Public Safety Announcements: How the Threat of Media Coverage Affects Workplace Regulatory Compliance

Matthew S. Johnson *
Boston University

May 7, 2014

Abstract

This paper investigates how media coverage detailing workplaces' regulatory performance influences their subsequent compliance behavior in the domain of workplace safety. If there are reputational costs to poor workplace safety, such media coverage increases the marginal cost of non-compliance. However, estimating the causal effects of media in this setting is a challenge, both because using variation in realized media coverage may understate its effect (if the threat of media coverage is a deterrence on its own) and because media coverage may be correlated with other events influencing compliance (such as accidents). This paper overcomes such bias by utilizing a 2009 policy change by the Occupational Safety and Health Administration (OSHA) which induced quasi-random variation in media coverage (largely in local newspapers and industry trade journals) detailing safety and health violations found by OSHA during an inspection. Because this policy was not announced to the public, employers were likely only made aware of it when they observed a press release. I separately estimate the effects of press releases on the establishment about which it is written ("specific deterrence"), and on all establishments in its same "peer group" which are most likely to observe a press release about one another ("general deterrence"). Using a Regression Discontinuity (RD) design, I find that, conditional on a future inspection, a press release leads to significantly higher compliance in the "general" sense.

---

*Email msjohn@bu.edu I wish to thank Johannes Schmieder for invaluable guidance, as well as Kavan Kucko, Kevin Lang, Michael Lipsitz, Alison Morantz, Claudia Olivetti, Benjamin Ogden, David Weil, Russell Weinstein, and seminar participants at Boston University for helpful discussion. I would also like to thank the staff at OSHA's headquarters and each of OSHA's regional Office of Public Affairs for providing me with details on OSHA's press release policies. Jing Li provided excellent research assistance. All errors are mine and mine alone.

1 Introduction

Scholars and law enforcement alike have long debated whether publicizing the punishment of law breakers is an effective deterrence mechanism, ranging from the public torture of criminals prominent in the late 18th century (Foucault 1977) to disclosure of regulatory performance of modern day workplaces. Indeed, in regulatory domains ranging from environmental to financial, the use of information disclosure has become a prominent supplement to enforcement and legal pressures to encourage compliance (Delmas, Montes-Sancho and Shimshack 2010).

This paper investigates the effects of public information disclosure on regulatory compliance through the lens of media coverage of poor safety and health conditions at workplaces in the U.S. In theory, if employers and workers/consumers have asymmetric information about firms' workplace safety, then such information provision should correct for market failures and affect firms' compliance behavior with safety and health regulation *if workplace safety is sufficiently valued by the general public and in the labor market.* Furthermore, the extent to which firms' compliance decisions respond to such publicity can provide information on how workplace safety is valued by the general public and in the labor market. However, to date there is no empirical evidence on whether information provision can provide such benefits in this regulatory domain.

The Occupational Safety and Health Administration (OSHA) is the regulatory agency charged with setting and enforcing standards to ensure safe and healthful working conditions for U.S. employees. Its primary tool to enforce these standards is inspections of workplaces. If during the inspection the inspector finds the workplace out of compliance with any OSHA standards, she issues violations with a corresponding financial penalty.

Beginning in 2009, OSHA instituted a policy whereby if the penalties associated with an inspection exceeded a particular threshold, it would issue a press release describing the types of violations found, the penalties issued, and other relevant details from the inspection. These press releases were then sent to, and typically reported by, local media. The nature of this policy admits a regression discontinuity design to estimate the causal effect of the publicity arising from these press releases on future compliance behavior. If whether penalty amounts end up "just above" or "just below" the press release cutoff is essentially random (which I argue below it is), we can estimate the "treatment effect" of publicity by comparing the future compliance behavior of establishments with an inspection yielding a penalty just above the press release cutoff to that of establishments with a penalty just below the cutoff.

The paper first evaluates the effect of publicity about poor workplace safety on "specific deterrence," or how press releases affect the subsequent compliance behavior of the publicized (i.e. "focal") facility. There is no evidence that the publicity arising from a press release affects the probability the establishment is inspected in the future. Conditional on having a future inspection, though, the point estimates suggest establishments receiving a press release exhibit higher subsequent compliance (in the form of lower penalties and violations at future inspections) than the comparison group receiving no press release, though the estimates are mostly statistically insignificant and are sensitive to the bandwidth choice used to define the sample "just above" and "just below" the cutoff, no doubt due to the relatively small sample size.

The main part of the paper's analysis turns to the effects of publicity on "general deterrence," or whether press releases written about one facility generate spillover effects that affect compliance at other facilities. We first sort establishments into "peer groups," which we define as all establishments sharing the same zip code and 2 digit industry code. We compare the compliance behavior of peer groups during the months following an inspection of an establishment in that group with penalties just above the press release cutoff to the compliance of peer groups during the months following an inspection in that group with penalties just below the cutoff. Again, there is no evidence that "treated" peer groups have a lower probability of future inspection than "non-treated" peer groups. However, conditional on future inspection, a press release about an establishment leads to significantly higher compliance in that establishment's peer group: "treated" peer groups have on average roughly 40 percent less in total financial penalties and 33 percent fewer violations than "non treated" peer groups. Consistent with a theoretical model describing why publicity would affect an establishment's optimal compliance behavior, these effects are stronger when the reputational costs of poor workplace safety are higher, when the probability of OSHA inspection is higher, and they dissipate over time the longer an active press release policy has been in place.

Several checks are provided to support the validity of the research design and the causal interpretation of the results. Substantial support is provided that the identification assumptions required for the regression discontinuity design are met, and a placebo test and a few robustness checks provide evidence the results are not driven by a spurious relationship.

This paper's findings provide a novel contribution to the literature on deterrence (which is defined broadly as the extent to which actions or policies affect compliance behavior). While a large literature has consistently found strong "specific deterrence" effects from regulatory enforcement,[1] the evidence on general deterrence is less defini-

[1] See Weil (1996) for OSHA inspections and Hanna and Oliva (2010) for EPA inspections. According to Gray and Shimshack (2011), recent survey evidence shows that, at least for environmental performance, regulatory monitoring and enforcement remains the number one motivation for plants'

tive, partly because the literature is far sparser (Gray and Shadbegian 2007), no doubt due in part to the causality issues that inevitably arise when comparing the behavior of an entity with that of an appropriate peer group. One paper found that EPA inspections resulting in a fine result in a substantial reduction in the statewide violation rate, whereas inspections with no fine have no detectable effect (Shimshack and Ward 2005), which the authors interpret as evidence that general deterrence operates through regulator reputation. Thornton et al (2005) conducted a survey among firms in a particular industry and found that the number of examples of enforcement actions at other firms that respondents could recall was significantly and positively associated with whether the respondent reported having taken action to improve environmental performance, though they (rightly) caution the causality could run in the opposite direction.

Along with the concerns about causality, an unanswered question in the general deterrence literature is understanding the mechanism through which general deterrence actually occurs (Gray and Shimshack 2011). For example, if an inspection at one establishment truly has spillover effects onto the compliance behavior of other establishments in its "peer group," how does word actually spread about the enforcement activity in question? By utilizing an arguably random variation in media coverage of OSHA enforcement activities, this study provides a unique opportunity to evaluate whether publicity (and the associated "public shaming" that comes with it) is a mechanism behind these effects.

This paper also contributes to a second strand of literature assessing the effect of information disclosure on subsequent outcomes. After all, a press release or newspaper story is just a form of information disclosure. Some papers have looked at how investors value information disclosure on firms' regulatory performance: such as financial information (Greenstone, Oyer and Vissing-Jorgensen 2006), and climate change plans (Beatty and Shimshack 2010), . Doshi, Dowell and Toffel (2013) evaluate how information on firms' release of toxic chemicals affect their subsequent emissions. Lee (2013) investigates how To my knowledge, this is the first study estimating the effects of information disclosure on workplace safety and health performance.

Press releases about violations of safety and health regulation are a particularly compelling setting to evaluate how informal pressures such as publicity affect regulatory compliance decisions. Negative publicity about poor workplace safety can alienate firms' host community, result in increased scrutiny by other regulators, or have other adverse economic consequents. By estimating the extent to which such publicity affects establishments' compliance behavior, and how these effects vary with various local characteristics, these results can shed light on the extent to which the promotion of

---

environmental compliance decisions.

safe and healthy workplaces is valued by the general public and in the labor market.

The remainder of this paper is organized as follows. Section 2 provides theoretical motivation of how publicity arising from press releases may affect compliance. Section 3 provides institutional background of OSHA's press release policy and describes the data, and Section 4 develops the empirical methodology. Section 5 provides the results of the empirical analysis, and Section **??** describes robustness checks to test the validity of the results. Finally, Section 6 concludes and offers future directions I plan to take with this study.

2 A Simple Model of Regulatory Compliance

Suppose that each period, an establishment chooses its level of non-compliance nc with OSHA regulation to solve

$$\max_{nc} \quad E\big[\pi(nc) - p^I\left(pen(nc) + p^{pub}r(nc)\right)\big]$$

Where $\pi(\cdot)$ is profit with $\pi' > 0, \pi'' < 0$, p^I is the probability the establishment is inspected, p^{pub} is the probability its compliance from an inspection is revealed to the public, and $r(\cdot)$ is a "reputation cost" if nc is revealed to the public, with $r' > 0, r'' > 0$. Such costs could arise through consumer substitution, financial markets, higher wages demanded by new workers, or through contracts with upstream firms.

$pen(\cdot)$ is the financial penalties levied by OSHA for a given level of nc. The actual relation between penalties issued by OSHA during an inspection and noncompliance is stochastic:

$$pen(nc) = f(nc) + \nu$$

Where $f(\cdot)$ is the deterministic function OSHA uses to assign financial penalties based on the establishment's level of nc with $f' > 0, f'' \geq 0$ and $\nu \sim N(0, \sigma)$ is random error. It is plausible, both in theory and in practice, that penalties levied by OSHA have a stochastic element. For example, different OSHA inspectors may have varying degrees of "toughness," and not every OSHA standard is checked at every inspection, and very often standards have been refined or eliminated over time (Weil 1996). Furthermore, OSHA inspectors are told to take several factors into account when calculating penalties, including her assessment of the "gravity" of each violation and how many employees she determines are exposed to the hazard caused by the violation (OSHA 2009). Such factors are, to an extent, likely outside the establishment's control.

2.1 Effect of press releases on optimal nc

Suppose initially that $p^{pub} = 0$, but a policy is introduced in which a press release revealing an establishment's noncompliance is issued to the public if and only if $pen(nc) \geq c^*$. Since press releases get picked up by local media read by the general public, the policy change in question changes p^{pub} so that $p^{pub} = p^{pub}(nc)$.

Assume this change in policy is not announced publicly, but that establishments get a signal about it if they observe a press release when issued (whether about themselves or another establishment). Observing a press release changes an establishment's belief about p^{pub} so that $E(p^{pub}) = Pr(pub|nc)$. With perfect information about the policy, $Pr(pub|nc) = Pr(pen(nc) \geq c) = 1 - \Phi\left[\frac{1}{\sigma}\left(c^* - f(nc)\right)\right]$. With imperfect information, we can be agnostic about the form of $Pr(pub|nc)$ and assume only it is weakly increasing in nc.

The optimal choice of nc equates the marginal benefit of non-compliance with the marginal cost:

$$\pi'(nc^*) = p^I\left(f'(nc^*) + \frac{\partial Pr(pub|nc^*)}{\partial nc^*} r(nc^*) + Pr(pub|nc^*) r'(nc^*)\right) \tag{1}$$

If an establishment does *not* observe a press release, the second and third terms on the RHS are zero. Upon observing one, however, both turn positive. Since there are increasing reputational costs to noncompliance, the change to p^{pub} increases the marginal cost of noncompliance, leading to a downward shift of nc^*.

2.2 Comparison if firms have perfect control over $pen(nc)$ and perfect information

Suppose establishments have perfect information about the policy and, contrary to our assumption above, the penalty function is completely deterministic so that $pen(nc) = f(nc)$.

If establishments are heterogeneous in their slope of profitability with respect to nc, $\pi'(nc)$, then when $p^{pub} = 0$ we will observe a nondegenerate distribution of nc in the cross section. Denote $\hat{nc}$ as the level of nc such that $f(\hat{nc}) = c^*$. Then, for levels of nc just below $\hat{nc}$, the payoff is $\pi(\hat{nc}) - p^I f(\hat{nc})$, and the payoff for levels of nc just above $\hat{nc}$ is $\pi(\hat{nc}) - p^I(f(\hat{nc}) + r(\hat{nc}))$. Thus there is a discontinuity in the establishment's payoff at nc^*, and it follows we will observe a discontinuity in the mass of observed penalties at $f(nc^*)$.

If, on the other hand, firms have imperfect control over the penalty function (but still have perfect information), then the payoff function approaching $\hat{nc}$ from both the right and left is $\pi(\hat{nc}) - p^I(f(\hat{nc}) + Pr[\nu \geq c^* - f(\hat{nc})] r(\hat{nc}))$ which is continuous at nc^*, and thus the mass of observed penalties should be continuous at c^*. This distinction—whether establishments can manipulate whether they are just above or just below c^*—will be very important in the empirical section below.

3 Institutional Background and Data

3.1 Institutional Background: OSHA's Press Release Policy

OSHA's primary tool to enforce its health and safety standards is inspections of workplaces. During these inspections OSHA personnel survey a workplace's operations and assess its compliance with standards. Inspections can be in response to a complaint (by an employee or member of the public) or what is called a fatality or catastrophe (hereafter called "fat/cat"), or otherwise pre-planned, for example as part of an emphasis program. If, during the inspection, the inspector finds the workplace out of compliance with any standards, she issues violations with a corresponding financial penalty. The inspector classifies these violations into various categories (such as "serious," "willful," etc) each of which is associated with a particular range of potential penalties, and the inspector determines the actual penalty amount based on a variety of factors, such as the "gravity" of the violation or the number of employees exposed to the hazard caused by the violation. These penalties—which are typically issued about six months after an inspection is opened— are "not designed as punishment for violations...[but rather] to serve as an effective deterrent to violations" (OSHA 2009, Ch.6 pg 1).

For at least the past decade, OSHA has followed a policy whereby it would issue a press release detailing the violations found and penalties issued at an inspection if it deemed one appropriate. These press releases are written by staff at the one of OSHA's ten regional offices around the country in whose vicinity the inspection took place. The regional office then sends the press release to local media, which very often takes up the story. Figure 1 gives an example of such news coverage: an inspection of a scrap metal recycling center in Moline, Ill. was begun in April 2012, and the inspector issued $64,680 in penalties on July 3, 2012. OSHA immediately issued a press release about the inspection describing violations found during the inspection, and the same day a story appeared in the local newspaper, the Moline Dispatch.

Before 2009, the criteria used to determine whether to issue a press release was largely left to OSHA's ten regional offices. Generally, each region used a cutoff whereby it issued a press release if penalties issued at an inspection were above this cutoff, but other factors also caused a press release regardless of the cutoff, such as if the violations found were considered "novel." These criteria varied substantially across regions, as different regions used different cutoffs (some not having a cutoff at all). For example, Regions 1, 4 and 6 (covering New England and parts of the South, respectively) had used \$40,000, and some other regions used \$100,000 (and given how rare penalties over \$100,000 are issued, these regions effectively never issued press releases).

However, in May 2009 OSHA's national headquarters in Washington D.C. attempted to standardize these criteria across regions. As a result, a common cutoff of \$40,000 was instituted for Regions 1-5, 6, 9, and 10, and a cutoff of \$45,000 for Regions 5, 7 and 8.[2]. The policy had was intended to "shame" exceptionally high violators, and also to provide publicity about OSHA's enforcement activity. It is this change in policy that is utilized in the analysis below.

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 get a press release anyway if, for example, "novel" violations are found. Furthermore, some inspections above the cutoff will not get press releases if the inspector does not send the necessary information to the regional office in time to be relevant. Furthermore, OSHA's 10 regions varied in their adherence to the policy. The "fuzziness" of this design is incorporated into the empirical analysis.

3.2 Data

The primary data source used in the analysis is OSHA's Integrated Management Information System (IMIS), which contains detailed information on each of OSHA's inspections started between January 2001 and June 2012. Key variables included are the date the inspection is opened, the type of inspection (complaint, accident, programmed, etc), establishment characteristics (address, industry, number of employees present, whether the employees are represented by a union, etc). As for compliance measures, a detailed report of each violation found (if any) is included with the type and gravity of each violation, its corresponding financial penalty, and the date the violations are issued (typically a few months after the date the inspection is opened). Thus, factoring in these compliance measures, the data are at the establishment-inspection-violation level. For the sake of tractability, I collapse the data to the establishment-inspection level by summing each type of violation and all penalties levied at each inspection. Since many establishments are inspected multiple times throughout the sample period, but at varying rates, the data constitute an unbalanced panel.[3]

For most of the analysis, I restrict attention to inspections with penalties issued May 2009 and after, since this is when OSHA made its press release policy relatively uniform,

[2] At this time I do not know the reason for the difference in this cutoff across regions.

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

and with penalties issued before July 2011, to provide sufficient post-inspection data through June 2012 (when the dataset ends). The press release policy does not cover the 22 states with state-run OSHA offices, so inspections in these states are excluded.

Summary statistics are provided in Table 1 separately for the entire sample of inspections, and for the subset of inspections with penalties within \$10,000 of the press release cutoff for its corresponding region. Most inspections result in little to no penalties: out of the nearly 162,000 inspections during this period, the average inspection results in just over \$5,400 in penalties (but is highly skewed) and just 2 percent result in penalties above the corresponding press release cutoff. That the press release cutoff is at the 98th percentile of the penalty distribution supports the idea the policy was intended to expose the highest violators. The average inspection finds 2 violation though, as would be expected, the average for the subset around the press release cutoff is much higher.

Roughly 60 percent of inspections in the whole sample are programmed (i.e. planned ahead of time) and 21% are in response to a complaint or "fat/cat." However, the share of complaint or fat/cat inspections rises to 32% in the "near cutoff" sample, which makes sense as these types of inspections are more likely to result in violations. The average establishment in the "near cutoff" subsample is nearly three times as large (in terms of employment) and slightly more likely to be unionized than the average establishment in the whole sample.

Since many of these variables are so skewed to the right, for the remainder of the analysis I topcode count variables (violations, # inspections) at their respective 99th percentiles, and I take logs of continuous variables (penalties, # employees).

Table A.1 contains a tabulation of industry groups based on each establishment's 2-digit NAICS code. OSHA inspections are concentrated largely among construction and manufacturing establishments, both in the whole sample as well as the subsample around the press release cutoff.

To determine the extent to which the cutoff rule for issuing press releases was followed in practice, I linked the IMIS data to the set of archived press releases on OSHA's website to create an indicator for each inspection in IMIS equal to 1 if the inspection resulted in a press release. Figure 2 uses the results of this linking to illustrate the discontinuity at the cutoff. The figure excludes two regions (Regions 2 and 3, covering mostly New York and New Jersey) which, upon inspection, evidently did not adhere to the cutoff rule. These two regions are excluded from the remaining analysis as well. The Figure makes clear the probability an inspection results in a press release jumps significantly at the cutoff by 35-40 percentage points, highlighting the presence of the discontinuity but also the imperfect adherence to the policy by OSHA.

4 Empirical Strategy

4.1 Measuring Compliance (and the effect of a press release on it)

The true state of an establishment's OSHA compliance is unobservable. The IMIS data provide a measure of compliance conditional on being inspected based on the assessment of the inspector. Recall that such inspections are not a regular occurrence: they are often a response to an event (accident, complaint, etc) and in general the occurrence of an inspection itself is endogenous. Suppose we are interested in using the number of violations (V) of OSHA standards conditional on inspection as a metric of compliance. Then, in expectation, observed compliance is E(V| I), and the treatment effect of a press release on measured compliance for this sample can be written as

$$
\begin{aligned}
&E[V|PR=1]-E[V|PR=0]\\
&=E[V|I,PR=1]Pr(I|PR=1)-E[V|I,PR=0]Pr(I|Pr=0)\\
&=(Pr(I|PR=1)-Pr(I|PR=0)(E[V|I,PR=1])\\
&-(E[V|I,PR=1]-E[V|I,PR=0])*Pr(I|PR=0)\ nonumber && (2)\\
&\equiv\mu_p(E[V|I,PR=1])-\mu_c Pr(I|PR=0) && (3)
\end{aligned}
$$

The causal effect of a press release on compliance has two components: the first term of Equation 3 which gives the difference in the probability an inspection is initiated (sometimes called a "participation" effect), and the difference in mean violations conditional on inspection (sometimes called a "Conditional on Positive" (COP) effect).

While the COP effect may be our primary outcome of interest, the participation effect may also be relevant. For example since many inspections are in response to a complaint or accident, the publicity arising from a press release could affect the likelihood these events take place, thus affecting the probability an inspection is initiated.

Another reason we must check the "participation effect" is that it can affect the interpretation of the COP effect. If $\mu^p \neq 0$ (i.e. a press release changes the probability of a future inspection), then a press release could change the composition of who gets inspected. Such an effect is a form of selection bias on the COP effect and could bias the estimate of the causal effect of press releases on observed compliance (Angrist and Pischke 2009, page 65) . For this reason, the analysis that follows first estimates μ^p to evaluate whether this selection bias is actually a concern, and then we turn to estimating μ^c.

If reputational costs matter, then μ^c should unambiguously be negative, as described in the model in Section 2. However, the sign of μ^p is ex ante ambiguous. On the one hand, if the publicity from a press release causes an establishment to improve its true state of compliance, then a press release may reduce the likelihood of an accident, complaint, or other event leading to an OSHA inspection, in which case $\mu^p < 0$. On the other hand, it could be that the publicity from a press release empowers employees to complain or report events to OSHA when they otherwise would not, in which case $\mu^p > 0$. Due to this ambiguity, we will consider μ^c as our preferred measure for the effect of the press release on the true state of compliance.

4.2 RD Method

The institutional features of OSHA's policy of issuing press releases allows us —if certain identification assumptions are met—to estimate the causal effect of these press releases on associated outcomes using a regression discontinuity (RD) design. As discussed above, OSHA issues a press release about the violations found in an inspection if it results in penalties above some cutoff c.

Let the data generating process for some outcome Y for each establishment i be given by the following equation:

$$Y_i = \alpha + D_i\tau + f(P_i^{first} - c) + \epsilon_i \tag{4}$$

Where

$$\begin{aligned} P_i^{first} &= \text{penalty levied at first inspection of establishment i in the sample period} \\ D_i &= \mathbb{1}\{P_i^{first} \geq c\} \end{aligned}$$

and τ is the treatment effect of media coverage arising from a press release which, since we are controlling flexibly for financial penalty, is identified from variation on those just below and just above the cutoff c.[4] Recall $c = \$45,000$ for Regions 5,7,8 and $\$40,000$ for all other regions, and $f(\cdot)$ is a functional form to be determined. The sample period begins in May 2009 (when the policy change took place).

Using P_i^{first} as the assignment variable may seem overly restrictive, as a more flexible specification would allow "treatment" to be "turned on" at any inspection after the policy has been in place, as opposed to just the first. However, given the relatively short sample period considered (2009-2012), along with the relative infrequency with which individual establishments are inspected multiple times, this specification ensures we have the most possible amount of follow-up data to measure subsequent compliance for the analysis.

[4] Note that now I am not allowing for temporal effects from press releases (i.e. different effects from a press release issued 6 months ago, a year ago, 2 years ago, etc). Such effects will be considered in a later version.

As shown in Equation 3, the treatment effect of a press release on measured compliance can be decomposed into its effect on the probability of inspection μ^p and its effect on compliance conditional on inspection μ^c. To estimate μ^p, we let Y_i be an indicator if establishment i has at least one inspection after the date of its first inspection, and 0 otherwise.

To estimate the effects of a press release on compliance conditional on a future inspection (μ^c), we adopt Equation 4 but now using panel data:

$$Y_{it} = \alpha + D_{it}\tau + f(P_i^{first} - c) + \epsilon_{it}$$

Where Y_{it} is a measure of compliance (such as violations or penalties) for establishment i at an inspection opened at time t (where $t >$ date of first inspection), and $D_{it} = \mathbb{1}\{P_i^{first} \geq c\}$.

Note that this model does not include fixed effects for each establishment i. Unlike traditional panel data settings, including fixed effects is unnecessary for identification in an RD design (Lee and Lemeuix 2010). Instead, one can conduct the RD analysis as if the data were repeated cross sections, and cluster the standard errors by establishment to account for within-establishment correlation over time.

Various strategies exist to approximate the ex ante unknown functional form of $f(\cdot)$. However, Hahn et al (2001) show that local linear regression—that is, estimating a standard linear regression restricted to a narrow bandwidth around the cutoff point c—is a non-parametric way to obtain an unbiased estimate of the treatment effect τ. To implement the local linear regression, we will estimate Equation 4 (or its panel data analog) locally around the cutoff c specifying $f(\cdot)$ as a linear function but allowing for different slopes on each side of the penalty cutoff c. Results will be reported using varying bandwidths around the cutoff point.

4.3 Checking the Validity of RD Design

The RD design rests on the assumption that whether inspected establishments end up just above or just below the relevant cutoff for press releases is random. This assumption is valid if those involved have imperfect control over the exact penalty amount issued, and it can be jeopardized if there is room for manipulation.

As discussed in Section 2, it is very plausible that establishments have imperfect control over the penalty from an inspection. If there are reputational costs to publicity about poor safety, the disutility from penalties is discontinuous at the cutoff c, and if establishments know the value of c they would prefer to "bunch up" just below it. However, the stochastic element of the penalty function introduces an element of randomness from the establishment's perspective, which would limit its ability to control whether the penalty levied based on its level of noncompliance ends up "just below" or "just above" the cutoff.

On the other hand, there is entirely room for manipulation by the OSHA inspectors, since they issue violations and associated penalties themselves. For example, one may worry that if an inspector thinks a certain employer is poorly run and "deserves" bad publicity from a press release, she may "tip the employer over" the penalty cutoff, which would be a clear violation of the "imprecise control" assumption. OSHA officials have assured me that the method inspectors use to determine penalties is very mechanical, and that any notion of whether the employer is above or below the press release cutoff never enters into the equation. However, it is still assuring to determine whether this lack of manipulation appears true quantitatively.

One test of the validity is whether the density of penalties associated with inspections is smooth around the cutoff c. If there is a discontinuity in the aggregate density at the cutoff, then one may suspect either establishments or inspectors are manipulating penalty amounts to be on one side or the other. Figure 3 examines the density around the cutoff visually. Penalty amounts are normalized by the corresponding regional cutoff c^* and are placed in equally sized bins of \$5000 (with care to ensure all bins are on only one side of each cutoff), and frequencies are calculated for each bin. The sample is restricted to an establishment's *first* inspection during May 2009-June 2012.

While the density appears overall quite smooth, there appears to be a slight increase at the cutoff. However, this discontinuity could be for a completely unrelated reason: because penalty amounts are typically levied in round numbers, it is more likely total penalties from an inspection would sum to \$40,000 than, say \$39,999. For this argument to be valid, we should also expect a discontinuity in the density at other round numbers such as \$20,000, \$30,000, etc, and further we should expect a similar jump in the densities prior to 2009 (before the policy was uniformly in place). The plot provides suggestive evidence that this is indeed the case, as slight increases in the density also appear at penalty amounts \$10,000 below and above the press releases cutoffs. Table 2 implements the test proposed by McCrary (2008) to determine whether these changes are statistically significant. The table provides evidence of a jump in the density at \$30,000 and \$50,000 (which have no relation to press release considerations), as well as a jump at \$40,000 before 2009, suggesting any change in the density is unrelated to the press release policy.

A second test of the validity of the "imprecise control" assumption is whether relevant baseline characteristics are smooth around the cutoff. Table 3 shows the results of local linear regressions using various bandwidths around the cutoff c. The results show no evidence of a significant discontinuity in any covariates, providing further support that the assumptions needed for identification using the RD design are met.

5 The Deterrence Effects of OSHA Press Releases

5.1 Specific Deterrence

As described in Section 4, we first evaluate the effect of press releases on the probability of future inspection (μ^p) and then turn to their effect on compliance conditional on future inspection (μ^c). Columns (1)-(3) of Table 4 display results of the local linear regression investigating the treatment effect on probability of future inspection. These regressions estimate Equation 4 with Y_i equal to a dummy variable if an establishment has any future inspection following its first inspection in the sample period. Recall that, due to the current data limitations described above, all regressions that follow are an Intention to Treat (ITT) analysis, in which we compare outcomes of those "just above" and "just below" the press release cutoff, not knowing who actually gets a press release.

About 20 percent of the sample has some kind of future inspection following its first one in the sample period (regardless of bandwidth choice), and the probability that "treated" establishments have a future inspection appears indistinguishable from "non-treated" establishments both for any type of inspection but also for complaint or "fat/cat" inspections. This insignificant effect should give us confidence that press releases are not changing the composition of who subsequently gets inspected, and thus any estimates of μ^c will not be contaminated by any selection bias.

Turning to compliance conditional on inspection, Figure 4 displays the graphical results for our two compliance measures conditional on inspection: violation counts and (log) penalties. It is evident both of these measures are quite noisy. Part of the reason for this noise is no doubt the relatively small sample size: given the relative infrequency with which establishments are inspected more than once, the potential data for these graphs are limited. It is unclear from the graphs whether there is any significant change in either variable at the cutoff.

Columns (4)-(5) of Table 4 display the regression results for compliance conditional on future inspection, based on Equation 5. The point estimates suggest establishments receiving a press release exhibit higher subsequent compliance (in the form of lower penalties and violations at future inspections) than the comparison group receiving no press release, though the estimates are imprecise and are sensitive to the bandwidth choice. The imprecision of the estimates is likely due at least in part to the small sample size available for the regressions.

5.2 General Deterrence

We next turn to the general deterrence effects of OSHA press releases—that is, the extent to which a press release written about establishment i affects the subsequent compliance behavior of all establishments in i's peer group. Understanding general deterrence effects are particularly compelling in this setting. Since establishments were not alerted about the policy, they presumably only learned about it by observing media coverage following issued press releases. Since observing press releases provided information to establishments about a change to the *threat* of negative media coverage, estimating these general deterrence effects provides a unique opportunity to estimate the deterrence effects of this threat.

A first question is, if a press release is issued about one establishment, which other establishments are likely to observe it? I group establishments into "peer groups" if they share the same zip code and industry classification (as described in Table A.1). This specification of peer groups is natural for the following reasons. The grouping by zip code is natural given the regional distribution of the press releases: since OSHA sends its press releases to local media, the press release is more likely to be read by establishments operating nearby. The grouping by industry is also important since many press releases are covered in industry publications, but also because the set of standards OSHA checks for in an inspection varies widely by industry (Weil 1996) , .

The variation in standards across industries adds a layer of noise to our compliance outcomes, as this variation creates extra variance in levels of violations. To mitigate this issue, in some specifications I restrict attention to establishments in Construction (NAICS code = 23). Furthermore, inspectors are told to always check for a subset of 100 standards related to physical safety in inspections of Construction workplaces (Weil 2001) . Restricting to these standards can decrease noise further, and so for some specifications I restrict my compliance measures to violations of these top 100 standards.

To create the sample, I collapsed the data to the peer-month level to create a balanced panel with the zip-industry/month as the unit of analysis. In months in which no inspection was opened at any establishment in a peer group, I code the "# inspections opened" to zero. When at least one inspection is opened in a peer-month, I take the average over all penalties and violations issued at each one to create my focal "group-level" compliance measures. When no inspections are opened, no penalties or violations are issued, and so I leave these as missing.

For the estimation, I adopt a slightly different specification than that used for specific deterrence. I adapt Equation 4 the following way:

$$Y_{jt} = \alpha + D_{jt}\tau + f(P^{max}_{j(t-1)} - c) + \epsilon_{jt}$$

Where Y_{jt} is a measure of compliance in group j at time t, and

$$P^{max}_{j(t-1)} = \max_{i \in j} \{\text{penalty levied at an inspection of i (opened after May 2009) prior to time t}\}$$

$$D_{jt} = \mathbb{1}\{P^{max}_{j(t-1)} \geq c\}$$

In this framework, D_{jt} switches to 1 as soon as one establishment in group j has penalties issued exceeding the threshold c and remains at 1 for the remainder of the sample period.

As before, we decompose the deterrence effects into μ^p and μ^c. For the former we let Y_{jt} be a dummy if at least one inspection is opened among establishments in group j in month t, and for the latter we let Y_{jt} be the average of violations or penalties found in all inspections opened in group j in month t.

The results of the local linear regressions are shown in Table 5 using a bandwidth of $10,000 around the cutoff. Columns (1)-(3) provide different estimates of μ^p, and columns (4)-(5) for μ^c.

As in the specific deterrence case, Columns (1)-(3) show no evidence that press releases affect the probability of future inspection, though the point estimates are slightly negative. The lack of statistical significance suggests we need not worry about selection bias in the estimates of the effect conditional on future inspection, and robustness checks below provide further support.

Finally we turn to the compliance measures conditional on inspection (estimating μ^c). Again, here the dependent variable Y_{jt} is a compliance measure without recoding missing values to zero. The graphical representation of the results is shown in Figure 5, first for the whole sample and then for the subset of Construction establishments (and for the subset of top 100 standards). Each observation (a peer-month) is placed into a bin according to its P^{max} (again with equally sized bins of $2500), and average values of each dependent variable are calculated for each bin. While the graphs make evident these averages are fairly noisy, the graphs do seem to depict a downward shift in violations for both samples just to the right of the cutoff c. Columns (4) and (5) of Table 5 show the regression results. Establishments in peer groups having observed a press release have significantly fewer violations found than those not having observed one, for both the whole sample and the Construction subsample, and for both types of violations for the construction sample. Compared to the mean violation count, observing a press release leads to between 40-50%.

5.3 Where Are General Deterrence Effects the Strongest? VERY PRELIMINARY

This section describes some very preliminary attempts to estimate where the deterrence effects of media coverage are strongest. Regression results for various split samples are shown in Table 6.

Columns 2-3 investigate how the effects differ by peer groups' compliance prior to 2009. On the one hand, we might expect previously "high violators" to be more responsive to the media coverage, since they are more likely to have penalties exceeding c^* and thus face a higher probability of a press release. On the other hand, these "high violators" may face significantly higher cost of improving their compliance. The results (in Panel 2 of Table 6) show the "low violators" are more responsive, though the effects for the two groups are not statistically significantly different from each other.

Finally, if establishments learn quickly about the policy by observing press releases, the effects of press releases should dissipate over time the longer the policy has been in place. To see if effects dissipate over time, recall that Region 1 and 4 had been using the $40,000 press release cutoff for several years before 2009, whereas the other regions were using much higher cutoffs of $100,000 (or no cutoff at all). Thus, by the time of the policy change in 2009, establishments in Regions 1 and 4 would have been exposed to press releases for several years and likely have already formed precise beliefs about p^{pub}, relative to establishments in other regions. Thus, we split the sample into Regions 1 and 4 versus all others. The results in Columns 8-9 imply no detectable effect of press releases on compliance in regions in which an active press release policy had been in place several years before the 2009 policy change.

5.4 Placebo Tests

We run two placebo tests to validate the causal interpretation of the above results. Firstly, we re-run the regressions corresponding to Equation 5 but replacing c^* with a placebo meaningless cutoff. If we still find a significant τ, we would worry the above significant estimates are spurious. For this exercise, we replace c^* with $\$30,000$.

We run a second placebo test to ensure the results are not driven by some other factor that "switches on" at penalty amounts exceeding $40,000 or $45,000. Recall that while Regions 1, 4 and 6 had adopted the $40,000 cutoff several years before 2009, all other regions had been using either a significantly higher cutoff or none at all. The intuition we use is that, for regions that did not utilize a cutoff rule for issuing press releases before the intervention in 2009 (i.e. other than Regions 1, 4 and 6), we should see no relation between a zip-industry's compliance behavior 2009 and after, and whether any establishment in that zip-industry had a penalty exceeding the *post-2009* press release cutoff issued *before 2009.*

To implement this placebo test, we again adopt the specification from Equation 5 :

$$Y_{jt} = \alpha + D_{jt}\tau + f(\hat{P}^{max}_{j(t-1)} - c) + \epsilon_{jt}$$

Where the sample is, as before, restricted to May 2009-June 2012, but now

$$\hat{P}^{max}_{j(t-1)} = \max_{i \in j} \{\text{penalty levied at an inspection of i (issued } \textbf{Jan 2003-Jan 2009}) \}$$

$$D_{jt} = \mathbb{1}\{\hat{P}^{max}_{j(t-1)} \geq c\} \quad \text{(with c=}\textbf{post 2009}\text{ cutoff)}$$

The results are shown in Table 7. Panel 1 implements the first placebo test, The estimated τ using the meaningless cutoff of $\$30,000$ is small in magnitude and statistically indistinguishable from zero. Panels 2-4 implement the second placebo test for various subsets of regions. Panel 2 restricts to Regions other than 1, 4 and 6 (where no policy was in place pre-2009) and Panels 3 and 4 restrict to Regions 6 and 1, respectively. Panel 2 provides no evidence of a significant change in any compliance outcome when $\hat{P}^{max}$ exceeds the post-2009 PR cutoff. On the contrary, Panel 3 shows a significant negative effect for Region 6, where the cutoff *was* already in place, which we would expect. However, Panel 4shows no evidence of an effect in Regions 1 and 4 (where the policy was also in place).[5]

5.5 Robustness checks

Next, we do a couple robustness checks on the primary specification. The results are shown in Table 8. While the estimates for μ_p consistently show no evidence that press releases significantly affect the composition of who gets inspected, we may still be worried about the selection bias on the Conditional on Positive (COP) effects discussed in Section 4. Columns 1-4 of Table 8 provide checks to this end. Columns 1-3 use a DV equal to 1 if total violations in a peer-month exceed 0, 3 and 5, respectively. Using these DVs illuminates the distributional effects of press releases, but also can mitigate the COP selection bias (Angrist and Pischke 2009). Column 4 uses the same DV as the primary specification but restricts attention to violations found in programmed inspections (omitting complaint and fat/cat inspections). Since these inspections are routine and pre-planned by OSHA, there is smaller scope they are endogenous to the media coverage. All 4 columns support the main findings.

Columns (5) includes fixed effects for the 10 OSHA regional offices, and Columns

[5] One possible explanation for the non-effect found in Region 1 is that Region 1 had been using a \$40,000 threshold for issuing press releases since at least 1991, and it could be that any effects of press releases had dissipated by 2009.

(6) include industry-zip fixed effects in Equation 5. Recall that including fixed effects in an RD panel data setting is not necessary for identification (Lee and Lemeuix 2010) , and furthermore given that the inclusion of fixed effects utilizes only within-group variation, our identifyinng variation can only come from zip-industry groups whose initial P^{max} is just below the cutoff but which later switches to a value just above the cutoff during the sample period. Still, it is instructive to see if their inclusion drastically changes the results. In both of these cases, the inclusion of fixed effects does not change the magnitude of the coefficients but significance is lost when ln(penalties) is the DV.

Finally, columns (8) uses a smaller bandwidth around the cutoff of $5,000 (whereas the primary specification uses $10,000). The results, if anything, are stronger.

Overall, the robustness tests provide credibility to the main results though suggest their statistical significance is somewhat tenuous.

6 Conclusions and Future Directions

This paper investigated whether media coverage of noncompliance with workplace safety and health regulation has a causal effect on establishments' subsequent compliance behavior. It found evidence that a press release about one establishment led to a significant increase in compliance among other establishments in its same region and industry. Higher subsequent compliance by the "focal" establishment was also found, though the estimates were imprecise. Taken as a whole, the results suggest that there are significant reputational costs to poor workplace safety and health.

Future versions will include temporal models to estimate the dynamic effects of press releases on compliance over time.

Finally, in future work I plan to look at whether press releases affect outcomes other than compliance, in particular market outcomes.

References

[1] Angirst, Joshua D. and Jorn-Steffen Pischke. 2009. Mostly Harmless Econometrics. Princeton University Press.

[2] Barreca, Alan I., Jason M. Lindo, Glen R. Waddell. 2012. Heaping-Induced Bias in Regression Discontinuity Designs.

[3] Beatty, Timothy KM, Jay P. Shimshack. 2010. The Impact of Climate Change Information: New Evidence from the Stock Market. B.E. Journals of Economic Analysis and Policy (Contributions). 10:1 (2010): Article 105.

[4] Delmas, Magali, Maria J. Montes-Sancho, Jay P. Shimshack. 2010. Mandatory Information Disclosure in the Electricity Industry. Economic Inquiry 48(2) (April 2010): 483-498.

[5] Doshi, Anil R., Glen W. Dowell, Michael.W. Toffel. 2013. How Firms Respond to Mandatory Information Disclosure. Strategic Management Journal (forthcoming).

[6] Foucault, Michael. 1977. Discipline and Punish: The Birth of the Prison. Random House LLC.

[7] Gray, Wayne B. and Ronald J. Shadbegian. 2007. The Environmental Performance of Polluting Plants: A Spatial Analysis. JOURNAL OF REGIONAL SCIENCE, VOL. 47, NO. 1, 2007, pp. 6384

[8] Gray, Wayne B. and Jay P. Shimshack. 2011. The Effectivenss of Environmental Monitoring and Enforcement: A Review of the Empirical Evidience. Review of Environmental Economics and Policy, volume 5, issue 1, winter 2011, pp. 324.

[9] Greenstone, M., Oyer, P., Vissing-Jorgensen, A. 2006. Mandated disclosure, stock returns, and the 1964 Securities Acts Amendments. Quarterly Journal of Economics 121(2): 399-460.

[10] Hahn, Jinyong, Petra Todd and Wilbert Van der Klaaw. 2001. Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design, Econometrica, January, 69 (1), 201209.

[11] Hanna RN, Oliva P. 2010. The impact of inspections on plant-level air emissions. The B.E. Journal of Economic Analysis & Policy 10(1) (Contributions), Article 19.

[12] Lee, David, and Thomas Lemieux. 2010. Regression Discontinuity Designs in Economics. Journal of Economic Literature 48(2), June. 281-355.

[13] McCrary, Justin. 2008. Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test. Journal of Econometrics, Volume 142 , Issue 2, February.

[14] Occupational Safety and Health Administration, 2009. OSHA's Field Operation Manual (FOM). Directive Number CPL 02-00-148

[15] Shimshack, J., and M. B. Ward. 2005. Regulator reputation, enforcement, and environmental compliance. Journal of Environmental Economics and Management 50: 51940.

[16] Thornton, D., N. Gunningham, and R. Kagan. 2005. General deterrence and corporate environmental behavior. Law and Policy 27: 26288.

[17] Weil, D. 1996. If OSHA Is So Bad, Why Is Compliance So Good? RAND Journal of Economics, Vol. 27, No. 3 (Autumn), pp. 61840.

[18] Weil, D. 2001. Assessing OSHA performance: New evidence from the construction industry Journal of Policy Analysis and Management. Vol. 20, Iss. 4.

Table 1: Summary Statistics

	(1) All inspections	(2) Penalties within 10000 of cutoff
Compliance measures		
D(initial penalties $\geq$ PR threshold)	0.02	0.34
	(0.12)	(0.47)
total initial penalties	5406.48	37509.37
	(87726.28)	(8893.07)
total number of violations	1.96	9.09
	(3.31)	(6.31)
Type of Inspection		
programmed inspection	0.61	0.46
	(0.49)	(0.50)
complaint inspection	0.19	0.25
	(0.39)	(0.43)
fatality or catastrophe inspection	0.02	0.07
	(0.14)	(0.25)
other type of inspection	0.18	0.24
	(0.39)	(0.43)
Establishment characteristics		
Number of employees in establishment	110.77	280.78
	(1497.98)	(1837.12)
union present	0.11	0.15
	(0.32)	(0.36)
N	161924	620

The table gives the mean of each variable with standard deviations in parentheses below. Sample in Column (1) includes all inspections opened Jan 2009-July 2012. The subsample in Column (2) consists of all inspections which are a) an establishment's first inspection in the sample period, b) have penalties issued within the given bandwidth of the relevant press release cutoff, and c) have penalties issued before July 2011, and d) not in Regions 2 or 3.

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

Table 2: Results from McCrary test

penalty cutoff value	30000	40000	45000	50000
Region 5,7,8 post 2009	2.687	-0.129	-0.073	0.196
	(0.264)	(0.144)	(0.179)	(0.216)
Region 5,7,8 pre 2009	2.284	0.454	0.295	0.121
	(0.271)	(0.196)	(0.228)	(0.234)
Region NOT 5,7,8 post 2009	2.046	0.329	0.207	0.345
	(0.143)	(0.106)	(0.120)	(0.139)
Region NOT 5,7,8 pre 2009	2.432	0.605	-0.006	0.092
	(0.201)	(0.133)	(0.138)	(0.149)

Results from McCrary test to estimate the change in the density of penalties at various cutoff values and for various subsamples. Point estimates of the magnitude of the jump in the density are reported, with standard errors below in parentheses. Test performed using a bin size of 1000 and bandwidth of 20,000.

Table 3: Smoothness of covariates around press release cutoff

	(1) **First stage** PR dummy	(2) Prog-rammed Insp	(3) Comp-laint Insp	(4) Fat-Cat Insp	(5) ln (emp)	(6) # prior inspec-tions	(7) Industry average # prior insps	(8) # prior viol-ations	(9) Industry average # prior viols
Window around cutoff = 5,000									
D(initial penalties $\geq$ PR threshold)	0.32	0.23	-0.080	-0.027	0.38	-0.38	-0.027	-1.79	-0.14
	(0.081)**	(0.13)+	(0.12)	(0.054)	(0.36)	(0.29)	(0.053)	(0.95)+	(0.17)
N	281	281	281	281	281	281	281	281	281
N above	117	117	117	117	117	117	117	117	117
N below	164	164	164	164	164	164	164	164	164
Mean DV	0.18	0.45	0.24	0.071	3.54	0.66	0.56	2.06	1.07
Window around cutoff = 7,500									
D(initial penalties $\geq$ PR threshold)	0.23	0.065	0.021	-0.0099	0.43	-0.15	-0.00025	-0.32	0.011
	(0.073)**	(0.11)	(0.099)	(0.047)	(0.30)	(0.23)	(0.043)	(0.93)	(0.14)
N	440	440	440	440	440	440	440	440	440
N above	161	161	161	161	161	161	161	161	161
N below	279	279	279	279	279	279	279	279	279
Mean DV	0.17	0.48	0.23	0.068	3.58	0.62	0.56	1.91	1.09
Window around cutoff = 10,000									
D(initial penalties $\geq$ PR threshold)	0.22	-0.010	0.059	-0.0028	0.39	-0.043	-0.0023	-0.039	0.026
	(0.067)**	(0.090)	(0.090)	(0.043)	(0.28)	(0.21)	(0.036)	(0.92)	(0.12)
N	623	623	623	623	623	623	623	623	623
N above	210	210	210	210	210	210	210	210	210
N below	413	413	413	413	413	413	413	413	413
Mean DV	0.16	0.46	0.25	0.069	3.63	0.58	0.56	2.07	1.10

The sample is restricted to an establishment's first inspections with penalties issued between May 2009-June 2011.
The coefficients estimate the magnitude of the change in the dependent variable as measured during an inspection in the sample period with penalties issued at the relevant press release cutoff. Each coefficient is estimated in a separate RD regression which controls linearly for penalty at initial inspection with different slopes on each side of the cutoff. Robust standard errors in parentehses +P<.1, *P<.05, **P<.01.
For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000.
Count variables topcoded at 99th percentiles (# previous inspections, # previous total violations). Industry averages taken over 2-digit NAICS groups.

Table 4: Specific Deterrence regressions

	(1)	(2)	(3)	(4)	(5)
	probability of inspection (μ^p)			compliance conditional on inspection (μ^c)	
	Any insp	Any complaint	Any fatcat	ln(Initial Penalties)	# Total violations
Window around cutoff = 5,000					
D($P\_i^{first} \geq c$)	0.0013	-0.044	-0.0037	-0.27	-1.06
	(0.082)	(0.044)	(0.024)	(1.33)	(1.34)
obs	414	414	414	98	98
obs P(max)$\geq c$	183	183	183	39	39
obs P(max)$< c$	231	231	231	59	59
Mean DV	0.18	0.058	0.0072	7.00	1.52
Window around cutoff = 7,500					
D($P\_i^{first} \geq c$)	-0.036	-0.0096	0.011	-0.12	-0.31
	(0.070)	(0.037)	(0.019)	(1.05)	(1.12)
obs	626	626	626	137	137
obs P(max)$\geq c$	250	250	250	48	48
obs P(max)$< c$	376	376	376	89	89
Mean DV	0.18	0.059	0.0080	7.13	1.99
Window around cutoff = 10,000					
D($P\_i^{first} \geq c$)	-0.051	-0.040	0.0051	-0.87	-2.13
	(0.059)	(0.031)	(0.014)	(0.86)	(0.94)*
obs	879	879	879	197	197
obs P(max)$\geq c$	317	317	317	67	67
obs P(max)$< c$	562	562	562	130	130
Mean DV	0.18	0.055	0.0068	7.08	1.98

For columns (1)-(3), the sample includes all establishments whose first post-May 2009 inspection results in penalties within the coresponding bandwidth around the press release cutoff. The DVs are equal to 1 if the establishment is inspected at least one time in the months that follow. For the remaining columns, the sample is restricted to inspections which follow the first inspection in the sample period.

The coefficients estimate the magnitude of the change in the dependent variable when penalties from the first inspection in the sample period just exceed the press release cutoff. Each coefficient is estimated in a separate RD regression which controls linearly for penalty at initial inspection with different slopes on each side of the cutoff. Robust standard errors clustered by establishment +P<.1, *P<.05, **P<.01.

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

Count variables topcoded at 99th percentiles.

Table 5: General Deterrence regressions at zipcode-industry level

	(1)	(2)	(3)	(4)	(5)
	Pr(inspection) (μ^p)			conditional on inspection (μ^c)	
	Any insp	Any complaint	Any fatcat	Average # violations	Avg. # top 100 viols
Whole sample					
P(max)$\geq c$	-0.026	0.0033	0.00071	-0.98	
	(0.029)	(0.011)	(0.0025)	(0.32)**	
Obs	11162	11162	11162	1568	
obs P(max)$\geq c$	3748	3748	3748	532	
obs P(max)$< c$	7414	7414	7414	1036	
Mean DV	0.14	0.038	0.0035	2.39	
Construction only					
P(max)$\geq c$	-0.082	-0.0019	-0.0019	-0.84	-0.54
	(0.053)	(0.021)	(0.0042)	(0.36)*	(0.27)+
Obs	3746	3746	3746	748	748
obs P(max)$\geq c$	1371	1371	1371	261	261
obs P(max)$< c$	2375	2375	2375	487	487
Mean DV	0.20	0.037	0.0032	1.91	1.36

All regressions use a bandwidth around the cutoff of 10,000 and include calendar year fixed effects. The unit of analysis is the peer-group /month. The assignment variable (P(max)) is the largest penalty issued at any establishment in a peer group at any point prior to the current month (but after April 2009). The sample period is restricted to June 2009-June 2012.
The coefficients estimate the magnitude of the change in the dependent variable during the months after which P(max) just exceeds the relevant press release cutoff. Each coefficient is estimated in a separate RD regression which controls linearly for penalty at initial inspection with different slopes on each side of the cutoff. Robust standard errors clustered by zip-industry +P<.1, *P<.05, **P<.01.
For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000. Count variables topcoded at 99th percentiles.

Table 6: Split sample General Deterrence regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		E(penalty)		Construction		Employment		Region group	
	Whole sample	Below Median	Above Median	No	Yes	Below Median	Above Median	NOT 1 or 4	Regions 1 and 4
		(Median=3750.00)				(Median=28.00)			
DV = 1 if any inspection opened this month (coefficient estimates μ^p)									
P(max)$\geq c$	-0.026	-0.010	-0.046	0.025	-0.082	-0.054	0.018	-0.020	-0.044
	(0.029)	(0.046)	(0.037)	(0.031)	(0.053)	(0.045)	(0.036)	(0.034)	(0.057)
Obs	11162	4436	6726	7416	3746	4727	6435	8221	2941
obs P(max)$\geq c$	3748	1587	2161	2377	1371	1629	2119	2705	1043
obs P(max)$< c$	7414	2849	4565	5039	2375	3098	4316	5516	1898
Mean DV	0.14	0.17	0.12	0.11	0.20	0.18	0.11	0.14	0.14
DV = total violations, conditional on inspection (coefficient estimates μ^c)									
P(max)$\geq c$	-0.98	-1.15	-0.74	-1.78	-0.84	-0.84	-1.84	-1.30	-0.29
	(0.32)**	(0.48)*	(0.44)+	(0.52)**	(0.36)*	(0.37)*	(0.50)**	(0.38)**	(0.55)
Obs	1568	739	829	820	748	865	703	1161	407
obs P(max)$\geq c$	532	292	240	271	261	295	237	377	155
obs P(max)$< c$	1036	447	589	549	487	570	466	784	252
Mean DV	2.39	2.15	2.60	2.82	1.91	2.01	2.85	2.47	2.14

All regressions use a bandwidth of 10,000. The unit of analysis is the zip-industry/month. The assignment variable (P(max)) is the largest penalty issued at any establishment in a zip-industry at any point prior to the current month (but after April 2009). The sample period is restricted to June 2009-June 2012.

For Columns 2-3, E(penalty) is calculated for each peer group as the median (maximum monthly penalty) during the period 2005-2008.

The coefficients estimate the magnitude of the change in the dependent variable during the months after which P(max) just exceeds the relevant press release cutoff. Each coefficient is estimated in a separate RD regression which controls linearly for penalty at initial inspection with different slopes on each side of the cutoff. Robust standard errors clustered by zip-industry +P<.1, *P<.05, **P<.01.

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

Table 7: Placebo tests: General Deterrence regressions at zipcode-industry level using a) fake threshold of 30,000 and b) using pre 2009 penalties as focal penalty

	(1) ln(Initial Penalties)	(2) # Total violations
Using placebo cutoff of 30,000 (All Regions)		
P(max)$\geq 30k$	-0.054	0.096
	(0.094)	(0.17)
Obs	4356	4356
obs P(max)$\geq c$	1600	1600
obs P(max)$< c$	2756	2756
Mean DV	7.70	3.14
Using pre-2009 penalties for focal penalty - Regions NOT 1,4,6		
Pre 2009 P(max)$\geq c$	-0.12	-0.18
	(0.25)	(0.51)
Obs	1998	1998
obs P(max)$\geq c$	866	866
obs P(max)$< c$	1132	1132
Mean DV	7.63	3.41

All regressions use a bandwidth of 7,500. The unit of analysis is the zip-industry/month. The sample period is restricted to June 2009-June 2012. The assignment variable (P(max)) is the largest penalty issued at any establishment in a zip-industry at any point prior to April 2009).
The coefficients estimate the magnitude of the change in the dependent variable (measured over the whole sample period) when P(max) just exceeds the relevant press release cutoff. Each coefficient is estimated in a separate RD regression which controls linearly for penalty at initial inspection with different slopes on each side of the cutoff. Robust standard errors clustered by zip-industry +P<.1, *P<.05, **P<.01.
For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000. Count variables topcoded at 99th percentiles

Table 8: Robustness Checks: General Deterrence regressions at zipcode-industry level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Avg total viols				
	Total # viols > 0	Total # viols > 3	Total # viols > 5	Programmed only	Region FE	peer group FE	window =5000	
P(max)$\geq c$	-0.14	-0.15	-0.19	-0.95	-0.93	-0.99	-1.60	
	(0.054)*	(0.053)**	(0.050)**	(0.53)+	(0.32)**	(0.40)*	(0.54)**	
Obs	1568	1568	1568	1568	1568	1568	731	
obs P(max)$\geq c$	532	532	532	532	532	532	308	
obs P(max)$< c$	1036	1036	1036	1036	1036	1036	423	
Mean DV	0.74	0.34	0.21	1.84	2.39	2.39	2.38	

All regressions use a bandwidth of 10,000, unless otherwise noted. The Programmed Only specification excludes penalties and violations from any complaint or fat/cat inspections.
The unit of analysis is the zip-industry/month. The sample period is restricted to June 2009-June 2012. The assignment variable (P(max)) is the largest penalty issued at any establishment in a zip-industry at any point prior to the current month (but after April 2009).
The coefficients estimate the magnitude of the change in the dependent variable during the months after which P(max) just exceeds the relevant press release cutoff. Each coefficient is estimated in a separate RD regression which controls linearly for penalty at initial inspection with different slopes on each side of the cutoff. Robust standard errors clustered by zip-industry +P<.1, *P<.05, **P<.01.
For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000. Count variables topcoded at 99th percentiles.

Table 9: Comparing treatment effects for different peer groups (DV = Monthly average(violations) in peer group)

	(1)	(2)	(3)
	Geographic Group		
	zip code	county	
Industry Group			
Coarsened 2-digit NAICS	-0.98	-0.44	
	(0.32)**	(0.36)	
Obs	1568	2296	
unique groups	419	291	
All industries	-0.45	-0.18	
	(0.26)+	(0.38)	
Obs	2751	1624	
unique groups	446	181	

All regressions use a bandwidth of 7,500 and a DV=total violations (topcoded at 99th percentile). The unit of analysis is the relevant peer group/month. The sample period is restricted to June 2009-June 2012. The assignment variable (P(max)) is the largest penalty issued at any establishment in a zip-industry at any point prior to April 2009).
The coefficients estimate the magnitude of the change in the dependent variable (measured over the whole sample period) when P(max) just exceeds the relevant press release cutoff. Each coefficient is estimated in a separate RD regression which controls linearly for penalty at initial inspection with different slopes on each side of the cutoff. Robust standard errors clustered by zip-industry +P<.1, *P<.05, **P<.01.
For OSHA regions 5, 7 and 8, the relevant cutoff is 45,000, and for all others it is 40,000. Count variables topcoded at 99th percentiles

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

Quad-Cities Online
Dispatch·Argus

Weather News+ Obituaries Sports+ Life+ Entertainment+ Extras+ Class

SEARCH QCOnline.com Yellow Pages Google GO

OSHA fines Midland-Davis on safety issues; recycler to appea

Posted Online: July 03, 2013, 6:00 pm
Comment on this story | Print this story | Email this story
By Stephen Elliott, selliott@qconline.com

MOLINE -- A Moline recycling center has been fined $64,680 for 19 safety violations after an April inspection by the U.S. Department of Labor's Occupational Safety and Health Administration.

Midland Davis Corp. has 15 business days from receipt of the citations and notice of proposed penalties to contest them before the independent OSHA review commission.

Mitch Davis, the company's vice president, said Wednesday Midland will appeal the fines.

"We intend to question them (OSHA) on this," Mr. Davis said. "Everything they cited us for has already been fixed and taken care of. It was nothing life threatening or anything like this.

"An example is we have a magnet on a crane that picks up iron all day. We got fined because the tag from the manufacturer isn't on the magnet.

"There was nothing blatant. I wouldn't ask any of our people here to do anything I wouldn't do myself."

Tom Bielema, OSHA's area director in Peoria, said in a news release that "failing to conduct periodic inspections and remove damaged equipment creates an atmosphere in which workers are vulnerable to injury on the job.

Figure 2: Probability of a Press Release Jumps at the Cutoff by 35-40 percentage points

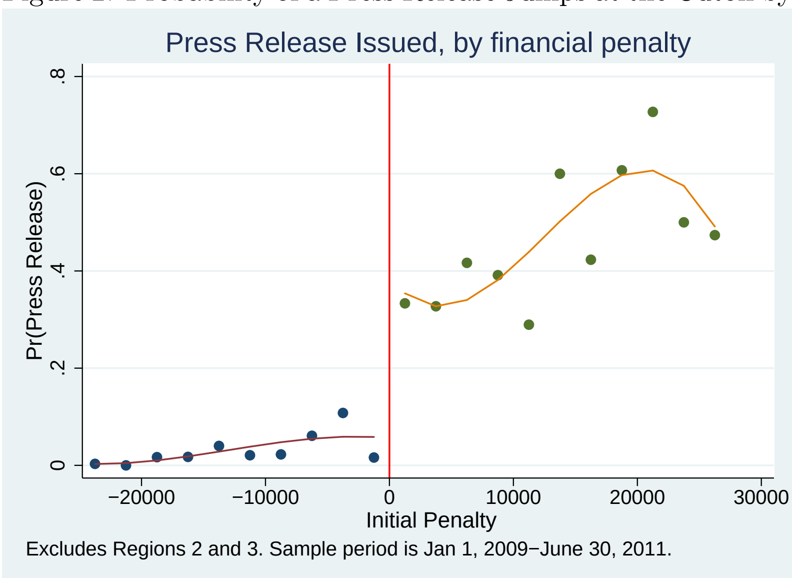


Figure 3: Frequency of Inspections Around Penalty Cutoffs for Press Release Issuance: May 2009-June 2012

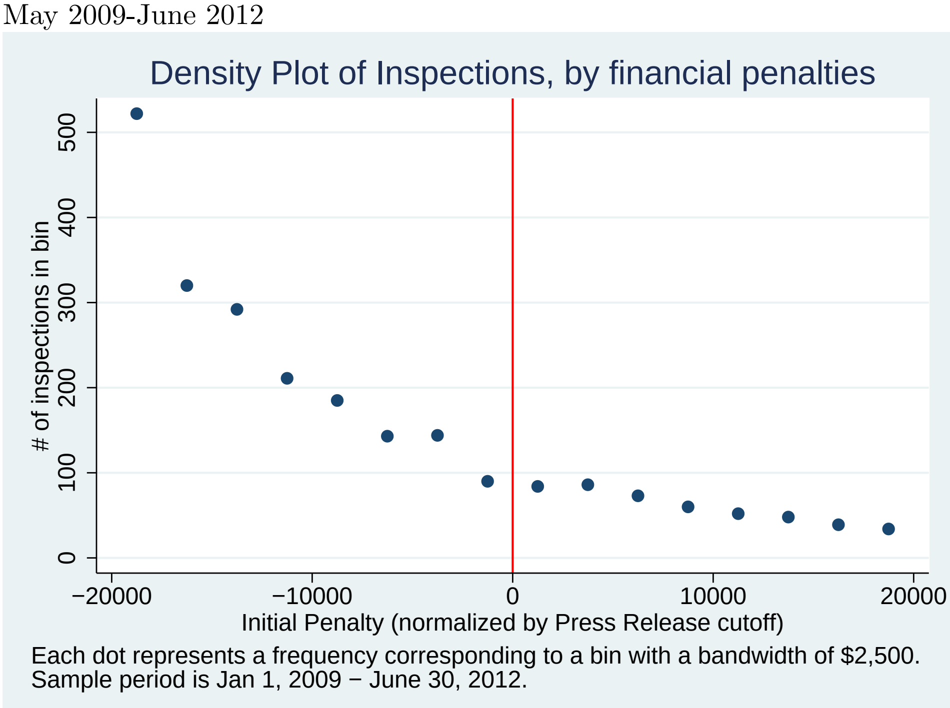


Figure 4: Specific Deterrence Plots: The Effect of a Press Release Written About Noncompliance of an Establishment on that Establishment's Subsequent Compliance (Conditional on Future Inspection) - May 2009-June 2012

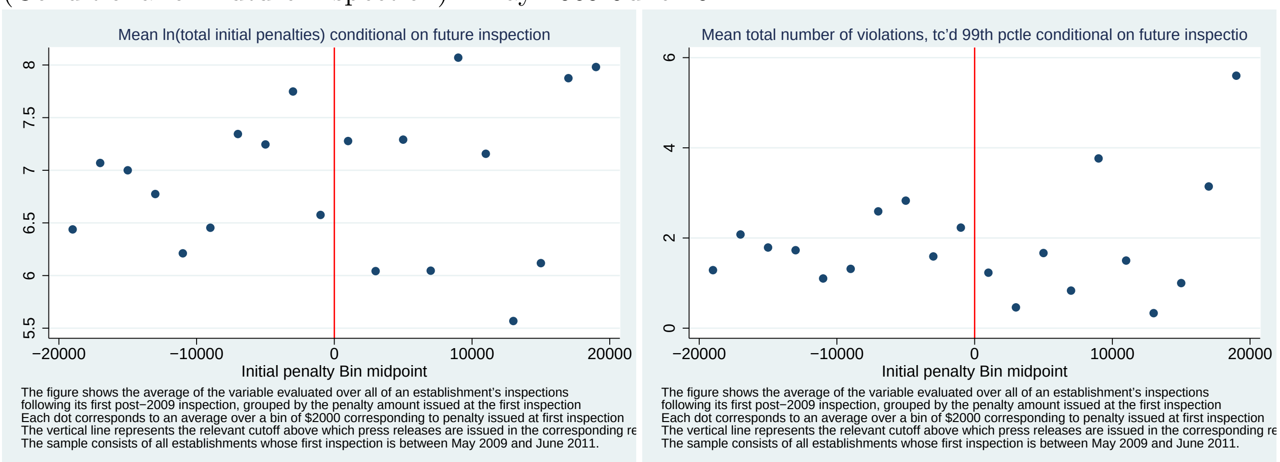
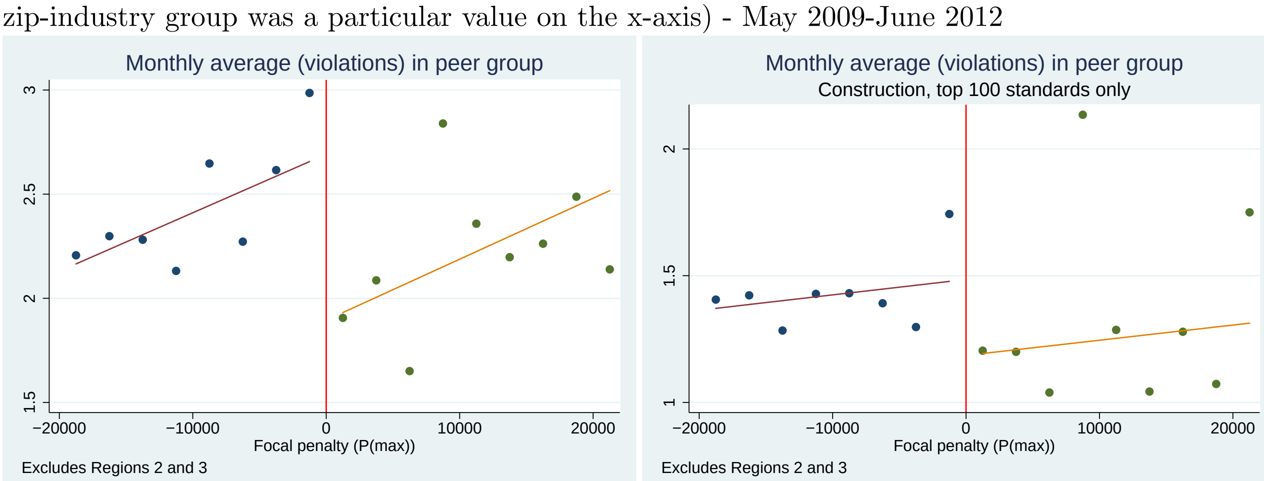


Figure 5: General Deterrence Plots: The Effect of a Press Release Written About Noncompliance of an Establishment on the Subsequent Compliance of all Establishments in its same Zip-Industry Group (Compliance measures summed over all inspections in a zip-industry-month, conditional on at least one inspection being opened in that month, then averaged over the months in which the maximum prior penalty in that zip-industry group was a particular value on the x-axis) - May 2009-June 2012



A Appendix Tables

Table A.1: Tabulation of Industry Groups (sample is all inspections opened November 2008-June 2012. The "within 10k of PR cutoff" subsample is restricted to an establishment's first inspection in the sample period and to inspections opened before July 2011.)

NAICS group	codes	all inspections Freq.	all inspections Percent	within 10k of PR cutoff Freq.	within 10k of PR cutoff Percent
Agriculture, Forestry, Fishing and Hunting	11	8,975	2.58	17	1.44
Mining	21	3,363	0.97	20	1.69
Utilities	22	3,744	1.08	15	1.27
Construction	23	164,603	47.32	418	35.3
Manufacturing	31-33	65,458	18.82	478	40.37
Wholesale Trade	42	10,940	3.14	49	4.14
Retail Trade	44-45	13,222	3.8	33	2.79
Transportation and Warehousing	48	11,357	3.26	33	2.79
Information	51	1,724	0.5	7	0.59
Finance, Insurance, Real Estate	52	3,036	0.87	5	0.42
Professional, Scientific and Technical Services	54	2,814	0.81	4	0.34
Management of Companies and Enterprises	55	50	0.01	0	0
Administrative, Support, Waste Management Services	56	13,006	3.74	33	2.79
Educational Services	61	4,849	1.39	10	0.84
Health Care and Social Assistance	62	9,795	2.82	9	0.76
Arts, Entertainment, and Recreation	71	2,274	0.65	9	0.76
Accommodation and Food Services	72	6,385	1.84	17	1.44
Other Services (except Public Administration)	81	8,869	2.55	25	2.11
Public Administratoin	92	13,339	3.83	2	0.17
Total		**347,873**	**100**	**1,184**	**100**