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

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Publicizing firms' socially undesirable actions may enhance firms' incentives to avoid such actions. In 2009, the Occupational Safety and Health Administration (OSHA) began issuing press releases about facilities that violated safety and health regulations. Using quasi-random variation arising from a cutoff rule OSHA followed, I find that publicizing a facility's violations led other facilities to substantially improve their compliance and experience fewer occupational injuries. OSHA would need to conduct 210 additional inspections to achieve the same improvement in compliance as achieved with a single press release. Evidence suggests that employers improve compliance to avoid costly responses from workers. (*JEL* J28, J81, K32, L51, M54)

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

Many sources, though, seek to disclose information only about firms whose quality or performance is low: that is, "shaming." For example, nongovernmental organizations and media outlets compile lists of firms that fail in some dimension

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

according to objective data sources, such as "Least Green Companies in America."[2] Increasingly, technology and social media have enabled customers, former workers, and other stakeholders to expose companies' actions ranging from tax avoidance,[3] to high medical drug prices,[4] to sexual harassment of employees.[5] Regulatory agencies have engaged in their own shaming; in 2018 the Food and Drug Administration released a list exposing pharmaceutical companies that used subversive tactics to prevent entry of generic drugs in their market (Yadin 2019). While one intent of such tactics is to pressure the entity being targeted to improve its behavior ("specific deterrence"), a broader and perhaps more important intent is to encourage improvements in quality at other entities who wish to avoid being the target of their own future negative publicity ("general deterrence"). Despite the growing prevalence of these policies, little is known about how firms respond to such disclosure policies targeted only at the worst performers. Estimating their effects poses substantial empirical challenges due to (i) the purposely nonrandom selection of entities that are publicized, (ii) the difficulty in knowing which other entities are the most likely to respond to general deterrence, and (iii) a dearth in data on outcomes typically under scrutiny.

This paper overcomes these challenges. I investigate a policy implemented by the Occupational Safety and Health Administration (OSHA), the regulatory agency charged with setting and enforcing workplace safety and health standards in the United States, in which it began issuing press releases about employers found to be violating its standards in a recent inspection. The policy was intended to expose egregious violators to public scrutiny and to publicize OSHA's enforcement actions. These press releases described the violations found and financial penalties levied in the inspection of an employer's facility,[6] and they implied that the employer was exposing its workers to substantial safety and health hazards.

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

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[2] Newsweek Staff, "Least Green Companies in America: Photos," *Newsweek*, October 16, 2011, https://www.newsweek.com/least-green-companies-america-photos-68107.

[3] Vanessa Barford and Gerry Holt, "Google, Amazon, Starbucks: The Rise of 'Tax Shaming,'" *BBC*, May 21, 2013, https://www.bbc.com/news/magazine-20560359.

[4] *Knowledge@Wharton*, "Social Media Shaming: Can Outrage Be Effective?" November 20, 2015, http://knowledge.wharton.upenn.edu/article/social-media-shaming-can-outrage-be-effective.

[5] Hansi Lo Wang, "Uber Orders Investigation into Sexual Harassment Claims," *NPR*, February 20, 2017, http://www.npr.org/2017/02/20/516292319/uber-orders-investigation-into-sexual-harassment-claims.

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

broad set of workplaces to detect health and safety violations and collects the results in an internal database, providing a timely and systematic data source to measure facilities' health and safety outcomes.

Understanding the extent to which such publicity affects workplace safety and health is useful not only to understand how firms respond to targeted information disclosure, but it is also an important question for public policy. Workplace injury rates have substantial welfare costs: there were 3.7 million work-related injuries and illnesses in 2015 (Bureau of Labor Statistics 2016), and these injuries and illnesses cost the United States an estimated $250 billion per year (Leigh 2011). At the same time, it is not obvious based on economic theory that publicizing facilities' safety and health record would be an effective means to improve it: in a competitive labor market in which workers are fully informed about job hazards, the level of safety is an efficient outcome in which workers are appropriately compensated for hazards (Rosen 1986), meaning there is no market imperfection that publicity would alleviate.

I find that press releases revealing OSHA noncompliance lead to substantial improvements in workplace safety and health. A press release leads to 73 percent fewer violations at "peer" facilities in the same sector within a 5 kilometer radius. To put this magnitude in perspective, one recent study found that a typical OSHA inspection leads to 48 percent fewer violations at later inspections of the same facility (Ko, Mendeloff, and Gray 2010). Combined with this finding, my estimate implies that publicizing violations of one facility leads neighboring facilities in the same sector to improve compliance by 50 percent more than if OSHA inspected each of these facilities instead. Improvements in compliance persist for facilities located up to 50 km away. Given that inspections are costly and that OSHA's budget constraints, like those in most regulatory agencies, dictate that it can inspect only a small subset of regulated workplaces, publicity appears to be a highly effective complement to inspections as a means to improve workplace safety.

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

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

health only when workers are most likely to be able to use information about an employer's safety record to respond in a costly way.

Finally, I show that press releases have larger deterrence effects when the publicized facility's local newspaper is more likely to cover it. This result highlights one channel through which facilities learn about the publicity of one another, and, given that newspaper stories tend to rapidly spread to many other types of news outlets, they also are consistent with press releases being more costly when a wider audience of stakeholders is likely to see it.

This paper's findings provide a novel contribution to a literature on the disciplinary effects of information provision. While a growing body of work (such as those papers cited in footnote 1) has investigated the extent to which information disclosure leads firms to improve their performance, this paper is one of the first to identify how providing information about some targeted firms can have broader effects on the behavior of other firms. Sharkey and Bromley (2015) finds evidence that third-party ratings of firms' environmental performance have spillover effects on unrated firms in some contexts. Other papers have explored the effect of "shaming" in other domains, such as public release of criminal records (Luca 2011) and tax delinquency (Pérez-Truglia and Troiano 2015). In politics, media coverage has been shown to affect politicians' incentives to engage in malfeasant behavior (Snyder and Strömberg 2010; Larreguy, Marshall, and Snyder 2014). This paper's findings on general deterrence effects of information provision complement work in Freedman, Kearney, and Lederman (2012) which finds that recalls on one set of consumer products has spillover effects on how consumers behave with respect to other products. This paper builds on these literatures by exploring how shaming, and targeted information disclosure, affects firm behavior in a regulatory environment.

Second, this paper provides new insight into the determinants of regulatory compliance in firms. Many prior studies have investigated the specific deterrence effects of OSHA inspections on future compliance of inspected facilities (Gray and Jones 1991; Weil 1996; Ko, Mendeloff, and Gray 2010), as well in other regulatory domains such as by the Environmental Protection Agency (see Alm and Shimshack 2014 for an overview). A smaller body of literature has sought to estimate the general deterrence effects of enforcement on other facilities. Thornton, Gunningham, and Kagan (2005) surveyed manufacturing firms and found that the number of examples of enforcement actions at other firms that respondents could recall was positively associated with whether the respondent reported having taken action to improve environmental performance. A particularly related paper is Shimshack and Ward (2005), which finds that EPA inspections resulting in a fine led to a substantial reduction in the statewide violation rate; one factor that distinguishes this paper is that my research design holds constant any general deterrence effects that might arise from large OSHA penalties, and it reveals large general deterrence effects of publicity, above and beyond and effects that penalties may have. Indeed, 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, p. 3). The findings of this paper suggest the media has been overlooked as powerful forces governing firms' compliance decisions, at least for safety and health.

I. Conceptual Framework

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

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

One set of stakeholders that certainly values facilities' OSHA compliance and safety performance is workers. While textbook labor economics theory says the level of workplace safety is an efficient equilibrium outcome based on workers' preferences and employers' costs (Rosen 1986), there is evidence that workers are not fully informed about job hazards. For example, Viscusi and O'Connor (1984) found that giving workers information about the hazards associated with their job increased their reservation wage and probability of quitting. This evidence suggests that workers begin their jobs with imperfect information about hazards and, as they learn over time, quit if their updated beliefs make the position sufficiently unattractive. Thus, publicity about OSHA violations could mitigate a market imperfection and lead current workers to update their beliefs about their job risks and in turn to quit, or lead potential new workers to be more informed at the outset of a job and in turn demand higher wages.[7]

Publicity about OSHA violations could lead other stakeholders that value workplace safety to update their beliefs as well. Consumers or downstream trading partners may infer that noncompliance with OSHA standards indicates labor unrest, which has been shown to worsen product quality (Mas 2008). Consumers might respond for other reasons: in the weeks following the widely publicized 2010 British Petroleum (BP) oil spill, which killed 11 workers and released millions of gallons of oil into marine waters, BP margins and volumes declined significantly (Barrage, Chyn, and Hastings 2014). In an example especially relevant to this study, Starbucks ended a relationship with a flavoring manufacturing it used as a supplier one week after that supplier was the subject of an OSHA press release detailing a history of widespread safety violations.[8] Publicity about violating safety standards thus may impose an additional cost on noncompliance, above and beyond enforcement penalties, insurance premiums, and other existing costs.

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

[8]Fred Hosier, "Starbucks Ends Business with Company Following Bad Safety Publicity," *Safety News Alert*, August 28, 2012.

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

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

II. Institutional Background and Data

A. *Background on OSHA*

OSHA, created in 1970, is the federal regulatory agency charged with assuring "safe and healthful working conditions" in the United States by establishing and enforcing standards.[9] Many employers are required to comply with hundreds of OSHA standards, which range from maintenance of specific capital equipment to more general restrictions that workers not be exposed to particular hazards. Organizationally, OSHA partitions the county into 10 regions, each with its own regional office, and 90 Area Offices that oversee the implementation of inspections and enforcement. OSHA has jurisdiction over 28 states; the remaining 22 states have received federal approval to operate their own state-run safety and health programs. Online Appendix Figure A.1 provides a map of which states are under OSHA's jurisdiction, and a map of how OSHA partitions the country into 10 regions.

Inspections are OSHA's primary tool for monitoring compliance with health and safety standards. Among non-construction industries, an inspection is typically conducted at the level of the establishment. In construction, inspections take place at a work site; if multiple companies are working on the same site, the inspector may conduct a separate inspection of each company. During inspections, inspectors review paperwork and tour a facility's operations to assess their hazards and compliance with relevant standards. When inspectors find facilities to be out of compliance with any standards, they issue citations for each violation they observe. Once inspections are completed, inspectors consult with the Director of the OSHA Area

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[9]OSHA, "About OSHA," https://www.osha.gov/about.html, accessed August 22, 2016.

Office to calculate the financial penalty for each violation, which is a function of the size of the employer, the number of workers exposed to the hazard, and the likelihood the violation would lead to a severe accident (US Occupational Safety and Health Administration 2009).

OSHA inspections can be initiated for two broad categories. "Programmed" inspections focus on particular industries or hazards, constituting roughly 60 percent of annual inspections. These inspections are pursuant to National Emphasis Programs (NEPs), which focus on nationwide priorities, or Local Emphasis Programs, which focus on regional priorities.[10] Because programmed inspections target facilities only based on their being in a particular industry or possessing a specific hazard, no other facility-specific factors (such as recent injuries) influence whether it receives a programmed inspection.[11] Furthermore, conditional on the criteria on which NEPs or LEPs are based (e.g., industry or region), OSHA typically randomly assigns inspections among plants that meet those criteria.[12]

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

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

B. *OSHA's Press Release Policy*

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

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[10] For example, one industry-specific NEP from 2016 focused on facilities that stored highly hazardous chemicals. An example of a hazard-specific NEP is one from 2008 that focused on reducing occupational exposure to lead, which targeted industries where such exposure was most likely. See OSHA NEP (https://www.osha.gov/dep/neps/nep-programs.html).

[11] One exception to this rule is OSHA's Site-Specific Targeting (SST) program, which focused inspections on plants with high recent injury rates from 1999–2011. However, SST is largely irrelevant to this paper, as it excluded construction (which makes up a majority of the sample in this paper's analysis), and the bulk of this paper's sample period is after SST ended.

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

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

Panel A
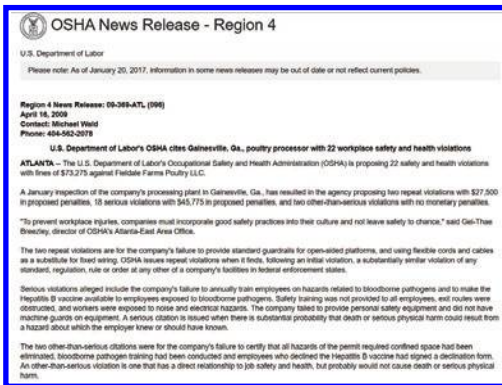
Panel B

FIGURE 1. EXAMPLE OF AN OSHA PRESS RELEASE AND SUBSEQUENT MEDIA COVERAGE

Notes: The source for panel A is downloaded from OSHA's archive of news releases, available at https://www.osha.gov/news/newsreleases/region4/04162009. The source for panel B is *The Gainesville Times*, accessed March 2014. The images are reprinted with permission from the Department of Labor and *The Gainesville Times*, respectively.

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

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

However, in May 2009 OSHA's national headquarters in Washington, DC standardized these criteria across regions. As a result, Regions 1–4, 6, 9, and 10 instituted a common cutoff of $40,000, Regions 5, 7, and 8 instituted a cutoff of $45,000.[14] OSHA did not announce these cutoffs publicly, and only communicated them internally, a detail important to support the validity of the empirical design that follows. Statements by OSHA officials reveal the policy was intended both to reveal exceptionally high violators to the general public, and to publicize OSHA's enforcement activity. Dr. David Michaels, then the Assistant Secretary of Labor and Director of OSHA, called press releases "regulation by shaming," suggesting the intent that press releases impose a cost on publicized employers and add a disincentive to violate OSHA regulations.[15] Additionally, OSHA hoped press releases would serve

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[14] In interviews, OSHA officials told me they were unsure of the reasons for the difference in this cutoff across regions.

[15] David Michaels, "OSHA at Forty: New Challenges and New Directions," July 19, 2010, available at https://www.osha.gov/as/opa/Michaels_vision.html (accessed January 2016).
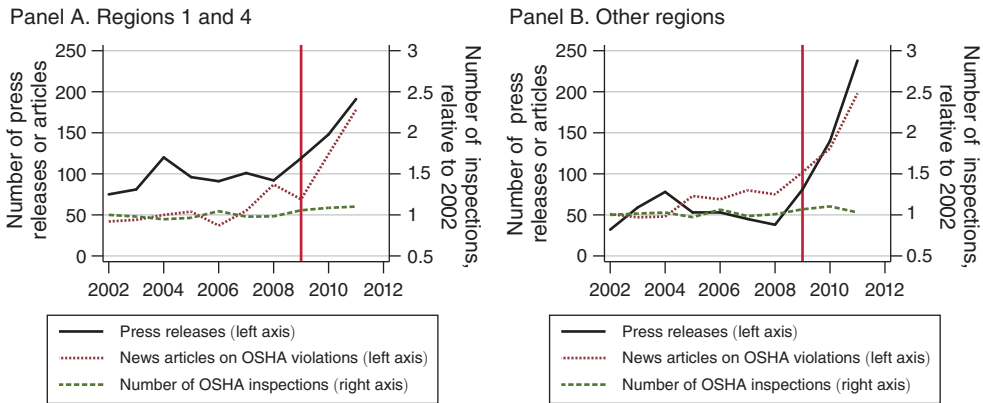
Panel A. Regions 1 and 4

Panel B. Other regions

FIGURE 2. PRESS RELEASES, MEDIA COVERAGE, AND INSPECTIONS BY YEAR

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

"educational and deterrent purposes for other companies in the same industry and geographic area."[16]

Figure 2 illustrates the effect of the 2009 policy change on the number of press releases issued by OSHA and media coverage of OSHA violations. For media coverage, I use the number of articles found on NewsLibrary.com[17] that contain "OSHA" in the title and "violations" anywhere in the text. Panel A plots these series for Regions 1 and 4, which were using the $40,000 cutoff rule at least as early as 2002, and panel B plots for the later-adopting regions. A few takeaways emerge from these figures. First, there is a marked increase in the number of press releases issued across all OSHA regions coinciding with the policy change in 2009, which is more dramatic outside of Regions 1 and 4. Second, the number of newspaper articles about OSHA violations exhibits a roughly one-to-one relationship with the number of press releases written, illustrating that the 2009 policy change significantly changed the frequency of media coverage about OSHA violations. This second point is important because information about OSHA inspections is publicly available on its web site and, in theory, journalists could cover large OSHA penalties (and other stakeholders could learn about them) even in the absence of OSHA-issued press releases. However, this information is not easily accessible,[18] and these figures reveal that press releases relaxed a constraint that otherwise limited the media's coverage of OSHA violations.

[16] Comments from Patrick Kapust, deputy director of OSHA Directorate of Enforcement Programs: see interview in Kyle W. Morrison, "Examining the Top 10," *Safety + Health,* December 1, 2012, https://www.safetyandhealthmagazine.com/articles/examining-the-top-10-2?page=2.

[17] NewsLibrary.com is a compendium of roughly 4,000 US newspapers and other news outlets .

[18] Users would have to know to go to OSHA's web site (osha.gov) to search for information about recent inspections, where they can view recent inspection regards for a specific establishment or OSHA area office.

While this policy change made the probability of a press release a discontinuous function of penalties, in practice the cutoff rule was not a sharp one. Some inspections with penalties below the cutoff resulted in a press release if, for example, the inspector found violations that posed a new and little-publicized kind of hazard. Some inspections above the cutoff did not get a press release if the inspector did not send the necessary information to the regional OPA in time. One regional officer mentioned in an interview that if two facilities were issued penalties above the press release cutoff on the same day, the office would issue a press release about the large one, but not the small one, so as not to crowd out its media connections. In general, the many layers of communication required to implement the policy (inspectors communicating with their Area Office, the Area Office communicating with the regional OPA), combined with the fact that the federal OSHA office did not enforce the cutoff rule with the regional offices, collectively led to less than perfect adherence. The empirical analysis that follows incorporates the fuzziness of this policy into the research design.

C. *Data*

This paper's primary data source is OSHA's Integrated Management Information System (IMIS), which is a database that contains detailed information on every OSHA inspection.[19] Key variables it includes are the date the inspection is opened, the reason the inspection was initiated (complaint, referral, accident, programmed, other), and facility characteristics (name, address, industry, number of employees present, whether the employees are represented by a union, etc.). I geocoded addresses using ArcGIS to get the latitude and longitude of each inspection. IMIS includes a detailed report of each violation found (if any), with the OSHA standard that was violated, its corresponding financial penalty, and the date the violations were issued. I collapse the data to the facility-inspection level by summing each type of violation and all penalties levied at each inspection. Since facilities are inspected at varying frequency, with some inspected multiple times, others inspected only once, the data constitute an unbalanced panel.[20]

For most of the analysis, I restrict attention to inspections with penalties issued in October 2009 and after. OSHA made its press release policy relatively uniform in May 2009, but conversations with OSHA officials revealed that it took a few months for the policy to catch on. The dataset ends in December 2013. The press release policy did not cover the 22 states with state-run OSHA offices, so I exclude inspections in these states. I also exclude Regions 2 and 3 (covering primarily New York and New Jersey), as the data suggest that these regions did not adhere to the cutoff rule for issuing press releases. I also exclude inspections in the mining industry ($<$1 percent of total inspections), as this industry is under the Mine Safety and

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TABLE 1—SUMMARY STATISTICS

	All inspections		Inspections with penalties within 10,000 of press-release cutoff	
	Variable mean (1)	Standard deviation (2)	Variable mean (3)	Standard deviation (4)
Panel A. Summary statistics				
Compliance measures				
Number of violations	2.01	(2.65)	8.12	(4.29)
Initial penalties ($)	4,629.95	(7,792.87)	37,388.66	(8,255.35)
Initial penalties $\geq$ press release cutoff	0.01	(0.11)	0.28	(0.45)
Facility characteristics				
Union present	0.09	(0.29)	0.13	(0.34)
	Variable count	Percent of total	Variable count	Percent of total
Panel B. Summary indicators				
Type of inspection				
Programmed inspection	89,923	59.7	528	41.9
Complaint inspection	32,020	21.3	360	28.6
Referral inspection	16,220	10.8	234	18.6
Fatality or catastrophe inspection	3,209	2.1	72	5.7
Related or other inspection	4,693	3.1	49	3.9
Industry				
Ag, forestry, fishing	1,009	0.7	9	0.7
Utilities	571	0.4	9	0.7
Construction	86,507	57.4	413	32.8
Manufacturing	3,4281	22.8	593	47.1
Wholesale trade	5,287	3.5	67	5.3
Retail trade	3,797	2.5	26	2.1
Transportation, warehousing	4,647	3.1	48	3.8
Services	14,574	9.7	95	7.5
Number of inspections	150,673		1,260	

Notes: The sample in columns 1 and 2 includes all inspections initiated between January 2009 and December 2012 in states under the jurisdiction of federal OSHA. The subsample in columns 3 and 4 consists of all inspections for which penalties were issued between October 2009 and November 2012 and were within the given bandwidth of the relevant press release cutoff, and excludes Regions 2 and 3. Inspections classified as Other include referral, monitoring, variance, follow-up, and other. For OSHA regions 5, 7 and 8, the relevant press release cutoff is 45,000, and for all others it is 40,000.

Health Administration's jurisdiction, rather than OSHA's, and is thus not technically eligible for OSHA inspections.

Table 1 provides summary statistics, separately for the entire sample of inspections initiated between January 2009 and December 2013 and for the subset of inspections with penalties issued between October 2009 and November 2012 and within $10,000 of the press release cutoff for its region ($30,000–$50,000 for Regions 1, 4, 6, 9, and 10, and $35,000–$55,000 for Regions 5, 7, and 8). Most inspections result in relatively small penalties: of the roughly 150,000 inspections during this period, the average inspection results in just over $4,600 in penalties (but is highly skewed) and only 1 percent result in penalties above the press release cutoff. The press release cutoff being at the ninety-ninth percentile of the penalty distribution reflects how OSHA intended press releases to expose the highest violators. The average inspection finds two violations, while the average inspection in the subset around the press release cutoff finds over eight violations.
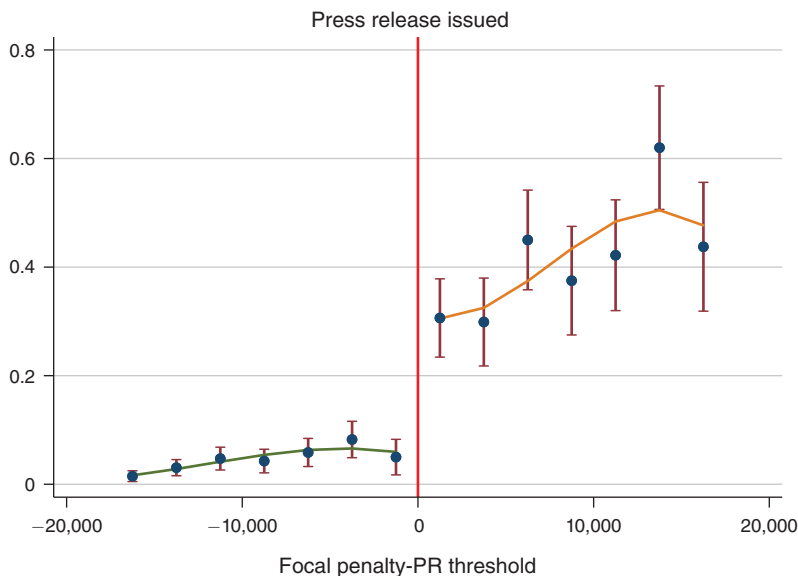
FIGURE 3. PROBABILITY OF A PRESS RELEASE JUMPS AT THE CUTOFF BY 20–25 PERCENTAGE POINTS

Notes: The figure shows the average of an indicator variable if an inspection resulted in a press release, ordered by the financial penalties levied at the inspection (*Focal penalty*). Each dot corresponds to an average over a $2,500 bin, with 95 percent confidence intervals included. The continuous lines represent third-order polynomials fitted separately on each side of the cutoff. The sample includes inspections with penalties issued from October 2009 through November 2012 and excludes Regions 2 and 3 and states not in OSHA jurisdiction.

Roughly 60 percent of inspections in the whole sample are programmed, and 34 percent are triggered by a complaint, referral, or "fat/cat" (fatality or catastrophe), with the remaining 3.1 percent classified otherwise.[21] The share of complaint, referral, or catastrophe inspections rises to 53 percent in the sample near the cutoff.

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

Because many of these variables are skewed to the right, I topcode count variables at their ninety-ninth percentiles and take logs of continuous variables to ensure that the analysis is not vulnerable to outliers.[23]

To determine the extent to which the cutoff rule for issuing press releases was followed in practice, with the help of a research assistant,[24] I hand-linked the set of

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

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

[23] I add the first nonzero percentile of each variable before taking the log to account for zeros. I do this rather than the more common approach of adding 1 before taking the log because of how financial penalties are distributed. In my sample, conditional on any penalties being levied, almost no inspections have less than $100 in penalties, and the first percentile is $1,000. I consider other approaches to handle outliers in robustness checks.

[24] Specifically, a research assistant looked up each press release OSHA posted on its web site between 2002 and 2013, and, using search terms like date, penalty issued, facility name, and location, identified the inspection record(s) corresponding to the press release. The archive of OSHA's press releases is available at OSHA's web site

archived press releases on OSHA's web site to the IMIS dataset to create an indicator for each inspection if the inspection resulted in a press release. Figure 3 uses the results to illustrate that the probability that an inspection results in a press release jumps at the cutoff by 20 to 25 percentage points, highlighting the discontinuity but also the imperfect adherence to the policy by OSHA. I, with a research assistant's help, also linked the archived press releases to NewsLibrary.com to identify if a press release was covered by a newspaper.[25]

III. Empirical Strategy

A. *Measuring General Deterrence Effects of Publicizing Facilities Caught Violating OSHA Regulations*

Estimating how a policy that publicizes violators of OSHA regulations affects facilities' compliance is fraught with empirical challenges. One challenge is identifying a set of "treated" facilities in which managers and/or workers become aware of the policy that violators will be publicized and a set of "control" facilities in which managers and workers remain unaware of this risk of publicity.

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

Other aspects reinforce the idea that managers of facilities in the same region and industry would view publicity about one another's OSHA compliance. Corporate networks have a significant geographic component (Davis and Greve 1997); managers located near each other thus have more contact than they do with those further away. In other domains, knowledge spillovers have been shown to decline with geographic distance: for example, from patents (Jaffe, Trajtenberg, and Henderson 1993; Belenzon and Schankerman 2013), and technology adoption (Agha and Molitor 2018), and to be stronger for firms in the same industry (e.g., management practices, Bloom et al. 2017). Furthermore, the set of standards that OSHA checks for in an inspection, as well as the likelihood that OSHA will inspect a particular facility, varies widely by industry (Weil 1996); thus the description of the violations in a press release is likely more relevant to other facilities in the same industry.

(https://www.osha.gov/news/newsreleases). I was unable to match about 3 percent of press releases to an inspection in IMIS.

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

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

B. *Estimating the Effects of Press Releases on Compliance When Inspections Are Endogenous*

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

However, this concern is easily addressed. When measuring compliance, we can focus on programmed inspections: because OSHA initiates such inspections for reasons exogenous to the facility (conditional on industry and other criteria), there is little scope for bias from comparing compliance conditional on such inspections between treated and nontreated facilities. As a slightly less stringent alternative, we can include all types of inspections to measure compliance, but control for whether the inspection is programmed or "unprogrammed." I adopt both of these approaches in the analyses that follow.

C. *Regression Discontinuity* (*RD*) *Method*

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

Fortunately, OSHA's procedures to issue press releases provide a set of inspected facilities that did and did not become the subject of a press release, but that were otherwise very similar. Because OSHA used a rule to issue a press release about violations only if the financial penalties were above a cutoff, one can estimate effects of these press releases using a regression discontinuity (RD) design, provided certain identification assumptions are met.

Suppose we are interested in the effect of a press release on some outcome for a facility that is publicized in a press release ("specific deterrence"). Whether the

facility is the subject of a press release is a function of the penalty issued at an OSHA inspection, or the "running" variable in RD terminology. Because penalties may also have their own direct effect on later outcomes, such as later OSHA compliance, it is important to control flexibly for the running variable itself to isolate the effects of the press release.

Suppose facility i is inspected and receives a penalty levied at date t amounting to Pen_{it}, and we are interested in an outcome for i observed at a date τ months relative to t. We can reorient a facility's inspection history around the "focal" penalty levied on date t the following way:

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

where

$$Pen_{it} = \text{ penalty levied at } i \text{ at time } t,$$

$$D_{it} = \mathbf{1}\{Pen_{it} \geq c\},$$

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

To estimate the effects of a press release on compliance conditional on a future inspection, $Y_{it\tau}$ is a function of assessed compliance at an inspection of i initiated at a date after t. For the main results, I measure compliance as the number of violations and the initial financial penalties,[26] and I restrict attention to inspections up to 36 months following the focal date ($\tau \in \{0, 36\}$).

Estimating the general deterrence, or spillover, effects of a press release on nonpublicized facilities requires a slightly different specification. Suppose again that facility i is inspected at time t and is issued penalties Pen_{it}, and that we are interested in an outcome at "peer" facility j, within a particular vicinity v of i. I create a new expanded dataset in which all facilities in vicinity v of focal facility i are reoriented around i's focal penalty date t, such that j's inspections occur at dates τ relative to the focal date t. The unit of observation is now a facility-focal date-inspection date ($jit\tau$). I model an outcome Y at peer facility j as a function of whether its focal penalty Pen_{it} was above the press release threshold. Again, because penalties at i may have their own independent effect on outcomes at j, it is important to control flexibly for the focal penalty:

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

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

Now, the running variable for all facilities within a vicinity v of the "focal" facility i is the focal penalty, Pen_{it}, assessed at i at time t.[27] If $Pen_{it} \geq c$, all facilities within vicinity v have been exposed to a press release, in an Intent-to-Treat (ITT) sense. Compliance is again measured as the number of violations or the financial penalties and $\tau \in \{0, 36\}$.

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

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

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

Because OSHA did not perfectly adhere to the cutoff rule to issue press releases, the coefficient γ in equations (1)–(3) estimates an Intent-to-Treat (ITT) effect of press releases. To obtain an estimate that accounts for this imperfect adherence (i.e., the Treatment-on-the-Treated, or TOT, effect), I employ the standard "fuzzy RD" method to rescale the ITT estimate by the "first stage" effect of the cutoff on the probability that a press release is issued, effectively instrumenting whether a focal facility is subject to a press release with whether its penalty is above the cutoff ($Pen_{it} \geq c$).

Since press releases were covered in local newspapers and industry publications, the vicinity v has both a geographic and an industry component. As the baseline specification, I define a facility j to be in the vicinity of i if it is within a 5 km geographic radius of i and in the same sector (as defined as in Table 1); while 5 km is an arbitrary numeration, it is intended to group facilities that are very close to each other and thus especially likely to communicate with and observe publicity about one another. I use expanded geographic rings in follow-on analysis. Because there may be correlation in OSHA compliance between facilities in close proximity to each other, the regressions cluster standard errors to allow arbitrary correlation in ϵ among all facilities in the same peer group.[28]

In all specifications, I use the approach developed by Calonico et al. (2019) to select the MSE-optimal bandwidth around the cutoff c. In the main specifications I use a triangular kernel around the cutoff (placing more weight on observations

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

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

closer to the cutoff) and a linear polynomial in the running variable Pen_{it}, allowing for different slopes on each side of the cutoff. Finally, I include two control variables when estimating equations (1) and (2). First, I include a dummy variable indicating an inspected facility is in the construction sector; because the construction industry has its own set of OSHA standards not applicable to other industries, and because inspections of construction sites are conducted differently than inspections of establishments in other industries (Weil 2001), this control substantially improves precision. Second, I include a dummy variable indicating if an inspection is "programmed" or "unprogrammed"; since these two categories of inspections occur for very different reasons, this control also substantially improves precision.[29]

D. *Checking the Validity of the RD Design*

The RD design rests on the assumption that whether the running variable (here, OSHA financial penalties) ends up just above or just below the cutoff for press releases is as good as random. This assumption is valid if those involved have imperfect control over the exact penalty issued, and it can be jeopardized if there is room for manipulation. For example, if there are reputational costs from publicity about poor safety, the disutility from penalties is discontinuous at the cutoff c. If managers know the value of c they may attempt to bunch just below it.

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

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

One test of the validity is whether the density of penalties is smooth around the cutoff c. Figure 4 illustrates this density. Penalty amounts are normalized by the corresponding regional cutoff c and are placed in equally sized bins of \$2,500 (ensuring all bins are on only one side of each cutoff), and frequencies are calculated for each bin. The sample includes inspections with penalties issued between October 2009 and November 2012. The density is overall quite smooth, and implementing the

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[29] As will be seen, controlling for programmed inspections yields essentially an identical point estimate as a specification that does not include this control and is restricted to programmed inspections, but has a smaller standard error.
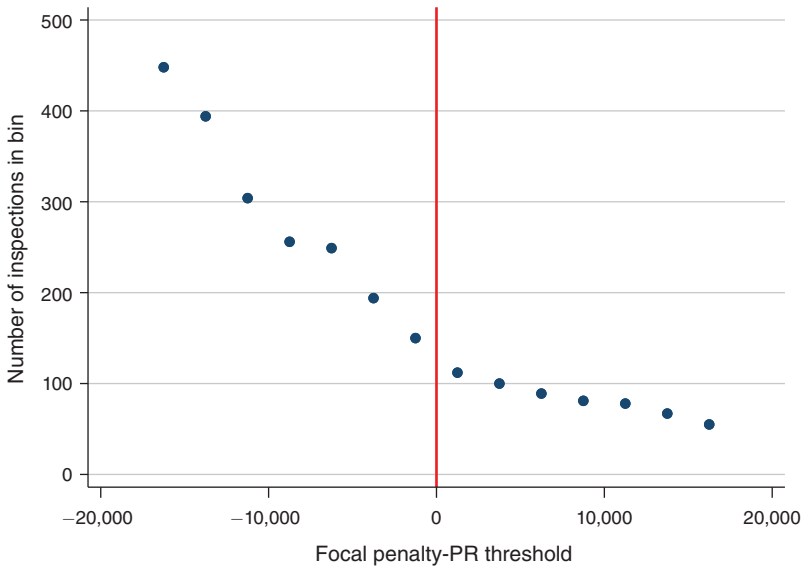
FIGURE 4. FREQUENCY OF INSPECTIONS AROUND PRESS RELEASE CUTOFF

Notes: The figure shows the density of the number of inspections, by the financial penalties levied at the inspection. Each dot plots the number of inspections in a bin, where bins are defined by $2,500 non-overlapping intervals of penalty issued. The sample includes inspections with penalties issued from October 2009 through November 2012.

test proposed by McCrary (2008) confirms that there is no statistically significant change in the density at the cutoff.

A second test of the validity of the "imprecise control" assumption is whether relevant baseline characteristics are smooth around the cutoff. Online Appendix Table A.1 shows results from estimating equation (1), with $\tau = 0$. In column 1, the dependent variable is whether a press release is issued in the inspection; the coefficient indicates that inspections with penalties just above the press release cutoff are 19 percentage points more likely to be the subject of a press release ($p < 0.01$), which roughly corresponds to the graphical discontinuity in Figure 3. In the remaining columns the outcome variable is equal to one of various baseline characteristics measured at the time of the focal inspection. The results show no evidence of a discontinuity in any characteristics, providing further support an RD design will yield valid identification in this setting.[30]

IV. The Effects of Publicizing OSHA Violations on Worker Health and Safety

This section investigates the extent to which press releases about severe OSHA violations affected facilities' safety and health. Section IVA reports "general

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[30] An alternative way to check for smoothness in baseline covariates is to run a regression with D_{it} as the dependent variable, include each baseline characteristics as a right-hand side variable, and to conduct an F-test that coefficients on all baseline covariates are equal to 0. The results of this specification, not shown, yield an F-statistic of 1.3 and p-value of 0.29.

Panel A. Total violations
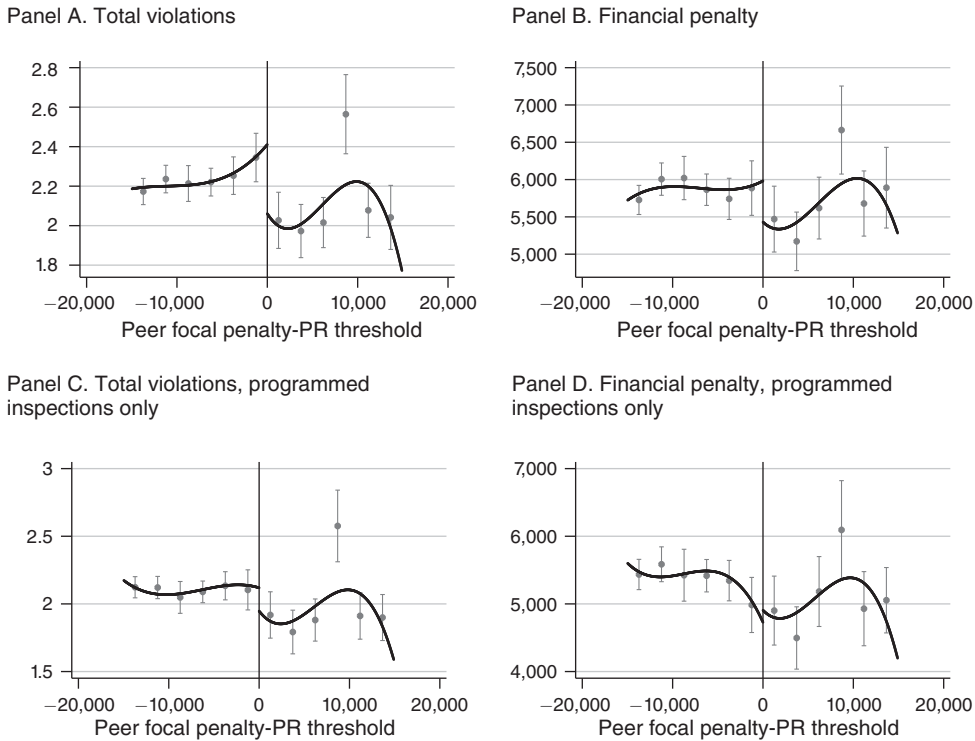
Panel B. Financial penalty

FIGURE 5. INTENT-TO-TREAT (ITT) GENERAL DETERRENCE EFFECT OF A PRESS RELEASE ON SUBSEQUENT COMPLIANCE OF OTHER FACILITIES IN A 5 KM RADIUS AND IN THE SAME SECTOR

Notes: The panels show noncompliance among facilities in a 5 km radius and the same sector as an inspection of a *focal* facility that was recently issued a *focal penalty*, for different measures of noncompliance and different sample restrictions. Each *focal penalty* is normalized by the cutoff c, above which OSHA was supposed to write a press release about the focal facility. Each dot corresponds to an average over a $2,500 bandwidth of focal penalty, with 95 percent confidence intervals included. The continuous lines represent third-order polynomials fitted separately on each side of the cutoff. The sample includes inspections occurring in the 36 months following the date the focal penalty was issued through December 2013, and for which the focal penalty was issued between October 2009 and November 2012.

deterrence" estimates, or how a press release about one facility affects the compliance of other facilities likely exposed to it. After verifying the robustness of the results in IVB, Section IVC estimates "specific deterrence" effects of a press release on the compliance of the publicized facility. Finally, Section IVD estimates the effects of press releases on the occurrence of worker injuries.

A. *General Deterrence Effects on Compliance*

Figure 5 graphically illustrates whether exposure to a press release leads facilities to subsequently change their compliance with OSHA standards. The sample includes inspections of facilities in the same sector, within 5 km, and within 36 months following the inspection of a facility with a focal penalty within $15,000 of the press release cutoff. Each facility is placed into a bin based on its focal penalty. In panels A and B, the dependent variables are number of violations and total financial penalty, respectively. In panels C and D, the dependent variables are the

same but the sample is restricted to programmed inspections. Each graph depicts a clear discontinuous downward shift in noncompliance among facilities whose focal penalty is just to the right of the cutoff c.[31]

The anomalous high average for the fourth bin to the right of the cutoff in Figure 5 merits discussion. This bin contains the fewest number of observations of any bin in these figures, as shown in panel A of online Appendix Figure A.2; given this small sample, this high average likely reflects sampling variation. Indeed, using larger bins of $3,000 instead of $2,500 (panel B of Figure A.2) makes this out-lier disappear. Furthermore, this outlier seems to arise because this bin contains an over-representation of certain states (Connecticut and Georgia), which happen to have higher-than-average violation rates, as well as a slight over-representation of non-construction inspections, which tend to have more violations than construction inspections. Online Appendix Figure A.3 (again using $2,500 bins) illustrates that this outlier disappears when the sample is restricted to construction inspections out-side of Connecticut and Georgia, but the figure still reveals a clear discontinuity at the cutoff. I further discuss the sensitivity of the estimates to this outlier below.

While the figures provide evidence that penalties above the press release cutoff lead to higher compliance in later inspections of peer facilities, as described above this ITT effect does not yield a magnitude representing the effects of press releases due to the fuzziness of the cutoff rule. To estimate the effects of press releases (the TOT estimate), I estimate a fuzzy RD regression, employing the procedure detailed in Calonico et al. (2019) to optimally select the bandwidth for a fuzzy RD design that allows for clustering (by peer group) and covariate adjustment (a dummy for construction and for whether an inspection is "programmed" or "unprogrammed"). The bandwidth that minimizes mean squared error, based on Calonico et al. (2019), is 3,976.8 to the left and 9,552.1 to the right of the cutoff. (As discussed below, the estimates are quite insensitive to the bandwidth selection.)

Table 2 reports regression estimates of the effect of press releases on peers' compliance. Panel A includes all inspections, and panel B restricts to programmed inspections (i.e., excludes inspections initiated by a serious accident worker com-plaint, or referral). The first column estimates the ITT effect of a press release on the number of violations detected in a subsequent inspection. Focusing on panel A, the ITT point estimate is -0.40 ($p < 0.01$). The estimated first stage, in column 2, is 0.24 ($p < 0.01$). Finally, the TOT estimate in column 3, essentially the ratio of col-umn 1 over column 2, is -1.67 ($p < 0.01$). Since facilities with a focal penalty to the left of the cutoff averaged 2.29 violations, the TOT estimate implies that a press release led to 73 percent fewer violations at later inspections of other facilities in the same sector within a 5 km radius. The story is similar when the dependent variable is instead the (log) of the dollar amount of financial penalties issued in an inspection (online Appendix Table A.2).

These estimates pool the 36 months after a focal penalty is issued, but the effect of a press release on peers' compliance could be dynamic, e.g., taking time to appear,

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[31] The discontinuity in noncompliance at the threshold is less clear in panel D, where the dependent variable is financial penalty and the sample is programmed inspections, due to a downturn in penalties in the bin just to the left of the cutoff. This apparent non-discontinuity disappears when I account for potential differential sector composi-tion of inspections near the cutoff: when I restrict the sample to the construction sector, there is clear discontinuity in penalties at the cutoff for the sample of programmed inspections (results not shown).

Table 2—Instrumental Variables (IV) Estimate of the General Deterrence Effect of a Press
Release on Compliance of Other Facilities within a 5 km Radius and in the Same Sector

	ITT (Dep var = number of violations) (1)	First stage (Dep var = press release in focal inspection) (2)	TOT (Dep var = number of violations) (3)
All inspections			
Focal penalty $\geq c$	−0.40 (0.14)	0.24 (0.058)	
Press release in focal inspection			−1.67 (0.64)
Robust *p*-value	0.003	0.000	0.009
Observations	9,761	9,761	9,761
Number of peer groups	481	481	481
Left bandwidth	3,977.3	3,977.3	3,977.3
Right bandwidth	9,552.1	9,552.1	9,552.1
Control mean dependent variable	2.29	0.07	2.29
Programmed inspections			
Focal penalty $\geq c$	−0.33 (0.17)	0.20 (0.08)	
Press release in focal inspection			−1.64 (0.91)
Robust *p*-value	0.050	0.008	0.071
Observations	10,873	10,873	10,873
Number of peer groups	674	674	674
Left bandwidth	8,220.7	8,220.7	8,220.7
Right bandwidth	6,667.0	6,667.0	6,667.0
Control mean dependent variable	2.03	0.07	2.03

Notes: The table shows regression estimates of the effect of a press release about one focal facility on the subsequent compliance of peer facilities, defined as those within a 5 km radius and in the same sector. The running variable is the focal penalty, and the threshold is whether the focal penalty is higher than the press release cutoff c. The sample in all regressions includes inspections of peers occurring in the 36 months following the date the focal penalty was issued through December 2013, and for which the focal penalty was issued between October 2009 and November 2012.
The regressions are estimated with a linear polynomial in the running variable and include controls for indicators that a facility is in the construction sector and (in the top panel) if an inspection was programmed. See Section IIIC for further details. Robust standard errors clustered by peer group.

or alternatively decaying over time. Online Appendix Table A.3 tests whether such temporal effects are present. I estimate a variant of equation (2) allowing the ITT effect of exposure to a press release to differ 0–6, 7–12, 13–24, and 25–36 months after the focal penalty is issued.[32] Effects show up immediately and remain through 36 months after the date the focal penalty is issued.

It is plausible that press releases would affect the behavior of facilities located further away than 5 km. Figure 6 explores how the general deterrence effects of

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[32] This regression takes the form

$$Y_{jit\tau} = \left(\sum_k \alpha_k + \gamma_k \times D_{it} \times \alpha_k \right) + f(Pen_{it} - c) + \epsilon_{jit\tau}$$

with $k = \{\tau \in \{0\text{–}6\}, \tau \in \{7\text{–}12\}, \tau \in \{13\text{–}24\}, \tau \in \{25\text{–}36\}\}$ months. Note that this regression uses the same bandwidth as the main specifications reported in Table 2, but use a uniform kernel instead of triangular kernel. Thus, column 1 (which reproduces the ITT estimate pooling all 36 post-months) yields a slightly different point estimate and standard error than the estimate reported in column 1 of Table 2.
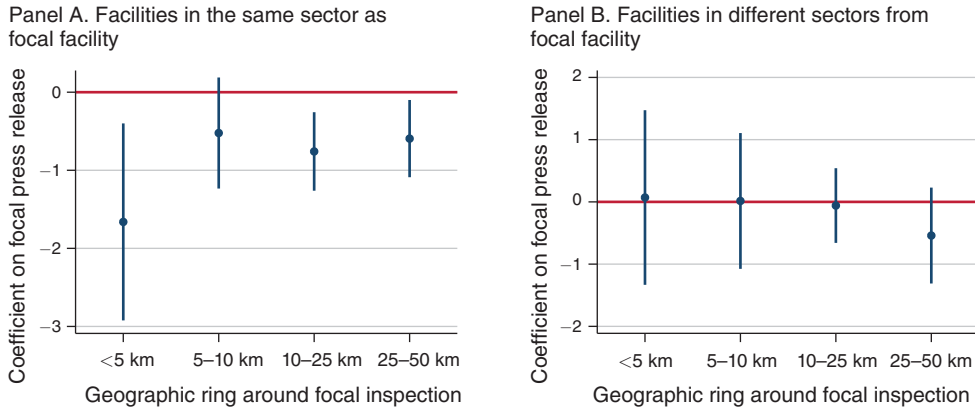
Panel A. Facilities in the same sector as focal facility

Panel B. Facilities in different sectors from focal facility

FIGURE 6. GENERAL DETERRENCE EFFECTS OF PRESS RELEASES AS A FUNCTION OF GEOGRAPHIC AND SECTORAL DISTANCE

Notes: The figures plot the point estimate and 95 percent confidence interval of the coefficient on *Press Release in Focal Inspection* from a regression in which the dependent variable is the number of violations found in an inspection, the sample includes inspections of peer facilities (for different definitions of peers) in the 36 months following the date a focal penalty is issued in a focal inspection, and *Press Release in Focal Inspection* is instrumented with *Focal Penalty* $\geq c$. In panel A, peer facilities are defined as being in the same sector as, and within various geographic radii of, the focal inspection. In panel B, peer facilities are defined as being in a different sector from, and within various geographic radii of, the focal inspection.

press releases change depending on how "vicinity" to the focal facility is defined. Panel A of Figure 6 plots the TOT point estimates and 95 percent confidence intervals from equation (2) with "vicinity" requiring shared sector, for non-overlapping radii around the focal facility of 5 km, 6 to 10 km, 11 to 25 km, and 26 to 50 km.[33] The point estimate is imprecisely measured for the 6–10 km group ($p = 0.14$), but are statistically significant for the larger vicinities and are roughly half as large in magnitude as for those within 5 km. Online Appendix Table A.4 displays the ITT, first stage, and TOT estimates for the shared-sector peer groups of various geographic vicinities, but using overlapping groups (i.e., up to 5 km, up to 10 km, up to 25 km, and up to 50 km).[34] The typical press release leads to 30 percent fewer violations among facilities located up to 50 km away ($\beta = -0.66$, relative to control mean of 2.22) ($p < 0.01$).

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

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[33] For all peer groups, I impose the requirement that two facilities be in the same commuting zone to be in the same peer group. Commuting zones are clusters of counties that prior studies have used to proxy for local labor markets (e.g., Autor, Dorn, and Hanson 2013). To identify each facility's commuting zone, I used the county-commuting zone crosswalk from David Dorn's web site (https://www.ddorn.net/data.htm). I identified each facility's county based on its geocoded latitude and longitude. For the rare cases in which I could not geocode a facility's address, I used the publicly available zip code-county cross walk from HUD (https://www.huduser.gov/portal/datasets/usps_crosswalk.html).

[34] For each of these vicinities, I re-estimate the MSE-optimal bandwidth around the cutoff.

To put the magnitude of the TOT estimate in perspective, a useful benchmark is the effect of inspections themselves on compliance. Like many regulatory agencies, inspections are, and have historically been, OSHA's primary tool to monitor, enforce, and promote compliance. One study by Ko, Mendeloff, and Gray (2010), which examined inspections of manufacturing plants between 1996 and 2006, estimated that a typical OSHA inspection led to 48 percent fewer violations at later inspections of the same facility. Another study of inspections of large construction firms between 1987 and 1993 found that inspections led to more modest improvements in compliance at inspected facilities (Weil 2001). Taking the estimate in Ko, Mendeloff, and Gray (2010) as an upper bound of the specific deterrence effect of a typical inspection, the results in online Appendix Table A.4 suggest that a press release about severe violations led the typical facility within a 5 km radius and the same sector to improve compliance by 50 percent more than if OSHA inspected each of those facilities directly, and the typical facility within a 50 km radius to improve by 62 percent as much.[35] Put another way, given that there are on average 340 inspections in the sample period among peers in the same sector and a 50 km radius of focal facilities, OSHA would have to conduct 210 (340×0.62) inspections to elicit the same level of deterrence as a single press release about severe violations.[36,37]

While this magnitude is strikingly large, it is not unbelievable. OSHA is statutorily limited in its ability to issue fines, and the likelihood that OSHA will repeatedly inspect a given facility is quite low. As a result, a standard model of crime (Becker 1968) would predict low potential for deterrence from inspections. On the other hand, the discussion in Section I illustrates several substantial potential costs of publicity about OSHA violations, and a manager may be much more incentivized to improve compliance to avoid such publicity than to avoid more fines at any future potential inspections. Indeed, the former director of OSHA has said that company lawyers told him that their clients worried more about seeing their names in OSHA press releases than about being fined.[38]

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[35] A caveat to this calculation is that the estimate from Ko, Mendeloff, and Gray (2010) is from a different time period and may not generalize to the time period analyzed in this paper. However, the comparative magnitudes of the deterrence effects of press release would still be substantial even if a typical inspection led to 100 percent reduction in violations of inspected facilities.

[36] This number could be an underestimate of the number of inspections OSHA would need to conduct to achieve the deterrence of a press release, given that it ignores any effects on uninspected facilities and on facilities located further away than 50 km, and since Weil (2001) finds the deterrence effects of inspections of construction firms (which make up a large portion of this analysis's sample) is smaller than the estimate of 48 percent from Ko, Mendeloff, and Gray (2010).

[37] At the same time, this number could be an overestimate if inspections themselves have general deterrence effects. Evidence of such effects, however, is minimal in the case of OSHA. One study (Johnson, Levine, and Toffel 2017) estimated the spillover effects of inspections conducted by OSHA under its Site-Specific Targeting program on injury rates of other geographically proximate facilities to be small and statistically insignificant. The point estimate in that study, while not statistically significant, implied that one OSHA inspection led to 3.7 percent lower injury rates at other facilities in the same sector and commuting zone. Even though this paper considers a different outcome (compliance, versus injury rates in Johnson, Levine, and Toffel 2017), suppose that we apply this estimate and one OSHA inspection leads to 3.7 percent fewer violations at other facilities in the same sector and 50 km radius. Given the sample mean number of violations as 2.2, this would imply that the number of inspections that would achieve the same reduction in violations as a press release is 8 $(2.2 \times (340 \times -0.66))/(2.2 \times (1 \times (-1.07) + 340 \times (-0.082))) = 7.7$, which is substantially lower than 210.

[38] Jim Morris, "Death in the Trench," Center for Public Integrity, December 14, 2017, https://apps.publicintegrity.org/the-trench.

The extent to which publicity about OSHA violations would elicit improvements in safety and health may differ by industry. For example, relative to other sectors, the construction sector faces different types of OSHA standards, different work environments, and different definitions of what a "facility" represents. Online Appendix Figure A.4 separates the visual evidence of the discontinuity in violations of peer facilities for focal penalties at the press release cutoff, separately for construction and manufacturing. Panels A and B use the baseline peer vicinity of within 5 km of the focal facility used in Figure 5. There is a clear discontinuous drop in violations for the construction sector, and less so for manufacturing. However, the sample size for the manufacturing figure is relatively small, as manufacturing only makes up 23 percent of the set of peers within a 5 km radius of a focal facility. Panels C and D expand the vicinity of peers to be 25 km. With this expanded vicinity, there is a clear discontinuous drop in violations at the cutoff for the manufacturing sector (and the discontinuity remains clear for construction).

Press releases might affect compliance with some types of OSHA standards more than others. Do press releases reduce the violations most likely to cause accidents? Or do they affect less serious violations that have little direct effect on safety and health? Online Appendix Table A.5 reports ITT estimates, based on equation (2), using different measures of compliance for the dependent variable. Column 1 considers the number of *Willful* (in which an employer has demonstrated either intentional disregard for requirements of the OSHA Act or a plain indifference to employee safety and health) or *Repeat* (if the employer has previously been cited for the same condition or hazard) violations. Column 2 considers violations of *Gravity* 10: violations are assigned a gravity on a scale of 1 to 10, with 10 being most likely to result in severe incident, and thus OSHA considers violations with a gravity of 10 to be the most hazardous. Exposure to a press release leads to significantly fewer of both of these, and the effect sizes are large in percentage terms. Columns 3 through 5 assess the effects on the distribution of violations by using a dependent variable equal to 1 if total violations exceed 0, 2, and 4, respectively. The magnitude of the effects in percentage terms monotonically increases across the columns, suggesting that press releases lead to an especially large decrease in especially high noncompliance.

B. *Checks on Validity of Results*

Robustness Checks.—Next, I conduct several tests to ensure the validity of the baseline results. First, I check the sensitivity of my estimates to the chosen bandwidth. I re-estimate the coefficient reported in panel A of Table 2, but varying the bandwidth around the cutoff. Online Appendix Figure A.5 displays the results. I start with the baseline bandwidth selected by the Calonico et al. (2019) procedure, and report estimates from regressions progressively decreasing the left and right bandwidths by 10 percent, then progressively increasing it. Panels A and B report, for the set of all inspections, point estimates and 95 percent confidence intervals at different bandwidths for ITT estimates and TOT estimates, respectively. In both cases, the point estimates are remarkably stable for different bandwidths. Panels C and D do the same, but restricting the sample to programmed inspections. In this case, the TOT estimates are more sensitive to the bandwidth choice, especially at smaller bandwidths where the sample size becomes low. Collectively, these

figures highlight that the main results are not materially affected by the bandwidth choice.

Second, online Appendix Table A.6 reports tests to ensure the estimates are robust to alternative specifications and are not driven by spurious relationships. Column 1 reproduces the baseline ITT estimate of the effect of exposure to a press release on the number of violations in subsequent inspections of peer facilities in the same sector and within 5 km. Columns 2 and 3 assess sensitivity to regression specification. Column 2 uses an Epanechnikov kernel (in which an observation's weight decreases parabolically with distance to the cutoff) instead of a triangular kernel. Column 3 uses a quadratic, rather than linear, polynomial in the running variable. In each case the estimate is essentially unchanged from the baseline.

Columns 4 and 5 assess sensitivity to outliers. The model in column 4 drops observations with violations above the ninety-ninth percentile, rather than topcoding their values and including them in the regression. The estimate remains largely unchanged. Column 5 uses as the dependent variable the Inverse Hyperbolic Sine (IHS) transformation of the number of (non-topcoded) violations: a method to mitigate the influence of outliers for variables that include values of zero. Here, the regression coefficient estimates the ITT effect of a press release on the *percent change* in the number of violations. The point estimate (-0.18) is identical to the baseline estimate in percent terms $(-0.40/2.29 = 17.5\%)$.

Columns 6 and 7 assess sensitivity to additional controls. The model in column 6 includes controls for the number of inspections, and the seventy-fifth percentile of penalties issued, between 2005 and 2008 in the county and sector of the focal inspection, as well as fixed effects for each OSHA region and the year the focal penalty was issued. The model in column 7 does not include these controls but instead includes state fixed effects. These additional controls should be uncorrelated with treatment for the RD design to be valid, but including them may improve efficiency. This appears to be the case; in both columns, the point estimate changes by a small magnitude only and the standard error shrinks.

Columns 8 and 9 address a potential concern that one facility may fall in the radius of multiple "focal" inspections. Column 8 restricts to a facility's earliest focal penalty above $25,000 in the sample period, and Column 9 restricts to a facility's maximum focal penalty. The coefficient is essentially identical for the former and twice as large for the latter; this increase in the magnitude could reflect that use of repeated observations mutes the effect of "treatment."

Finally, column 10 defines facilities in the "vicinity" of a focal inspection as those in the same zip code rather than with a geographic radius. The sample size shrinks, but the point estimate remains highly significant and very similar to the baseline estimate.

Placebo Tests.—I run three placebo tests to validate the causal interpretation of the above results. First, I re-run the regressions corresponding to equation (2) but replacing the true cutoff c with a series of placebo meaningless cutoffs. If we were to find a significant coefficient using any of these meaningless cutoffs, one would worry that the above significant estimates are spurious. Table 3 displays the results. Using all cutoffs other than the true press release cutoff, the estimated coefficient is tiny and statistically indistinguishable from zero, whether the dependent variable is number of violations or log of penalties.

TABLE 3—COMPARING INTENT-TO-TREAT GENERAL DETERRENCE EFFECTS OF PRESS RELEASES USING
THE TRUE PRESS RELEASE CUTOFF VERSUS PLACEBO CUTOFFS

	25k (1)	30k (2)	$c =$ PR cutoff (3)	55k (4)	65k (5)
Dependent variable = number of violations					
Focal penalty $\geq c$	0.11	−0.00	−0.40	0.07	0.08
	(0.09)	(0.09)	(0.14)	(0.15)	(0.25)
Robust p-value	0.216	0.965	0.003	0.650	0.749
Observations	106,670	59,729	9,761	8,760	4,683
Control mean dependent variable	2.17	2.18	2.29	2.05	2.00
Dependent variable = ln(penalties)					
Focal penalty $\geq c$	0.06	0.00	−0.14	0.02	−0.13
	(0.04)	(0.04)	(0.05)	(0.07)	(0.11)
Robust p-value	0.100	0.965	0.002	0.816	0.237
Observations	106,670	59,729	9,761	8,760	4,683
Control mean dependent variable	8.25	8.27	8.27	8.19	8.13

Notes: The table shows the effects of a penalty levied on one focal facility (the *focal penalty*) that is above various cutoffs on the subsequent compliance of peer facilities, defined as those within a 5 km radius and in the same sector, including the true press release cutoff and other placebo cutoffs. For OSHA regions 5, 7 and 8, the true press release cutoff is 45,000, and for all other regions it is 40,000. The sample in all regressions includes inspections of peers occurring in the 36 months following the date the focal penalty was issued through December 2013, and for which the focal penalty was issued between October 2009 and November 2012. The running variable in all regressions is the focal penalty, and the threshold is whether the focal penalty is higher than the cutoff c in the corresponding column. The regressions are estimated with a linear polynomial in the running variable and include controls for indicators that a facility is in the construction sector and if an inspection was programmed. See Section IIIC for further details. Robust standard errors clustered by peer group.

Second, I examine inspections of peer facilities that happen *before* the focal penalty is issued. Outcomes in such inspections should not be affected by future events. Online Appendix Figure A.6 is identical to panel A of Figure 5, except that it uses inspections conducted in the 36 months *before* the focal penalty is issued. Reassuringly, there is no change in violations at the press release cutoff.

Third, I ensure that the results are not driven by another factor that "switches on" at penalty amounts exceeding \$40,000 or \$45,000. Recall that Regions 1 and 4 adopted the \$40,000 cutoff to issue press releases several years before 2009, but that all other regions had been using either a much higher cutoff or none at all. Running the regression corresponding to equation (2) but oriented around inspections with penalties levied before 2009, and specifying c as \$40,000, one expects a significant coefficient on D_{it} for Regions 1 and 4, and zero for all others.

Table 4 tests these predictions. Panel A estimates the first-stage effect of having a penalty above \$40,000 on the likelihood a press release is issued among inspections with penalties issued between 2002 and 2008. Column 1 shows that in Regions 1 and 4, the coefficient is 0.24 ($p < 0.01$), similar to the whole sample after the 2009 policy change, and column 2 shows that the first-stage effect is essentially zero in other regions. Panel B estimates the general deterrence effects on the compliance of peer facilities in the same sector and within 5 km. The coefficient for Regions 1 and 4 is −0.53 ($p < 0.01$), and the coefficient is essentially zero and nowhere near statistically significant in other regions. These estimates are corroborated in online Appendix Figure A.7, which illustrates the discontinuous increase in the probability a press release is issued, and the decrease in subsequent violations among peer

Table 4—The Effect of Receiving Penalties above $40,000 Prior to 2009 on (i) the Probability That a Press Release Is Issued, and (ii) the Future Compliance of Peer Facilities

	Regions 1, 4 (Press release policy in place, 2002) (1)	Regions NOT 1, 4 (Press release policy begins in 2009: placebo check) (2)
Dependent variable: press release issued in focal inspection		
Focal penalty $\geq c$	0.24	−0.00
	(0.09)	(0.03)
Robust *p*-value	0.007	0.927
Observations	539	509
Control mean dependent variable	0.05	0.01
Dependent variable: violations at later inspections of peer facilities		
Focal penalty $\geq c$	−0.53	−0.08
	(0.17)	(0.21)
Robust *p*-value	0.001	0.721
Observations	4,314	4,623
Control mean dependent variable	2.41	2.27

Notes: The top panel estimates the effect of a *focal* facility receiving a penalty above 40,000 between 2002 and 2007 on the likelihood it results in a press release, and the sample includes inspections of *focal* facilities that were issued penalties between 2002 and 2007. The bottom panel estimates the effect of a focal penalty above 40,000 on the subsequent compliance of peer facilities, defined as those within a 5 km radius and in the same sector (bottom panel) and for inspections occurring in the 36 months following the date the focal penalty was issued. Column 1 restricts to facilities in OSHA regions 1 and 4, which were using a cutoff rule of 40,000 to issue press releases since 2002. Column 2 restricts to facilities in other regions that were not using a cutoff rule during this time, thus serving as a placebo check. The regressions are estimated with a linear polynomial in the running variable and include controls for indicators that a facility is in the construction sector, an indicator if an inspection was programmed, and an indicator that a facility is located in OSHA region 1. Robust standard errors clustered by peer group.

facilities, for focal penalties issued between 2002 and 2008 in Regions 1 and 4, and no discontinuity in either case for regions other than 1 and 4.

C. *Specific Deterrence Effects on Publicized Facilities*

The above results provide evidence that facilities exposed to a peer's press releases substantially improved their OSHA compliance. A separate question is how press releases affected the subsequent compliance of the *publicized* facility. On one hand, publicized facilities may be eager to clean up after a loss to their reputation. On another hand, the specific and general deterrence effects of "shaming" may differ: if publicized facilities suffer a loss to reputation, and subsequently have few opportunities to signal improvements to stakeholders, they may face weak incentives to improve compliance (Board and Meyer-ter-Vehn 2013).

Estimating specific deterrence effects of publicity is challenged since, in construction, the concept of a "facility" is ill-defined. If OSHA issues penalties to a construction contractor at one site, the next time OSHA inspects that contractor may be at a different site, making it difficult to create a facility identifier. The task is more straightforward for non-construction: a manufacturing plant stays in one place and is relatively easy to track across repeat inspections. For this analysis, I define a "facility" as inspections sharing the same sector and an identical latitude and longitude. Furthermore, to increase the sample size as much as possible, I combine focal penalties issued between October 2009 and November 2012 (the baseline sample)

with focal penalties in Regions 1 and 4 issued between 2002 and 2008 (which, recall, were using the $40,000 cutoff since at least 2002).

Online Appendix Figure A.9 graphically tests the effects of a press release on number of violations detected at later inspections. Panel A includes all types of inspections, and panels B restricts to later non-complaint, -referral or -accident inspections.[39] The plots hint at a discontinuous downward jump in noncompliance at the cutoff, but really are too noisy to provide conclusive evidence either way.

Online Appendix Table A.7 displays ITT regression results, corresponding to equation (1). Columns 1 and 2 include all inspections, and columns 3 and 4 restrict to non-complaint, -referral or and -accident inspections. The point estimates for the number of violations are large and negative in both cases and statistically significant ($p = 0.012$) for the sample including all inspections. The estimates are noisier when the dependent variable is financial penalties assessed. Thus, press releases might have had large specific deterrence effects, though given the small sample size and imprecision these estimates should be viewed as suggestive.

D. *Does Publicizing OSHA Violations Lead to Fewer Work-Related Injuries?*

The previous sections provided evidence that OSHA's press releases caused substantial improvements in compliance with OSHA regulations. A more direct measure of the social benefit is whether press releases led to improved health and safety *outcomes*. This section investigates this question.

To measure safety and health outcomes, I use the occurrence of OSHA "fat/cat" inspections: those triggered by a fatal injury or by the hospitalization of three or more workers. I calculate the number of such inspections that occur in a peer group over the 36 months following the date of the focal penalty.

Table 5 shows ITT regression estimates, corresponding to equation (3). I again employ the Calonico et al. (2019) method to select the MSE-optimal bandwidth around the cutoff. Because fat/cat inspections are rare, these regressions include additional controls (the year of the focal inspection and the number of inspections in the focal facility's sector and county in 2005–2008) to improve precision.[40]

Panel A defines peer groups as facilities in the same sector as and within a 5 km radius as the focal facility. The estimate in column 1 implies that a penalty above the press release cutoff leads to 0.08 fewer accident inspections, which is 32 percent of the mean among controls but not statistically significant at conventional levels ($p = 0.15$).

A challenge to interpreting the estimate in column 1 of panel A is that only 19 percent of peer groups, defined using the 5 km radius, have at least one fat/cat inspection in the post-period. Such rare events pose difficulties for statistical inference. Thus, panels B and C consider peers within 10 km and 25 km, 38 percent and

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[39] Unlike panels C and D of Figure 5, which restricted to programmed inspections, the "non-complaint, -referral or -accident inspections" sample includes what OSHA calls "follow-up" or "monitoring" inspections. As described in US Occupational Safety and Health Administration (2009), "The primary purpose of a follow-up inspection is to determine if the previously cited violations have been corrected." Since facilities in this sample were recently fined tens of thousands of dollars, they were prime candidates for follow-up inspections.

[40] Omitting these controls results in slightly higher standard errors but does not qualitatively affect the point estimates.

	Number of inspections of peer facilities that are:	
	Triggered by a serious injury (1)	Programmed (falsification test) (2)
Peers: same sector, within 5 km		
Focal penalty $\geq c$	−0.09	−0.41
	(0.06)	(2.43)
Robust p-value	0.140	0.867
Observations	1,836	1,836
Control mean dependent variable	0.26	13.97
Effect relative to control mean	−0.33	−0.03
Peers: same sector, within 10 km		
Focal penalty $\geq c$	−0.22	−5.37
	(0.13)	(5.35)
Robust p-value	0.095	0.316
Observations	1,604	1,604
Control mean dependent variable	0.70	36.92
Effect relative to control mean	−0.32	−0.14
Peers: same sector, within 25 km		
Focal penalty $\geq c$	−0.55	−11.83
	(0.31)	(13.47)
Robust p-value	0.077	0.380
Observations	1,571	1,571
Control mean dependent variable	2.52	125.98
Effect relative to control mean	−0.22	−0.09

Notes: The table shows ITT estimates of the effect of a *focal* facility receiving a penalty above the press release cutoff c on the number of inspections in the next 36 months of peer facilities, defined as those in the same sector and within the specified radius of the focal facility, for two categories of inspections: inspections triggered by a fatal injury or hospitalization of three or more workers, and programmed inspections that are pre-planned by OSHA and unrelated to events at the facility. The sample includes focal penalties issued between October 2009 and November 2012. The running variable is the focal penalty, and the threshold is whether the focal penalty is higher than the press release cutoff c. The regressions are estimated with a linear polynomial in the running variable and include controls for indicators that the focal facility is in the construction sector, the year the focal penalty was issued, and the number of inspections in the focal facility's county-industry 2005–2008. See Section IIIC for further details. Robust standard errors in parentheses.

65 percent of which have at least one fat/cat inspection, respectively. The estimated effect are again negative and economically meaningful, and they are each statistically significant at the 10 percent level ($p = 0.095$ and 0.077, respectively). Online Appendix Figure A.10 corroborates these estimates with graphical evidence.

These estimates would be misleading if press releases affect the rate of OSHA inspections overall, not just those inspections triggered by a serious accident. To address this concern, column 2 of panels A–C estimates the effect on the number of programmed inspections. Because these are exogenous to events at individual facilities, these results effectively serve as a falsification test. Reassuringly, in each panel the coefficient is tiny (relative to the control mean) and nowhere near statistically significant. Online Appendix Figure A.8 corroborates that a focal penalty above the press release threshold does not affect the number of programmed inspections of

peer facilities: the figure plots the density of the number of programmed inspections of peer facilities within 5 km in the 36 months following the date the focal penalty is issued. The density is smooth around the cutoff and indicates no discontinuous change in the frequency of programmed inspections.

Overall, these estimates, while imprecise, provide evidence that OSHA's press releases led facilities not only to improve their compliance with OSHA regulations, but also to experience fewer serious workplace injuries and illnesses.

V. Why Do Press Releases Lead to Better Safety and Health?

In this section, I first examine two potential mechanisms through which OSHA's publicity about large fines could lead employers to improve safety and health: (i) changing employers' beliefs about the expected costs of OSHA enforcement, and (ii) providing information to stakeholders that could lead them to take costly actions against firms. I then investigate whether media coverage is a channel through which employers learned about OSHA's press releases.

A. *Do Press Releases Change Employers' Beliefs about OSHA Enforcement?*

As described in Section I, publicity about a facility caught violating safety regulations could affect the *regulator's* reputation by changing managers' beliefs about the expected costs of OSHA enforcement. This view, however, is not supported by the specific deterrence results. Managers at publicized facilities, already subject to an inspection and fine, learn nothing new about OSHA enforcement from a press release. The estimate that publicized facilities improve compliance relative to facilities that were also inspected and fined nearly identical penalties suggests managers and/or workers subject to a press release change their behavior for reasons other than learning about OSHA enforcement.

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

Online Appendix Table A.8 tests this prediction. For each focal inspection, I identify the set of OSHA standards violated ("focal" violations), and then label all other violations as "non-focal." I calculate the number of focal and non-focal violations for subsequent inspection of peer facilities in the same-sector and 5 km radius. The ITT estimates for both focal (column 1) and *non-focal* (column 2) violations are negative and of nearly identical magnitude. These results thus provide no evidence that general deterrence effects operate through updating beliefs about the priorities of enforcement.

B. *Do Employers Improve Safety to Avoid Costly Responses from Workers?*

If, then, it is the *facility's*, rather than the regulator's, reputation that a press release affects, which stakeholders care? As described in Section I, one set of

stakeholders for which information about violations of safety and health standards is especially relevant is workers. Potential new workers may choose to work elsewhere or demand higher wages upon learning an employer is unsafe, and existing workers may update their beliefs about risks and demand better conditions or quit. However, if workers lack bargaining power, they may have little scope to leverage a press release to make such demands from their employer. A press release, then, is less likely to lead to a costly response when workers' bargaining power is low. If press releases lead to a greater improvement in OSHA compliance when workers have more bargaining power, it would suggest employers are seeking to avoid costly demands from workers.

To test this idea, I examine whether two measures of workers' bargaining power moderate the effect of press releases on compliance. One proxy for workers' bargaining power is the strength of labor unions. A longstanding theory says that the presence of unions leads nonunion employers to improve working conditions to forestall unionization, often called the union "threat effect" (Freeman and Medoff 1985), and prior work has found that an increase in local unionization leads to higher nonunion wages (Neumark and Wachter 1995). Thus, regardless of workers' own union status, they may be in a better bargaining position to leverage press releases to demand better conditions when unions have a stronger presence in their local labor market. Unions could play a more direct role by enabling collective voice that ensures workers are better able to speak up to employers (Morantz 2018), or by facilitating dissemination of press releases through union newsletters and other outlets.

I measure the strength of labor unions in two ways. The first measure is the percent of OSHA inspections in a focal facility's county between 2005 and 2008 in which a union was present (*baseline unionization rate*). The second is whether a facility is located in a Right-to-Work (RTW) state (http://www.ncsl.org/research/labor-and-employment/right-to-work-laws-and-bills.aspx).[41] RTW laws allow workers to decline to pay union dues even if they are covered by a collective bargaining agreement, leading to free-rider problems, and they have been shown to decrease union membership and to limit unions' bargaining strength (Ichniowski and Zax 1991). RTW laws are also correlated with other "pro-business" policies that disproportionately benefit employers over workers (Holmes 1998), potentially another channel through which RTW laws proxy for low worker bargaining power.

To visualize how bargaining power moderates the deterrence effect of press releases, the panels of Figure 7 plot the change in violations for facilities in the same sector and within 5 km of a focal facility with a penalty just above the press release cutoff, separately for facilities in low and high union density areas. These figures restrict to programmed inspections. The top panels are split on whether the *baseline unionization rate* is below or above the sample median,[42] and the bottom panels split on whether a facility is in a non-RTW or RTW state. In both cases, there is no clear discontinuity at the press release cutoff for facilities in areas with low union density. On the other hand, there is a clear discontinuous drop in the number of violations at later inspections for facilities in high union-density areas. Online

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[41] The correlation between these two measures is −0.61.
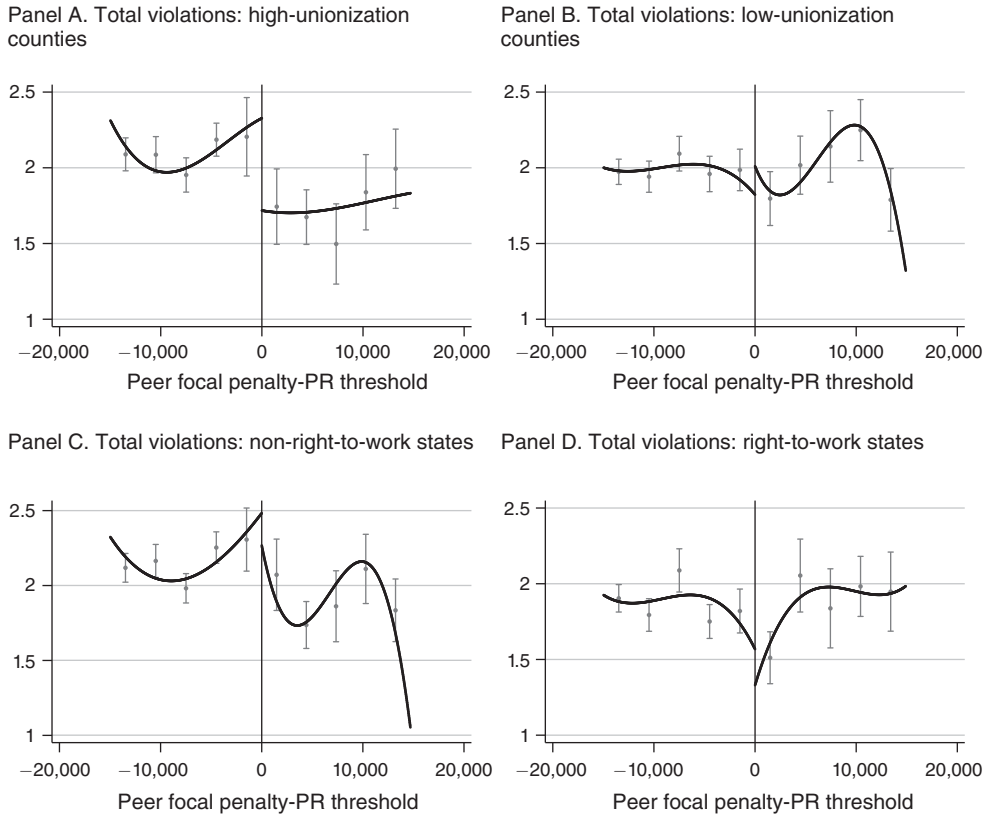[42] The sample median is 15 percent.

FIGURE 7. GENERAL DETERRENCE EFFECT OF A PRESS RELEASE ON SUBSEQUENT COMPLIANCE DEPENDS ON
THE STRENGTH OF WORKER BARGAINING POWER

Notes: The panels show the number of violations among facilities in a 5 km radius and the same sector as an inspection of a *focal* facility that was recently issued a *focal penalty*, depending on whether the facility is located in an area with low union density (left column) or high union density (right column). The top panels measure union density with whether the percent of OSHA inspections conducted in the focal facility's county between 2005 and 2008 in which the workers were unionized is below or above the sample median. The bottom panels measure low/high union density as whether a facility is in a Right-to-Work (RTW) or non-RTW state. Each *focal penalty* is normalized by the cutoff *c*, above which OSHA was supposed to write a press release about the focal facility. Each dot corresponds to an average over a $3,000 bandwidth of focal penalty, with 95 percent confidence intervals included. The continuous lines represent third-order polynomials fitted separately on each side of the cutoff. The sample includes programmed inspections occurring in the 36 months following the date the focal penalty was issued through December 2013, and for which the focal penalty was issued between October 2009 and November 2012.

Appendix Figure A.11 shows a similar relationship for the number of subsequent inspections of peer facilities triggered by a serious accident.

Table 6 reports corresponding regression estimates. The first panel reports ITT estimates and the second panel reports TOT estimates. Columns 1 and 2 split the sample by whether the peer group's *baseline unionization rate* is above or below the sample median, and columns 3 and 4 split by RTW and non-RTW states. With both measures, the TOT effect is insignificant and small in magnitude for facilities in areas with weak union presence, and large and statistically significant for facilities in areas with high union presence. A press release leads to 3.97 fewer violations ($p < 0.01$) and 4.27 fewer violations ($p < 0.01$) among peer facilities in high *baseline unionization rate* counties and non-RTW states, respectively.

TABLE 6—DO PRESS RELEASES HAVE A STRONGER EFFECT ON COMPLIANCE WHEN WORKERS
HAVE MORE BARGAINING POWER?

| | Sample: peers within 5 km radius | | | |
| | Share of pre-period inspected facilities that are unionized is: | | State is right-to-work: | |
	Low (1)	High (2)	Yes (3)	No (4)
Intent-to-treat estimates				
Focal penalty $\geq c$	−0.065	−0.71	−0.13	−0.55
	(0.20)	(0.10)	(0.13)	(0.17)
Robust p-value	0.749	0.000	0.326	0.001
Observations	4,708	5,053	3,093	6,668
Left bandwidth	3,977	3,977	3,977	3,977
Right bandwidth	9,552	9,552	9,552	9,552
Mean dependent variable	2.21	2.35	1.87	2.47
Treatment-on-treated estimates				
Press release in focal inspection	−0.24	−3.96	−0.33	-4.27
	(0.72)	(1.33)	(0.33)	(1.75)
Robust p-value	0.738	0.003	0.308	0.015
Observations	4,708	5,053	3,093	6,668
Left bandwidth	3,977	3,977	3,977	3,977
Right bandwidth	9,552	9,552	9,552	9,552
Mean dependent variable	2.21	2.35	1.87	2.47

Notes: The table shows estimates of the effects of a press release about a *focal* facility on the subsequent compliance of peer facilities, defined as those within a 5 km radius and in the same sector. The dependent variable in all regressions is the number of violations detected in inspections of peer facilities, and the sample includes peer inspections in the 36 months following the date the focal penalty is issued (through December 2013). The running variable is the focal penalty, and the threshold is whether the focal penalty is higher than the press release cutoff c. The estimates in the bottom panel account for the fuzziness in adherence to the cutoff rule. Columns 1 and 2 split the sample by whether the percent of inspections between 2005 and 2008 in the peer group's focal facility's county that were of unionized workplaces is below or above the sample median. Columns 3 and 4 split the sample by whether a facility is located in a right-to-work state. The regressions are estimated with a linear polynomial in the running variable and include controls for indicators that a facility is in the construction sector and if an inspection was programmed. See Section IIIC for further details. Robust standard errors clustered by peer group.

One unintuitive implication of the TOT estimates in Table 6 is that, in high-union areas, press releases lead to a reduction in violations that is greater than 100 percent of the sample mean. This appears to arise because the "first stage" is smaller for the high-union samples in this table than for the overall sample.[43] This difference is plausibly due to sampling variation leading to small changes in the denominator of the IV estimate that cause fairly substantial changes in the overall estimate. When I recreate this table but expanding peer facilities to be within a 10 km radius instead of 5 km (online Appendix Table A.9), the magnitudes relative to the sample means for high-union areas are much more reasonable (primarily due to a larger first stage), and there is still zero estimated effect for low-union areas.

These results imply that press releases lead facilities to improve compliance by a greater amount when robust labor unions enable workers to make demands on their employers, consistent with the logic outlined above. But these results

[43] The ratios of the ITT and TOT estimates imply a first stage of $(-0.71/-3.97 =)$ 0.18 for high-unionization counties, and $(-0.55/-4.27 =)$ 0.13 for the non-Right-to-Work states, both of which smaller than the first stage for the overall sample.

also imply that press releases appear to have *no* effect on compliance when labor unions are relatively weak. This relationship is consistent with evidence in other settings regarding how information provision affects behavior. For example, numerous studies have found that the Toxic Release Inventory (TRI), an EPA policy that required facilities to disclose their release of toxic chemicals into the environment, led to a reduction in toxic releases (e.g., Doshi, Dowell, and Toffel 2013). Kalnins and Dowell (2017) finds that TRI led to much larger reductions for facilities located in high-income areas than for facilities located in low-income areas, which they interpret as low-income communities lacking the political capital to make demands on local facilities. In a very different setting, interventions to provide information about financial aid and college admissions to high-achieving, low-income students only seems to work when information is accompanied by individualized assistance or mentoring (Bettinger et al. 2012, Carrell and Sacerdote 2017). Thus, the findings reported above are consistent with a range of studies finding that information provision changes behavior only when those receiving the information have the resources, support, and power to use it.

There could be differences between areas with high and low union presence, other than worker bargaining power, that could lead to differential responses by employers to OSHA's press releases. While it is not feasible to isolate the role of bargaining power, I briefly consider one alternative channel by which union presence could moderate responsiveness. Another stakeholder that could punish firms for poor safety and health is consumers. Consumers' propensity to punish firms for this information would plausibly be lower if they have an unfavorable view of government regulation in the first place, and views on regulation could be correlated with union presence. I use responses to the 2014 Pew Survey on Political Polarization in the American Public (Dimock et al. 2014) to construct each state's percent of respondents who report an unfavorable view of regulation.[44] Among the states included in the main analysis, this percent is 50 percent and 48 percent in low and high *baseline unionization rate* counties, respectively, and 51 percent and 46 percent in RTW and non-RTW states, respectively.[45] Thus, there are slight differences in views on regulation based on union presence, but the differences are not large enough to plausibly cause large differences in consumer responses to OSHA's press releases.

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

C. *The Role of Media Take-Up*

OSHA posted its press releases on its web site. In the absence of any media coverage, it is unlikely that these releases would be noticed by many people, given that

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[44] Specifically, respondents were asked which statement more closely aligned with their views: "Government regulation of business usually does more harm than good" versus "Government regulation of business is necessary to protect the public interest," as well as "Stricter environmental laws and regulations cost too many jobs and hurt the economy" versus "Stricter environmental laws and regulations are worth the cost."
[45] Results are very similar for views on environmental regulation.

usual web traffic to OSHA's news releases page is likely light. As described above, OSHA leveraged relationships with media outlets to get their press releases covered in local newspapers and/or industry trade press. In this section, I analyze whether the deterrence effect of OSHA's press release was larger when newspapers were more likely to cover them.

I focus on media take-up by newspapers. I measure whether a newspaper covered a press release by whether a research assistant found a newspaper article about the press release on NewsLibrary.com. Among press releases written between August 2009 and December 2012, 66 percent were covered in at least one newspaper.

To test whether media take-up enhanced the deterrence effect of press releases, I must first identify conditions under which newspapers were more or less likely to cover a press release. I leverage the fact that smaller newspapers were more likely to cover press releases than larger newspapers.[46] As shown in Figure 1, *The Gainesville Times*, not *The New York Times*, covered a press release about a poultry processing facility in Gainesville, Georgia. The presence of robust local newspapers is highly variable across the United States (George and Waldfogel 2006, Napoli et al. 2017). To identify characteristics of local newspapers where facilities are located, I use data from the Alliance for Audited Media (AAM) (https://auditedmedia.com/data/media-intelligence-center/newspaper-analyzer), an organization that collects and verifies detailed data about the circulation of over 4,000 newspapers in the United States (representing roughly 70 percent of all newspapers).[47] From AAM, I obtain data on total circulation by zip code for each daily newspaper in operation in January 2016.

I define the *dominant newspaper* in each zip code as the daily newspaper with the highest circulation in that zip code. I then measure the size of a zip code's dominant newspaper in two ways: (i) the total *national* circulation of that newspaper, and (ii) the number of zip codes in which that newspaper has any circulation.

Online Appendix Figure A.12 illustrates that the likelihood that a press release is covered in a newspaper is decreasing in the size of the dominant newspaper where the facility subject to the release is located. The panels report a binned scatterplot in which the sample is all press releases written between August 2009 and November 2012. The variable on the vertical axis is an indicator if the press release was covered in at least one newspaper, and the variable on the horizontal axis is the size of the facility's dominant newspaper, measured by its total national circulation (panel A) and number of zip codes it serves (panel B). Both scatter-plots are residualized after controlling for the log of the penalty levied in the inspection, an indicator if the facility is in construction, and if the inspection was programmed. Using either measure, there is a clearly negative, roughly linear, relationship between the size of a facility's dominant newspaper and the likelihood that its press release gets covered by a newspaper. Setting other variables at their means, a press release about a facility whose dominant newspaper has national circulation of 16,800 (the tenth percentile) has a 76 percent chance of being covered in a newspaper, whereas one

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[46] Several Public relations officials at OSHA confirmed this relationship to me.
[47] The AAM Newspaper Analyzer database is a proprietary database that can be accessed for a fee. See https://auditedmedia.com/about/contact for more information.

about a facility whose dominant newspaper has circulation of 302,000 (the ninetieth percentile) has a 49 percent chance.[48]

I then test whether the general deterrence effect of press release is larger when the focal facility's dominant newspaper is more likely to cover it. The sample for this analysis is peer facilities located between 5 and 50 km away. I exclude those up to 5 km away (the sample for much of the preceding analyses) to focus on those further away and thus less likely to learn about press releases through a channel other than the news.[49] Using this sample, online Appendix Figure A.13 plots the number of violations in inspections of peer facilities following the date the focal penalty is issued, separately for peer groups in which the focal facility newspaper's size is below the sample median (and thus more likely to cover it) and above the sample median (less likely to cover it). The discontinuous drop in violations at the cutoff is present in all cases, but the drop appears twice as large when the focal facility's dominant newspaper is smaller. Online Appendix Table A.11 corroborates these figures with regression estimates. Press releases lead peer facilities to improve compliance by over twice as much when the focal facility's newspaper's size is below the sample median, relative to when size is above the sample median.

Collectively, these results indicate that media coverage amplified the deterrence effects of press releases. Media coverage could have this effect either through facilitating the dissemination of information that OSHA was writing press releases about violations, and it also could have raised the costs of press releases by bringing their attention to a larger set of stakeholders.

VI. Conclusion

Increasingly, customers, nongovernmental organizations, and other actors are using platforms to "shame" companies for actions perceived as wrongdoing. Such tactics are essentially a form of targeted information disclosure. Despite a large literature assessing how broadly applied information disclosure affects the behavior of the firms whose actions or attributes are disclosed, there is no empirical evidence to date of how targeted information disclosure, and how the *threat* of such disclosure, affects firms' behavior. This paper investigated a disclosure policy in which a government agency publicized employers found to be egregiously violating workplace safety and health regulations, finding that such publicity led to substantial reductions in regulatory noncompliance and workplace injuries.

Two caveats are important to note. First, it is not possible to say that OSHA's press releases were welfare-improving without an estimate of facilities' compliance costs. Such an estimate is beyond the scope of this paper. However, given the enormous social costs of work-related injuries and illnesses, the evidence that press releases led to fewer injuries, though imprecise, suggests the social benefit of press releases was large. Compliance costs would need to be very high for press releases

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[48] A potential concern with interpreting this correlation is that the characteristics of local news could be correlated with labor union presence, and it could be labor unions, not "small" newspapers, that drive media coverage of press releases. However, there is essentially zero correlation between the size of a facility's dominant newspaper and the two measures of union density considered in the prior section (*baseline unionization rate* and a state's RTW status).

[49] The implications of the results that follow are unchanged regardless of which vicinity I use.

to be welfare-diminishing. Second, this paper's findings were local to a threshold that publicized especially egregious violators. It is difficult to say whether OSHA should lower the threshold to write more press releases, since publicity about lesser violations might have smaller deterrence effects.

This paper's findings have several broad implications. First, they shed light on how workplace safety is provided in the labor market. While classical economic theory is ambivalent about the need for information disclosure in this domain, workers may lack full information about job hazards and firms' safety and health record. Such imperfect information, if present, leads to inefficiently high job hazards. The paper's analysis implies that at least part of the reason press releases elicit safety improvements is due to employers seeking to avoid costly responses from workers. These effects would be hard to explain if the labor market was characterized by perfect information about safety and health.

Second, this paper provides insight into the conditions under which information disclosure policies effectively incentivize agents to improve the quality of their attribute under scrutiny. OSHA's press releases only elicited safety and health improvements when workers' bargaining power, proxied by the presence of labor unions, was high. These results thus imply that information disclosure is only effective when the stakeholders to whom the information is targeted are in a position to leverage it.

Third, this paper has implications for regulatory agencies. Like other regulators, OSHA has traditionally relied on inspections and fines to enforce standards and promote safety. This paper showed that publicizing severe violations is a powerful complement to inspections (a complement since inspections are needed to identify who to publicize). Regulatory agencies, many of which have seen their resources plateau or decline in recent decades, could likely better achieve their objectives through devoting resources to making information accessible and salient to market participants, along with traditional enforcement efforts.

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