The Effect of Workplace Inspections on Worker Safety

December 18, 2017

Ling Li
Department of Economics
Center for Policy Research
Syracuse University
426 Eggers Hall
Syracuse, NY 13244

Perry Singleton
Associate Professor
Department of Economics
Center for Policy Research
Syracuse University
426 Eggers Hall
Syracuse, NY 13244

Abstract: The Occupation Safety and Health Administration (OSHA) enforces safety regulations through workplace inspections. We estimate the effect of inspections on worker safety by exploiting a feature of OSHA's Site Specific Targeting plan. The plan targeted establishments for inspection if their baseline case rate exceeded a cutoff. This generated a discontinuous increase in inspections, which we exploit for identification. Using the fuzzy regression discontinuity model, we find that inspections decrease the rate of cases involving days away from work, job restrictions, and job transfers in the calendar year immediately after the inspection cycle. We find no effect for other case rates or in subsequent years. Effects are most evident in manufacturing and less evident in health services, the largest two-digit industries represented in the data.

JEL Codes: J28, K32
Keywords: OSHA, worker safety, regression discontinuity

For valuable comments and suggestions, the authors thank Gary Engelhardt, Christopher Carpenter, Michael (Misha) Dworsky, Alfonso Flores-Lagunes, Hugo Jales, Matthew Johnson, Jeffrey Kubik, Thomas Kniesner, and seminar participants at the meeting of the Association for Public Policy Analysis and Management.

Lead Author: Perry Singleton; Associate Professor, Department of Economics; Senior Research Associate, Center for Policy Research; Syracuse University. Contacts: 426 Eggers Hall; Syracuse University, Syracuse, NY 13244; (315) 443-3114; psinglet@syr.edu.

Additional results and copies of the computer programs used to generate the results presented in the paper are available from the lead author at psinglet@syr.edu.

In 2007, the estimated cost of on-the-job injuries in the US was $192 billion (Leigh 2011). While employers may independently invest in workplace safety, investment may be suboptimal if employers do not internalize the full costs of worker injuries. To attempt to achieve the social optimum, governments could enforce safety and health regulations through workplace inspections, the primary responsibility of the US Occupational Safety and Health Administration (OSHA). However, this approach depends on whether regulations and workplace inspections are effective. This is difficult to determine empirically since inspections are generally targeted at high-risk establishments (Kniesner and Leeth 2014; Smith 1979). As a result, inspections and worker safety would be negatively correlated, which would confound any positive, causal effect of the former on the latter.

In this study, we attempt to identify the causal effect of inspections on worker safety. The identification strategy exploits quasi-experimental variation in inspections generated by OSHA's Site Specific Targeting (SST) plan. The SST plan, implemented in 1999, targeted establishments with high rates of accidents and injuries for inspection. The plan used data from the OSHA Data Initiative (ODI), which collected establishment-level data on accidents and injuries directly from employers. Using these data, the 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. This process generated a discontinuous increase in inspections at the cutoff, particularly for the primary inspection list. Using the fuzzy regression discontinuity (FRD) design, the discontinuity in inspections is used to identify the causal effect of inspections on worker safety.

Data on accidents and injuries come from the ODI, conducted annually from 1996 to 2011. These data are used to predict inspections during the SST plan and to measure worker safety outcomes after the SST plan. The data report the rate of cases involving days away from work, job restrictions, and job transfers (DART). To determine whether an establishment is inspected during the SST plan, the ODI data are matched to OSHA's Integrated Management Information System (IMIS), which contains data on all establishments inspected during the analysis period.

We first estimate the discontinuous increase in inspection outcomes, particularly at the DART rate cutoff for the primary inspection list. Using local linear regression, the cutoff is associated with a 22.7 percentage point increase in inspections related to the SST plan, a 17.5 percentage point increase in any citations, and a 15.4 percentage point increase in any penalty. The cutoff is not associated with a change in "unprogrammed" inspections, which are unrelated to the SST plan.

We then estimate the effect of an inspection on worker safety. Using the FRD design and local linear regression, the average effect of an inspection on the DART rate is -1.792 per 100 full-time equivalent workers – a reduction of 20 percent relative to the post-inspection DART rate near the cutoff. Moreover, the effect on the DART rate is most evident for manufacturing establishments, particularly below the 90th percentile of the DART distribution post-inspection. Treatment effects are less evident for other case rates and for other industries. Treatment effects also do not differ significantly between establishments with and without union activity.[1] Given the empirical strategy, the treatment effect estimates pertain only to establishments near the cutoff, and thus are not generalizable to establishments away from the cutoff.

Because case-rates are self-reported, a valid concern is that employers may underreport their case rate to avoid inspection (Ruser and Smith 1988). If the tendency to underreport is

[1] Morantz (2009, 2013) explores the relationship between unionization and worker safety.

greater among recently inspected employers under the SST plan, underreporting could account for the results of this study. This may not be the case for four reasons. First, under the SST plan, employers report their case rates before the SST cutoffs are determined, which limits the ability of employers to avoid inspection entirely. Second, the case rate distribution is smooth at the cutoff, suggesting that employers did not underreport case rates to avoid inspection, at least not locally. Third, OSHA inspections include an audit of previously-recorded case rates, which may deter employers from underreporting.[2] Finally, citations for record-keeping violations were extremely rare among establishments that were inspected again within one year after the SST inspection cycle. Among establishments above the cutoff under the SST plan, only 0.32 percent were cited for a record-keeping violation during a subsequent inspection. Below the cutoff, only 0.35 percent were cited.

Regarding efficiency, an important question is whether the gains from the additional inspections exceed the marginal costs. The gains include the statistical value of averted injuries as well as the fiscal externalities through, for example, social insurance programs.[3] The costs include both the cost of the inspection to OSHA and the cost of compliance to employers.[4] To improve efficiency, OSHA should target establishments for inspection in which the effect on worker safety is greatest. In this study, the effect is most evident in manufacturing and less evident in health services, the largest two-digit industries represented in the ODI data.

Background

[2] Kniesner and Leeth (2014) note that deterrence effects are limited by the low likelihood of inspection and relatively small financial penalties.

[3] For a review of estimates on the value of statistical injury, see Viscusi and Aldy (2003).

[4] For a discussion on compliance costs, see Kniesner and Leeth (2014).

Occupational Safety and Health Administration

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 safety guards for machinery or equipment, and performance standards, such as 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 from non-employees, whereas 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.[5] 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.

OSHA Inspections and Worker Safety

The literature on OSHA inspections and worker safety finds a wide range of effects, depending on the identification strategy, analysis period, firm size, definition of treatment (inspection versus citation or penalty), and worker safety outcome (overall injuries versus

[5] These figures exclude State Plan inspections, which are conducted by states under the purview of OSHA. In fiscal year 2016, State Plan inspections totaled 43,105.

specific types).[6] For identification, some studies exploit the timing of an inspection, arguing that establishments inspected earlier in the year have more time to remediate workplace hazards (McCaffrey 1983; Ruser and Smith 1991; Smith 1979). These studies find no effect of inspections on case rates, except for a small decrease in 1973 (Smith 1979). These estimates may be downward biased, however, since establishments inspected earlier have higher rates of accidents (Gray and Scholz 1993). Cooke and Gautschi (1981) relate changes in case rates to the number of citations issued during an inspection. They find that citations decrease days lost from injury by 23 percent in plants with more than 200 workers. However, the relatively large effect may be attributable to mean reversion, whereby a high case rate in one period, which may precipitate an inspection, is followed by a lower rate the next period (Ruser 1995). A study by Levine, Toffel, and Johnson (2012) uses experimental data from California in 1996 to 2006. By exploiting random assignment of an inspection among 409 establishments, they find that inspections reduce injuries by 9.4 percent, with no detectable effect on employment, sales, or firm survival. A limitation of their study is that it is restricted to high-risk industries in California and therefore is not generalizable to other industries or states (Kniesner and Leeth 2014). Finally, a report by Summit Consulting (Peto et al. 2016) uses the same identification strategy as this study, but only uses ODI data collected in 2007. They find a small and statistically insignificant effect of an inspection on worker safety.[7]

Site Specific Targeting Plan

[6] Kniesner and Leeth (2014) provide a review of the literature. Some studies differentiate inspections by whether they result in a citation or penalty, arguing that only these inspections should affect worker safety (Cooke and Gautschi. 1981; Mendeloff and Gray 2005; Gray and Scholz 1993; Haviland et al. 2010).

[7] When we limit our sample specifically to 2007, we also find small and statistically insignificant effects.

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 on accidents and injuries collected by the Bureau of Labor Statistics. However, after an inspection, many establishments in high-risk industries were found to be relatively safe, revealing a high degree of within-industry variation in worker safety. Thus, targeting high-risk industries seemed to be relatively inefficient at targeting high-risk establishments.

To better target high-risk establishments, OSHA created the ODI in 1996. The goal of the ODI was to collect data on accidents and injuries directly from employers at the establishment level. To facilitate data collection, OSHA required establishments to record accidents and injuries using OSHA's Form 300, provided in the Appendix. Per the form, employers must separately record cases by four outcomes: (1) death, (2) days away from work, (3) job restrictions or transfers, or (4) medical attention beyond first aid. These data were then used to calculate aggregate case rates for the calendar year. (The data do not report case rates for the four outcomes separately.) The total case rate (TCR) includes all four cases. A second rate includes only days away, job restrictions, and job transfers (DART). [8] A third case rate includes only days away from work (DAFWII). The rates are calculated annually per 100 full-time equivalent workers.[9]

The ODI data were then used to implement OSHA's SST plan. To target high-rate establishments, the SST plan prioritized establishments using case-rate cutoffs. For example, the

---

[8] In 2002, the DART replaced a rate that includes only cases involving lost work days due to injury or illness (LWDII), though the DART and the LWDII are nearly identical.
[9] To calculate 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.

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 inspection calendar for other SST cycles are provided in the Appendix Table.) 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 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 rate was 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, not all establishments were inspected, and those that were inspected did not always have the highest case rates (US Department of Labor 2012).[10] The low inspection rate was attributed, in part, to limited resources (US Department of Labor 2012). To address this limitation, each of the 81 OSHA Area Offices determined the number of establishments it could reasonably inspect, and then randomly selected a subset of establishments for inspection. However, treatment assignment did not perfectly predict inspection outcomes (Johnson, Levine, and Toffel 2017).

Importantly, the ODI data were collected in 45 US states and the District of Columbia, but the SST plan was implemented in only 35 states. This includes 29 states that are covered directly by OSHA and 6 states that are covered by state-level agencies – known as State Plans – approved by OSHA.

[10] For example, from August 2010 through September 2011, only 16 percent of establishments on the primary and secondary inspection list were ultimately inspected.

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.

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 based on treatment: worker safety without an inspection, denoted $Y_i(0)$, and worker safety with an 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:

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

In this case, the likelihood of treatment increases at the cutoff c. Second, the likelihood of an inspection increases monotonically at the cutoff. This implies that the increase in inspections at the cutoff is due only 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 (Imbens and Lemieux 2008). Third, the conditional mean functions $E[Y(0)|X_i = x]$ and $E[Y(1)|X_i = x]$ are continuous near the cutoff 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) $$\tau_{FRD} = \frac{\lim_{x \downarrow c} E[Y|X = x] - \lim_{x \uparrow c} E[Y|X = x]}{\lim_{x \downarrow c} E[D|X = x] - \lim_{x \uparrow c} E[D|X = x]}.$$

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.

Distributional Effects

The FRD estimand measures the average treatment effect 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:

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

Below the cutoff, the conditional CDF is given by:

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

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 $F_{Y(1)|\Omega}(y) - F_{Y(0)|\Omega}(y)$.

Estimation

Treatment effects are estimated using nonparametric, local linear regression. An advantage of local linear regression is that observations can be weighted more near the cutoff where the estimands are evaluated (Imbens and Lemieux 2008). For example, the term $\lim_{x \downarrow c} E[Y|X = x]$ is estimated by solving

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

The term $X_i - c$ is the distance of observation i to the cutoff c, among establishments with X_i betweeb c and $c + h_{YR}$, so that α_{YR} corresponds to $\lim_{x \downarrow c} E[Y|X = x]$. The parameters are estimated by minimizing the sum of the squared deviations, weighted by the kernel function $\mathrm{K}\left(\frac{X_i - c}{h}\right)$. Estimation is accomplished using a procedure developed by Calonico, Cattaneo, and Titiunik (2014), which estimates the optimal bandwidth h and provides a robust, bias-correction for $\hat{\tau}_{FRD}$. The standard errors are clustered by establishment. The kernel function $\mathrm{K}(.)$ is triangular.

Data

Data for the running variable X_i and the outcome variable Y_i come from OSHA's Data Initiative (ODI). The data were collected annually from 1996 to 2011, with approximately 60,000 to 80,000 establishments surveyed each year. The goal of the ODI was to survey all establishments meeting the target criteria at least once every three years.[11] The survey targeted establishments in manufacturing and other industries with injury rates above the national average, excluding establishments in construction and with fewer than 40 employees.[12] In regard 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. Case rates are measured per 100 full-time equivalent workers.

To construct the analysis sample, the ODI data were first pooled across years 1996 to 2011, yielding 1,018,600 establishment-by-year observations. Observations were dropped if they appear to be a duplicate record or if the establishment's name and address are missing, eliminating 0.46 percent of the sample. The observations were then stacked by establishment based on the establishment's name and address, yielding 341,302 unique establishments, of which 188,178 have more than one observation.[13]

The data were then limited to pairs of observations spaced four calendar years apart, yielding 252,382 paired observations. The first observation is used for the running variable X_i, and second observation is used for the outcome variable Y_i. The lag of four years was chosen so that the second observation corresponds to the first calendar year after the SST plan. For

[11] OSHA identifies establishments using Dun & Bradstreet data.
[12] In 1999, the ODI excluded establishments with fewer than 60 employees.
[13] Establishment name and address were standardized before linking. See Appendix for more details.

example, the data for 2002, collected in 2003, were used to target programmed inspections from April 2004 to August 2005. Thus, the outcome variable Y_i is measured in 2006.

Three additional restrictions are imposed on the sample. First, the sample is restricted to years 1997 to 2011, since data in 1996 were not used to implement the SST plan. Second, the sample is restricted to states that participated in the SST plan, which includes all 29 states under federal jurisdiction with respect to OSHA and six states that operate state plans. Third, observations pairs are excluded if the case rate from the ODI is missing or exceeds 100, eliminating 1.9 percent of the sample.[14] The remaining sample contains 154,808 paired observations among 61,702 unique establishments, for an average of 2.5 paired observations per establishment. 25,460 establishments are observed only once.

The cutoff c was identified from administrative reports from the SST plan. The cutoffs varied by inspection list (primary versus secondary), case type, industry, and SST cycle (see Appendix Table).

To measure the inspection indicator D_i, the ODI data are merged to OSHA's IMIS. These data contain the universe of OSHA inspections as of September, 2016. For each inspection, the data report the type of inspection, 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 only during the SST plan cycle. Thus, in the example above, D_i equals one if an establishment matches to an inspection record in the IMIS from April 2004 to August 2005 and zero otherwise.

[14] This restriction eliminates extreme outliers, but has no impact on the results.

The ODI data were merged to the IMIS based on the name and address of the establishment, including the street number, street name, city, state, and zip code.[15] Although the data were cleaned and standardized before matching, there may be both false-negative and false-positive matches. A false-negative match occurs if an establishment had been inspected during the SST cycle, but did not match to its inspection record in the IMIS. A concern is that, if the match rate is biased downward by a proportional factor $0 < \pi < 1$, then the estimate of τ_{FRD} would be biased upwards by a factor of $1/\pi$. Conversely, false-positive matches could lead to a downward bias, but this is less of a concern given the stringency of the matching criteria. Our best estimate of π is 82.7 percent, which is the match rate to the IMIS among establishment that, according to administrative records of the SST plan, had completed the SST cycle. Thus, the estimate of τ_{FRD} ranges from $\hat{\pi}\hat{\tau}_{FRD}$ to $\hat{\tau}_{FRD}$.

The covariates 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 Standard Industrialization Classification codes (SIC). Using the SIC codes, 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. Using the FMCS data, the union indicator variable equals one if there is any union activity from 2004 to 2016 and zero otherwise. It should be noted that not all

[15] Additional details of the merging procedure are provided in the Appendix.

union establishments are expected to have filed with the FMCS during the data period, so union status is measured with error, particularly with false-negative errors.[16]

Sample Summary

We initially focus on the DART cutoff for the primary inspection list. This cutoff is located near the top of the DART rate distribution. In column one of Table 1, the mean DART rate was 7.33, and the mean cutoff was 13.67. In columns two and three, the sample is split between establishments above and below the DART rate cutoff for the primary inspection list. According to the number of observations, only 14.08 percent of establishments exceeded the cutoff. The distribution of the DART rate relative to the cutoff is illustrated in Figure 1. As shown, the distribution is skewed to the right.

According to Table 1, the likelihood of a programmed inspection was greater above the cutoff than below: 30.3 percent and 5.1 percent, respectively. However, this difference pertains to all establishments above and below the cutoff, not necessarily at the cutoff. To illustrate the change at the cutoff, Figure 2 plots the likelihood of a programmed inspection by the DART rate relative to the cutoff. The markers denote the mean outcome within intervals of 0.5, and the lines are derived from local linear regression, estimated separately above and below the cutoff. As shown, the increase in inspections occurs at cutoff, as required for identification using the FRD design. Additionally, the increase in programmed inspections led to greater rates of citations and penalties (Figure 2). In contrast, the likelihood of an unprogrammed inspection, which is unrelated to the SST plan, did not change at the cutoff (Figure 2).

[16] In the Appendix, we compare union status information in the IMIS to the match rate to the FMCS. We find that a match to the FMCS is highly correlated with union status.

The FRD model assumes that, despite the discontinuity in inspections, the conditional mean functions $E[Y(0)|X_i = x]$ and $E[Y(1)|X_i = x]$ are continuous. This assumption is supported by two observations. First, the density of the DART rate is smooth near the cutoff, as shown in Figure 1.[17] This suggests that establishments do not bunch just below the cutoff to avoid inspection. This seems reasonable, since establishments report their DART rates before the SST cutoffs are determined.[18] It also suggests that inspections did not affect firm survival. For example, if inspections negatively affected firm survival, then there would be greater density just below the cutoff than above.

Second, establishments appear similar just above and below the SST cutoff with respect to observable characteristics. According to column one of Table 1, approximately 61.0 percent of establishments are in manufacturing, 17.5 percent are in health services, and 12.5 percent exhibit union activity, according to FMCS data. Figure 3 plots these characteristics relative to DART rate, which show no measurable change at the cutoff.[19] The figure also plots the likelihood of an inspection during the year before the SST cycle. As shown, there is no discontinuity in the likelihood at the cutoff.[20]

Table 1 also shows that the case rates four years later are substantially lower than the baseline case rates, denoted by the subscripts $t + 4$ and t, respectively. Among all

[17] A test by McCrary (2008) rejects that there is a discontinuity in the distribution at the cutoff. The smoothness at the cutoff also suggests that the increase in inspections did not affect firm survival.

[18] In some years, the cutoffs remained unchanged (Appendix Table), allowing establishments to form expectations of the cutoffs over time. As a robustness check, we limit the analysis to establishments first observed when a new SST cutoff was implemented.

[19] Using local linear regression, the changes in these characteristics at the cutoff are small and statistically insignificant.

[20] Using local linear regression, the discontinuity in the likelihood of an inspection during the calendar year before the first observation in the ODI is .007 percent and statistically insignificant.

establishments, the TCR decreases from 12.8 to 9.5, and the DART decreases from 7.3 to 5.7. These decreases are greater among establishments above the cutoff: the TCR decreases from 27.0 to 14.4, and the DART decreases from 19.1 to 9.5. This is consistent with mean reversion in case rates (Ruser 1995), particularly at the top of the case rate distribution. This is also consistent with a general decrease in case rates over time.[21] These factors should not invalidate the identification strategy, however, if their impacts are similar above and below the cutoff.

Results

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 percentage point increase in programmed inspections, a 17.6 percentage point increase in citations, and a 15.7 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.

The nature and severity of the citations and penalties are examined using the FRD estimand in equation (2), where the treatment variable is a programmed inspection and the outcome variable is the number of citations or the penalty amount.[22] Among compliers at the

[21] Among the full sample, the mean TCR decreased from 13.17 in 1996 to 6.23 in 2011.
[22] The model includes the full set of control variables: year fixed effects, state fixed effects, industry fixed effects, and an indicator for union activity.

cutoff, a programmed inspection increased the average penalty by $6,156 in 2009 dollars, with a standard error of $1,011 (not shown). Table 3 presents the results for the number of all citations and of the nine most common citations among the analysis sample. The most common citations are associated with manufacturing, with the exception of "bloodborne pathogens". As shown in the table, a programmed inspection increased the number of all citations by 5.06 and the top nine citations combined by 1.34.

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. The first panel plots the TCR, and the second panel plots the DART. In both panels, the mean case rate appears to decrease discontinuously at the cutoff, suggesting that inspections improved 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 4 presents the baseline estimates separately for the TCR and the DART. As shown, an inspection decreases both the TCR and the DART. However, the standard errors do not rule out a large range of effects, and only the effect on the DART 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.[23] Relative to the post-inspection DART rate near the cutoff of

[23] Mentioned above, false-negative matches of the ODI to the IMIS may lead to overestimating the effect of workplace inspections on worker safety. Using a bias factor of 1/0.827, the estimated effect on the TCR ranges from -0.471 to -0.569, and the estimated effect on the DART rate ranges from -1.329 to -1.607.

eight (Figure 4), the effect on the DART amounts to a decline of approximately 20 percent. The estimates are similar with the inclusion of covariates: -0.769 and -1.792, respectively.

The right side of Table 4 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. With covariates, the estimated effect on the DART is -2.068. 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 more than 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.

Robustness to Bandwidth and Order of Polynomial

In Table 5, we examine the robustness of the baseline results with respect to the order of the polynomial and the bandwidth. In Table 4, the order of the polynomial is one, and the bandwidth is chosen optimally using the procedure developed by Calonico, Cattaneo, and Titiunik (2014). Under these specifications, and controlling for observable characteristics, the optimal bandwidth is 3.17, and the estimated effect of an inspection on the DART rate is -1.792. In Table 5, the order of the polynomial varies across rows, from one to three, and the bandwidth varies across columns, from 50 percent to 150 percent of the optimal bandwidth 3.17. As shown, the estimated effect is negative in all specifications, ranging from -1.101 to -2.650. Moreover, the estimates are more statistically significant with either a narrow bandwidth and a lower-order polynomial or a large bandwidth and a higher-order polynomial. This makes sense intuitively, as a larger bandwidth requires a more flexible function form with respect to the running variable.

Instrumental Variable Regression

In Table 6, we examine the robustness of the baseline results using instrumental variable (IV) regression, rather than local linear regression. The IV regression model takes the following form:

$$Y_i = \gamma + \delta D_i + f(X_i - c) + I(X_i \geq c)g(X_i - c) + Z_i + \varepsilon_i.$$

As before, Y_i is the worker safety outcome, and D_i is an indicator of an inspection. The instrument is an indicator of whether an establishment's case rate exceeds the cutoff $I(X_i \geq c)$. The model controls for a polynomial of the running $f(X_i - c)$, a separate polynomial for establishments exceeding the cutoff $g(X_i - c)$, and observable characteristics Z_i. By including the polynomials, the coefficient of interest δ represents the causal effect of an inspection on the worker safety outcome at the cutoff.

The estimates of the model are presented in Table 6. Similar to Table 5, the order of the polynomial varies across rows, and the bandwidth varies across columns. As shown, the estimated effect is negative in all specifications. With narrow bandwidth (50 percent) and a first-order polynomial, the estimated effect is -2.059. With a larger bandwidth (150 percent) and a third-order polynomial, the estimated effect is -2.072. These estimates are similar in magnitude to the baseline estimate of -1.792 in Table 4.

Alternative Samples

In Table 7, we examine the effect of an inspection on worker safety using alternative samples. The baseline estimate of -1.792 is reported in column one. In columns two and three, we consider longer lags between the first and second observations. In column two, the observations are spaced five years apart, and, in column three, the observations are spaced six years apart. The longer lag decreases the sample size, which may reflect that some establishments no longer exist. In both columns, the estimates are smaller, positive, and statistically insignificant. This suggests that the effect of an inspection on worker safety may be ephemeral. However, the larger standard errors, due in part to fewer observations, do not rule out a wide range of effects.

In columns four through six, we focus on establishments that are less able to anticipate the SST plan and the DART cutoff. In column four, the sample is restricted to establishments that are observed exactly twice, spaced four years apart. In column five, the sample is restricted to the earliest paired observation. In column six, the sample is restricted to the earliest paired observation in the first year a new cutoff was implemented. These restrictions decrease the sample size considerably, with only 13,101 observations in column six. Nonetheless, the estimates remain negative, though statistically insignificant, ranging from -1.109 to -1.973.

Distributional Effects

The effect of an inspection may vary across the post-inspection rate distribution, conditional on being near the 85th percentile pre-inspection. 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.

Figures 5 and 6 illustrate the estimated distributional effects for the DART rate. The first panel in Figure 5 plots the estimates of $F_{Y(1)|\Omega}(y)$ and $F_{Y(0)|\Omega}(y)$, and Figure 6 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 of the post-inspection rate distribution, conditional on being near the 85th percentile pre-inspection.

Effects by Industry and Union Activity

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 8 presents mean effects, and Figure 5 illustrate distributional effects. For brevity, the estimates of $F_{Y(1)|\Omega}(y)$ and $F_{Y(0)|\Omega}(y)$ are plotted, but not their differences.

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 -1.050 per 100 full-time equivalent workers, compared to 0.626 for health services and -0.124 for "other" industries. However, none of the mean 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 effect 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 to workplace inspections, in particular (Morantz 2009; Morantz 2013).

The right side of Table 8 presents the mean effects, and Figure 5 illustrates the distributional effects. According to the results, the effect of an inspection on worker safety is qualitatively similar among establishments with and without union activity. In Table 8, the estimates for union and non-union establishments are -3.546 and -1.413, 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 establishments with and without union activity, though, as noted above, our indicator of union activity is measured with error with respect to unionization.

Additional Considerations

Secondary Inspection List and Letter

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.

Regarding 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. These findings are illustrated in Figure 7. 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.[24] Similarly, the cutoff for the letter is associated with a small increase in programmed inspections, but there is no measurable change in worker safety. These findings are illustrated in Figure 8. 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.

ODI Data Recorded in 1996 and Collected in 1997

ODI data were recorded in 1996 and collected in 1997, but these data were not used to implement the SST plan. Thus, as a placebo test, the empirical analysis is repeated for the ODI

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

data recorded in 1996 as if the SST plan had been implemented. The same sample restrictions are imposed on these data, including limiting the analysis to states under federal jurisdiction. Establishments observed in 1996 are assigned the DART cutoff for the primary inspection list as if they were observed in 1997.

As expected, the data reveal no discontinuity in either programmed inspections or the DART rate. These findings are illustrated in Figure 9. Using local linear regression, the discontinuity in programmed inspections is 3.56 percentage points, with a standard error of 4.66, and the discontinuity in the DART rate is -0.496, with a standard error of 0.711.

Non-Participating States

ODI data were collected in 45 US states and the District of Columbia, but the SST plan was only implemented in 35 states. Thus, as a placebo test, the empirical analysis is repeated for states where ODI data were collected, but the SST plan had not been implemented. Establishments in non-participating states are assigned the DART cutoff for the primary inspection list as if they resided in states that implemented the SST plan.

Again, the data reveal no discontinuity in either programmed inspections or the DART rate. These findings are illustrated in Figure 10. Using local linear regression with covariates, the discontinuity in programmed inspections is -0.062 percentage points, with a standard error 1.12, and the discontinuity in the DART rate is -0.283, with a standard error of 0.284. 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.

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. The effect is identified specifically among establishments near the 85th percentile of the DART rate distribution pre-inspection that were inspected as a result of the SST plan. Using the fuzzy regression discontinuity design and local linear regression, the causal effect of an inspection on the DART rate is approximately -1.792 per 100 full-time equivalent workers. Relative to the mean, this effect is a reduction of approximately 20 percent. The effect is most evident for manufacturing establishments below the 90th percentile of the DART rate distribution post-inspection.

The estimated effect of an OSHA inspection on worker safety found in this study is large compared to related studies. As noted, most studies find little to no effect of inspections on worker safety, and studies that do find effects may suffer from statistical biases or lack generalizability. However, it is difficult to reconcile this study to most related studies, since they differ in regards to identification strategy, data, population of interest, and worker safety outcomes.

Regarding efficiency, an important question is whether the gains from the additional inspections exceed the marginal costs. According to Viscusi and Aldy (2003), the value of statistical injury ranges from $20 thousand to $70 thousand. If equated to the DART, the mean effect of an inspection on the DART rate of -1.792 ranges in value from $35.8 thousand to $125.4 thousand annually per 100 full-time equivalent workers. This range represents the average private gain of an inspection and excludes fiscal externalities through, for example, social insurance programs. The marginal cost includes the cost of an inspection, which equaled

$6.5 thousand on average in 2016, as well as compliance costs to employers.[25] Although compliance costs are difficult to estimate, they can be bounded to determine efficiency.[26] For example, in an establishment of 40 employees, the minimum establishment size in the ODI, an inspection would inefficient if compliance costs exceeded roughly $7.8 thousand to $43.7 thousand plus the external gains from reducing workplace injuries.[27] To improve efficiency, OSHA should target establishments for inspection in which the effect on worker safety is greatest. In this study, the effect is most evident in manufacturing and less evident in health services, the largest two-digit industries represented in the ODI data.

[25] Financial penalties are direct monetary transfers from establishments to OSHA and thus do not affect social welfare.

[26] The average cost of an inspection is derived by dividing the total OSHA budget on federal enforcement of $208 million by the number of federal OSHA inspections of 31,948. For a thorough discussion on compliance costs, see Kniesner and Leeth (2014).

[27] This calculation assumes that the average cost of an inspection for an establishment with 40 employees is equal to $6.5 thousand, the average cost of an inspection among all establishments in 2016. However, it is likely that that the costs of an inspection and compliance increase with establishment size. In 2016, the median size of establishments inspected by OSHA was 11.

References

Calonico, Sebastian, Matias Cattaneo, and Rocio Titiunik. 2014. Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica* 82(6): 2295-2326.

Cooke, William and Frederick Gautschi. 1981. OSHA, Plant Safety Programs, and Injury Reduction. *Industrial Relations* 20(3): 245-257.

DiNardo, John and David Lee. 2004. Economic Impacts of New Unionization on Private Sector Employees: 194-2001. *Quarterly Journal of Economics* 199(4): 1383-1442.

Eaton, Adrienne and Thomas Nocerino. 2000. The Effectiveness of Health and Safety Committees: Results of a Survey of Public-Sector Workplaces. *Industrial Relations* 39: 265–290.

Frandsen, Brigham, Markus Frolich, and Blaise Melly. 2012. Quantile Treatment Effects in the Regression Discontinuity Design. *Journal of Econometrics* 168: 382-395.

Gray, Wayne and John Scholz. 1993. Does Regulatory Enforcement Work? A Panel Analysis of OSHA Enforcement. *Law & Society Review* 177-214.

Hahn, Jinyong, Petra Todd, and Wilbert Van der Klaauw. 2001. Identification and Estimation of Treatment Effects with Regression-Discontinuity Design. *Econometrica* 69(1): 201-209.

Haviland, Amelia, Rachel Burns, Wayne Gray, Teague Ruder, and John Mendeloff. 2010. What Kinds of Injuries do OSHA Inspections Prevent? *Journal of Safety Research* 41(4): 339-345.

Holland, Paul. 1986. Statistics and Causal Inference. *Journal of the American Statistical Association* 81(396): 945-960.

Imbens, Guide and Thomas Lemieux. 2008. Regression Discontinuity Designs: A Guide to Practice. *Journal of Econometrics* 142: 615-635.

Johnson, Matthew, David Levine, and Michael Toffel. 2017. Improving Regulatory Effectiveness through Better Targeting: Evidence from OSHA. Mimeo.

Kniesner, Thomas and John Leeth. 2014. Chapter 9: Regulating Occupational and Product Risks. *Handbook of the Economics of Risk and Uncertainty*, *Volume 1* Eds. M. Machina and K. Viscusi. Amsterdam: Elsevier.

Leigh, Paul. 2011. Economic Burden of Occupational Injury and Illness in the United States. *Milbank Quarterly* 89(4): 728-772.

Levine, Toffel, and Matthew Johnson. 2012. Randomized Government Safety Inspections Reduce Worker Injuries with No Detectable Job Loss. *Science* 336: 907-911.

Li, Ling, Shawn Rohlin, and Perry Singleton. 2017. Labor Unions and Occupational Safety: Event-Study Analysis Using Unions Elections.” Working Paper 205, Center for Policy Research, Maxwell School, Syracuse University.

McCaffrey, David. 1983. An Assessment of OSHA’s Recent Effects on Injury Rates. *Journal of Human Resources* 18(1): 131-146.

McCrary, Justin. 2008. Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test. *Journal of Econometrics* 142.2: 698-714.

Mendeloff, John and Wayne Gray. 2005. Inside the Black Box: How Do OSHA Inspections Lead to Reductions in Workplace Injuries? *Law & Policy* 27(2): 219-237.

Morantz, Alison. 2009. The Elusive Union Safety Effect: Toward a New Empirical Research Agenda. LERA 61st Annual Proceedings.

Morantz, Alison. 2013. Coal Mine Safety: Do Unions Make A Difference? *ILR Review* 66(1): 88-116.

Peto, Valint, Laura Hoesly, George Cave, David Kretch, and Ed Dieterle. 2016. Evaluation of the Occupational Safety and Health Administration’s Site-Specific Targeting Program – Final Report. Summit Reporting.

Rubin, Donald 1974. Estimating Causal Effects of Treatments in Randomized and Non-Randomized Studies. *Journal of Educational Psychology* 66: 688-701.

Ruser, John and Robert Smith. 1988. The Effect of OSHA Records-Check Inspections on Reported Occupational Injuries in Manufacturing Establishments. *Journal of Risk and Uncertainty* 1(4): 415-435.

Ruser, John and Robert Smith. 1991. Reestimating OSHA’s Effects: Have the Data Changed? *Journal of Human Resources* 26(2): 212-235.

Ruser, John. 1995. Self-Correction versus Persistence of Establishment Injury Rates. *Journal of Risk and Insurance* 62(1): 67-93.

Smith, Robert. 1979. The Impact of OSHA Inspection on Manufacturing Injury Rates. *Journal of Human Resources* 14(2): 145-170.

US Department of Labor. 2012. OSHA’s Site Specific Targeting Program Has Limitations on Targeting and Inspecting High-Risk Worksites. Office of Inspector General – Office of Audit. Report Number 02-12-202-10-105.

Viscusi, W. Kip and Joseph Aldy. 2003. The Value of a Statistical Life: A Critical Review of Market Estimates Throughout the World. *Journal of Risk and Uncertainty* 27(1): 5-76.

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 five 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. Second, the first criteria is repeated using the second name, if applicable. Third, establishments are matched based on establishment name and 5-digit zip code within the same city and state. Fourth, establishments are matched based on the first six letters of the establishment name and street address (excluding spaces). Fifth, 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. Among establishments with a match, 57 percent match using the first criteria, two percent match using the second criteria, 16 percent match using the third criteria, 18 percent match using the fourth criteria, and seven percent match using the fifth criteria.

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 with the FMCS to modify an existing contract, a notice indicates 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.

Appendix Table: SST Timing and Cutoffs

ODI Data		SST Cycle		ODI	Primary Inspection List					
					DART			DAFWII		
Recorded	Collected	Begin	End	Outcome	SIC 20-39	SIC 80	Other	SIC 20-39	SIC 80	Other
1996	1997	-	-	2000	-	-	-	-	-	-
1997	1998	19-Apr-99	4-Feb-00	2001	16	24	16	-	-	-
1998	1999	4-Feb-00	13-Jul-01	2002	14	14	14	-	-	-
1999	2000	13-Jul-01	13-Jul-02	2003	14	14	14	-	-	-
2000	2001	7-Jul-02	10-Jun-03	2004	14	-	14	-	-	-
2001	2002	10-Jun-03	19-Apr-04	2005	14	17	14	9	-	9
2002	2003	19-Apr-04	5-Aug-05	2006	15	17.75	15	10	-	10
2003	2004	5-Aug-05	12-Jun-06	2007	12	14.65	12	9	-	9
2004	2005	12-Jun-06	14-May-07	2008	12	15.15	12	9	-	9
2005	2006	14-May-07	19-May-08	2009	11	14.17	11	9	-	9
2006	2007	19-May-08	20-Jul-09	2010	11	13.7	11	9	-	9
2007	2008	20-Jul-09	22-Oct-10	2011	8	17	15	6	14	13
2008	2009	22-Oct-10	9-Sep-11	-	7	16	15	-	-	-
2009	2010	9-Sep-11	4-Jan-13	-	7	16	15	5	13	14
2010	2011	4-Jan-13	2-Feb-14	-	7	-	15	5	-	14
2011	2012	2-Feb-14	2-Feb-15	-	7	-	15	5	-	14

Appendix Table (continued): SST Timing and Cutoffs

ODI Data		Secondary Inspection List						
		DART			DAFWII			
Recorded	Collected	SIC 20-39	SIC 80	Other	SIC 20-39	SIC 80	Other	Letter
1996	1997	-	-	-	-	-	-	-
1997	1998	10	-	10	-	-	-	8
1998	1998	8	8	8	-	-	-	8
1999	1998	8	8	8	-	-	-	8
2000	1998	8	8	8	-	-	-	8
2001	1998	8	8	8	4	4	4	6
2002	1998	8	-	8	4	-	4	7
2003	1998	7	-	7	5	-	5	6.5
2004	1998	7	-	7	5	-	5	6
2005	1998	7	-	7	4	-	4	5.3
2006	1998	7	-	7	5	-	5	5.4
2007	1998	6	15	6	4	11	4	5
2008	1998	5	13	7	4	11	5	4.5
2009	1998	5	13	5	4	11	4	2.5
2010	1998	5	-	7	4	-	5	2
2011	1998	5	-	7	4	-	5	-

OSHA's Form 300 (Rev. 01/2004)

Log of Work-Related Injuries and Illnesses

Attention: This form contains information relating to employee health and must be used in a manner that protects the confidentiality of employees to the extent possible while the information is being used for occupational safety and health purposes.

Year 20__ __

U.S. Department of Labor
Occupational Safety and Health Administration

Form approved OMB no. 1218-0176

You must record information about every work-related death and about every work-related injury or illness that involves loss of consciousness, restricted work activity or job transfer, days away from work, or medical treatment beyond first aid. You must also record significant work-related injuries and illnesses that are diagnosed by a physician or licensed health care professional. You must also record work-related injuries and illnesses that meet any of the specific recording criteria listed in 29 CFR Part 1904.8 through 1904.12. Feel free to use two lines for a single case if you need to. You must complete an Injury and Illness Incident Report (OSHA Form 301) or equivalent form for each injury or illness recorded on this form. If you're not sure whether a case is recordable, call your local OSHA office for help.

Establishment name ______________________

City ______________________ State ______________

Identify the person			Describe the case			Classify the case: CHECK ONLY ONE box for each case based on the most serious outcome for that case:				Enter the number of days the injured or ill worker was:		Check the "Injury" column or choose one type of illness: (M)					
(A) Case no.	(B) Employee's name	(C) Job title (*e.g., Welder*)	(D) Date of injury or onset of illness	(E) Where the event occurred (*e.g., Loading dock north end*)	(F) Describe injury or illness, parts of body affected, and object/substance that directly injured or made person ill (*e.g., Second degree burns on right forearm from acetylene torch*)	Death (G)	Days away from work (H)	Remained at Work: Job transfer or restriction (I)	Remained at Work: Other record-able cases (J)	Away from work (K)	On job transfer or restriction (L)	Injury (1)	Skin disorder (2)	Respiratory condition (3)	Poisoning (4)	Hearing loss (5)	All other illnesses (6)
			/ month/day			☐	☐	☐	☐	___ days	___ days	☐	☐	☐	☐	☐	☐
			/ month/day			☐	☐	☐	☐	___ days	___ days	☐	☐	☐	☐	☐	☐
			/ month/day			☐	☐	☐	☐	___ days	___ days	☐	☐	☐	☐	☐	☐
			/ month/day			☐	☐	☐	☐	___ days	___ days	☐	☐	☐	☐	☐	☐
			/ month/day			☐	☐	☐	☐	___ days	___ days	☐	☐	☐	☐	☐	☐
			/ month/day			☐	☐	☐	☐	___ days	___ days	☐	☐	☐	☐	☐	☐
			/ month/day			☐	☐	☐	☐	___ days	___ days	☐	☐	☐	☐	☐	☐
			/ month/day			☐	☐	☐	☐	___ days	___ days	☐	☐	☐	☐	☐	☐
			/ month/day			☐	☐	☐	☐	___ days	___ days	☐	☐	☐	☐	☐	☐
			/ month/day			☐	☐	☐	☐	___ days	___ days	☐	☐	☐	☐	☐	☐
			/ month/day			☐	☐	☐	☐	___ days	___ days	☐	☐	☐	☐	☐	☐
			/ month/day			☐	☐	☐	☐	___ days	___ days	☐	☐	☐	☐	☐	☐
			/ month/day			☐	☐	☐	☐	___ days	___ days	☐	☐	☐	☐	☐	☐
					Page totals	___	___	___	___	___	___	___ Injury (1)	___ Skin disorder (2)	___ Respiratory condition (3)	___ Poisoning (4)	___ Hearing loss (5)	___ All other illnesses (6)

Be sure to transfer these totals to the Summary page (Form 300A) before you post it.

Public reporting burden for this collection of information is estimated to average 14 minutes per response, including time to review the instructions, search and gather the data needed, and complete and review the collection of information. Persons are not required to respond to the collection of information unless it displays a currently valid OMB control number. If you have any comments about these estimates or any other aspects of this data collection, contact: US Department of Labor, OSHA Office of Statistical Analysis, Room N-3644, 200 Constitution Avenue, NW, Washington, DC 20210. Do not send the completed forms to this office.

Table 1: Summary Statistics

	All	DART<Cutoff	Dart>=Cutoff
TCR t	12.81	10.49	26.96
	(0.03)	(0.02)	(0.08)
DART t	7.33	5.41	19.06
	(0.02)	(0.01)	(0.05)
Cutoff	13.67	13.71	13.41
	(0.01)	(0.01)	(0.02)
Inspection, programmed	0.086	0.051	0.303
	(0.001)	(0.001)	(0.003)
Citation, programmed	0.064	0.036	0.230
	(0.001)	(0.001)	(0.003)
Penalty, programmed	0.057	0.033	0.203
	(0.001)	(0.000)	(0.003)
Inspection, unprogrammed	0.046	0.044	0.062
	(0.001)	(0.001)	(0.002)
Manufacturing	0.610	0.622	0.536
	(0.001)	(0.001)	(0.003)
Health services	0.175	0.177	0.166
	(0.001)	(0.001)	(0.003)
Union activity	0.125	0.122	0.143
	(0.001)	(0.001)	(0.002)
TCR t+4	9.51	8.71	14.38
	(0.02)	(0.02)	(0.06)
DART t+4	5.69	5.08	9.47
	(0.01)	(0.01)	(0.05)
Establishments	61,702	55,247	6,455
Observations	154,808	133,013	21,795

The sample is derived from ODI. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The TCR is the rate of cases involving death, days away from work, job transfers and restrictions, and medical attention beyond first aid; the DART includes cases involving days away from work and job transfers and restrictions; the DAFWII includes cases involving days away from work. All rates are measured per 100 full-time employees. The subscript t denotes the first of the two observations; the subscript t+4 denotes the second. The cutoff is the DART rate cutoff for the primary inspection list. The inspection outcomes come from OSHA's IMIS.

Table 2: Discontinuity in Inspection Outcomes

Inspection Outcome	Programmed	Citation	Penalty	Unprogrammed
A. Without covariates				
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$	0.227***	0.176***	0.157***	0.005
	(0.013)	(0.011)	(0.011)	(0.006)
Bandwidth h	3.57	3.68	3.55	6.06
B. With covariates				
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$	0.224***	0.174***	0.155***	0.006
	(0.012)	(0.011)	(0.011)	(0.006)
Bandwidth h	3.62	3.74	3.63	4.76
Observations	154,808	154,808	154,808	154,808

The sample is derived from ODI. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The estimates come from a regression discontinuity model using local linear regression, with inspections as the outcome variable. The data on inspections come from OSHA's IMIS. The covariates include year fixed effects, state fixed effects, industry fixed effects (manufacturing, health services, and other), and an indicator of union status. The parameter α_{DR} represents the mean outcome just above the SST cutoff; the parameter α_{Dl} represents the mean outcome just below the SST cutoff. ***, **, and * indicate statistical significance at the one, five, and ten percent levels, respectively.

Table 3: Effect of Inspection on Citations

Citation category	$\hat{\tau}_{FRD}$
All citations	5.063***
	(0.367)
The control of hazardous energy (lockout/tagout)	0.199***
	(0.026)
Wiring design and protection	0.213***
	(0.026)
General requirements for all machines	0.199***
	(0.028)
Electrical, general	0.184***
	(0.026)
Hazard communication	0.167***
	(0.025)
Respiratory protection	0.097***
	(0.016)
Mechanical power-transmission apparatus	0.102***
	(0.021)
Abrasive wheel machinery	0.069***
	(0.021)
Bloodborne pathogens	0.111***
	(0.018)

The sample is derived from ODI. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The estimates come from a regression discontinuity model using local linear regression, with the number of citations as the outcome variable and a programmed inspection as the treatment variable. The data on inspections come from OSHA's IMIS. The covariates include year fixed effects, state fixed effects, industry fixed effects (manufacturing, health services, and other), and an indicator of union status. ***, **, and * indicate statistical significance at the one, five, and ten percent levels, respectively.

Table 4: Effect of Inspection on Worker Safety

ODI Year	1997-2007		1998-2007		
Outcome Variable	TCR	DART	TCR	DART	DAFWII
A. Without covariates					
$\hat{\tau}_{FRD}$	-0.569	-1.607**	-1.294	-1.877**	-0.511
	(1.143)	(0.787)	(1.121)	(0.844)	(0.639)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$	0.227***	0.227***	0.217***	0.218***	0.217***
	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)
Bandwidth h	3.65	3.57	3.90	3.56	3.63
B. With covariates					
$\hat{\tau}_{FRD}$	-0.769	-1.792**	-1.717	-2.068**	-0.554
	(1.150)	(0.814)	(1.215)	(0.872)	(0.607)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$	0.224***	0.224***	0.215***	0.215***	0.215***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Bandwidth h	3.45	3.17	3.25	3.13	3.57
Observations	154,808	154,808	139,220	139,220	139,220

The sample is derived from ODI. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The estimates come from a regression discontinuity model using local linear regression, with the case rate as the outcome variable and a programmed inspection as the treatment variable. The data on inspections come from OSHA's IMIS. The covariates include year fixed effects, state fixed effects, industry fixed effects (manufacturing, health services, and other), and an indicator of union status. The parameter α_{DR} represents the mean outcome just above the SST cutoff; the parameter α_{Dl} represents the mean outcome just below the SST cutoff. ***, **, and * indicate statistical significance at the one, five, and ten percent levels, respectively.

Table 5: Effect of Inspection on Worker Safety by Bandwidth and Order of Polynomial

	Order of Polynomial	Bandwidth 50%	75%	100%	125%	150%
$\hat{\tau}_{FRD}$	1	-2.446**	-1.880*	-1.792**	-1.508**	-1.101
		(1.235)	(0.968)	(0.814)	(0.762)	(0.708)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$		0.208***	0.224***	0.224***	0.227***	0.226***
		(0.018)	(0.015)	(0.013)	(0.012)	(0.011)
$\hat{\tau}_{FRD}$	2	-1.903	-2.489**	-2.144*	-2.026**	-2.129**
		(1.829)	(1.403)	(0.156)	(1.012)	(0.922)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$		0.187***	0.204***	0.217***	0.223***	0.226***
		(0.025)	(0.020)	(0.018)	(0.016)	(0.015)
$\hat{\tau}_{FRD}$	3	-2.360	-2.268	-2.650	-2.306*	-2.056*
		(2.275)	(1.957)	(1.620)	(1.371)	(1.207)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$		0.192***	0.183***	0.194***	0.207***	0.216***
		(0.031)	(0.026)	(0.022)	(0.020)	(0.018)
Bandwidth h		1.58	2.37	3.17	3.96	4.75
Observations		154,808	154,808	154,808	154,808	154,808

The sample is derived from ODI. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The estimates come from a regression discontinuity model using local linear regression, with the case rate as the outcome variable and a programmed inspection as the treatment variable. The data on inspections come from OSHA's IMIS. The covariates include year fixed effects, state fixed effects, industry fixed effects (manufacturing, health services, and other), and an indicator of union status. The parameter α_{DR} represents the mean outcome just above the SST cutoff; the parameter α_{Dl} represents the mean outcome just below the SST cutoff. ***, **, and * indicate statistical significance at the one, five, and ten percent levels, respectively.

Table 6: Effect of Inspection on Worker Safety, Instrumental Variable Regression

	Order of Polynomial	Bandwidth 50%	75%	100%	125%	150%
$\hat{\tau}_{FRD}$	1	-2.059**	-1.194	-1.028	-0.675	-0.221
		(0.911)	(0.739)	(0.653)	(0.590)	(0.545)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$		0.219***	0.221***	0.218***	0.217***	0.214***
		(0.014)	(0.011)	(0.009)	(0.008)	(0.007)
$\hat{\tau}_{FRD}$	2	-2.147	-2.189*	-1.622*	-1.753**	-1.548**
		(1.664)	(1.173)	(0.954)	(0.864)	(0.788)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$		0.177***	0.207***	0.221***	0.220***	0.221***
		(0.020)	(0.016)	(0.014)	(0.012)	(0.011)
$\hat{\tau}_{FRD}$	3	-0.502	-2.553	-2.636*	-1.720	-2.072*
		(2.241)	(1.814)	(1.463)	(1.192)	(1.067)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$		0.171	0.176***	0.191***	0.210***	0.215***
		(0.027)	(0.022)	(0.019)	(0.016)	(0.014)
Bandwidth h		1.58	2.37	3.17	3.96	4.75
Observations		12,752	19,257	26,092	33,383	41,301

The sample is derived from ODI. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The estimates come from an instrumental variable regression, with the case rate as the outcome variable, a programmed inspection as the treatment variable, and an indicator of a DART rate greater than the cutoff as the instrument. The data on inspections come from OSHA's IMIS. The covariates include year fixed effects, state fixed effects, industry fixed effects (manufacturing, health services, and other), and an indicator of union status. The parameter α_{DR} represents the mean outcome just above the SST cutoff; the parameter α_{Dl} represents the mean outcome just below the SST cutoff. ***, **, and * indicate statistical significance at the one, five, and ten percent levels, respectively.

Table 7: Effect of Inspection on Worker Safety, Alternative Samples

	DART					
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\tau}_{FRD}$	-1.792**	0.064	0.336	-1.292	-1.109	-1.973
	(0.814)	(0.938)	(0.933)	(1.954)	(1.153)	(2.426)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$	0.224***	0.219***	0.214***	0.218***	0.275***	0.258***
	(0.013)	(0.014)	(0.014)	(0.034)	(0.022)	(0.046)
Bandwidth h	3.17	3.29	4.26	7.00	4.49	3.61
Observations	154,808	125,245	103,514	25,460	61,702	13,101

The sample is derived from ODI. The estimates come from a regression discontinuity model using local linear regression, with the case rate as the outcome variable and a programmed inspection as the treatment variable. Column 1 shows the main results as presented in Table 4, column 2; column 2 and column 3 show longer run results, measured two and three years after the SST plan; column 4 shows results among establishments observed exactly twice in t and t+4 in ODI; column 5 shows results using the earliest paired observation from t to t+4; and column 6 shows results using the earliest paired observation from t to t+4 when a new cutoff was implemented. The data on inspections come from OSHA's IMIS. The covariates include year fixed effects, state fixed effects, industry fixed effects (manufacturing, health services, and other), and an indicator of union status. The parameter α_{DR} represents the mean outcome just above the SST cutoff; the parameter α_{Dl} represents the mean outcome just below the SST cutoff. ***, **, and * indicate statistical significance at the one, five, and ten percent levels, respectively.

Table 8: Effect of Inspection on Worker Safety by Industry and Union Activity

	Industry			Union	
	Manufacturing	Health Services	Other	Yes	No
$\hat{\tau}_{FRD}$	-1.050	0.626	-0.124	-3.546*	-1.413
	(0.859)	(1.317)	(1.532)	(2.141)	(0.820)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$	0.208***	0.238***	0.245***	0.204***	0.226***
	(0.013)	(0.022)	(0.024)	(0.030)	(0.013)
Bandwidth h	6.37	6.11	3.07	3.83	3.81
Observations	94,410	27,136	33,262	19,293	135,515

The sample is derived from ODI. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The estimates come from a regression discontinuity model using local linear regression, with the case rate as the outcome variable and a programmed inspection as the treatment variable. The data on inspections come from OSHA's IMIS. The covariates include year fixed effects, state fixed effects, industry fixed effects (manufacturing, health services, and other), and an indicator of union status. The parameter α_{DR} represents the mean outcome just above the SST cutoff; the parameter α_{DL} represents the mean outcome just below the SST cutoff. ***, **, and * indicate statistical significance at the one, five, and ten percent levels, respectively.

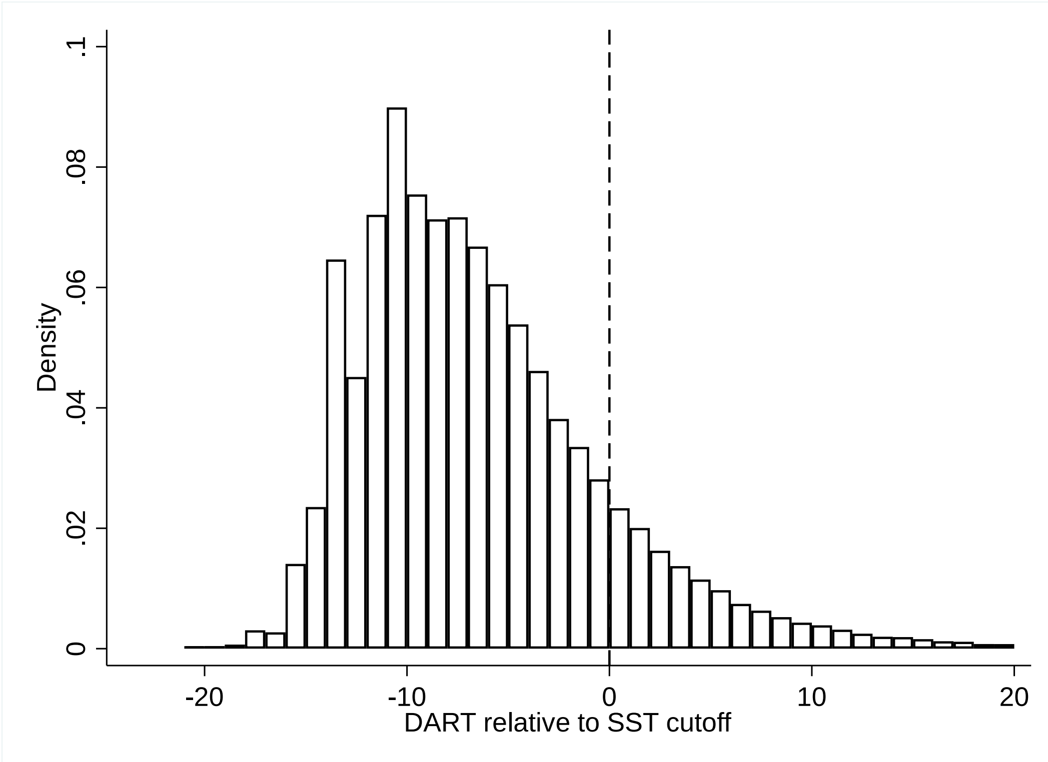

Figure 1: Distribution of DART case rate relative to SST Cutoff

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The DART is the rate of cases involving days away from work and job transfers or restrictions per 100 full-time employees. The x-axis is the DART rate from the first observation relative to the DART cutoff for the primary inspection list.

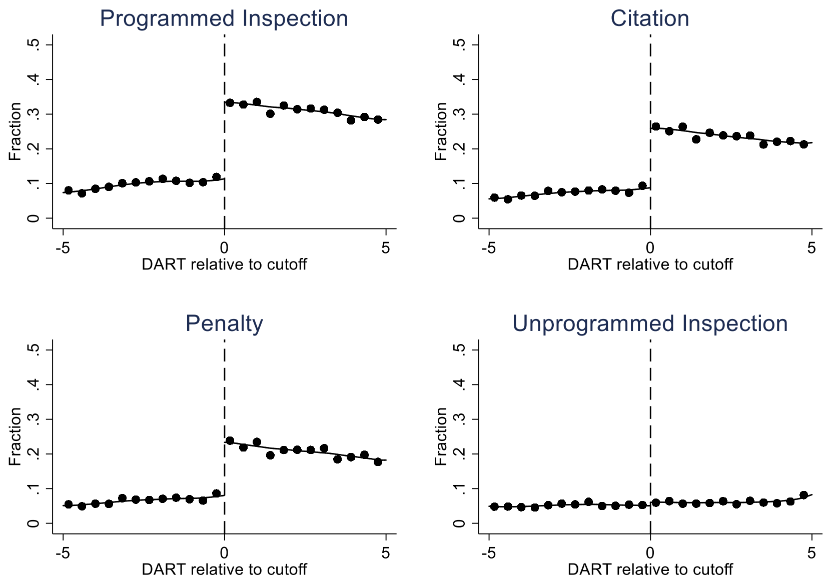

Figure 2: Inspection Outcomes by DART relative to SST Cutoff

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The DART is the rate of cases involving days away from work and job transfers or restrictions, per 100 full-time employees. The x-axis is the DART rate from the first observation relative to the DART cutoff for the primary inspection list. The inspection outcomes are derived from OSHA's Integrated Management Information System. The markers denote the mean outcome within intervals of 0.5; the line is derived from local linear regression.

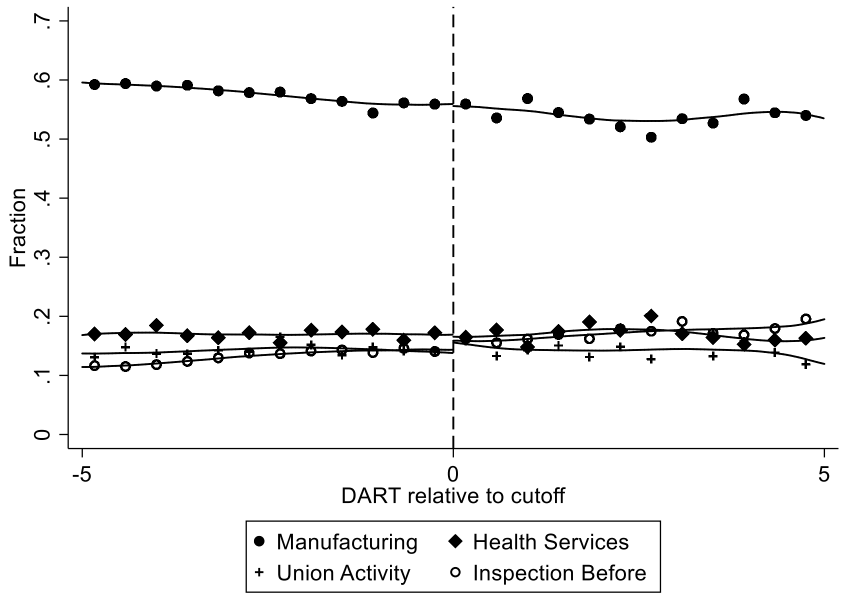

Figure 3: Establishment Characteristics by DART relative to SST Cutoff

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The DART is the rate of cases involving days away from work and job transfers or restrictions per 100 full-time employees. The x-axis is the DART rate from the first observation relative to the DART cutoff for the primary inspection list. The markers denote the mean outcome within intervals of 0.5; the line is derived from local linear regression.

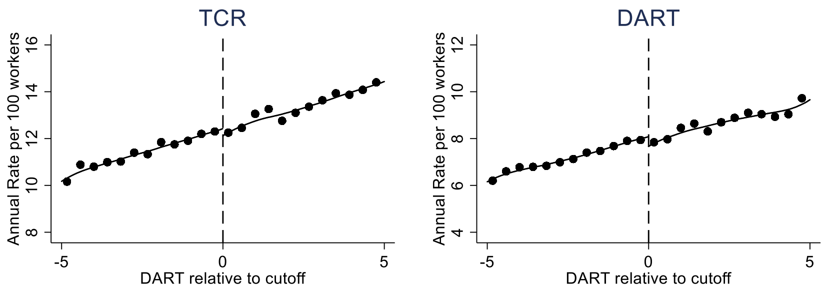

Figure 4: Case Rate Outcomes by DART relative to SST Cutoff

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The TCR includes cases involving death, days away from work, job transfers and restrictions, and medical treatment beyond first aid, and the DART includes cases involving days away from work and job transfers or restrictions, both measured per 100 full-time employees. The x-axis is the DART rate from the first observation relative to the DART cutoff for the primary inspection list. The markers denote the mean outcome within intervals of 0.5; the line is derived from local linear regression.

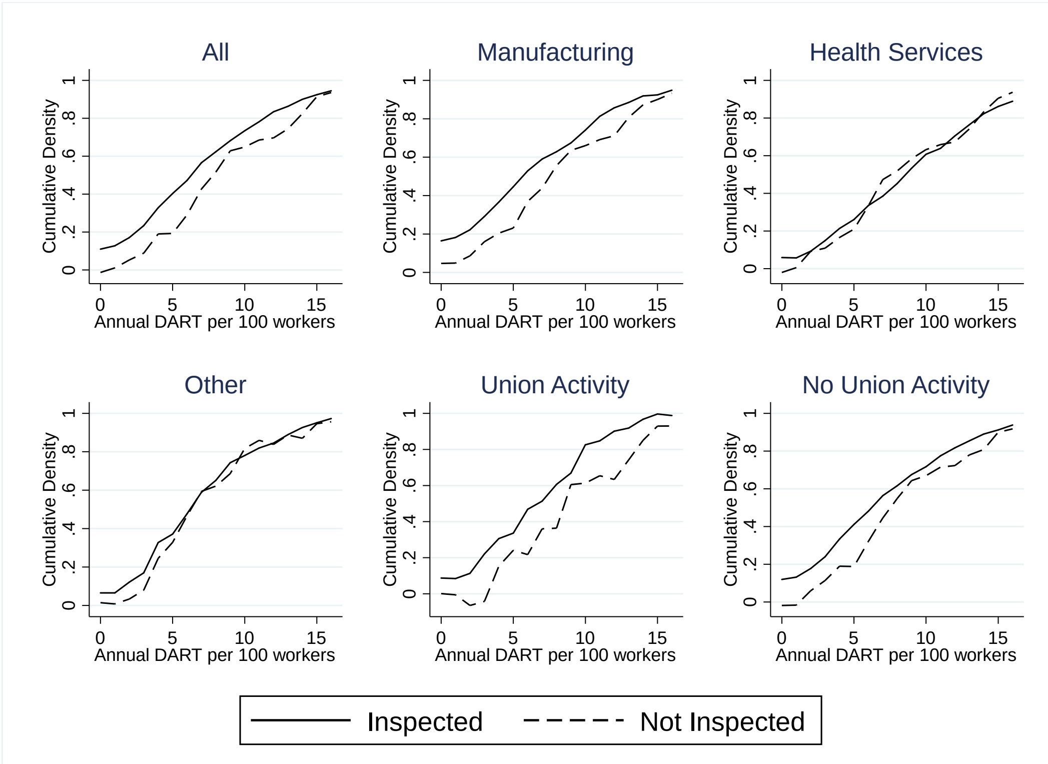

Figure 5: Distributional Effects of Inspection on DART Rate

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The panels plot the cumulative density functions of the DART rate among compliers just above the cutoff, that are inspected, and counterfactual compliers just below the cutoff, that are not inspected. The DART is the rate of cases involving days away from work and job transfers or restrictions per 100 full-time employees.

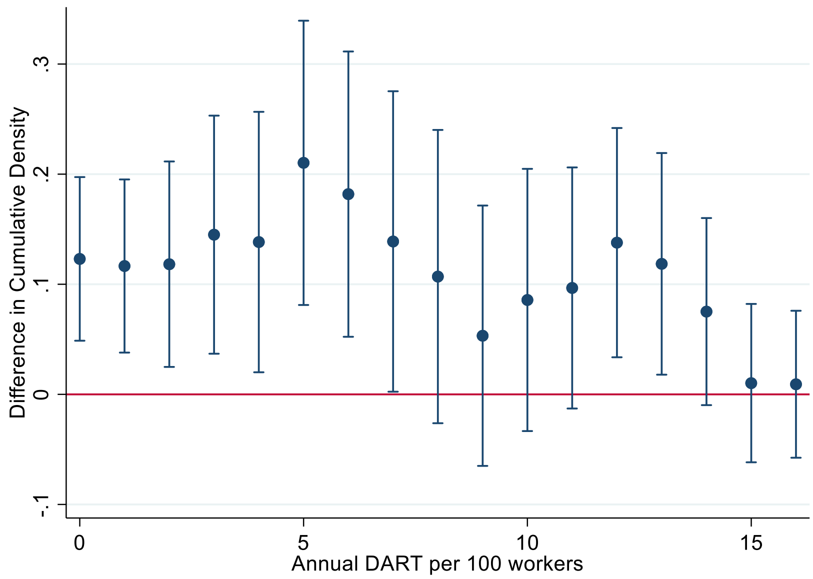

Figure 6: Distributional Effects of Inspection on DART Rate

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The figure plots the difference in the cumulative density functions plotted in the first panel of Figure 5. The DART is the rate of cases involving days away from work and job transfers or restrictions per 100 full-time employees.

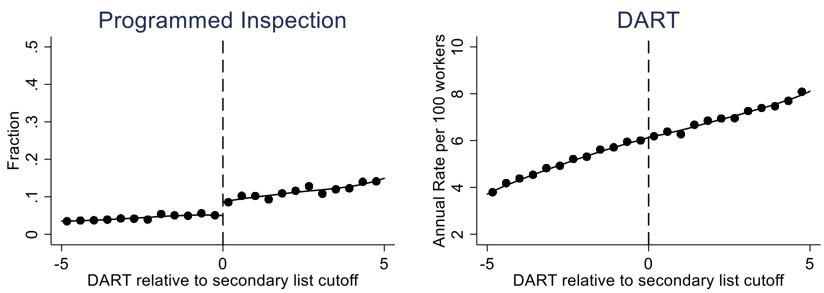

Figure 7: Secondary Inspection List

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The DART rate includes cases involving days away from work and job transfers or restrictions, per 100 full-time employees. Establishments are deleted if there is no secondary inspection list for the SST cycle. The x-axis is the DART rate from the first observation relative to the DART cutoff for the secondary inspection list. The markers denote the mean outcome within intervals of 0.5; the line is derived from local linear regression.

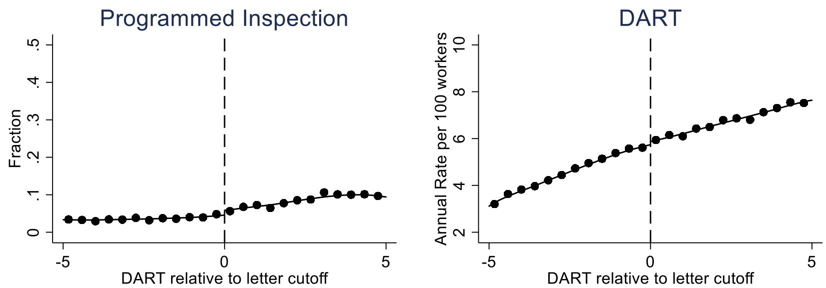

Figure 8: Letter List

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The DART rate includes cases involving days away from work and job transfers or restrictions per 100 full-time employees. The x-axis is the DART rate from the first observation relative to the cutoff for a letter. The markers denote the mean outcome within intervals of 0.5; the line is derived from local linear regression.

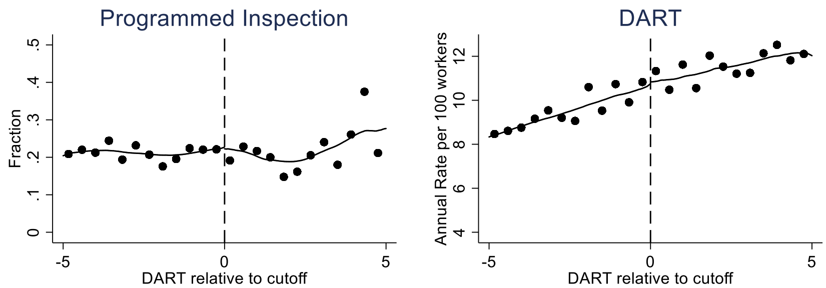

Figure 9: ODI Data Recorded in 1996 and Collected in 1997

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The DART rate includes cases involving days away from work and job transfers or restrictions per 100 full-time employees. The ODI Data Recorded in 1996 were not used by the SST plan. The x-axis is the DART rate from the first observation in 1996 relative to the DART cutoff for the primary inspection list in 1997. The markers denote the mean outcome within intervals of 0.5; the line is derived from local linear regression.

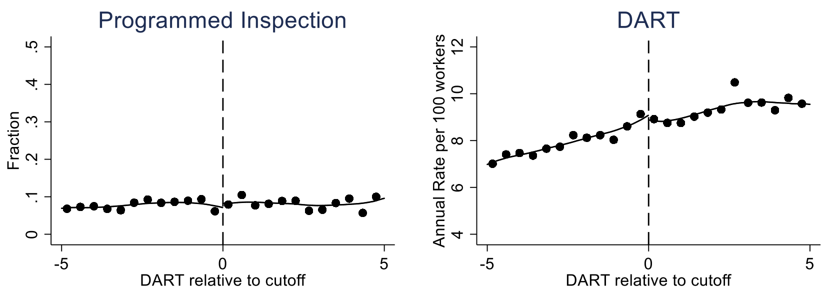

Figure 10: Non-Federal States

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The DART rate includes cases involving days away from work and job transfers or restrictions per 100 full-time employees. The SST plan was not implemented in non-federal states. The x-axis is the DART rate from the first observation relative to the DART cutoff for the primary inspection list in federal states. The markers denote the mean outcome within intervals of 0.5; the line is derived from local linear regression.