The Effect of Workplace Inspections on Worker Safety

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Abstract

The Occupation Safety and Health Administration enforces safety regulations through workplace inspections. To identify the effect of inspections on worker safety, this study exploits quasi-experimental variation in inspections due to OSHA's Site Specific Targeting plan. The SST plan used establishment-level data on accidents and injuries to target establishments for inspection. The primary inspection list consisted of establishments with case rates exceeding a cutoff. This cutoff generated a discontinuous increase in inspections, which is used to identify the effect of inspections on worker safety. Using the fuzzy regression discontinuity design and local linear regression, the estimated effect of an inspection on cases involving days away from work, job restrictions, and job transfers is -1.607 per 100 full-time equivalent workers. The effect is most pronounced among manufacturing establishments below the 90th percentile of the case-rate distribution.

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I. Introduction

In 2016, the Occupational Safety and Health Administration conducted approximately 32 thousand workplace inspections, with a fiscal year budget of $553 million. While the goal of inspections is to enforce federal regulations of safety and health, identifying the effect of inspections on worker safety presents a challenge. One reason is that OSHA specifically targets high-risk industries and establishments for inspection (Kniesner and Leeth 2014). This generates a negative correlation between inspections and worker safety, which confounds any positive, causal effect of the former on the latter.

To identify the effect of inspections on worker safety, this study exploits quasi-experimental variation in inspections generated by OSHA's Site Specific Targeting (SST) plan. The plan, implemented in 1999, targeted establishments with high rates of accidents and injuries for inspection. The data to implement the SST plan come from the OSHA Data Initiative (ODI), which collected establishment-level data on accidents and injuries directly from employers. Using these data, the SST plan prioritized establishments for inspection using case-rate cutoffs. One set of cutoffs defined the primary inspection list, and a lower set of cutoffs defined the secondary inspection list. An even lower cutoff was used to notify employers by mail that their case rates were high relative to the national average. The cutoff for the primary inspection list is associated with a discontinuous increase in inspections, which is used to identify the effect of inspections on worker safety.

To exploit the discontinuity in inspection for identification, the empirical strategy follows the fuzzy regression discontinuity (FRD) design (Hahn, Todd, and Van der Klaauw 2001; Imbens and Lemieux 2008). The FRD design requires that establishments are similar just above and below the cutoff, and that the discontinuity in inspection is due to compliers, defined as establishments

that were inspected just above the cutoff, but would have not have been inspected in the absence of the SST plan. These assumptions are supported by examining establishment characteristics and inspections unrelated to the SST plan. If the identification assumptions are valid, the FRD estimand measures the average effect of an inspection among compliers at the cutoff. The study also examines whether the effect of an inspection varies across the distribution of worker safety outcomes. This will determine whether the average effect of an inspection among compliers, estimated using the FRD estimand, varies between establishments with high versus low case rates after the SST plan. The estimands for distributional effects come from Frandsen, Frolich, and Melly (2012). To estimate both the mean and distributional effects, the empirical strategy uses a robust, bias-correction procedure developed by Calonico, Cattaneo, and Titiunik (2014).

The primary data come from OSHA's Data Initiative (ODI), conducted annually from 1996 to 2011. The data report the employer name, establishment address, and industry. In regards to accidents and injuries, the data report the total case rate (TCR) and the DART rate. The TCR includes cases involving (1) death, (2) days away from work, (3) job transfers or restrictions, or (4) medical attention beyond first aid, whereas the DART rate excludes cases involving death and medical attention beyond first aid. These rates are calculated annually per 100 full-time equivalent workers. The ODI data are used to determine both the proximity to the inspection-list cutoff before the SST plan, as well as worker safety outcomes following the SST plan. Thus, the data are restricted to establishments observed at least twice, with two observations spaced four years apart.[1] To determine inspection outcomes during the SST plan, the ODI data are linked to OSHA's Integrated Management Information System. To determine union activity within an

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[1] While the ODI data were not designed as a panel, many establishments are observed in the ODI in multiple years.

establishment, the ODI data are linked to "notices of bargaining" filed with the Federal Mediation and Conciliation Service.[2]

The empirical analysis focuses on the DART rate cutoff for the primary inspection list. Using local linear regression, this cutoff is associated with a 22.7 percentage point increase in "programmed" inspections, a 17.5 percentage point increase in any citations, and a 15.4 percentage point increase in penalties. The cutoff is not associated with a change in "unprogrammed" inspections, which are unrelated to the SST plan. Thus, the SST plan provides a sizeable discontinuity in inspections for identification.

The effect of inspection on worker safety is most evident for the DART rate. Using the FRD design and local linear regression, the average effect of an inspection on the DART rate among compliers is -1.607 per 100 full-time equivalent workers. This estimate is statistically significant at the five percent level and robust to the inclusion of establishment characteristics. Moreover, the effect on the DART rate is most pronounced among manufacturing establishments below the 90th percentile of the DART distribution post-inspection. Treatment effects are less evident for other case rates, such as the TCR, and for other industries. Treatment effects also do not differ significantly between union and non-union establishments, suggesting that unionization is neither a substitute nor a complement to workplace inspections.[3]

This study contributes to a literature on the effects of OSHA inspections on worker safety. In general, most studies find little to no "abatement" effect of inspections on worker safety, and studies that do find an effect may suffer from confounding biases (Kniesner and Leeth 2004).[4]

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[2] DiNardo and Lee (2004) use "notices of bargaining" filed with the Federal Mediation and Conciliation Service to determine union activity following a union vote.

[3] The effect of unionization on worker safety has been examined by Morantz (2009, 2013).

[4] Studies in this literature include Cooke and Gautschi (1981); Gray and Scholz (1993); Gray and Mendeloff (2005); Haviland et al. (2010); McCaffrey (1983); and Ruser and Smith (1991). The little to no effect of an OSHA inspection

These studies differ by the analysis period, the definition of treatment (inspection versus citation or penalty), and worker safety outcome (overall injuries versus specific accidents). To address selection bias, some studies exploit the timing of an inspection, arguing that establishments inspected earlier in the year have more time to remediate workplace hazards. One study, by Levine, Toffel, and Johnson (2012), use experimental data from California in 1996 to 2006. By exploiting random assignment of inspections among 409 establishments, they find that inspections reduce injuries by 9.4 percent, with no detectable effect on employment, sales, or firm survival. However, their sample was limited to high-risk industries in California, so their findings may not be generalizable to other industries or US states.

The estimates from this study appear at the high end of effects. Relative to the average DART rate at the cutoff, the average effect of -1.607 amounts to a 20 percentage point reduction in injuries. However, this estimate pertains only to compliers near the cutoff, which is located at 85th percentile of the DART distribution pre-inspection. While this suggests that the effect of an inspection on worker safety may be greater among high-risk firms, the effect is concentrated below the 90th percentile of the DART distribution post-inspection. This study also differs from related studies with respect to the data and analysis sample. Thus, it is difficult to reconcile the estimates of this study with those of the related literature.[5] In regards to efficiency, the gains from reducing the DART rate at the cutoff are offset by the cost of an inspection for OSHA and the cost of compliance for employers.

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on worker safety may reflect that OSHA penalties are too low (Viscusi 1979) or that safety and health standards are not directly related to workplace accidents (Bartel and Glenn 1985).

[5] A report by Summit Consulting (Peto et al. 2016) pursues a similar identification strategy as this study, but this report uses ODI data collected in 2007 only. They find small and statistically insignificant effects of an inspection on worker safety. A qualitatively similar result is obtained in this study when the data are restricted to the ODI data collected in 2007 (not shown).

II. Background

The goal of the Occupational Safety and Health Act, passed by the US Congress in 1970, is "to assure safe and healthful working conditions for working men and women." To achieve this goal, the Occupational Safety and Health Administration (OSHA) was created to codify and enforce safety and health regulations. Regulations include specification standards, such as requiring safety guards for machinery or equipment, as well as performance standards, such as requiring limits on exposure to hazardous chemicals (Kniesner and Leeth 2014). To enforce regulations, OSHA educates employers and employees, inspects worksites for workplace hazards, and levies financial penalties on employers for serious or repeated violations.

OSHA inspections are either programmed or unprogrammed. Unprogrammed inspections result from fatal or catastrophic accidents, employee complaints, or referrals, while programmed inspections are intended to identify and abate workplace hazards before an accident or illness occurs. In fiscal year 2015, OSHA conducted 16,527 programmed inspections and 19,293 unprogrammed inspections.[6] Among unprogrammed inspections, 912 were due to fatal or catastrophic accidents, 9,037 were due to employee complaints, 4,705 were due to referrals, and 4,639 were due to other reasons.

In 1999, OSHA drastically changed its procedure for targeting programmed inspections. Before 1999, programmed inspections were targeted at industries with high rates of accidents and injuries. This was accomplished using industry-level data collected by the Bureau of Labor Statistics. However, after an inspection, many establishments in high-risk industries were found to be relatively safe. Thus, targeting high-risk industries was relatively inefficient at targeting high-risk establishments, since worker safety varied substantially within industries.

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[6] These figures exclude State Plan inspections, which totaled 43,105 in fiscal year 2016.

To better target high-risk establishments, OSHA created the OSHA Data Initiative (ODI) in 1996. The goal of the ODI was to collect data on accidents and injuries directly from employers at the establishment level. The ODI survey was conducted annually from 1997 to 2011, with approximately 80,000 establishments surveyed each year. The survey covers manufacturing and industries with injury rates above the national average, except construction, and excludes establishments with fewer than 40 employees.[7] To facilitate data collection, OSHA required establishments to record accidents and injuries using OSHA's Form 300. Per the form, cases must be recorded if they involve death, days away from work, job transfers or restrictions, or medical attention beyond first aid. These data were then used to calculate case rates for the calendar year. The DAFWII includes cases involving days away from work; the DART includes cases involving days away from work and job transfers or restrictions, and the TCR includes all case types, including death.[8] These rates are calculated and reported per 100 full-time equivalent workers.[9]

The ODI data were then used to implement OSHA's Site-Specific Targeting (SST) plan. The goal of the SST plan was to target programmed inspections at establishments with high rates of accidents and injuries. To target high-rate establishments, the SST plan prioritized establishments using case-rate cutoffs. For example, the ODI in 2003 collected case-rate data for calendar year 2002, and the SST plan used these data to target programmed inspections from April 2004 to August 2005. The primary inspection list included establishments with a DART rate greater than 15 or a DAFWII rate greater than 10. A secondary inspection list included establishments with a

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[7] In 1999, the ODI excluded establishments with fewer than 60 employees.

[8] In 2002, DART replaced LWDII due to a change in recordkeeping. The LWDII includes only cases involving lost work days due to injury or illness and is nearly identical to DART.

[9] To calculate case rates, the ODI asks employers to report the number of employees and the total hours worked by employees during the previous calendar year. This information is not reported in the ODI data.

DART rate greater than 10 or a DAFWII rate greater than 4. Additionally, all establishments with a DART rate greater than 7 were mailed a letter stating that their DART rates were high relative to the national average. Occasionally, the case-rate cutoffs changed, reflecting changes in the case-rate distribution and OSHA's resources to conduct inspections. While all establishments on the primary list were targeted for an inspection, only 20 percent were ultimately inspected, and those that were inspected did not always have the highest case rates (US Department of Labor 2012). The low inspection rate was attributed, in part, to limited resources (US Department of Labor 2012).

III. Methodology

The empirical objective is to identify the causal effect of an OSHA inspection on worker safety. The effect is identified using quasi-experimental variation in inspections generated by OSHA's SST plan. Specifically, the SST established a case-rate cutoff, and establishments exceeding the cutoff were targeted for a programmed inspection. If this process generated a discontinuous increase in inspections at the cutoff, and if establishments just above and below the cutoff are similar, then the increase in inspections at the cutoff may be used to identify the causal effect of an inspection on worker safety.

A. Fuzzy Regression Discontinuity

The empirical strategy utilizes the fuzzy regression discontinuity (FRD) design (Hahn, Todd, and Van der Klaauw 2001; Imbens and Lemieux 2008) using the potential outcomes framework of Rubin (1974) and Holland (1986). Using this framework, each establishment has two potential outcomes: worker safety without an OSHA inspection, denoted $Y_i(0)$, and worker safety with an OSHA inspection, denoted $Y_i(1)$. For each establishment, the causal effect of an OSHA inspection

is defined as $\tau_i = Y_i(1) - Y_i(0)$. If worker safety is measured by the case rate, then $\tau_i < 0$ indicates an improvement worker safety. The fundamental problem for identifying τ_i is that only one outcome – either $Y_i(1)$ or $Y_i(0)$ – is observed for each establishment.

To plausibly identify causal effects, the FRD design requires three main assumptions. First, the inspection outcome, denoted by the variable D_i, must be partially determined by whether a running variable X_i exceeds a cutoff c:

$$(1) \quad \lim_{x\uparrow c} E[D_i|X_i = x] < \lim_{x\downarrow c} E[D_i|X_i = x].$$

In this case, the likelihood of treatment increases at the cutoff c. Second, the change in inspections at the cutoff is due to compliers, defined as those who are treated just above the cutoff, but would not have been treated in the absence of the SST plan. Third, the conditional mean functions $E[Y(0)|X_i = x]$ and $E[Y(1)|X_i = x]$ are continuous near the cutoff c with respect to the running variable X_i. If so, $\lim_{x\uparrow c} E[Y|X = x]$ represents the counterfactual of $\lim_{x\downarrow c} E[Y|X = x]$ in the absence of the SST plan.

With these assumptions, the FRD estimand is given by:

$$(2) \quad \tau_{FRD} = \frac{\lim_{x\downarrow c} E[Y|X = x] - \lim_{x\uparrow c} E[Y|X = x]}{\lim_{x\downarrow c} E[D|X = x] - \lim_{x\uparrow c} E[D|X = x]}.$$

The numerator measures the difference in the mean outcome Y above and below the cutoff, and the denominator measures the difference in the treatment D above and below the cutoff. By dividing the former by the latter, the FRD estimand measures the average effect of treatment among compliers. The FRD estimand is comparable to the Wald estimator, which arises from instrumental variable regression with a binary instrument.

B. Distributional Effects

The FRD estimand measures the average treatment effect of among compliers. However, the effect among compliers may differ across the distribution of the outcome variable Y. On one hand, establishments with high Y, which are presumably more dangerous, have greater scope for remediating workplace hazards. On the other hand, these establishments may face greater idiosyncratic risk beyond the purview of OSHA regulations and enforcement. Thus, the effect of an inspection across the distribution of the outcome variable Y is ambiguous.

To estimate distributional effects, the cumulative density function (CDF) for Y is estimated among compliers just above the cutoff, where they are treated, and among counterfactual compliers just below the cutoff, where they are not treated. The estimands for the conditional CDFs are provided by (Frandsen, Frolich, and Melly 2012). Above the cutoff, the conditional CDF is given by:

$$(3) \qquad F_{Y(1)|\Omega}(y) = \frac{\lim\limits_{x\downarrow c} E[1(Y \le y)D|X = x] - \lim\limits_{x\uparrow c} E[1(Y \le y)D|X = x]}{\lim\limits_{x\downarrow c} E[D|X = x] - \lim\limits_{x\uparrow c} E[D|X = x]}.$$

Below the cutoff, the conditional CDF is given by:

$$(4) \qquad F_{Y(0)|\Omega}(y) = \frac{\lim\limits_{x\downarrow c} E[1(Y \le y)(1 - D)|X = x] - \lim\limits_{x\uparrow c} E[1(Y \le y)(1 - D)|X = x]}{\lim\limits_{x\downarrow c} E[1 - D|X = x] - \lim\limits_{x\uparrow c} E[1 - D|X = x]}.$$

Both CDFs are conditional on compliers, denoted by Ω. At each value of $Y = y$, the distributional impact of treatment among compliers is measured by $F_{Y(1)|\Omega}(y) - F_{Y(0)|\Omega}(y)$.

C. Estimation

Treatment effects are estimated using nonparametric, local linear regression. Local linear regression is appropriate because the estimands are evaluated at the cutoff, and because the

cutoff is located at the boundary of the bandwidth (Imbens and Lemieux 2008). The term $\lim_{x \downarrow c} E[Y|X = x]$ is estimated by solving

$$(5) \quad \min_{\alpha_{YR}, \beta_{YR}} \sum_{c \leq X_i \leq c + h_{YR}} \left(Y_i - \alpha_{YR} - \beta_{YR}(X_i - c)\right)^2 \mathrm{K}\left(\frac{X_i - c}{h_{YR}}\right).$$

The outcome Y_i is a linear function of $X_i - c$, with α_{YR} as the intercept and β_{YR} as the slope. The objective is to minimize the sum of the squared deviations of Y_i from the linear function, weighted by the kernel function $\mathrm{K}\left(\frac{X_i - c}{h_{YR}}\right)$, using observations within the bandwidth of length h_{YR}, from c to $c + h_{YR}$. The solution to the minimization problem is denoted by $\hat{\alpha}_{YR}$ and $\hat{\beta}_{YR}$, so the estimate of Y given X is $\hat{\alpha}_{YR} + \hat{\beta}_{YR}(X - c)$, and the estimate of $\lim_{x \downarrow c} E[Y|X = x]$ is $\hat{\alpha}_{YR}$. Similarly, $\lim_{x \uparrow c} E[Y|X = x]$ is estimated by solving

$$(6) \quad \min_{\alpha_{YL}, \beta_{YL}} \sum_{c - h_{YL} \leq X_i < c} \left(Y_i - \alpha_{YL} - \beta_{YL}(X_i - c)\right)^2 \mathrm{K}\left(\frac{X_i - c}{h_{YL}}\right).$$

The terms $\lim_{x \downarrow c} E[D|X = x]$ and $\lim_{x \uparrow c} E[D|X = x]$ are estimated by replacing Y with D.

The estimation is accomplished using a robust, bias-correction procedure developed by Calonico, Cattaneo, and Titiunik (2014). This procedure determines the optimal bandwidth h, corrects for the bias of $\hat{\tau}_{FRD}$ when estimated using local linear regression, and calculates robust standard errors that reflect that the bias correction is stochastic. The procedure also allows for controls for observable characteristics. The optimal bandwidth is calculated for the outcome Y, assuming $h_{YL} = h_{YR}$, and this bandwidth is used for the inspection outcome D. The default for the kernel function $\mathrm{K}(.)$ is triangular.

IV. Data

The primary data come from OSHA's Data Initiative (ODI). Stated above, the ODI collected data on accidents and injuries at the establishment level from 1996 to 2011. The data report the

establishment name and address, including the street number, street name, city, state, and zip code. The data also report the establishment's industry using the Standard Industrialization Classification codes (SIC). In regards to accidents and injuries, the data report the TCR, the DART, and the DAFWII, though the DAFWII is only available for calendar years 2002 and beyond.

The empirical analysis focuses on the DART cutoff for the primary inspection list, so the running variable X_i is measured by the DART rate reported in the ODI. While the SST plan had multiple cutoffs for the primary and secondary inspection lists, only the DART cutoff exhibits a sizeable discontinuity in inspections. Alternative cutoffs are discussed in section VII.

The inspection indicator D_i is obtained by merging the ODI data to the universe of OSHA inspections as of September, 2016.[10] These data come from OSHA's Integrated Management Information System. For each inspection, the data report whether the inspection was programmed or unprogrammed, the citations recorded during the inspection, and the penalties levied for each citation, if any. The inspection indicator D_i is measured within the SST plan cycle. For example, the ODI in 2003 collected case-rate data for calendar year 2002, and the SST plan used these data to target programmed inspections from April 2004 to August 2005. Thus, for that cycle, D_i measures inspection outcomes from April 2004 to August 2005.

The outcome variable Y_i is measured from the ODI in the calendar year after the SST cycle. In the example above, inspections under the SST plan occurred from April 2004 to August 2005, so Y_i is the case rate in the ODI data in 2006. While the ODI data were not designed as a panel, many establishments are observed in the ODI in multiple years.

The covariates Z_i include sets of dummy variables for calendar year, state, industry, and an indicator of union activity. State and industry are reported in the ODI. Using the SIC codes,

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[10] The merging procedure is described in the Appendix.

industry is categorized into three groups: manufacturing (SIC 20 to 39), health services (SIC 80), and other. To obtain information on union activity, the ODI data are merged to "notices of bargaining" filed with the Federal Mediation and Conciliation Service (FMCS). A notice must be filed to modify a union contract and thus indicate union activity within an establishment. The FMCS data include all notices filed from 2004 to 2016. The data report the date of the notice and the name and address of the establishment, including the street number, street name, city, state, and zip code. Using the FMCS data, a union indicator variable is created that equals one if there is any union activity and zero otherwise.

The steps used to match the ODI to the IMIS and the FMCS are discussed in the Appendix.

V. Sample

The analysis sample is derived from the ODI using five restrictions. First, the data are restricted to years 1997 to 2011, since ODI data in 1996 were not used to implement the SST plan. The pooled data contain 951,077 observations for 328,751 establishments. Second, observations are excluded if they appear to duplicate a record or if the establishment's name and address are missing, eliminating 0.44 percent of the sample. Third, the data are stacked by establishment by year, and the sample is restricted to pairs of observations spaced four years apart. This yields 225,215 observation pairs, with the first year ranging from 1997 to 2007. Fourth, the data are restricted to states covered by the SST plan, which includes all states under federal jurisdiction with respect to OSHA and a few states that are not. In total, twenty-seven states were covered by the SST plan throughout the analysis period, and an additional eight states were covered at some point during the analysis period. Finally, observations pairs are excluded if the case rate from the ODI is missing or exceeds 100, eliminating 1.9 percent of the sample. The resulting analysis sample contains 154,808 observation pairs.

Table 1 presents summary statistics of the analysis sample. The first column reports statistics for the entire sample; the second column reports statistics for establishments with DART rates from four below the cutoff to four above the cutoff; and the last two columns divide establishments into those just below and above the SST cutoff.

As shown, the SST cutoff is located near the top of the DART rate distribution. Among all establishments, the mean DART rate is 7.33, and the mean SST cutoff is 13.67. To illustrate the distribution, Figure 1 plots the distribution of the DART relative to the SST. The distribution is skewed to the right, and the cutoff is located near the 85th percentile. This means that, in this study, compliers are relatively high-risk firms. Additionally, the density is smooth near the cutoff.[11] This suggests that establishments do not bunch just below the cutoff to avoid inspection, which could potentially violate the identification assumption that establishments are similar just above and below the threshold. The absence of bunching through gaming seems reasonable, since establishments report their DART rates before SST cutoffs are determined.

Consistent with the SST plan, programmed inspections were more frequent just above the cutoff than just below. In Table 1, the rate of inspections were 31.9 percent just above the cutoff, but 10.2 percent just below. To illustrate the difference at the threshold, panel A of Figure 2 plots the likelihood of a programmed inspection by the DART rate relative to the SST cutoff. The markers denote the mean outcome within intervals of 0.5, and the lines are derived from second-order local polynomial regression, estimated separately above and below the cutoff. As shown, the difference in inspection above and below the cutoff occurs at cutoff, as required for identification using the FRD design. Additionally, increased programmed inspections led to greater rates of citations and penalties, as shown in Table 1 and panels B and C of Figure 2. In

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[11] A test by McCrary (2008) rejects that there is a discontinuity in the distribution at the cutoff.

contrast, there is no change in unprogrammed inspections at the cutoff, as shown in panel D of Figure 2.

Establishments also appear similar just above and below the SST cutoff with respect to observable characteristics. Among all establishments, approximately 61.0 percent are in manufacturing, 17.5 percent are in health services, and 12.5 percent exhibit union activity. Figure 3 plots these characteristics by the DART relative to the threshold, which shows that these characteristics are relative stable throughout the DART distribution, with no measurable change at the discontinuity.[12]

Finally, the TCR and the DART exhibit substantial mean reversion, a finding consistent with Ruser (1995). This is evident in Table 1 by comparing the baseline case rates, denoted by the subscript t, to the rates four years later, denoted with the subscript $t+4$. Among all establishments, the TCR falls from 12.8 to 9.5, and the DART falls from 7.3 to 5.7. Mean reversion should not invalidate the identification strategy, however, if the extent of mean reversion is similar above and below the cutoff.

VI. Results

A. Inspection Outcomes

The first step is to estimate the discontinuity in inspection outcomes at the cutoff. Panel A of Table 2 presents the estimated discontinuity and the optimal bandwidth using local linear regression without covariates. As shown, the cutoff is associated with a 22.7 percent point increase in programmed inspections, a 17.5 percentage point increase in citations, and a 15.4

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[12] The change in these characteristics at the cutoff are estimated using local linear regression. All estimated changes are small and statistically insignificant.

percentage point increase in penalties. These estimates are statistically significant at the one percent level and robust to the inclusion of covariates, as shown in panel B.

The final column of Table 2 presents the results for unprogrammed inspections, which were not directly affected by the SST plan. As expected, there is no discontinuous change in unprogrammed inspections at the cutoff.

B. Mean Effects

The increase in programmed inspections at the cutoff is used to identify the effect of an inspection on worker safety. To examine this effect graphically, Figure 4 plots case rates in the first calendar year after the SST cycle. Panel A plots the TCR, and panel B plots the DART. In both figures, the mean case rates appear to decrease discontinuously at the cutoff, suggesting that inspections improve worker safety.

The FRD estimand in equation (2) relates the change in case rates to the change in inspections, both measured at the cutoff. With the assumptions outlined above, the FRD estimand represents the causal effect of an inspection among compliers.

The left side of Table 3 presents the baseline estimates of the FRD estimand. The estimates are presented separately for the TCR and the DART, and the inspection outcome is any programmed inspection during the SST cycle. As shown, an inspection has a negative effect on both the TCR and the DART, but only the latter effect is statistically significant. Without covariates, the estimated effect on the TCR is -0.569, and the estimated effect on the DART rate is -1.607, measured per 100 full-time equivalent workers. Relative to the mean DART rate of 7.5 (Table 1), the effect on the DART amounts to a decrease of approximately 20 percent. The estimates are robust to the inclusion of covariates.

The right side of Table 3 presents estimates using data from 1998 to 2007. This allows consideration of a third outcome, the DAFWII, which is only available for calendar years 2002 and beyond. As shown, all three estimated effects are negative, but only the effect on the DART is statistically significant. Without covariates, the estimated effect on the DART is -1.877 per 100 full-time equivalent workers. The estimated effects on the TCR and the DAFWII are smaller in magnitude and statistically insignificant.

In both panels, the estimates for the DART rate are larger than the estimates for the TCR or the DAFWII. A possible mechanism is that inspections reduced the severity of cases involving job restrictions or transfers to require only medical attention beyond first aid. Cases involving job restrictions and transfers are included in the TCR and the DART, but not the DAFWII, and cases involving medical attention beyond first aid are included in the TCR, but not the DART or DAFWII. Thus, the proposed mechanism would decrease the DART rate, but less so the TCR and the DAFWII. However, the standard errors for all the estimated effects are large and thus do not rule out a wide range of effects.

C. Distributional Effects

The effect of an inspection may vary between high-risk and low-risk firms. To explore this possibility, the distributional effects of an inspection are examined using equations (3) and (4). Equation (3) presents the CDF of compliers when treated, and equation (4) represents the CDF of counterfactual compliers when not treated. The equations are estimated separately for integers of $Y = y$, from zero to sixteen, using local linear regression.

Figure 5 illustrates the estimated distributional effects for the DART rate. Panel A plots the estimates of $F_{Y(1)|\Omega}(y)$ and $F_{Y(0)|\Omega}(y)$, and panel B plots their difference and its 95 percent confidence interval. As shown, the effect of inspections is concentrated at bottom of the DART

distribution. Starting at $Y = 0$, the difference in the conditional CDFs is approximately 11 percentage points, which is statistically significant at the one percent level. The difference remains positive and statistically significant up to $Y = 8$, though the 95 percent confidence intervals widen substantially. The difference then converges towards zero near the 92nd percentile. At that point, the difference is approximately one percent and statistically insignificant. Thus, the effects of an inspection on the DART rate occur predominately below the 90th percentile.

D. Effects by Industry and Union Status

The effect of an inspection may also differ by industry. Differences may arise due to different occupational hazards, effective regulatory standards, and scopes for improvement. To explore this possibility, the models are estimated separately for manufacturing, health services, and "other" industries, with the DART rate as the outcome. The left side of Table 4 presents mean effects, and Figure 6 illustrates distributional effects. For brevity, the figure plots the estimates of $F_{Y(1)|\Omega}(y)$ and $F_{Y(0)|\Omega}(y)$, but not their difference.

According to the results, the effect of an inspection on worker safety is most evident for manufacturing, particularly below the 90th percentile. In regards to the mean effect, the estimate for manufacturing is -1.257 per 100 full-time equivalent workers, compared to 0.626 for health services and -0.124 for "other" industries. However, none of these estimates is statistically significant. In regards to distributional effects in manufacturing, there are sizeable differences between the conditional CDFs up to the 90th percentile, most of which are statistically significant. In contrast, there are no statistically significant differences in the conditional CDFs in health services or "other" industries.

Another consideration is whether the effects of an inspection vary by union status. On one hand, unions may increase the effect of an inspection, since union officials often accompany safety inspectors during the workplace inspection. On the other hand, unions may decrease the effect of an inspection, since union officials often work with management independently to improve worker safety (Eaton and Nocerino 2000). Thus, unions may be a complement or substitute to regulatory oversight – and workplace inspections, in particular (Morantz 2009; Morantz 2013).

The right side of Table 4 presents the mean effects, and Figure 7 illustrates the distributional effects. According to the results, the effect of an inspection on worker safety is qualitatively similar among union and non-union establishments. In Table 4, the estimates for union and non-union establishments are -3.536 and -1.414, respectively, though neither estimate is statistically significant. In regards to distributional effects, there are notable differences in the conditional CDFs up to the 90th percentile for both union and non-union establishments. The only differences that are statistically significant are those among non-union establishments below the 65th percentile. Taken together, the results cannot reject that the effect of an inspection on worker safety is similar among union and non-union establishments.

VII. Additional Considerations

A. Treatment Variable

Thus far, the treatment variable has been defined as a programmed inspection. However, the effect of an inspection on worker safety may be limited to those that result in citations or penalties (Gray and Scholz 1993; Gray and Mendeloff 2005; Haviland et al. 2012). This is because

the mechanism for the effect is through the identification and remediation of workplace hazards, which is most likely to occur if the inspection results in a citation.

To address this issue, the model of the DART rate with covariates is estimated separately with a citation and penalty as the treatment variable. In the baseline case, where the treatment variable is a programmed inspection, the discontinuity in inspections is 22.4 percentage points, and the estimated effect of a programmed inspection on the DART rate is -1.607 per 100 full-time equivalent workers. When the treatment variable is a citation, these estimates are 17.2 percentage points and -2.22, respectively; when the treatment variable is a penalty, these estimates are 15.3 percentage points and -2.46, respectively. All estimates are statistically significant at the five percent level. Thus, if the treatment is defined as an inspection resulting in either a citation or penalty, the estimated treatment effect increases.

B. Alternative SST Cutoffs

Thus far, the empirical analysis has focused on the DART cutoff for the primary inspection list. However, stated above, a lower set of cutoffs defined a secondary inspection list, and an even lower cutoff determined which establishments received a letter stating that their case rate was high relative to the national average. An important consideration is whether these cutoffs affected the likelihood of an inspection or worker safety.

In regards to the secondary inspection list, the DART cutoff is associated with a small increase in programmed inspections, but there is no measurable change in worker safety. For example, using local linear regression with covariates, the discontinuity in programmed inspections is 3.76 percentage points, which is statistically significant at the five percent level, but the change in the

DART rate is 0.074, with a standard error of 0.127.[13] Similarly, the cutoff for the letter is associated with a small increase in programmed inspections, but there is no measurable change in worker safety. Using local linear regression with covariates, the discontinuity in programmed inspections is 1.47 percentage points, which is statistically significant at the five percent level, but the change in the DART rate is 0.201, with a standard error of 0.121. Thus, alternative cutoffs are not associated with a substantial increase in programmed inspections or a change in worker safety.

C. Non-Federal States

ODI data were collected in 45 US states and the District of Columbia, but the SST plan was only implemented in 35 states, including 29 states under federal jurisdiction with respect to OSHA. Thus, as a placebo test, the empirical analysis is repeated for non-federal states, as if the SST plan had been implemented. As expected, the DART cutoff for the primary inspection list is not associated with a change in programmed inspections or worker safety. For example, using local linear regression with covariates, the discontinuity in programmed inspections is 1.05 percentage points, with a standard error 1.09, and the discontinuity in the DART rate is -0.028, with a standard error of 0.250. Thus, as expected, the DART cutoff is not associated with a change in programmed inspections or worker safety in states that are not covered by the SST plan.

VIII. Discussion and Conclusion

This study examines the effect of an OSHA inspection on worker safety. To identify the effect, the study exploits quasi-experimental variation in inspections due to OSHA's SST plan. Due to the identification strategy, the effect of an OSHA inspection is identified among establishments near

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[13] A secondary inspection list was not specified in some years, so the sample size is reduced to 137,848.

the 85th percentile of the DART rate distribution pre-inspection. Using the fuzzy regression discontinuity design and local linear regression, the causal effect of an inspection on the DART rate is approximately -1.607 per 100 full-time equivalent workers. Relative to the mean, this effect is a reduction of approximately 20 percent. The effect is most pronounced among manufacturing establishments below the 90th percentile of the DART rate distribution post-inspection.

The estimated effect of an OSHA inspection on worker safety from this study is large compared to related studies. As stated, most studies find little to no "abatement" effect of inspections on worker safety (Kniesner and Leeth 2004). However, it is difficult to reconcile the results from this study, since studies differ in regards to identification strategy, data, population of interest, and worker safety outcomes.

An important consideration is whether the gains from reducing the DART rate exceed the costs. According to Viscusi and Aldy (2003), the value of statistical injury ranges from $20 thousand to $70 thousand. Thus, the value of reducing the DART rate by -1.607 ranges from $32 thousand to $112 thousand per 100 full-time equivalent workers. This value is offset by the cost of an inspection for OSHA and the cost of compliance for employers. In 2016, the average cost of an OSHA inspection was $6,511. This figure is derived by dividing the total OSHA budget on federal enforcement in 2016 of $208 million by the number of federal OSHA inspections of 31,948. The cost of compliance for employers is more difficult to estimate. To shed light on these costs, the IMIS data are used to determine the most common citations among establishments in the analysis sample and to estimate the effect of an inspection on the number of citations at the cutoff. Table 5 presents the results for all citations as well as the nine most common citations separately. As shown, an inspection increased the number of all citations by 5.251. The most of

the common citations are associated with manufacturing, with the exception of "bloodborne pathogens." To address additional citations after an inspection, employers face direct costs, such as retrofitting machines, as well as indirect costs, such as lost productivity. While no attempt is made here to quantify these costs, previous research suggests that compliance costs may be substantial (Kniesner and Leeth 2014).

Appendix

This study uses establishment-level data from OSHA's Data Initiative (ODI) matched to records on notices of union bargaining from the Federal Mediation and Conciliation Service (FMCS) and inspection records from the Integrated Management Information System (IMIS). This section provides the procedures used to link these datasets.

The analysis sample is derived from the ODI, which provides establishment-level data on accidents and injuries. The sample is limited to establishments observed at least twice, with the two observations spaced exactly four years. To link multiple observations of an establishment across years, the establishment name and address were standardized. All special characters, such as @, #, and /, were removed. For the establishment name, common words such as "Company", "Corporation", and "Co," were deleted. Some establishments operated under a different name as their parent company, often indicated by "DBA," an acronym for, "doing business as." In these cases, the establishment name is separated into two, with the second name as a new variable. For the establishment address, floor numbers, suite numbers, and room numbers were removed. Common words such as "Street", "Road", and "Avenue" are standardized to abbreviations "St", "Rd", and "Ave". For city names, we construct a list of all the city-state combinations that appear in ODI and matched them to a list of city names from Census. Any city-state combinations with no match to the list were checked manually for errors in either the state or the spelling of the city. Duplicates of the same establishment (based on the identifier we generated) in the same year are deleted (less than one percent of the sample).

The ODI data are then linked to the inspection data during the SST cycle from the Integrated Management Information System (IMIS). IMIS includes the universe of the inspections conducted by OSHA from 1970 and reports the name and address of the inspected establishments, including

street address, zip code, city, and state, which are used to link ODI data. The establishment name and address are standardized using the same method used to standardize the ODI data. The ODI data are then matched to the IMIS data using several criteria. If an establishment is matched successfully based on one criterion, the establishment and its inspection record are removed from subsequent matching. First, establishments matched based on the establishment name and street address within the same city and state. This step is repeated for establishments with two names. Then, establishments are matched based on establishment name and 5-digit zip code within the same city and state. Third, establishments are matched based on the first six letters of the establishment name and street address (excluding spaces). Finally, establishments are matched based on street address within the same city and state, after manually verifying a match of the establishment name, and on establishment name, after manually verifying a match on the street address.

The ODI data are also linked to the universe of notices of bargaining filed with Federal Mediation and Conciliation Service (FMCS). The universe of notices are available from 2004 to 2016. Because unions must file a notices with the FMCS to modify an existing contract, these notices indicate whether any collective bargaining activity occurs within an establishment (DiNardo and Lee, 2004). Again, the establishment name and address are standardized, and the ODI data are matched to the FMCS data using several criteria. An establishment is assumed unionized if there is any match to a record in FMCS. This assumption can be checked among establishments matched to both the FMCS and the IMIS, since the inspection data also report whether the establishment is unionized. Among these establishments, 89.3 percent with a match to the FMCS are unionized according to the IMIS, and only 10.8 percent without a matched to the FMCS are unionized. Thus, a match to the FMCS is highly correlated with union status.

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