Organizational and Geographic Spillover Effects of Regulatory Inspections: Evidence from OSHA

DOL Scholars Final Report

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1 Introduction

A crucial question for the Occupational Safety and Health Administration (OSHA) and other regulatory agencies in the Department of Labor is the extent to which enforcement inspections lead to general deterrence—that is, improve compliance and performance at non-inspected workplaces. The magnitude and scope of spillovers has major implications for how OSHA should target its enforcement resources to maximize their impact on the health and safety of workers.

However, identifying spillover effects of inspections entails overcoming several substantial empirical challenges. One challenge is data limitations to identify plausible sources of spillover effects. For example, inspections could lead to improved outcomes at other establishments in the same firm of an inspected establishment, but most datasets do not provide a way to identify such "corporate siblings" (establishments sharing the same corporate parent). A more fundamental challenge stems from the difficulty in disentangling spillover effects from unobserved variables that jointly affect the behavior of a focal entity and its peers (Manski 1993). Establishments in the same firm face the same corporate culture, and establishments in the same local area face the same institutional environment (e.g. labor market), both of which might affect injury rates. Because OSHA targets many of its inspections to establishments that experienced recent injuries or complaints, such unobserved common shocks challenge the interpretation of the relationship between inspections and outcomes of peers.

To overcome these challenges, in this paper we estimate to what extent randomized OSHA inspections affect injury rates of non-inspected establishments. In particular, we investigate spillover effects to other establishments owned by the same firm as an inspected establishment ("corporate siblings"), and neighboring establishments regardless of corporate ownership ("neighbors"). We hypothesize spillover effects will be stronger for facilities that are closer to an inspected establishment in terms of ownership, geography and industry.

Our research design utilizes the subset of inspections under OSHA's Site-Specific Targeting (SST) program that were allocated using random assignment from 2001-2010. To estimate spillover effects of SST inspections, we compare safety outcomes of corporate siblings and neighbors of establishments randomly selected for SST inspections to those of siblings and neighbors of establishments that were eligible—but not selected—for SST inspections.

We find evidence that OSHA's SST inspections led to statistically significant and economically meaningful improvements in injury rates at corporate siblings of inspected establishments: one additional SST inspection of an establishment in a multi-unit firm on average led to a 2.2 percent decline in injuries involving days away from work per full employee (the "DAFW rate") among the inspected establishment's corporate siblings over the following four years. Furthermore, consistent with our hypotheses, these effects were stronger for corporate siblings in closer geographic proximity; among siblings in the same OSHA region (roughly equivalent to the 10 Census regions partitioning the country), an inspection led to a 7.7 percent decline in the DAFW rate. Our point estimates suggest effects were even stronger for siblings located in the same OSHA Area Office's boundary, but this result is not quite statistically significant. Unlike the role of geographic proximity, our results do not support the hypothesis that these within-firm spillover effects are stronger for siblings sharing the same industry.

On the other hand, we are unable to detect spillover effects across firms. We test whether an SST inspection affects the DAFW rates of other establishments sharing the same a) Commuting Zone (i.e. local labor market) and 2-digit NAICS industry code (i.e. broad sector), b) Commuting Zone and 4-digit NAICS code (i.e. detailed industry), c) zip code and 2-digit NAICS code, and d) zip code and 4-digit NAICS code. In all cases, our point estimates suggest that an SST inspection led to a small decline in DAFW injury rates among other establishments in each of these four groups, but none of the results are statistically significant.

2 Conceptual Framework

To determine likely sources of spillover effects of OSHA inspections, it is first informative to discuss mechanisms through which inspections may lead to improvements in safety performance of *inspected* establishments. For example, OSHA inspections that find violations may lead to improvements in compliance with OSHA regulations, which may lead to fewer injuries. Additionally, because OSHA inspections are irregular and infrequent, an inspection may lead managers and workers to update their beliefs of the probability of future enforcement. Similarly, inspections could make safety issues more salient to managers and workers, irrespective of any effects on beliefs of future enforcement. Another possibility is that inspectors may impart knowledge about good safety practices, as knowledge about such practices and OSHA regulations varies across establishments (Vickers et al 2005).

There is a long and active empirical literature examining the direct effects of OSHA inspections on outcomes of inspected establishments. Prior studies have found a range of estimated effects, with some finding little or no correlation (Smith 1979; Viscusi 1986; Ruser et al. 1991), and others that OSHA inspections correlate with a decline in injury rates (Gray and Scholz. 1993; Mendoloff et al. 2005; Haviland et al. 2012; Levine et al. 2012; Johnson et al 2017).

Beyond their effect on inspected establishments, OSHA inspections could affect the safety and health of non-inspected establishments as well. However, there is little evidence on spillovers from inspections. A few papers have found EPA inspections lead to improved compliance among nearby plants (Shimshack and Ward (2005); Gray and Shadbegian (2007)). Additionally, Weil (2012) finds inspections of fast food chains by the Wage & Hours Division of the Department of labor lead to improvements in compliance with the Fair Labor Standard Act among other fast food chains in the same zip code. Johnson (2016) finds press releases about penalties issued by OSHA lead to significant improvements in compliance at workplaces located nearby and in the same industry as the publicized workplace. While each of these studies has found evidence of geographic spillover effects on compliance, there has been no study we are aware of that has investigated spillover effects of OSHA inspections, spillover effects of inspections to corporate siblings within firms, spillover effects on performance (e.g. injuries and illnesses) rather than compliance, and how spillover effects vary as a function of distance to the inspected establishment.

We now discuss hypotheses regarding how we expect spillover effects from OSHA's SST inspections to occur.

2.1 Proximity Effects

We anticipate spillover effects from SST inspections to be strongest among establishments "closest" to the inspected establishment. We consider three dimensions of "closeness:" organizational, geographic, and industry.

First, inspections could have spillover effects on corporate siblings in the same firm. Researchers have shown that knowledge transfers across organizations within firms (e.g., Darr, Argote and Epple 1995), and a similar process could arise if inspectors provide helpful safety practices or information about the expected costs of future OSHA enforcement.

There are institutional reasons that within-firm spillover effects of SST inspections could arise. For example, a few of OSHA's procedures, such as the Enhanced Enforcement Program[1] (which in 2010 became the Severe Violator Enforcement Program) provides incentive for spillovers. Under EEP, if OSHA issued a high gravity violation (a violation that, based on the inspector's assessment, would likely lead to serious injury or illness) at one establishment, other establishments of the same firm were prioritized for inspections. Additionally, OSHA can issue a "repeat" violation to an establishment if another site of the same firm had been cited for the same standard in the prior three years (U.S. DOL 2016). Employer procedures could also institutionalize spillovers. For example, an OSHA safety inspection at one establishment that identifies problems could potentially induce a firm to implement company-wide standard procedures across all its establishments.

Second, an inspection of one establishment could also have spillover effects on behavior among geographically proximate facilities ("neighbors"), regardless of corporate ownership. At least since

[1] The EEP program began in 2003. The 2008 OSHA directive explaining the institutional details of the EEP program (a revised version of the 2003 directive) can be found here: https://www.osha.gov/pls/oshaweb/owadisp.show_document?p_table=DIRECTIVES&p_id=3749

Marshall (1890), theories of economic geography have emphasized how knowledge flows in local areas. Knowledge spillovers of patents (measured by citations) have a strong geographic component (Belenzon and Schankerman 2013), as does diffusion of adoption of new technologies (Agha and Molitor 2016). Thus, we would hypothesize that spillover effects of inspections decrease with distance to the inspected establishment (both within firms and across them).

Third, spillover effects from OSHA inspections would be especially likely among other establishments in its same industry. Transferring knowledge and capabilities between establishments is more difficult when the establishments have different operating procedures (Szulanski 1996; Maritan and Brush 2003), suggesting the transfer of knowledge learned from an OSHA inspection would occur more easily within industries than across industries. Prior evidence has shown firms learn good management practices from proximate firms, but only those in the same industry (Bloom et al 2016), further suggesting the potential importance of shared industry.

3 The OSHA Site-Specific Targeting (SST) Program

OSHA's Site-Specific Targeting (SST) program, which ran from 1999 to 2014, targeted for inspection high-injury workplaces in historically hazardous industries. The process OSHA used to assign workplaces to the SST program began with OSHA identifying a set of hazardous industries based on Bureau of Labor Statistics (BLS) data.[2] Each year from 1996 to 2011, OSHA surveyed between 60,000 and 80,000 workplaces with at least 40 employees in these industries through the OSHA Data Initiative (ODI).[3] OSHA sent the ODI survey mid-year, and establishments reported summary data on their injuries, illnesses, and employment from the previous calendar year based on logs they are required to maintain to record every work-related injury and illness.[4]

Each year from 1999 to 2014, OSHA used the ODI data to create two target lists for the SST program: a primary list and a secondary list. Each year, the primary list consisted of the roughly 3,500 establishments that two years earlier had reported the highest DART or DAFW rates—that is, the rate of injuries (per 100 full-time employees) that required Days Away From Work (DAFW), Restricted work, or a Transfer (collectively referred to as DART). The secondary list contained the roughly 7,000 establishments with the next highest rates.[5]

Each year between April and August,[6] OSHA sent each of the 81 OSHA Area Offices in the 29 states OSHA directly regulates[7] the subset of the primary list that contained the establishments under the purview of that Area Office (henceforth, "office"). If an office did not have sufficient resources to inspect all establishments on its entire primary list, the office inputted the number it could inspect into OSHA software. The software then randomly assigned which establishments from the primary list the office was to inspect. If the office inspected all of these establishments before headquarters issued the next year's list, the office used the same procedure to select a new random set of establishments from the remainder of its primary list.

[2] Specifically, the BLS Survey of Occupational Injuries and Illnesses gathered data each year from a sample of approximately 200,000 establishments drawn from all private-sector industry establishments. OSHA selected for the SST program a subset of industries that BLS classified as "High Hazard Industries." OSHA used BLS annual "High Hazard Industries" lists until 2003 when BLS stopped updated it, and thereafter used the 2003 edition.
[3] OSHA intended to survey each establishment meeting these criteria (with at least 40 employees and in the specified hazardous industries) at least once every three years.
[4] OSHA Form 300 that OSHA standard 29 CFR 1904 requires employers to complete is available at https://www.osha.gov/recordkeeping/RKform300pkg-fillable-enabled.pdf.
[5] For example, in 2008, the primary list included establishments reporting a DART rate of 11 or more or a DAFW rate of 9 or more, and the secondary list included establishments reporting a DART rate between 7 and 11 or a DAFW rate between 5 and 9. The DAFW rate refers to the number of DAFW injuries per 100 full-time employees; DAFW injuries are a subset of DART injuries. The specific cutoffs for the primary and secondary list changed each year, and beginning in 2009, varied by industry.
[6] For example, the 2007 SST Directive was effective between May 14, 2007 and May 19, 2008.
[7] Workplaces in the remaining 21 states are monitored by state agencies according to an OSHA-approved state-run plan.

When an office exhausted its entire primary list, OSHA headquarters and the office repeated this process with the SST secondary list (for details, see U.S. Department of Labor 2008).

Not all establishments that offices assigned for inspection were actually inspected. First, because OSHA issued the annual SST directives between April and August each year, Area Offices did not make the corresponding assignments to SST inspections until the middle of the directive year, if not later, and the corresponding inspections occurred subsequently. As a result, only 19% of establishments assigned to inspection received an SST inspection by the end of the directive year. Second, being *assigned* for an SST inspection does not correspond perfectly to being *inspected*. According to our interviews with OSHA personnel, some establishments assigned to inspection were not inspected when the inspector could not physically locate the establishment, or when an Area Office successfully petitioned OSHA Headquarters to not complete the set of establishments it had assigned to inspection.[8] Even one year after the directive year, 22% of establishments assigned to inspection had not received an SST inspection.[9] We describe below how our empirical strategy accounts for this distinction between assignment to SST inspection and the occurrence of inspections.

4 Data and Outcomes

4.1 Data

We merged four datasets to undertake our analysis: (1) OSHA's annual SST target lists and (2) annual ODI survey data on injuries and employment, both obtained from OSHA's Office of Statistical Analysis;[10] (3) OSHA inspection data from the agency's Integrated Management Information Systems (IMIS) database; and (4) annual Dun and Bradstreet data on employment, credit rating, and other business outcomes from the National Establishment Time Series (NETS) database.

The annual SST primary and secondary target lists report the set of establishments at risk of SST inspection each year during 2001-2010, each establishment's associated Area Office, and whether or not each establishment was assigned to receive an SST inspection.[11]

The ODI dataset contains the annual survey results that establishments reported to OSHA from 1996 to 2011. These data include annual counts of DAFW injuries and injuries involving job transfers or restrictions, which together comprise DART injuries, as well as annual average employment and total labor hours worked over the year.[12] The ODI dataset is an unbalanced panel because it includes a different (but overlapping) set of establishments each year. The ODI sought to survey all establishments with at least 40 employees in the target industries every three years. Beginning around 2005, the ODI resurveyed establishments the following year that reported a DART rate of at least 7. Many establishments on the SST target lists report ODI data nearly every year[13] because annual ODI reporting was triggered by a DART threshold that was below the threshold used to create the SST primary target list (and at or above the threshold used to create the secondary list), and because injury rates are serially correlated across years.[14]

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[8] In addition, OSHA personnel told us that Area Offices did not face repercussions if they failed to complete a set of establishments selected for inspection, so it is possible some offices did so without informing headquarters.
[9] It is possible OSHA inspected some of these workplaces, but our procedure to link SST with OSHA's information system (IMIS) failed to find the corresponding inspection in IMIS.
[10] We obtained these data from OSHA's Office of Statistical Analysis in 2012 after signing a Memorandum of Understanding.
[11] While the SST program operated from 1999 through 2014, OSHA's Office of Statistical Analysis was only able to provide us with target lists for 2001 through 2010.
[12] The ODI dataset also includes the number of fatal injuries, the number of injuries and illnesses not meeting the DART criteria, and the number of various types of illnesses (in categories such as skin disorders, respiratory problems, poisoning, and hearing loss),.
[13] Among the establishments that were ever on an SST target list, 25% reported injury data in at least 10 of the 16 years of the ODI program (1996-2011), and 50% reported injury data in at least 7 years of these 16 years.
[14] For example, among all establishments ever on the SST list, the correlation between establishments' DART rate in successive years is 0.55.

We downloaded OSHA inspection records from the agency's IMIS database in January 2014.[15] These records include, for each OSHA inspection, the name and address of the inspected establishment, the inspection date, whether the inspection was completed, and what triggered the inspection (e.g., SST, a serious accident, an employee complaint, etc.). These records also provide the number and severity of violations, and each violation's "gravity" (the severity of the hazard cited by the violation).

We obtained from NETS establishments' annual industry, geographic location, and firm affiliation. NETS seeks to include all establishments in the U.S. in operation at any point since 1990, and is an annual panel dataset extracted from Dun & Bradstreet data.

We linked SST target lists, ODI records, and NETS records via the DUNS number, a unique establishment-level identifier included in all of these datasets. Because OSHA draws the ODI survey sample from Dun & Bradstreet data, in theory every establishment in ODI and SST should also be in NETS. In reality, we found 97% of the SST target list establishments in NETS. Because IMIS does not include DUNS numbers or any other unique establishment identifier, we linked the SST target list to IMIS inspection records by fuzzy matching establishment names, addresses, and industries using *MatchIt* software, the Stata *reclink* command, and a manual process.[16]

4.2 Outcomes

Our outcome measure of workplace safety is number of injuries involving days away from work (DAFW) per 100 full-time workers, or DAFW rate. We choose this measures as our primary outcome for a few reasons. First, among all types of injuries reported to ODI, DAFW injuries are likely to be the most accurate. DAFW injuries are the most serious reported to ODI, and serious injuries are more likely to be reported by employees to supervisors (Biddle and Roberts 2003), and to be reported by employers on an OSHA log (Boden et al 2010).[9] Second, because of their severity, DAFW injuries impose enormous costs and are thus an outcome of substantial policy interest. In 2005—the median year of our sample—there were 1.2 million DAFW injuries in the private sector in 2005 (U.S. Bureau of Labor Statistics 2005). This corresponds to a total economic loss of over $60 billion that year given that the average non-fatal DAFW injury has been estimated to cost $50,000.[17] To calculate FTEs, we divide total working hours by 200,000 (the number of hours 100 full-time workers would work in a year), and to calculate the DAFW rate per 100 FTE, we divide DAFW injury count by this measure of FTEs. To reduce the effect of very large outliers of DAFW injury rate (which exhibits positive skew), we add the first non-zero percentile of FTE to the denominator of the DAFW rate, and we then top-code this variable at its 99th percentile.

5 Empirical Strategy

5.1 Estimating Spillover Effects Over Distance and Industry

Section 2 discusses how we expect spillover effects to be strongest among "closer" establishments, where we consider organizational, geographic, and industry dimensions of "closeness." To investigate these hypotheses regarding proximity, seek to investigate the presence of spillovers for multiple peer groups. We organize our peer groups into "within-firm" and "any-firm" peer groups.

For within-firm spillovers, we will separately test for spillover effects of SST inspections to other establishments in the same firm and

[15] The Department of Labor's IMIS database is publicly available at https://enforcedata.dol.gov/views/data_summary.php

[16] This process resulted in us finding 82% of establishments on the 2001-2010 SST target lists in the IMIS database. Of course, failure to match could indicate either that a) an establishment was in fact inspected, but our matching technique failed to find the inspection in IMIS, or b) the establishment was not in fact ever inspected.

[17] Waehrer et al. (2007) estimate the cost of a DAFW injury in 2002 was $37,017. We update this estimate to reflect the cost of a DAFW injury in 2005 (the median year in our sample), in 2016 dollars. See Appendix D for details of this calculation.

- Anywhere in U.S.
- Anywhere in U.S., same 4-digit NAICS
- Same OSHA region
- Same OSHA region, same 4-digit NAICS
- Same OSHA Area Office
- Same OSHA Area Office, same 4-digit NAICS

For any-firm spillovers, we will separately test for spillover effects of SST inspections to other establishments in

- Same commuting zone[18], same 2-digit NAICS
- Same commuting zone, same 4-digit NAICS
- Same zip code, same 2-digit NAICS
- Same zip code, same 4-digit NAICS

5.2 Regression Specification

It is not easy to study spillovers (Manski 1993). For example, whatever caused a peer to receive an inspection (*e.g.*, dangerous industry, corporate culture, weak labor market that lowers employees' bargaining power, etc.) may also affect the safety and health outcomes of the "focal" establishment. As a result, it can be difficult to disentangle true spillover effects from a common unobserved variable that jointly affects the safety outcomes of a focal establishment and its peer.

We briefly elaborate the challenges to studying spillover effects specific to our setting, and how we overcome them. Suppose we have outcome y (number of injuries or injury rate) at establishment i that belongs to peer group j (specifically, a firm, an industry, and/or a geographic area) in year t. We are interested in learning the causal impact on y (through a spillover effect) of the number (or share) of other members of group j (that is, $-i,j$) that have been SST inspected τ years prior to t. We include dummy variables for each year to control for time-varying shocks common to all establishments, denoted by γ_t, as well as a vector of time-varying control variables $\boldsymbol{X_{ijt}}$. We assume that each establishment-year observation has an i.i.d. error component (ϵ):

$$y_{ijt} = \beta * \#\, peers\ SST\ Inspected_{-i,j,t-\tau} + \boldsymbol{\Gamma X_{ijt}} + \gamma_t + \epsilon_{ijt} \qquad (1)$$

However, due to the way SST inspections were targeted, β does not yield a clean estimate of the causal effect we seek. To see this more clearly, we know that

$$\#\, peers\ SST\ Inspected_{-i,j,t-\tau} = f(\begin{cases} peers\ on\ SST\ target\ list\ (\boldsymbol{A}) \\ \%\ drawn\ by\ peers'\ Area\ offices\ (\boldsymbol{B}) \\ \nu\ (random\ draw\ of\ which\ estabs\ are\ selected)\ (\boldsymbol{C}) \\ \%\ of\ those\ selected\ that\ are\ inspected\ (\boldsymbol{D}) \end{cases}$$

Thus, estimating a regression of Equation (1) would yield coefficient β that would incorporate all of these

[18] Commuting Zones which are clusters of counties characterized by strong commuting ties (Tolbert and Sizer 1996), have become seen as a logical geographic unit to characterize local labor markets. We identify establishments' commuting zone using a county-commuting zone cross walk available on David Dorn's website, using the establishment's county in the NETS database.

factors:

- **A** is determined by how many peers' injuries two years prior ($y_{-i,j,t-2}$) were above the SST threshold. However, if there are common shocks within peer groups (which would trigger spatial correlation when *j* is geographic, intra-industry correlation when *j* is industry, intra-firm correlation when *j* is firm), **A** would also be a function of $y_{i,j,t-2}$ and thus would be correlated with $y_{i,j,t}$ if serial correlation is also present. Note this is problematic only if there is correlation *both* within *j* and across *t* (within *i*).

- **B** could have an independent effect on establishment *i*'s injuries. For example, Area Offices that get through more of their Target Lists may do so when they are especially tough on enforcement, and having peers in the boundary of a tough Area Office may have an independent effect on *i's* injuries (especially if we are looking at peers in close geographic proximity)

- **C** is random *conditional on **B***, since offices used random assignment to determine which establishments to assign to inspection on the primary (and secondary, if they got to it) lists once they determined what share of their lists they could inspect.

- **D** is a choice variable of the Area Office. For various reasons described above, not every establishment selected for SST inspection is actually inspected, for example if the Office becomes inundated with other higher-priority inspections

Our goal is to isolate variation in $\#peers\ SST\ Inspected_{jt-\tau}$ that is a function only of C, and not A, B or D, so that its coefficient is interpretable.

To do so, recall first that, conditional on the share of its primary or secondary list that an office assigned to SST inspection, which establishments were ultimately assigned was randomly assigned. Thus, to isolate random variation in the number of an establishment's peers that have been SST inspected, we modify Equation (1) the following way:

$$y_{ijt} = \beta_1 * \#\ Peers\ SST\ Assigned_{-i,j,t-\tau} + \beta_2 * \#\ Peers\ Expected_{-i,j,t-\tau} + \boldsymbol{\Gamma X_{ijt}} + \gamma_t + \epsilon_{ijt} \qquad (2)$$

Where

$$\#\ Peers\ Expected_{-i,j,t-\tau} = peers\ on\ SST\ list\ and\ eligible * average(\%\ Assigned\ in\ peers'\ Area\ Office\ Primary/Secondary\ list)$$

In words, $\#\ Peers\ SST\ Expected$ captures the number of an establishment's peers that would be expected to be assigned to SST inspection in the prior period, based on how many of its peers were eligible for SST inspection, and the likelihood that those peers were drawn by their respective offices for inspection. For example, if an establishment had two peers on the SST Target List in the prior period, and these establishments were on the primary (or secondary) lists of offices that drew 25 and 75 percent of their lists for inspection, respectively, the number of its peers expected is 0.25+0.75 = 1. Controlling for $\#\ Peers\ SST\ Expected_{-i,j,t-\tau}$, the remaining variation in $\#\ peers\ SST\ Assigned$ is only driven by C (random chance of which establishments are actually drawn). Thus, β_1 in Equation (2) yields a causal estimate of having one additional peer assigned to SST inspection prior to year t on the establishment's outcome *y*.

However, we ultimately are interested in the causal effect of having one additional peer *inspected.* To obtain such an estimate, we use an instrumental variables (IV) strategy, instrumenting the number of an establishment's peers that have been inspected with the number of its peers that were assigned to inspection. In IV nomenclature, Equation (2) is an "Intent to treat" specification, but we are interested in estimating the Treatment Effect on the Treated (TOT). To estimate the TOT effect, we estimate the following variant of Equation (2):

$$y_{ijt} = \beta_1 * \#\, Peers\ SST\ \widehat{Inspected}_{-\iota,J,t-\tau} + \beta_2 * \#\, Peers\ SST\ Expected_{-i,j,t-\tau} + \boldsymbol{\Gamma X_{ijt}} + \gamma_t + \epsilon_{ijt} \qquad (3)$$

Here, $\#\, peers\ \widehat{SST}\ Inspected$ is estimated from the following "first stage" regression

$$\#\, Peers\ SST\ Inspected_{-i,j,t-\tau} = \beta_1 * \#\, Peers\ SST\ Assigned_{-i,j,t-\tau} + \beta_2 * \#\, Peers\ Expected_{-i,j,t-\tau} + \boldsymbol{\Gamma X_{ijt}} + \gamma_t + \epsilon_{ijt} \qquad (4)$$

Our approach to instrument *# peers Inspected* with *# peers Assigned* meets the requirements for $\widehat{\beta_1}$ in Equation 3 to identify the effect of having an additional peer SST-inspected in the prior period on outcome *y*. First, as we will show below, the first-stage relationship modelled in Equation (4) is strong. Second, consider the exclusion restriction, that *Assigned* cannot directly affect outcome *y* except through its influence on *Inspected*. There is no plausible reason that this exclusion restriction is violated because establishments were never informed that they were assigned to SST inspection, and assignment to inspection had no effect on inspectors' actions other than allocating SST inspections.

We note several further details behind our empirical strategy. Note these regressions do not include establishment (or peer group) fixed effects. Thus, rather than use variation within establishments over-time, we are using cross-sectional variation across establishments. To improve precision, we include an establishment's peer group's baseline (i.e. prior to the start of the sample period) average DAFW injury rate.[19]

We restrict attention to spillover effects of inspections within four years prior to the current year ($\tau \in \{1, 4\}$). We examine ranges of τ years so that $\#\, Peers\ SST\ Inspected_{-i,j,t-\tau}$ includes the sum over the 4 years prior to year *t*.

Our regressions do not control or account for any other types of OSHA intervention that establishments (or establishments' peers) may experience. Establishments in our sample are likely relatively hazardous (given that they have peers with high enough DART rates to be eligible for SST inspections), and thus may be more likely to experience OSHA complaint, referral, or other types of inspections. However, this detail does not imperil our empirical strategy: while such interventions may have an independent effect on establishments' injury rates, they are orthogonal to our independent variable of interest since, conditional on *# Peers Expected, # Peers Assigned* is random. Thus, such other interventions belong in the error term and do not bias our focal coefficients.

For regressions corresponding to within-firm spillovers, we cluster standard errors by firm. For those corresponding to any-firm spillovers, we cluster by Commuting Zone (the highest geographic level we consider).

5.3 Sample

To construct our sample, we begin with all establishments that reported to the OSHA Data Initiative (ODI) database. We drop a small percent of ODI responses that OSHA considered to be poor quality, and a small percent of ODI respondents that were not found in the NETS database.

To estimate within-firm spillover effects, we further restrict attention to establishments that belong to multi-unit firms. We identify corporate siblings (establishments in the same firm) as establishments sharing the same corporate headquarters according to the NETS database.

When estimating Equations (2)-(4) for each peer group, we identify $\widehat{\beta_1}$ using variation in the number of an

[19] In an earlier draft of this report, we used a regression specification that did include peer group fixed effects, thus using within-group variation to identify our coefficients of interest. We obtained broadly similar results using that specification.

establishment's peers that would be expected to be assigned to SST inspection, and the number that were actually assigned; we gain no information from establishments that have zero peers expected to be assigned. Thus, when estimating Equations (2)-(4) for each peer group separately, we drop firms that never have an establishment eligible for an SST inspection over the sample period when estimating within-firm spillovers, and we drop Commuting Zone / 2-digit NAICS groups that never had an establishment eligible when estimating "any-firm" spillovers. Additionally, because we do not include establishment fixed effects, we did not want to include more than one observation for each establishment in our regressions to avoid comparing an establishment to itself across years. Thus, when estimating our regressions for each peer group, if an establishment would be included in the regression sample for multiple years, we only include the earliest year.

We estimate our regressions pooling years t between 2004 and 2011.

Table 1 provides summary statistics for our key outcome measure (DAFW injury rate), as well as our key independent variables (*# Peers Expected, # Peers Assigned*) for each peer group we consider. Panel (a) provides this information for the within-firm peer groups and panel (b) for the any-firm peer groups.

6 Regression Results

6.1 Within-firm spillovers

Table 2 walks through Equations (2)-(4) for one peer group: establishments in the same firm, anywhere in U.S. Column (1) reports the coefficient on $\#\ peers\ SST\ Assigned_{-i,j,t-\tau}$ from Equation (2) (the Intent-to-treat specification). As described above, the sample includes all establishments reporting to ODI between 2004 and 2011 in multi-unit firms in which at least one establishment was eligible for an SST inspection at any point during 2001-2010. The coefficient implies that having an additional corporate sibling assigned to SST inspection in the prior 4 years leads an establishment's DAFW rate to decline by 0.029 (p=.012). Column (2) reports the first stage relationship that having one additional corporate sibling assigned to inspection is associated with 0.57 additional sibling actually inspected (p<.01). Finally, Column (3) reports the IV specification, which effectively scales the ITT effect by the first stage coefficient: having one additional corporate sibling SST inspected in the prior four years leads to a decline in the DAFW rate of 0.05 (p=.011), or 2.2 percent relative to the sample mean DAFW rate of 2.3.

Table 3 investigates how within-firm spillover effects of SST inspections vary as a function of "closeness" to the inspected establishment. The table reports the IV specification corresponding to Equation (4) for the 6 within-firm peer groups. Spillover effects display a monotonic relationship with geographic distance. Having an additional corporate sibling inspected recently in the same region (Column 3) leads to a 0.175 lower DAFW rate (p-.02), or 7.7 percent; a sibling inspected in the same area office (Column 5) leads to a decline of 0.289, or 12.5 percent, though this result is not quite statistically significant at conventional levels (p=.13).

Though the results in Table 3 reveal that spillover effects within firms operate more strongly as a function of geographic proximity, there is no evidence that industry proximity plays a role Columns (2), (4) and (6) illustrate that adding the "same 4-digit NAICS" criterion has little effect on the coefficients, and actually leads the point estimates to *decline* by a small magnitude.

6.2 Any-firm spillovers

Table 4 investigates spillover effects of SST inspections to other establishments regardless of corporate ownership. The table reports the IV specification corresponding to Equation (4) for 4 peer groups (Same Commuting Zone / 2-digit NAICS; same Commuting Zone /4-digit NAICS; same zip code; same zip code /4-digit NAICS. Each of the point estimates is negative, but none are close to statistically significant. These results thus imply that spillover effects of SST inspections across firs may exist, but they are likely much smaller than spillover effects within firms.

7 Discussion

Our results imply OSHA's SST inspections had statistically significant and economically meaningful spillover effects within firms. We find that the average SST inspection of an establishment in a multi-unit firm led to a 2.2 percent lower DAFW injury rate among corporate siblings across the country, and a 7.7 percent lower rate among siblings in the same region. As a point of comparison, in an adjacent project, we have estimated that the average SST inspection led to 9.1 percent fewer DAFW injuries (Johnson Levine Toffel 2017).

Though we found no evidence of spillover effects of SST inspections across firms *on average*, it is possible that they are present under certain conditions. Certain industries may be more connected, and certain local labor markets may facilitate knowledge transfer across firms more easily than others. In future work, we will investigate heterogeneity in this effect.

In terms of policy recommendations, our results imply that OSHA's overall effectiveness can potentially have a large multiplier effect if it targets enforcement resources to establishments in hazardous firms.

Our study has several limitations. Though this study provided credible estimates of spillover effects of SST inspections, it was unable to measure how the general *threat* of OSHA inspections affects establishments' safety outcomes, which is another aspect of "general deterrence." Also, because our study focuses on randomized SST inspections, we also cannot say anything about the spillover effects of OSHA inspections triggered by a complaint, referral or catastrophe.

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Table 1 Summary Statistics

Panel (a): Samples for Within-firm spillovers		
	N	mean
DAFW rate per 100 FTE	116,742	2.31
Peer Group = Same Firm		
# Peers Expected 1-4 years prior	35,461	5.49
# Peers Assigned to SST Inspection 1-4 years prior	35,461	5.36
Peer Group = Same Firm, 4-digit NAICS		
# Peers Expected 1-4 years prior	32,811	4.17
# Peers Assigned to SST Inspection 1-4 years prior	32,811	3.96
Peer Group = Same Firm, Region		
# Peers Expected 1-4 years prior	33,326	1.07
# Peers Assigned to SST Inspection 1-4 years prior	33,326	1.01
Peer Group = Same Firm, Region, 4-digit NAICS		
# Peers Expected 1-4 years prior	30,199	0.84
# Peers Assigned to SST Inspection 1-4 years prior	30,199	0.76
Peer Group = Same Firm, Area Office		
# Peers Expected 1-4 years prior	29,972	0.23
# Peers Assigned to SST Inspection 1-4 years prior	29,972	0.22
Peer Group = Same Firm, Area Office, 4-digit NAICS		
# Peers Expected 1-4 years prior	27,406	0.19
# Peers Assigned to SST Inspection 1-4 years prior	27,406	0.17
Panel (b): Samples for Any-firm spillovers		
	N	mean
DAFW rate per 100 FTE	287,479	2.07
Peer Group = Same Commuting Zone, 2-digit NAICS		
# Peers Expected 1-4 years prior	112,124	14.43
# Peers Assigned to SST Inspection 1-4 years prior	112,124	14.47
Peer Group = Same Commuting Zone, 4-digit NAICS		
# Peers Expected 1-4 years prior	106,413	2.17
# Peers Assigned to SST Inspection 1-4 years prior	106,413	2.09
Peer Group = Same zip code, 2-digit NAICS		
# Peers Expected 1-4 years prior	97,194	0.34
# Peers Assigned to SST Inspection 1-4 years prior	97,194	0.35
Peer Group = Same zip code, 4-digit NAICS		
# Peers Expected 1-4 years prior	76,168	0.09
# Peers Assigned to SST Inspection 1-4 years prior	76,168	0.08

In panel (a), the sample begins with all establishments reporting to ODI between 2004 and 2011that are in multi-unit firms in which at least one member of the firm was eligible for an SST inspection between 2001 and 2010. For each peer group listed, the sample is further restricted to an establishment's first observation in the sample period. In panel (b), the sample begins with all establishments reporting to ODI between 2004 and 2011 in which at least one establishment in its same Commuting Zone / 2-digit NAICS was eligible for an SST inspection between 2001 and 2010. As in panel (a), for each peer group listed, the sample is further restricted to an establishment's first observation in the sample period.

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Table 2 Within-firm spillover effects: Intent-to-treat, First Stage, and Instrumental Variables (IV) Results

	(1) Intent-to-treat	(2) First stage	(3) IV
Dep Var =	DAFW rate	# SST Inspected 1-4 years prior	DAFW rate
# Assigned to SST Inspection Same Firm 1-4 years prior	-0.029**	0.569***	
	(0.011)	(0.140)	
# SST Inspected Same Firm 1-4 years prior			-0.050**
			(0.020)
Observations	35461	35461	35461
Mean Dep Var	2.305	3.126	2.305
\# peer groups (firms)	8851	8851	8851

Standard errors in parentheses clustered by firm

All regressions control for the number of Expected Assignments to SST inspection in the firm and the firm's average DAFW rate between 1998-2003.

The sample is establishments in multi-unit firms in which at least one sibling was at risk of being assigned to inspection in the sample period.

If an establishment meets these criteria more than once only the earliest observation is used.

* p<.1, ** p<.05, ***p<.01

Table 3 Within-firm spillover effects: The Role of Proximity (Instrumental Variables specification)

Peer Group =	(1) Same Firm	(2) Same Firm-NAICS4	(3) Same Firm-Region	(4) Same Firm-Region-NAICS4	(5) Same Firm-Area Office	(6) Same Firm-Area-NAICS4
# SST inspected in peer group 1-4 years prior	-0.050**	-0.046**	-0.175**	-0.135	-0.289	-0.201
	(0.02)	(0.02)	(0.08)	(0.09)	(0.19)	(0.19)
Observations	35461	32811	33326	30199	29972	27406
Mean Dep Var	2.305	2.355	2.349	2.425	2.417	2.496
\# peer groups	8851	11664	12519	14912	17061	18191

Standard errors in parentheses clustered by firm

All regressions control for the number of Expected Assignments to SST inspection in each corresponding peer group and the peer group's average DAFW rate between 1998-2003.

The sample is establishments in multi-unit firms in which at least one corresponding peer was at risk of being assigned to inspection in the sample period.

If an establishment meets these criteria more than once only the earliest observation is used.

* p<.1, ** p<.05, ***p<.01

Table 4 Any-firm spillover effects (Instrumental Variables specification)

Peer Group =	(1) Same CZ /NAICS2	(2) Same CZ /NAICS4	(3) Same zip /NAICS2	(4) Same zip /NAICS54
# SST inspected in peer group 1-4 years prior	-0.072	-0.090	-0.090	-0.125
	(0.070)	(0.126)	(0.082)	(0.223)
Observations	112121	106410	97191	76165
Mean Dep Var	1.946	1.962	1.982	2.134

Standard errors in parentheses clustered by commuting zone

All regressions control for the number of Expected Assignments to SST inspection in each corresponding peer group and the peer group's average DAFW rate between 1998-2003.

The sample is establishments in which at least one corresponding peer was at risk of being assigned to inspection in the sample period.

If an establishment meets these criteria more than once only the earliest observation is used.

* p<.1, ** p<.05, ***p<.01