Does Publicizing Enforcement Deter Future Crime? Evidence from Press Releases on Workplace Safety Compliance

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Abstract

This paper asks whether publicizing firms' regulatory performance influences their subsequent compliance behavior in the domain of workplace safety. It utilizes a policy change by the Occupational Safety and Health Administration (OSHA) whereby it started issuing a press release detailing safety and health violations found during an inspection if that inspection resulted in financial penalties exceeding a certain threshold. Such a policy admits a regression discontinuity (RD) research design to estimate the causal effect of these press releases—and the publicity arising from them—on the subsequent compliance behavior of those workplaces affected by the press release. The results provide no evidence that publicity arising from press releases affects the probability of being inspected again in the future, either for the establishment about which it is written ("specific deterrence") or other establishments in its same zip code and industry ("general deterrence"). However, conditional on a future inspection, a press release leads to higher compliance in both the "specific" and "general" sense, though the specific deterrence estimates are imprecise and not statistically significant. The general deterrence effects are stronger when the reputational costs of poor workplace safety are more increasing, and they appear to dissipate over time the longer the policy has been in place. These results can potentially provide insight into the extent to which workplace safety and health are valued by the general public and in the labor market.

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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. 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 50 percent less in total financial penalties and 41 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' environmental compliance decisions.

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. 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 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 6 describes robustness checks to test the validity of the results. Finally, Section 7 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

$$\max_{nc} \quad \pi(nc) - p^I\Big(f(nc) + p^{pub} * r(nc)\Big)$$

Where $\pi(\cdot)$ is profit with $\pi' > 0, \pi'' < 0$, p^I is the establishment's belief of the probability of being inspected, p^{pub} is its belief of the probability the establishment's compliance behavior 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 be abstract, such as "public shaming," but could also be pecuniary such as higher wages demanded by new workers.

$f(\cdot)$ is the function OSHA uses to assign financial penalties based on the establishment's level of nc with $f' > 0, f'' \geq 0$. However, the actual relation between penalties and noncompliance is stochastic:

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

Where $\nu \sim N(0, \sigma)$ and is uncorrelated with nc. 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.

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

$$\pi'(nc) - p^I\Big(f'(nc) + p^{pub} r'(nc)\Big) = 0 \tag{1}$$

Suppose a policy is introduced in which a press release about an establishment's noncompliance is issued to the public if and only if $pen(nc) \geq c^*$. Such a policy could affect an establishment's optimal noncompliance via multiple channels. We consider these channels separately.

2.1 Effect of press releases on p^{pub}

Since press releases get picked up by local media read by the general public, press releases affect p^{pub}. If initially $p^{pub} = 0$, then using the Implicit Function Theorem the initial effect of the PR policy on nc via its effect on p^{pub} is

$$\begin{aligned} \left.\frac{\partial nc}{\partial p^{pub}}\right|_{p^{pub}=0} \times \Delta p^{pub} &= \frac{p^I r'(nc)}{\pi''(nc) - p^I f''} \times \Delta p^{pub} \\ &= \frac{r'(nc)}{\frac{\pi''(nc)}{p^I} - f''} \times \Delta p^{pub} \end{aligned} \tag{2}$$

Which is < 0 given our assumptions on derivatives above: since there are increasing reputational costs to noncompliance, the increase in p^{pub} increases the marginal cost of noncompliance. Note that if, contrary to our assumption, $r'(nc)$ equalled zero, the increase in p^{pub} has no effect on compliance.

One intuitive insight from Equation 2 is that the initial (when $p^{pub} = 0$) magnitude of $\frac{\partial nc}{\partial p^{pub}}$ is larger when $r'(\cdot)$ is larger—in other words when reuptational costs are more increasing in noncompliance. This is unambiguously true because, optimal nc is orthogonal to $r'(\cdot)$ when $p^{pub} = 0$. However, as soon as p^{pub} becomes positive, the repsonse on nc is increasing in $r'(\cdot)$.

In reality, the press release policy in question does not just change p^{pub} from 0 to a positive constant. Rather, it changes p^{pub} to a discontinuous function of nc so that $p^{pub} = Pr(f(nc) + \nu \geq c^*$. However, we assume establishments do not actually observe c^* once the policy is introduced, but rather form some expectation of it based on what they do observe each time a press release is issued. Thus, from the establishment's point of view:

$$p^{pub} = Pr[f(nc) + \nu \geq E_n(c)]$$

Where

$$E_n(c) = c^* + \psi/n$$

With $n = \#$ *press releases an establishment has observed*, and $\psi > 0$ is a random variable observed by all establishments the first time a press release is issued. ψ could be interpreted as the difference between the penalty amount revealed in the first press release OSHA releases and c^*. In the limit, establishments perfectly learn the value of c^*.

Using this notation,

$$
\begin{aligned}
p^{pub} &= Pr[f(nc) + \nu \geq c^* + \frac{\psi}{n}] \\
&= Pr[\nu \geq \frac{\psi}{n} + (c^* - f(nc))] \\
&= 1 - \Phi\Big[\frac{1}{\sigma}(\frac{\psi}{n} + (c^* - f(nc)))\Big]
\end{aligned}
$$

And thus the effect of the n^{th} press release on noncompliance is

$$\frac{\partial nc}{\partial p^{pub}} \times \Delta_n p^{pub}$$

With

$$
\Delta_n p^{pub} = \begin{cases} 1 - \Phi\Big[\frac{1}{\sigma}\big(\psi + (c^* - f(nc))\big)\Big] & \text{if } n = 1 \\ \Phi\Big[\frac{1}{\sigma}\big(\frac{\psi}{n-1} + (c^* - f(nc))\big)\Big] - \Phi\Big[\frac{1}{\sigma}\big(\frac{\psi}{n} + (c^* - f(nc))\big)\Big] & \text{if } n > 1 \end{cases}
$$

Which $\rightarrow 0$ as $n \rightarrow \infty$. In other words, the marginal effect of a press release (via its effect on p^{pub}) should converge to zero the longer the press release policy has been in place.

However, while the *marginal* effect of a press release should dissipate over time, there will be persistent effects of the policy on the *level* of noncompliance. In the long run (when $E_n(c) = c^*$), it can be shown that the optimal choice of nc satisfies

$$\pi'(nc) - p^I\Big(f'(nc)[1 + \phi(c^* - f(nc))r(nc)] + (1 - \Phi[c^* - f(nc)]r'(nc)\Big) = 0$$

In which optimal nc will be lower than that determined by Equation 1 (when evaluated at $p^{pub} = 0$).

2.2 Effect of press releases on p^I

Of course, if $r'(\cdot) = 0$ (there is no slope of reputational costs with respect to noncompliance), then the change in p^{pub} will have no effect on nc. However, another channel through which press releases issued by OSHA may affect establishments' level of nc is through their effect on p^I. Since press releases provide information on OSHA's enforcement activity, establishments that read a press release about a *different* establishment may update their priors on the probability of being inspected.

The initial effect (when $p^{pub} = 0$) of the change in p^I is

$$\left.\frac{\partial nc}{\partial p^I}\right|_{p^{pub}=0} \times \Delta p^I = \frac{f'(nc)}{\pi''(nc) - p^I f''} \times \Delta p^I \tag{3}$$

Which is < 0 given assumptions above. Note that, unlike $\frac{\partial nc}{\partial p^{pub}}$, the magnitude of $\frac{\partial nc}{\partial p^I}$ does not depend on $r'(\cdot)$.

The effect of the n^{th} press release is

$$\frac{\partial nc}{\partial p^I} \times \Delta_n p^I$$

We are agnostic about the functional form of $\Delta_n p^I$ but assume it $\rightarrow 0$ as $n \rightarrow \infty$. While the first few press releases may alert establishments to OSHA's enforcement activity, the "innovation" of this information likely decreases over time.

2.3 Summary

The total effect of the nth press release on noncompliance for establishment i is characterized by

$$\frac{\mathrm{d}nc_i}{\mathrm{d}PR_n} = \frac{\partial nc}{\partial p^{pub}} \times \Delta_n p^{pub} + \mathbb{1}\{\text{PR not about } i\}\frac{\partial nc}{\partial p^I} \times \Delta_n p^I$$

In summary, the introduction of the press release policy leads to a decrease in noncompliance, and this effect will be moderated by the following factors:

- If increasing reputational costs are actually present, initial effect on noncompliance will be larger when $r'(\cdot)$ is larger in magnitude

- The marginal effect of each press release should dissipate the longer the policy has been in place

- Despite the diminishing marginal effects, levels of observed noncompliance should be lower in the long run than if the policy had not been in place

- The "general deterrence" effects (subsequent compliance of establishments about which the press release is *not* written) of a press release are weakly greater than the "specific deterrence" effects (compliance of establishment about which the press release *is* written) due to the information about OSHA enforcement activity it includes.

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" or 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 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]. It is this change in policy that is utilized in the analysis below.

It is important to note that the probability a press release is issued does not jump exactly from 0 to 1 at the cutoff. As stated above, some inspections with penalties below the cutoff will 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. At this point, I do not have a precise estimate of the jump in the probability at the cutoff, though my initial estimate based on a random sampling of inspections around the cutoff suggests it jumps between 50 and 75 percentage points.[3] For now, I do not take into consideration the "fuzziness" of this design, and I thus in my analysis below I estimate an Intention to Treat (ITT) model.

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.[4]

For most of the analysis, I restrict attention to inspections with penalties issued May 2009 and after, since this is when OSHA made its press release policy relatively uniform, and with penalties issued before July 2011, to provide sufficient post-inspection data through June 2012 (when the data ends). Summary statistics are provided in Table 1

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

[3] In the future I plan to link the set of OSHA press releases to their corresponding inspection in OSHA's inspection database to be able to estimate this jump precisely.

[4] 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.

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 361,757 inspections during this period, the average inspection results in $3,088 in penalties (but is highly skewed) and just 1 percent result in penalties above the corresponding press release cutoff. The average inspection finds 1.5 violation though, as would be expected, the average for the subset around the press release cutoff increases to 9.

Almost two thirds 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 38% in the "near cutoff" sample, which makes sense as these types of inspections are more likely to result in violations. The average establishment in inspected 6.6 and 2.2 times between 2001 and 2012, and has 129 and 326 employees, in the entire and the "near cutoff" subsample, respectively.

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

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.[5] Then, in expectation, observed compliance is $E(\mathrm{V}|\ \mathrm{I})$, and the effect of a press release on measured compliance is

$$\frac{\partial E(\mathrm{V}|\ \mathrm{I})}{\partial PR} \equiv \mu^c$$

Suppose, however, we suspect a press release may also affect the probability an in-

[5] The below exercise is no different if V is measured in logs or levels.

spection is initiated: 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. Suppose we observe an establishment over many periods, during some of which an inspection happens and not during others. Then in any particular period, an establishment's expected observed compliance will be

$$E(\mathrm{V}) = p^I * E(\mathrm{V}|\ \mathrm{I}) + (1 - p^I) * 0$$

Where p^I is Pr(Inspection) during a given period. The effect of a press release on measured compliance is thus

$$\begin{aligned} \frac{\partial E(\mathrm{V})}{\partial PR} &= \frac{\partial p^I}{\partial PR} E(\mathrm{V}|\mathrm{I}) + p^I \frac{\partial E(\mathrm{V}|\ \mathrm{I})}{\partial PR} \\ &\equiv \mu^p E(\mathrm{V}|\mathrm{I}) + p^I \mu^c \\ &\equiv \mu \end{aligned} \tag{4}$$

A press release can affect measured compliance through two channels: its effect on probability of being inspected (μ^p), and its effect on compliance conditional on inspection (μ^c). The analysis below will consider these two channels separately.

If reputational costs matter, then μ^c should unambiguously be negative, as described in the model in Section 2. However, 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 and thus who gets their true state of compliance "revealed" from an inspection. Such an effect is a form of selection bias and could bias the estimate of the causal effect of press releases on observed compliance (Angrist and Pischke 2009, page 65) . For this reason, in the analysis that follows we first estimate μ^p to evaluate whether this selection bias is actually a concern, and then we turn to estimating μ^c.

Furthermore, 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.

Using this design to estimate the specific deterrence effects of OSHA press releases—that is, the extent to which a press release about poor safety at establishment i affects the subsequent compliance behavior of establishment i— we can model the data generating process for some outcome Y for each establishment i using the following equation:

$$Y_i = \alpha + D_i\tau + f(P_i^{first} - c) + \epsilon_i \tag{5}$$

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 average treatment effect we want to estimate,[6] $c = \$45,000$ 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.

As shown in Equation 4, 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 5 but now using panel data:

[6] 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). Later I plan to incorporate such effects.

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

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 5 (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 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 2 examines the density around the cutoff visually first for Regions 5, 7 and 8, and then for all other regions. Penalty amounts are placed in equally sized bins of \$1000 (with care to ensure all bins are on only one side of each cutoff), and frequencies are calculated for each bin. We restrict the sample to each establishment's *first* post-2009 inspection (for reasons explained below).

While the density appears overall quite smooth, there appears to be a slight increase at the cutoff for the "not 5,7, 8" group. Indeed, implementing the test proposed by McCrary (2008) confirms a statistically significant jump in the density at \$40,000 for the "not 5, 7, 8" group, but no significant change for the other group. 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). Table **??** shows the results and 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. Firstly, we can check this visually. We again group inspections into equally sized bins of \$1000. Now, rather than calculate frequencies of each bin, we calculate the average of each baseline characteristic for each bin. The plots are given for a few key baseline characteristics in Appendix B. The graphs do not suggest any evidence of a discontinuity in any baseline characteristics. While visually the characteristics appear smooth around the cutoff, we can also test this in a regression framework using Equation 5 with Y as a baseline characteristic such as *# previous inspections at the establishment*. Table 2 shows the results of local linear regressions using various bandwidths around the cutoff c (note that $c = \$45,000$ for Regions 5, 7 and 8, and $c = \$40,000$ for all other regions). 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 3 display results of the local linear regression investigating the treatment effect on probability of future inspection. These regressions estimate Equation 5 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 3 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 3 display the regression results for compliance conditional on future inspection, based on Equation 6. 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. A first question is what is the relevant peer group. 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, as the set of standards OSHA checks for in an inspection differs by industry (Weil 1996) , and furthermore it seems less likely that, for example, a construction contractor would pay much attention to a press release about a retail trade company.

To create the sample, I collapsed the data to the zip/industry-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 particular zip-industry, I code the "# inspections opened" to zero. When at least one inspection is opened in a zip-industry/month, I sum all penalties and vioations 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 it is unclear how to treat these missing observations. In some specifications, I keep them as missing, and in others I recode missing values to zero (akin to the strong assumption that if no inspection is opened, an establishment is in perfect compliance).

For the estimation, I adopt a slightly different specification than that used for specific deterrence. Whereas in specific deterrence the "focal" inspection was restricted to an establishment's first inspection beginning in May 2009, this restriction may be overly restrictive in this context. We adapt Equation 5 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

$P^{max}_{j(t-1)} = \max_{i \in j}$ {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 sum of violations or penalties found in all inspections opened in group j in month t. In specifications that recode compliance measures in months with no inspection as zero, τ is an estimate of μ, whereas in specifications leaving these observations as missing τ is an estimate of μ^c.

The results of the local linear regressions are shown in Table 7 for three different bandwidths. Columns (1)-(3) provide different estimates of μ^p, columns (4)-(5) for μ, and columns (6)-(7) for μ^c. Note that, unlike in the specific deterrence specification, we are able to get a direct estimate of μ due to how the sample here is constructed: by the construction of the balanced panel, here we see certain periods (months) with an inspection and others with none.

As in the specific deterrence case, Columns (1)-(3) show no evidence that press releases affect the probability of future inspection, suggesting we need not worry about selection bias in the estimates of the effect conditional on future inspection. Turning to the "complete" effect μ, we see an effect on measured compliance is negative, but generally statistically insignificant and sensitive to the bandwidth choice as well as the compliance measure chosen.

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 4. Each observation (a zip-industry/month) is placed into a bin according to its P^{max} (again with equally sized bins of \$1000), 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 both penalties and violations just to the right of the cutoff c, particularly for the "5,7,8" regions. Columns (6) and (7) of Table 7 show the regression results. A treatment effect is found that is robust to the bandwidth choice and which is highly significant. Based on the results using a bandwidth of \$7,500, the average zip-industry group with at least one previous inspection with penalties above the relevant cutoff has 50 percent less in total monthly penalties, conditional on having at least one inspection that month, than a group with no such inspections ($\beta = -.70, exp(\beta) - 1 = -0.503$). Relative to a mean level of penalties of \$1737 (Mean DV = 7.46, exp(7.46)=1737), this translates into \$876 less in penalties. In terms of violations, "treated" zip-industries have 1.21 fewer violations found, or 41.5% fewer relative to a mean of 2.91.

As a check on the interpretation of the coefficients, we compute an estimate of μ based on the estimates of μ^p in Column (1) and μ^c in Columns 6-7. Using Equation

4, we use the appropriate coefficients as estimates of μ^p and μ^c, and use the mean of the dependent variable in Column (1) as a proxy for p^I and the mean of the dependent variable in Columns (6) and (7) to proxy for E(penalties | I) and E(violations | I), respectively. For example, using penalties and using the bandwidth of $5,000, this calculates the expression $\mu = (0.0013) * (7.44) + (-0.71) * (0.13) = -0.083$, which is almost exactly the coefficient in the regression model in Column (4).

There are reasons we should interpret these estimates of μ with caution. For one, they are sensitive to the point estimate of μ^p in Column (1) even if the estimate is not statistically significant. Furthermore, given the ambiguity of how we should interpret any effect of a press release on Pr(Inspection) described above, μ^c is arguably a more unambiguous proxy for the treatment effect on "true state of compliance." For this reason, μ^c is our preferred estimate for the (local) average treatment effect of a press release on the establishment's true state of compliance.

5.3 Where Are General Deterrence Effects the Strongest?

Recall the model described in Section 2 suggests that, if reputational costs of noncompliance are nontrivial (i.e. $r'(\cdot) \neq 0$), the effects of the increased publicity from press releases on subsequent compliance behavior should be stronger when reputational costs have a larger slope with respect to noncompliance. Additionally, the effects of press releases should dissipate over time the longer the policy has been in place. This section explores whether such heterogenous effects are present.

We use two proxies to differentiate groups with high and low slope of reputational costs of noncompliance. Firstly, we would expect such costs are increasing in the competitiveness of the local labor market. If many establishments are competing with each other (for both final goods and for labor), bad publicity about poor safety may bring real consequences if customers and workers can choose to take their business elsewhere. On the other hand if, say, a meatpacking plant is "the only game in town," it may not care if the public finds out about its safety noncompliance. Thus, we proxy for the competitiveness of the local labor market by comparing whether a zip-industry is above or below the median *# establishments ever inspected by OSHA in a zip-industry.*

Furthermore, we should expect costs are more increasing for industries which are more localized (i.e. in which the operations of a firm are contained in a regional level). Establishments in such industries a) have a greater amount to lose from regional bad publicity, and b) are more likely to have senior management responsible for workplace safety located in the focal region. Construction is an example of an industry that is still very localized: while several national construction firms are very prominent, they almost exclusively contract out work to smaller regional contractors. Thus, we split the sample into Construction (NAICS 2-digit code = 23) and non-Construction zip-industry groups.

To see if effects dissipate over time, recall that Region 1 and 6 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 6 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 6 versus all others.

Regression results for various split samples are shown in Table 5. There do not appear to be any heterogenous effects on the probability of future inspection. Consistent with the model, though, the treatment effect on subsequent penalties and violations (conditional on inspection) is larger when reputational costs are more increasing using both the *# establishments* split and the construction/non-construction split. Finally, we see 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.

6 Robustness Checks

We take two approaches to check the robustness of the results regarding general deterrence: for now we omit robustness checks on the specific deterrence results due to the concerns regarding power discussed above.

Firstly, to ensure the results are not driven by some other factor that "switches on" at penalty amounts exceeding $40,000 or $45,000, we run a "placebo test" the following way. Recall that while Regions 1 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 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 6 :

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

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

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

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

The results are shown in Table 6. Panel 1 restricts to Regions other than 1 and 6 (where no policy was in place pre-2009) and Panels 2 and 3 restrict to Regions 6 and 1, respectively. Panel 1 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 2 shows a significant negative effect for Region 6, where the cutoff *was* already in place, which we would expect. However, Panel 3 shows no evidence of an effect in Region 1 (where the policy was also in place).[7].

As a second robustness test, we check for misspecification in the RD specification used in Table 7. Firstly, we implement the "donut" specification described in Barreca, Lindo and Waddell (2012) which accounts for the possibility of nonrandom "heaping" in penalties at particular values. Recall from earlier that penalty amounts at round numbers (30,000, 40,000, 50,000, etc) may be more likely given how penalty amounts are calculated, and one might worry that inspections with penalties "heaping" at these values might be nonrandom: i.e. maybe some inspectors like round numbers more than others. Following the advice of (Barreca et al 2012) , in Panel 1 of Table 7 we re-run the main specification but dropping groups whose P^{max} is 30,000, 40,000, 45,000 or 50,000. The results are essentially unchanged.

Secondly, we include industry-zip fixed effects in Equation 6. 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. Results are shown in Panel 2 of Table 7. The inclusion of fixed effects does not change the sign of the coefficients but signficance is mostly lost, which is not unexpected given the limited variation used in the regression.

[7] 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 dissipated over time

7 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.

As said in the text, since I currently do not have a precise estimate of the exact jump in the probability a press release is issued at the cutoff, all the analysis in this paper is conducted in an Intention to Treat (ITT) framework. Later on, I plan to link the set of OSHA press releases (archived on its website) to the inspection-level data in IMIS to get an estimate of this jump to incorporate into a "fuzzy RD" design.

Currently, the theoretical model in Section 2 does not distinguish between the specific and general deterrence effects of publicity, and I plan to incorporate this distinction later on. In addition, I plan to explore whether I can use this setting to get an estimate of how much workplace safety and health is valued by the general public and in the labor market.

Finally, in the future I want to look at whether press releases affect outcomes other than compliance, such injuries/fatalities. Data on occupational injuries and fatalities at a relatively fine geographic level are available from the Bureau of Labor Statistics.

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Table 1: Summary Statistics

	(1) All inspections	(2) Penalties within 10000 of cutoff
Compliance measures		
D(initial penalties $\geq$ PR threshold)	0.01	0.35
	(0.09)	(0.48)
total initial penalties	3088.31	37286.63
	(56354.72)	(8371.72)
total number of violations	1.46	8.77
	(2.64)	(6.33)
Type of Inspection		
programmed inspection	0.62	0.42
	(0.49)	(0.49)
complaint inspection	0.17	0.22
	(0.37)	(0.42)
fatality or catastrophe inspection	0.04	0.16
	(0.19)	(0.36)
other type of inspection	0.18	0.21
	(0.39)	(0.41)
Establishment characteristics		
Number of employees in establishment	129.49	325.90
	(1514.96)	(2368.49)
union present	0.16	0.19
	(0.36)	(0.39)
# previous inspections	4.33	0.85
	(74.89)	(2.12)
# inspections of establishment 2001-2012	6.57	2.24
	(92.79)	(2.70)
# total viols prior to this inspection	1.12	2.63
	(5.60)	(6.93)
N	361757	1184

The table gives the mean of each variable with standard deviations in parentheses below. Sample includes all inspections opened Nov 2008 and after. The subsample in the second column 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.
For OSHA regions 5, 7 and 8, the relevant press release cutoff is 45,000, and for all others it is 40,000.

Table 2: Smoothness of covariates around press release cutoff

	(1) Prog-rammed Insp	(2) Comp-laint Insp	(3) Fat-Cat Insp	(4) ln (emp)	(5) # prior inspec-tions	(6) Industry average # prior insps	(7) # prior viol-ations	(8) Industry average # prior viols
Window around cutoff = 5,000								
D(initial penalties $\geq$ PR threshold)	-0.078	-0.013	-0.073	0.45	0.19	-0.099	0.14	-0.11
	(0.080)	(0.082)	(0.063)	(0.25)+	(0.45)	(0.18)	(1.55)	(0.26)
N	562	562	562	562	562	562	562	562
N above	251	251	251	251	251	251	251	251
N below	311	311	311	311	311	311	311	311
Mean DV	0.75	0.22	0.16	3.57	0.93	0.85	2.49	1.47
Window around cutoff = 7,500								
D(initial penalties $\geq$ PR threshold)	-0.061	0.027	-0.029	0.43	0.41	-0.052	1.31	0.022
	(0.066)	(0.066)	(0.054)	(0.23)+	(0.40)	(0.14)	(1.37)	(0.22)
N	844	844	844	844	844	844	844	844
N above	335	335	335	335	335	335	335	335
N below	509	509	509	509	509	509	509	509
Mean DV	0.75	0.21	0.16	3.64	0.85	0.82	2.47	1.47
Window around cutoff = 10,000								
D(initial penalties $\geq$ PR threshold)	-0.089	0.061	0.016	0.27	0.29	-0.061	1.27	0.024
	(0.058)	(0.057)	(0.047)	(0.21)	(0.35)	(0.12)	(1.17)	(0.21)
N	1184	1184	1184	1184	1184	1184	1184	1184
N above	420	420	420	420	420	420	420	420
N below	764	764	764	764	764	764	764	764
Mean DV	0.75	0.22	0.16	3.65	0.85	0.83	2.59	1.49

The sample period is restricted to 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 3: 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	-0.020	-0.056	-0.95	-2.19
	(.)	(0.067)	(0.070)	(0.91)	(1.03)*
obs	562	562	562	157	157
obs P(max)$\geq c$	251	251	251	63	63
obs P(max)$< c$	311	311	311	94	94
Mean DV	1	0.85	0.82	6.91	1.57
Window around cutoff = 7,500					
D($P_i^{first} \geq c$)	0	0.025	0.012	-0.16	-0.47
	(.)	(0.057)	(0.059)	(0.74)	(0.78)
obs	844	844	844	219	219
obs P(max)$\geq c$	335	335	335	78	78
obs P(max)$< c$	509	509	509	141	141
Mean DV	1	0.86	0.83	7.01	1.98
Window around cutoff = 10,000					
D($P_i^{first} \geq c$)	0	0.024	0.051	-0.68	-1.44
	(.)	(0.048)	(0.051)	(0.60)	(0.65)*
obs	1184	1184	1184	318	318
obs P(max)$\geq c$	420	420	420	107	107
obs P(max)$< c$	764	764	764	211	211
Mean DV	1	0.86	0.83	6.99	1.95

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 4: General Deterrence regressions at zipcode-industry level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pr(inspection) (μ^p)			treat no inspection as zero (μ)		conditional on inspection (μ^c)	
	Any insp	Any complaint	Any fatcat	ln(Initial Penalties)	# Total violations	ln(Initial Penalties)	# Total violations
Window around cutoff = 5,000							
P(max)$\geq c$	0.0013	0.00070	0.00089	-0.086	-0.17	-0.71	-1.25
	(0.026)	(0.0092)	(0.0074)	(0.20)	(0.100)+	(0.28)*	(0.44)**
Obs	12906	12906	12906	12906	12906	1689	1689
obs P(max)$\geq c$	5299	5299	5299	5299	5299	754	754
obs P(max)$< c$	7607	7607	7607	7607	7607	935	935
Mean DV	0.13	0.032	0.011	0.97	0.38	7.44	2.88
μ estimate	.	.	.	.	.	**-0.083**	**-0.16**
Window around cutoff = 7,500							
P(max)$\geq c$	0.014	0.0057	0.0049	0.014	-0.12	-0.70	-1.21
	(0.022)	(0.0078)	(0.0062)	(0.17)	(0.085)	(0.24)**	(0.38)**
Obs	19022	19022	19022	19022	19022	2437	2437
obs P(max)$\geq c$	6825	6825	6825	6825	6825	948	948
obs P(max)$< c$	12197	12197	12197	12197	12197	1489	1489
Mean DV	0.13	0.032	0.0099	0.96	0.37	7.46	2.91
μ estimate	.	.	.	.	.	**0.018**	**-0.11**
Window around cutoff = 10,000							
P(max)$\geq c$	0.0088	0.00095	0.0077	-0.018	-0.12	-0.62	-1.05
	(0.019)	(0.0068)	(0.0050)	(0.15)	(0.077)	(0.20)**	(0.33)**
Obs	27133	27133	27133	27133	27133	3422	3422
obs P(max)$\geq c$	8774	8774	8774	8774	8774	1231	1231
obs P(max)$< c$	18359	18359	18359	18359	18359	2191	2191
Mean DV	0.13	0.033	0.0090	0.94	0.38	7.45	3.05
μ estimate	.	.	.	.	.	**-0.012**	**-0.11**

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.

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.

The μ estimate is calculated by computing the following expression: (coefficient in corresponding column)*(Mean DV in column 1) + (coefficient in column 1)*(Mean DV in corresponding column). See Equation 4 for explanation. 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: Split sample General Deterrence regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		# estabs in zip-ind		Construction		Pr(inspection)		Region group	
	Whole sample	Below Median	Above Median	No	Yes	Below Median	Above Median	NOT 1 or 6	Regions 1 and 6
		(Median= 11)				(Median= 0.69)			
DV = 1 if any inspection opened this month (coefficient estimates μ^p)									
P(max)$\geq c$	0.014	0.018	-0.0062	0.031	-0.018	-0.0098	0.030	0.030	-0.035
	(0.022)	(0.010)+	(0.032)	(0.022)	(0.043)	(0.029)	(0.032)	(0.026)	(0.039)
Obs	19022	9362	9660	12333	6689	9444	9578	14878	4144
obs P(max)$\geq c$	6825	3163	3662	4413	2412	3285	3540	5344	1481
obs P(max)$< c$	12197	6199	5998	7920	4277	6159	6038	9534	2663
Mean DV	0.13	0.038	0.22	0.095	0.19	0.11	0.15	0.14	0.10
DV = ln(initial penalty, missing converted to zero (coefficient estimates μ))									
P(max)$\geq c$	0.014	0.13	-0.23	0.21	-0.35	-0.098	0.073	0.11	-0.28
	(0.17)	(0.073)+	(0.24)	(0.16)	(0.33)	(0.24)	(0.23)	(0.19)	(0.32)
Obs	19022	9362	9660	12333	6689	9444	9578	14878	4144
obs P(max)$\geq c$	6825	3163	3662	4413	2412	3285	3540	5344	1481
obs P(max)$< c$	12197	6199	5998	7920	4277	6159	6038	9534	2663
Mean DV	0.96	0.27	1.62	0.68	1.46	0.81	1.10	1.00	0.79
DV = ln(initial penalty, conditional on inspection (coefficient estimates μ^c))									
P(max)$\geq c$	-0.70	0.26	-0.82	-0.16	-1.04	-0.19	-0.93	-0.74	-0.088
	(0.24)**	(0.54)	(0.26)**	(0.32)	(0.35)**	(0.40)	(0.28)**	(0.26)**	(0.66)
Obs	2437	356	2081	1166	1271	1001	1436	2017	420
obs P(max)$\geq c$	948	132	816	471	477	373	575	780	168
obs P(max)$< c$	1489	224	1265	695	794	628	861	1237	252
Mean DV	7.46	7.08	7.52	7.20	7.69	7.63	7.33	7.38	7.84

All regressions use a bandwidth of 7,500. 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.
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: Placebo test: General Deterrence regressions at zipcode-industry level using pre 2009 penalties as focal penalty

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pr(inspection) (μ^p)			treat no inspection as zero (μ)		conditional on inspection (μ^c)	
	Any insp	Any complaint	Any fatcat	ln(Initial Penalties)	# Total violations	ln(Initial Penalties)	# Total violations
Regions other than 1 or 6 (where PR policy was not in place)							
Pre 2009 P(max)$\geq c$	-0.0082	-0.0071	-0.0096	-0.035	0.016	0.17	0.33
	(0.017)	(0.0061)	(0.0044)*	(0.13)	(0.077)	(0.18)	(0.32)
Obs	44655	44655	44655	44655	44655	5701	5701
obs P(max)$\geq c$	17550	17550	17550	17550	17550	2429	2429
obs P(max)$< c$	27105	27105	27105	27105	27105	3272	3272
Mean DV	0.13	0.033	0.017	0.92	0.40	7.17	3.16
Region 6 (where the PR policy was in place)							
Pre 2009 P(max)$\geq c$	0.0055	-0.0090	0.0072	-0.037	-0.12	-0.86	-1.68
	(0.038)	(0.0067)	(0.0058)	(0.29)	(0.13)	(0.45)+	(0.81)*
Obs	4446	4446	4446	4446	4446	429	429
obs P(max)$\geq c$	1287	1287	1287	1287	1287	125	125
obs P(max)$< c$	3159	3159	3159	3159	3159	304	304
Mean DV	0.096	0.0092	0.0045	0.73	0.27	7.60	2.76
Region 1 (where the PR policy was in place)							
Pre 2009 P(max)$\geq c$	-0.051	0.0040	-0.00071	-0.37	-0.097	0.23	0.93
	(0.064)	(0.025)	(0.0025)	(0.51)	(0.33)	(0.66)	(1.85)
Obs	3588	3588	3588	3588	3588	363	363
obs P(max)$\geq c$	1833	1833	1833	1833	1833	172	172
obs P(max)$< c$	1755	1755	1755	1755	1755	191	191
Mean DV	0.10	0.028	0.0017	0.76	0.35	7.55	3.48

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 7: Robustness Checks: General Deterrence regressions at zipcode-industry level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pr(inspection) (μ^p)			treat no inspection as zero (μ)		conditional on inspection (μ^c)	
	Any insp	Any complaint	Any fatcat	ln(Initial Penalties)	# Total violations	ln(Initial Penalties)	# Total violations
Donut specification							
P(max)$\geq c$	0.028	0.010	0.0078	0.10	-0.086	-0.74	-1.20
	(0.024)	(0.0086)	(0.0072)	(0.18)	(0.092)	(0.25)**	(0.40)**
Obs	17071	17071	17071	17071	17071	2228	2228
obs P(max)$\geq c$	5960	5960	5960	5960	5960	849	849
obs P(max)$< c$	11111	11111	11111	11111	11111	1379	1379
Mean DV	0.13	0.033	0.010	0.97	0.38	7.47	2.93
with industry-zip fixed effects							
P(max)$\geq c$	0.028	0.0067	0.0037	0.18	-0.032	-0.37	-1.10
	(0.020)	(0.0074)	(0.0054)	(0.16)	(0.10)	(0.23)	(0.38)**
Obs	19022	19022	19022	19022	19022	2437	2437
obs P(max)$\geq c$	6825	6825	6825	6825	6825	948	948
obs P(max)$< c$	12197	12197	12197	12197	12197	1489	1489
Mean DV	0.13	0.032	0.0099	0.96	0.37	7.46	2.91

The donut specification drops observations whose focal penalty is 30000, 35000, 40000, 45000, or 50000.
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 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.

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

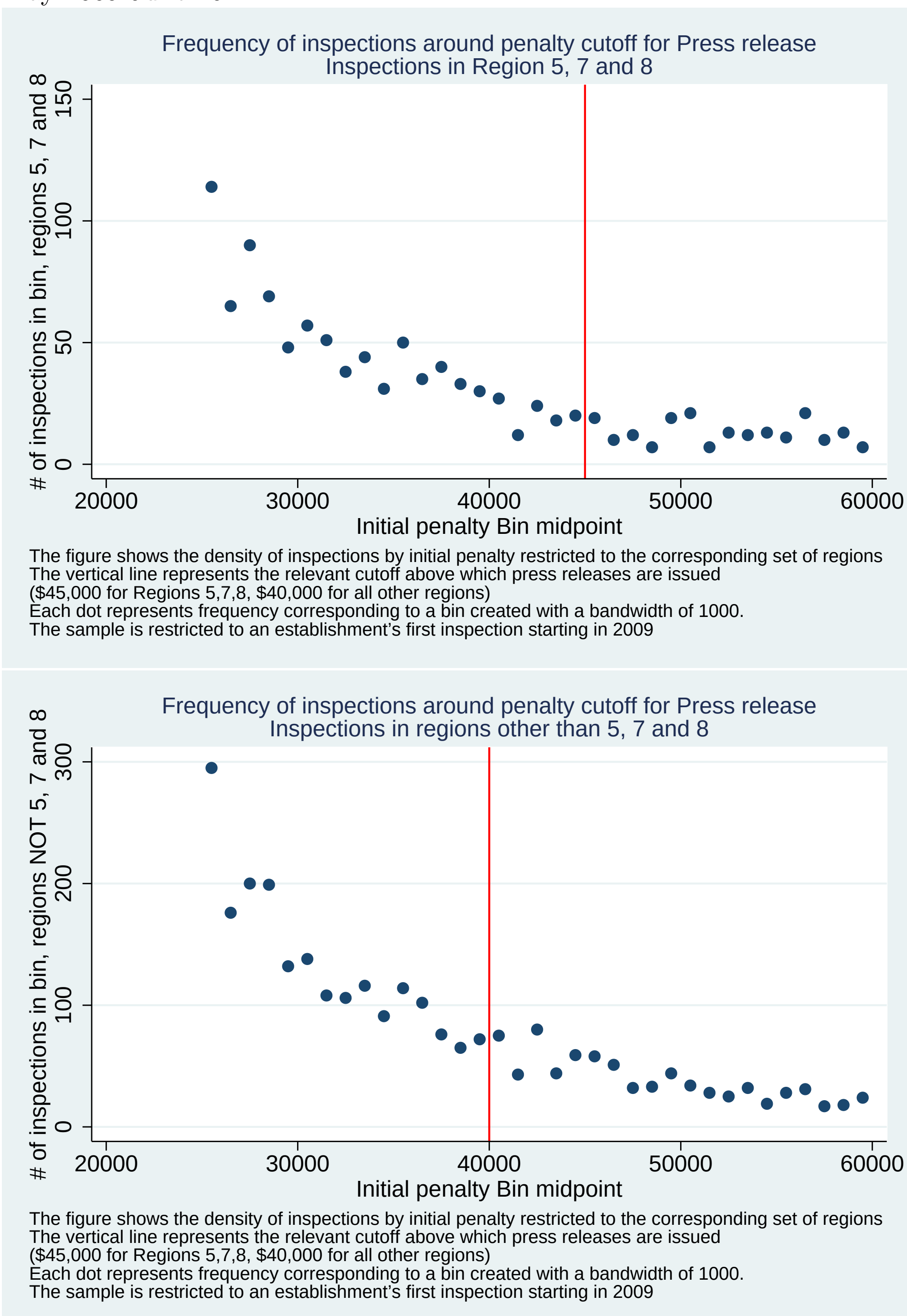

Figure 3: Specific Deterrence Plots: The Effect of a Press Release Written About Noncompliance of an Establishment on that Establishment's Subsequent Compliance (Conditional on Future Inspection) - May 2009-June 2012

Figure 4: 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.)

		all inspections		within 10k of PR cutoff	
NAICS group	codes	Freq.	Percent	Freq.	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**

B Graphs depicting smoothness of baseline covariates around cutoffs

Figure B.1: log(employment)

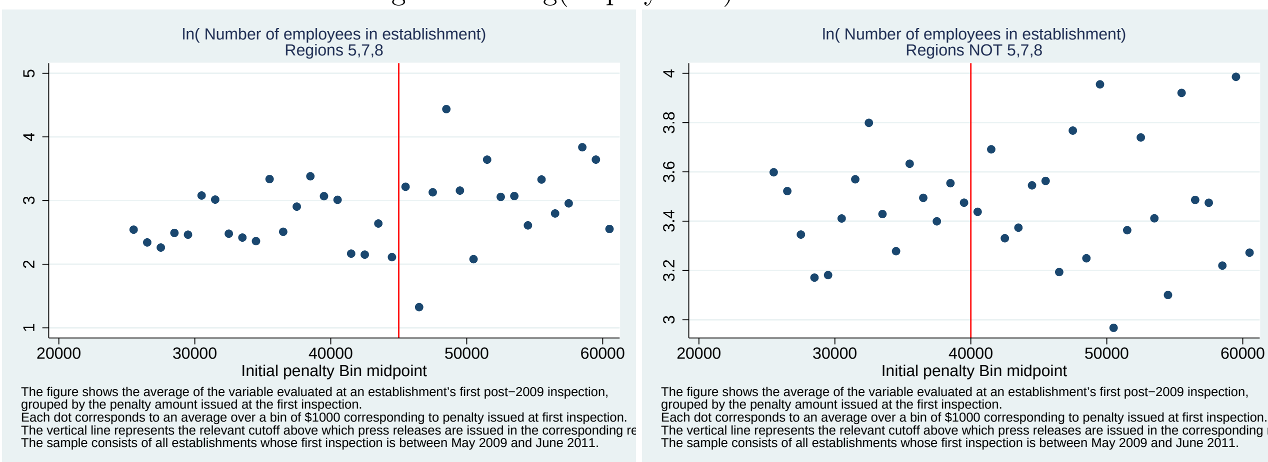

Figure B.2: Previous Violations

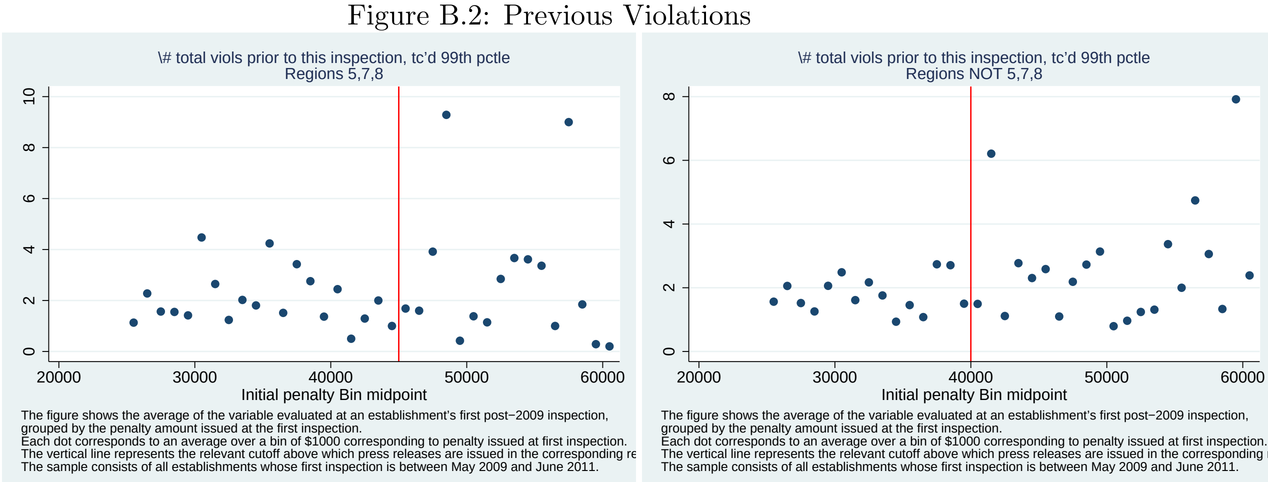

Figure B.3: # previous inspections