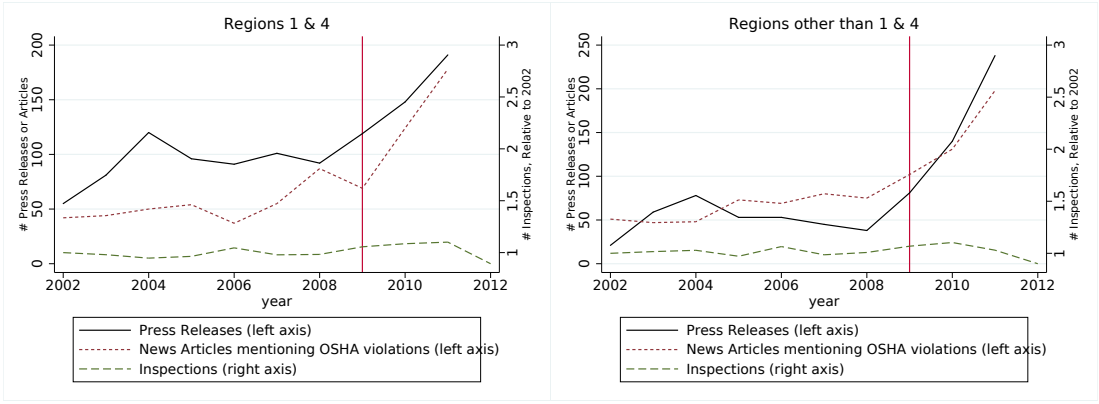
The two repeat violations are for the company's failure to provide standard guardrails for open-sided platforms, and using flexible cords and cables as a substitute for fixed wiring. The agency has proposed fines of $27,500 for the repeat offenses.

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

Here's a note

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

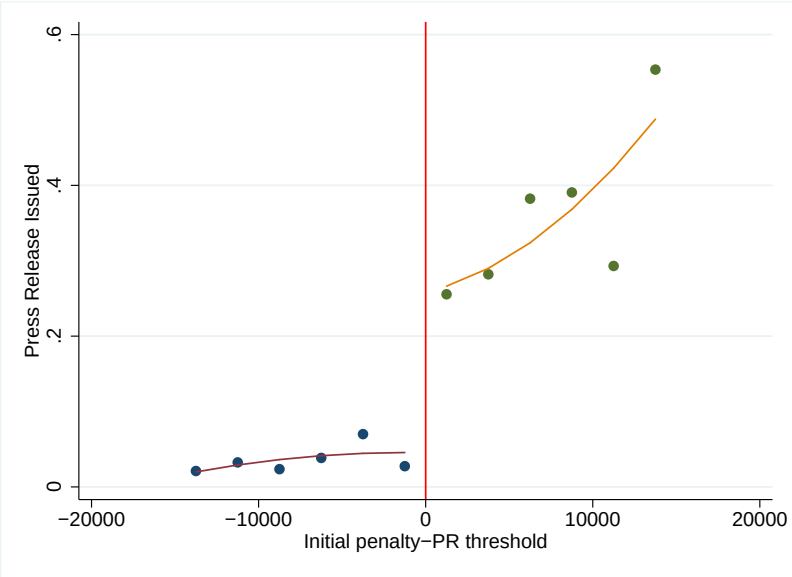


(a) Regions 1& 4

(b) Other regions

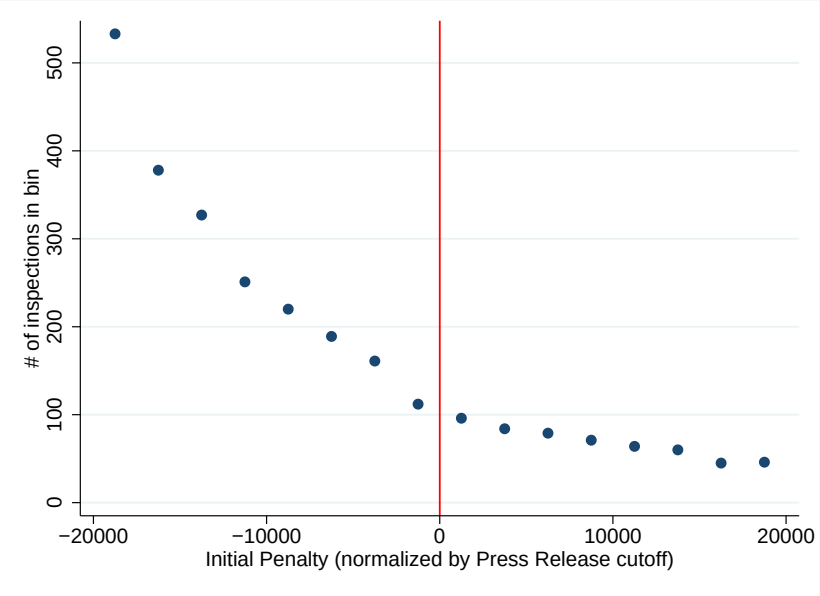
The figure gives the number of press releases about enforcement issued by OSHA each year, the number of newspaper articles in newslibrary.com mentioning "OSHA" in the title and "violations" anywhere in the text, and finally an index of the number of inspections, normalized by the number in 2002. each year 2002-2011. The first panel does so for Regions 1 and 4 (using a cutoff of $40,000 for the entire sample period) all other regions (adopting the cutoff rule for issuing press releases in 2009).

Figure 3: Probability of a Press Release Jumps at the Cutoff by 25-30 percentage points



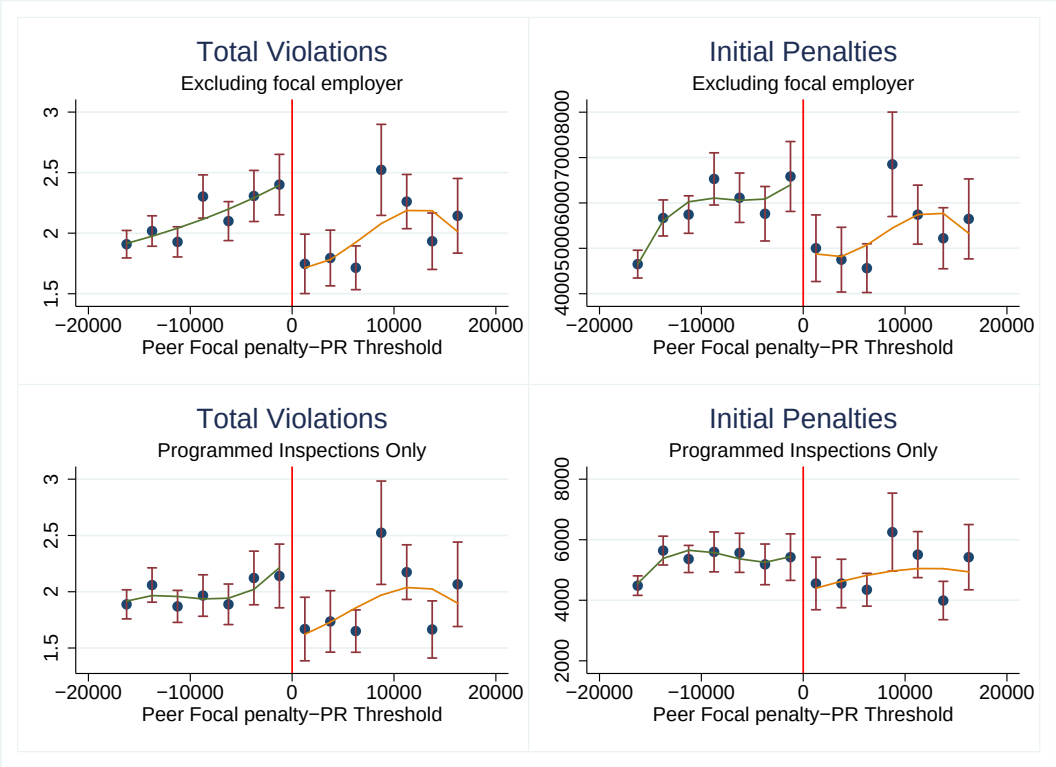
The figure shows the average of an indicator variable equal to one if an inspection resulted in a press release, ordered by the financial penalties levied at the inspection. Each dot corresponds to an average over $2,500. The continuous lines represent quadratic polynomials fitted separately on each side of the cutoff. Sample period includes inspections with penalties issued May 2009- Dec 2011, and excludes state-plan states and Regions 2 and 3.

Figure 4: Frequency of Inspections Around Penalty Cutoffs for Press Release Issuance



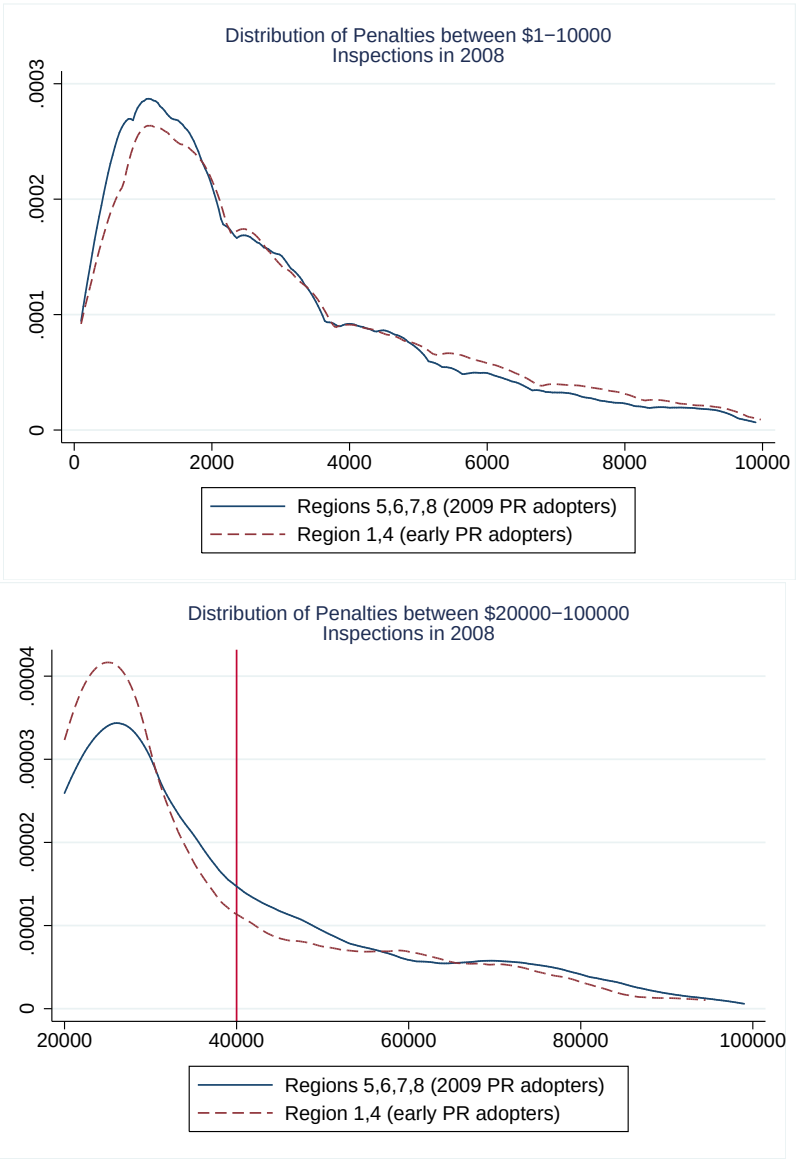
The figure shows the density of the number of inspections, by the financial penalties levied at the inspection. Each dot plots the number of inspections in a bin, where bins are defined by $2500 non-overlapping intervals of penalty issued. Sample period includes inspections with penalties issued May 2009- Dec 2011, and excludes state-plan states and Regions 2 and 3.

Figure 5: General Deterrence Plots: The Effect of a Press Release on Subsequent Compliance of all Workplaces in the Same Peer Group



The panels show compliance for different measures of compliance, and different sample restrictions, in an inspection of a workplace, by its "focal penalty," or the highest penalty levied at a previous inspection of any member of its peer group beginning May 2009. Peer groups are defined based on shared sector (as tabulated in Table **??**) and zip code. Each dot corresponds to an average over a $2,500 bandwidth of focal penalty, with 90% confidence intervals included. The continuous lines represent quadratic polynomials fitted separately on each side of the cutoff. Sample period is May 2009- Dec 2012, and excludes state-plan states and Regions 2 and 3.

Figure 6: The Effect of Press Releases on the Distribution of OSHA Penalties



The panels show a kernel density estimation of the density of initial penalties occurring in 2008, excluding state-plan states. The bottom panel includes a vertical line at $40,000, the cutoff used in Regions 1 and 4 at the time for issuing press releases. Other regions were either not using using a cutoff rule, or were using a much higher cutoff, at this time.

A Appendix Tables and Figures

Table A.1: Results using rdrobust package

VARIABLES	(1) Viols	(2) Penalties
RD_Estimate	-0.745	-0.426
Observations	4088	4588
Robust 95% CI	[-1.23 ; -.29]	[-.8 ; -.1]
Kernel Type	Triangular	Triangular
Robust Std. Error	0.241	0.178
Robust p-value	0.00155	0.0122
Order Loc. Poly. (p)	1	1
BW Loc. Poly. (h)	11683	12686

Table A.2: General deterrence regressions: which types of violations are most affected by press releases

	(1)	(2)	(3)	(4)	(5)	(6)
	Type of Violation			Distribution of Violations		
	# total viols	# OSH viols	# high hazard viols	Total # viols > 0	Total # viols > 2	Total # viols > 5
Post inspection of peer with Pen$\geq c$	-1.03	-0.96	-0.30	-0.12	-0.12	-0.088
	(0.27)**	(0.26)**	(0.13)*	(0.051)*	(0.048)*	(0.027)**
Obs	3270	3270	3270	3270	3270	3270
Control Mean	2.25	2.13	0.67	0.69	0.31	0.11

OSH violations are of standards specifically under an Occupational Safety and Health category. High-gravity violations are those with gravity 10 (out of a 1-10 scale), which means the violations is most likely to result in severe incidence.

Peer groups are defined as all workplaces sharing the same industry and zip code. All regressions use a bandwidth around the press release cutoff of 10,000 and include focal year, region and industry fixed effects. The running variable is the largest penalty issued in a peer group between May 2009 and the previous month. The sample period is restricted to June 2009-Dec 2012 and to peer groups whose running variable is between May 2009 and Dec 2011. Regions 2 and 3 not included.

Each coefficient is estimated in a separate regression which controls linearly for P(max) with different slopes on each side of the cutoff. 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 A.3: Sensitivity of general deterrence regressions to different bandwidths and polynomials in the running variable

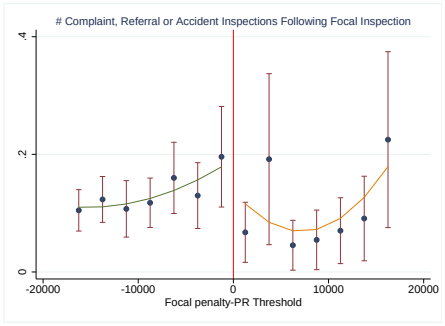
	(1)	(2)	(3)
	window around c		
	5000	10000	15000
Degree of polynomial in running variable			
1st (linear)			
Post inspection of peer with Pen$\geq c$	-0.95	-1.03	-0.73
	(0.36)**	(0.27)**	(0.21)**
Obs	1543	3270	5947
2nd (quadratic)			
Post inspection of peer with Pen$\geq c$	-1.13	-0.87	-1.04
	(0.53)*	(0.40)*	(0.31)**
Obs	1543	3270	5947
3rd (cubic)			
Post inspection of peer with Pen$\geq c$	-0.76	-0.79	-0.86
	(0.69)	(0.52)	(0.40)*
Obs	1543	3270	5947

Peer groups are defined as all workplaces sharing the same industry and zip code. All regressions include focal year, region and industry fixed effects. The running variable is the largest penalty issued in a peer group between May 2009 and the previous month. The sample period is restricted to June 2009-Dec 2012 and to peer groups whose running variable is between May 2009 and Dec 2011. Regions 2 and 3 not included.

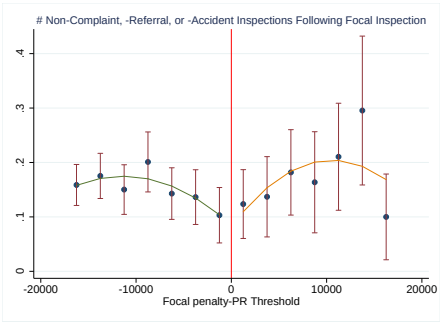
Each coefficient is estimated in a separate regression which controls linearly for P(max) with different slopes on each side of the cutoff. Robust standard errors clustered by peer group +P<.1, *P<.05, **P<.01.

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.

Figure A.1: The Effect of a Press Release on Later Inspections of the Publicized Workplace



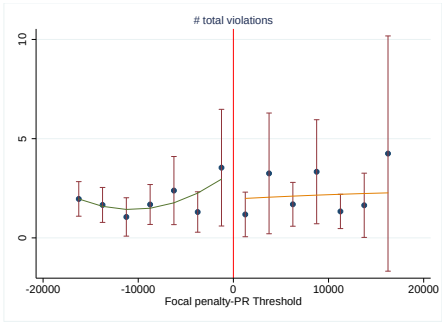
(a) Complaint, Referral, or Accident Inspections



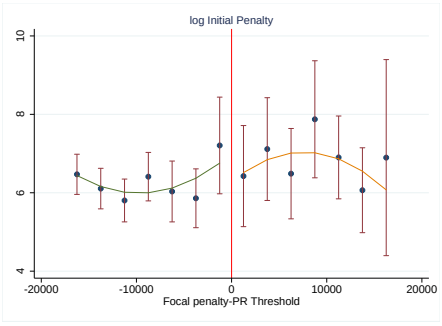
(b) Other Inspections

The left panel shows the number of future inspections triggered by a complaint, referral or accident, by the financial penalties levied at a previous inspection occurring between May 2009-Dec 2011.The right panel shows the number of later programmed and follow-up inspections. Each dot corresponds to an average over $2,500. The continuous lines represent quadratic polynomials fitted separately on each side of the cutoff. Sample period is May 2009- Dec 2012, and excludes state-plan states and Regions 2 and 3.

Figure A.2: The Effect of a Press Release on Subsequent Compliance of the Publicized Workplace



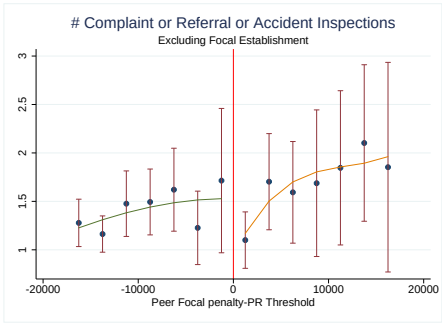
(a) Violations



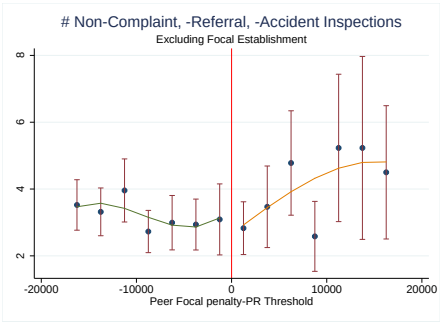
(b) Penalties

The left panel shows the number of violations found, and the right panel the financial penalties, in an inspection of a workplace, by the financial penalties levied at a previous inspection occurring between May 2009-Dec 2011. Each dot corresponds to an average over $2,500. The continuous lines represent quadratic polynomials fitted separately on each side of the cutoff. Sample period is May 2009- Dec 2012, and excludes state-plan states and Regions 2 and 3.

Figure A.3: The Effect of a Press Release on Number of Inspections in the Publicized Workplace's Peer Group



(a) Complaint, Referral, or Accident Inspections



(b) Other Inspections

The left panel shows the number of inspections in a peer group following its focal date, by the group's "focal penalty," or the highest penalty levied at a previous inspection of any member of the peer group beginning May 2009. Peer groups are defined based on shared sector (as tabulated in Table **??**) and zip code. Each dot corresponds to an average over a $2,500 bandwidth of focal penalty, with 90% confidence intervals included. The continuous lines represent quadratic polynomials fitted separately on each side of the cutoff. Sample period is May 2009- Dec 2012, and excludes state-plan states and Regions 2 and 3.