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Supplementary Materials for

Randomized Government Safety Inspections Reduce Worker Injuries with No Detectable Job Loss

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Supplemental Text

In this section, we present a stylized model to illustrate the effects of OSHA randomized inspections under three sets of assumptions; namely, those underlying perfect competition and those espoused by OSHA's critics and its proponents. The model makes extreme assumptions; we present it to fix ideas.

Assume a workplace has N employees and a large set of hazards. Remediation of a specific workplace hazard h costs the firm C_h per employee. We assume workers are identical and that, if they possess full information, they value remediation of hazard h at $\alpha_h C_h$ ($\alpha_h \geq 0$). In addition, remediating hazard h lowers the firm's costs by $\delta_h C_h$ per employee through lower workers' compensation costs, less employee absenteeism, and so forth ($\delta_h \geq 0$).

Perfect-competition Model

The perfect-competition model assumes that all managers and workers have full information about all workplace hazards and remediation costs, that there are no externalities such as publicly funded long-term disability insurance, and that employees are perfectly mobile across employers. Under these very strong assumptions, there are compensating wage differentials such that remediation of hazard h lowers wages by the amount at which workers value the safer work environment, $\alpha_h C_h$ (*26*, *27*). An important result from this perfect competition model is that a profit-maximizing firm voluntarily remediates hazard h if and only if the value to workers plus the value to the employer of remediation is larger than the cost of remediation; that is, when

$$\alpha_h + \delta_h \geq 1 \tag{1}$$

Now consider a case of this perfect-competition model in which a randomized OSHA inspection mandates remediation of a hazard, h, which the firm had not already remediated voluntarily. Because wages fall by $\alpha_h \cdot C_h$, employees are not on average made better off by the remediation; employees value remediation of the hazard, but face wage declines that fully usurp this value (*28*). At the same time, remediation of hazard h following the inspection increases the employers' cost per employee by

$$C_h \cdot (1 - \alpha_h - \delta_h) > 0 \tag{2}$$

Under almost any assumption about market structure, increased costs per employee due to inspections would reduce employment, increase marginal costs, increase prices, and reduce sales and profits. The resulting lower profits would lead to lower rates of firm survival. Firms that had sunk costs or that were earning above-normal profits prior to the inspection would not necessarily exit, but paying for remediation might cause some to borrow or have problems paying bills, leading to poorer credit ratings.

Even if OSHA mandates remediation of a hazard whose value to the firm and the workers almost equals the cost of remediation, injuries and wages would still decline. In this case, profits would still fall, but only by a small amount. Thus, under perfect competition, even relatively cost-effective mandated remediation of hazards would lead to small declines in employment, sales, credit ratings, and firm survival.

Assumptions of OSHA Critics

OSHA critics posit that many of OSHA's regulatory mandates are poorly designed and therefore raise employer costs while achieving minimal reductions in injuries (e.g., Wilson 1995; Reiman 2003). In this case, employees would place a very low value on the mandated remediation (α_h is near zero) and employers would not benefit from lower injury-related costs (δ_h is also near zero). With these assumptions, mandatory remediation increases employer costs by C_h, while injury rates and wages remain virtually unchanged. Such mandates would reduce employment, sales, credit ratings, and firm survival and the declines would be larger than if the regulation had been well designed.

Assumptions of OSHA Proponents

OSHA proponents posit that employers and employees operate with imperfect information; in many cases, they do not perceive—or fully perceive—a particular hazard or its consequences. Assume that employees observe and value a portion γ_h of the true safety improvement that results from remediating hazard *h* (e.g., they might value the reduced risk of acute harm but overlook the longer-term health benefits) and employers perceive a portion φ_h of the true direct benefits, with γ_h and φ_h both ranging from zero to one (inclusive). In this case, employers only voluntarily remediate when their perceived savings from lower wages and direct benefits outweigh their actual (full) remediation costs:

$$\gamma_h \alpha_h + \varphi_h \delta_h \geq 1 \tag{3}$$

With this decision rule, employers provide less safety than would be socially efficient whenever (a) their *perceived* savings and benefits from remediating a hazard *h* are exceeded by the remediation cost but (b) the remediation cost is lower than the *full* (but not fully perceived) savings and benefits they and their employees would obtain:

$$\gamma_h \alpha_h + \varphi_h \delta_h < 1 < \alpha_h + \delta_h \tag{4}$$

Because there is imperfect information, such hazards are not voluntarily remediated. But they would be voluntarily remediated under the full-information assumption in the perfect-competition model. Whenever OSHA mandates remediation of a hazard *h* that satisfies inequality (4), the remediation is socially efficient. In these cases, the firm's costs will change by $C_h (1 - \gamma_h \alpha_h - \delta_h)$, which can be positive or negative.

As long as workers perceive any benefits from remediating hazard *h* ($\gamma_h \alpha_h > 0$), there will be some decline in wages. When workers perceive a sufficient share of the benefits of the improvement in safety such that $\gamma_h > (1 - \delta_h)/\alpha_h$, then efficient remediation of hazard *h* reduces wages sufficiently to reduce the employer's costs. In this case, employment, sales, credit ratings, and firm survival all increase after an OSHA inspection.

If workers perceive a lower portion of the benefits (that is, $\gamma_h < (1 - \delta_h)/\alpha_h$), then even though the increase in safety is socially efficient, the remediation cost outweighs the firm's cost savings from lower injury costs and wages. In this case, workers do not suffer as large a decline in wages, but remediating hazard *h* will decrease employment, sales, credit ratings, and firm survival.

It is straightforward to complicate this illustrative model with heterogeneous workers, imperfect labor mobility, remediation with a fixed (not per-employee) cost, wage rigidities, and so forth, but even this minimalist version yields sufficient insights to structure the discussion below of our empirical results.

Materials and Methods

Causal identification

In each year of our study period, Cal/OSHA identified a list of industries with high injury rates, typically based on data from the U.S. Bureau of Labor Statistics (*19*). For each of these industries, Cal/OSHA used Dun & Bradstreet and other sources to compile a list of establishments with 10 or more employees, then randomly selected a subset of each list. These subset lists of randomly chosen establishments were then sent to the appropriate northern or southern district managers (each district covers roughly half the state). Within each district, inspectors attempted to inspect all of the randomly chosen establishments, though managers could prioritize based on factors such as avoiding industries they felt were not as dangerous or workplaces that had had an OSHA inspection in the prior two years.

At 13 percent of treatment establishments Cal/OSHA chose for a randomized inspection (and at 7 percent of the treatments in our sample), the inspection was not carried out, typically because the inspector could not find the establishment, the establishment had gone out of business, or the inspector determined that the establishment was not eligible for a random inspection after all (for example, if the inspector found out the establishment actually had fewer than 10 employees). As we could not filter the control sample on these criteria, we included in the treatment group the establishments at which random inspections were attempted, even if they were not carried out. Thus, our estimates measure the causal effect of an intention to inspect and might (slightly) underestimate the causal effect of the random inspections that actually occurred. This is therefore a conservative approach. As the vast majority of attempts were successful, we usually simplify our language by dropping the qualifier "attempted" and referring to our estimates as the causal effect of randomized inspections.

Constructing the matched sample

OSHA's Integrated Management Information System (IMIS) database had records for 1,752 establishments that Cal/OSHA had randomly inspected at least once during our sample period. Table S1 summarizes how this population of inspections was reduced to those used in our matched sample of randomly inspected establishments ("treatments") and control establishments.

Using matchIT software, we linked 1,392 of the randomly inspected establishments to the NETS database based on names, addresses, and industry codes. Because injury data from workers' compensation systems are available primarily at the company level, we restricted our analysis to single-establishment firms, as in (*13*, *14*, *29*). Restricting the sample to single-establishment firms does not substantially impact generalizability because approximately 89 percent of manufacturing plants in California were single-establishment firms in 2005, according to the California NETS database. Restricting the sample to single-establishment firms reduced the number of randomly inspected establishments to 935. We were concerned that Cal/OSHA might not have recorded some attempted inspections at workplaces that had shut down. Thus, we dropped all establishments absent from the NETS data in the year of the random inspection. Because Cal/OSHA performed random inspections only at establishments with at least 10 employees, we included only establishments with at least 10 employees in the random inspection year or either of the two preceding years. These restrictions left us with 698 treatment establishments.

Potential controls for a given treatment included all single-establishment firms in NETS that shared its industry code but were not themselves randomly inspected during the sample period. 1,135,530 establishments met these criteria. Imposing the same restrictions as above (at least 10 employees, etc.) left us with 54,676 potential controls. Analogous to our requirement that treatment establishments be present in NETS in the year of random inspection, we dropped all potential controls for a given treatment that were not in business in that treatment's random inspection year—what we termed the "match year." Using Federal Employer Identification Numbers (FEINs) when available or else names and addresses, we linked 482 treatments and 6600 potential controls to WCIRB records (*30*).

We sought to exclude establishments that had operated for only a portion of a year during either of the two years preceding the match year. To do so, we dropped establishments whose *Average annual wage* (calculated as *Payroll* from WCIRB divided by *Employment* from Dun & Bradstreet) in either of the two years preceding the match year was less than $7,020, the amount a full-time worker would earn over a six-month period at California's 2002 minimum wage. We also excluded establishments from being considered treatments or potential controls when they lacked WCIRB data in all four years prior to the match year. These two restrictions left us with 451 treatments and 6,156 potential controls. Finally, Cal/OSHA aimed to avoid randomly inspecting establishments that had been inspected during the previous two years, so we dropped treatments and potential controls that had had inspections in the two years preceding the match year. These restrictions left us with 431 randomized inspections and 6,055 potential controls.

Randomization of inspections effectively took place separately in Cal/OSHA's north and south districts. Thus, we matched each treatment with all potential controls sharing its region and industry code, using the level of aggregation Cal/OSHA had used that year for that industry (e.g., four-digit SIC code). When this matching process yielded more than one eligible control, we selected the one whose employment (averaged over the two years preceding the treatment's randomized inspection year) was most similar to that of the treatment, breaking any ties by randomly selecting among them. We excluded from our final matched sample all treatments for which we failed to identify a matched control. We also imposed a common-support restriction by excluding matched pairs when either member's *Injury count* value was outside the common support of the treatments and potential controls, which led us to drop one pair.

Our matching process yielded 409 matched pairs. Table S2 reports the industry distribution of the matched sample and Table S3 reports summary statistics.

Evaluation model

We measure the causal effect of inspections via a difference-in-differences analysis. Specifically, we estimate the following model for each outcome Y_{it} at establishment i in year t:

$$Y_{it} = \alpha_i + \beta \cdot \mathit{Has\ been\ randomly\ inspected}_{it} + \Sigma_k\, \gamma_k \cdot \mathbf{X}_{ikt} + \Sigma \delta_t \cdot \mathit{year}_t + \varepsilon_{it} \qquad (5)$$

where α_i is a complete set of establishment-specific intercepts. *Has been randomly inspected*$_{it}$ is coded "1" for the year an establishment is randomly inspected and each year thereafter and is coded "0" otherwise. Of primary interest is β, which represents the estimated effect of a random inspection—the average change in outcome levels pre- versus post-inspection. $\mathbf{X}_{ikt}$ refers to various controls (subscripted k) such as Average occupational riskiness and Log employment that are included in some specifications. All models include a full set of year dummies ($year_t$).

Attributing an establishment's current performance to a random inspection becomes increasingly unlikely the longer ago the inspection occurred. We therefore restrict our analysis to a period from four years prior to four years after the random inspection.

Balance tests

We tested whether Cal/OSHA's process of selecting establishments for randomized inspections (and our attempt to replicate that process) resulted in the selected establishments being similar to those that were eligible but not selected. Specifically, we compared their characteristics in the years prior to the random inspection year via several balancing tests, including t-tests to compare means and Wilcoxon rank-sum and Kolmogorov-Smirnov equality-of-distributions tests to compare medians. Our test results indicate that, in terms of levels and trends, treatments and controls were statistically indistinguishable in size, whether measured as sales, employment, or payroll (see Table S4). However, treatments have statistically significantly higher injury levels and lower injury trends prior to the inspection, both in terms of count and cost. For example, in the four-year period preceding the randomized inspection, treatments averaged 3.7 injuries per year whereas controls averaged 3.1 (t-test p-value = 0.06).

We also examined kernel density plots to compare the treatments' and matched controls' distributions of several key variables in the year before the match year (see Figures S1-S3). The distribution of each variable appears nearly identical for treatments and controls, including the variables for which the statistical tests found significant differences, suggesting that the statistically significantly different results could be due to sampling error. Kolmogorov-Smirnov equality-of-distributions tests, reported in Column 10 of Table S4, indicate that the two groups had indistinguishable distributions for levels in each variable other than *Average occupational riskiness*, *Injury count*, and *Injury cost* and for all trend variables other than log difference in *Injury cost*.

In addition to the many individual comparisons of treatment and control groups' levels and trends during the pre-period, we also assessed whether pre-period focal variables collectively predicted which workplace in each pair was randomly inspected. To do so, we estimated a cross-sectional logit regression model with conditional fixed effects for each matched pair. A test of joint significance finds that the pre-inspection characteristics collectively predict which workplace in each pair is randomly inspected (Column 12).

Cal/OSHA claims to randomize the inspections we focus on and we closely replicated their procedures to create the pool of establishments at risk of a randomized inspection each year. Cal/OSHA had no information on employer injury rates for any of these establishments, so we believe the imbalance is due to sampling error, not conscious selection by Cal/OSHA (*31*).

The evaluation model in Equation (5) includes a fixed effect for each employer. Thus, it will provide unbiased estimates even if Cal/OSHA selects persistently dangerous (or safe) workplaces for randomized inspection.

The remaining risk to the evaluation is that Cal/OSHA selects workplaces with a transitory high injury rate (or with transitory high injury costs) prior to inspection; mean reversion then leads to an improvement in injuries after the inspection, with the inspection being causally linked to the decline. We can divide the injury rate and injury cost into two pieces: that predicted by the hazardousness of the employer's occupational mix and a residual. The hazardousness of the employer's occupational mix changes slowly, with a correlation at controls of 0.82 between its average two years prior to inspection and its average in the inspection year and the next four years. Thus, as expected, inspections had no effect on changes in the average occupational riskiness at inspected workplaces.

When we focus on injury rates relative to the hazardousness of the employer's occupational mix (prior to inspection), treatment no longer has a large or statistically significant effect in predicting pre-inspection injury rates or costs (see Table S11). Thus, the unexpectedly high pre-inspection injury rates for treatments appear to be due solely to a more hazardous mix of occupations rather than to transitory high injuries. In short, the evidence does not show that randomization at Cal/OSHA was imperfect in ways that threaten the validity of the results of this evaluation.

However, even if randomization was carried out perfectly, our treatments and controls were not perfectly balanced on pre-inspection injury rates. Thus, our evaluation models often control for pre-inspection characteristics—the $\mathbf{X}_{ikt}$ in Equation (5) above.

<u>Injury rates</u>

Injury count is a count dependent variable that exhibits overdispersion (mean=3.26, variance=29.29) relative to the Poisson model (which assumes the variance equals the mean). Thus, we estimate negative binomial regressions with establishment-level conditional fixed effects to predict *Injury count*.

With no control variables, the point estimate indicates that randomized inspections reduce annual injuries by 9.4 percent ($\beta = -0.099$, $p = 0.013$, IRR = 0.906, Table 1, Column 1). The effect is nearly identical when we control for *Log payroll* and is reduced slightly in magnitude and statistical significance when we also control for *Average occupational riskiness* (not shown).

The effects of randomized inspections might attenuate after a few years or might take a few years to emerge. To test for such changes in the effects of inspections over time, we replace the single *Has been randomly inspected* dummy with a series of dummies coded "1" for inspected establishments in the randomized inspection year and one, two, three, and four years after the randomized inspection and coded "0" otherwise. The results in Column 2 of Table 1 reveal that inspections significantly reduce injuries in the random inspection year and three and four years later, marginally reduce them one year later, and have no significant effects two years later. Including *Log payroll* yields similar results (not shown). In short, there is no evidence that the reduction in injuries after randomized inspections is transitory.

Our main results are robust to an alternative approach to controlling for establishment size, using *Log employment* instead of or in addition to *Log payroll* (not shown). We continue to find significant evidence that inspections reduce injury rates and log injury costs when we also control for *Average occupational riskiness* and for *Log payroll* (not shown). When we simultaneously control for *Average occupational riskiness*, *Log employment*, and *Log payroll*, the coefficient magnitude falls slightly (but not statistically significantly). The increased standard error renders the coefficient on randomized inspections no longer statistically significant at conventional levels when predicting injury rate ($p = 0.12$; not shown), though we continue to find statistically significant support that inspections reduce *Log injury cost*.

Injury cost

We use OLS regression with establishment-level fixed effects to estimate the effect of randomized inspections on injury cost and we report standard errors clustered by establishment (except as noted below). Our initial basic model includes a dummy for *Has been randomly inspected* and year dummies. On average, randomly inspected establishments exhibit a 26 percent decline in injury cost ($\beta = -0.298$, thus IRR = 0.742, $p < 0.01$, Table 1, Column 3). Results (not shown) are little changed when we control for size (*Log payroll*) or for *Average occupational riskiness*.

We next interact *Has been randomly inspected* with a series of dummies indicating the number of years since the inspection (Table 1, Column 4). The point estimates suggest that randomized inspections always reduce injury costs, although the year-specific treatment effects are only statistically significant in the year of random inspection and years three and four after the inspection. These results indicate that the injury-cost reduction is not transitory.

Workplace survival

As the perfect-competition model makes clear, inspections that lead to lower injury rates may nevertheless not be socially efficient—or even beneficial for employees—if the cost of remediation leads to outcomes such as large losses of wages or employment. To examine these possible unintended consequences, we now examine the effects of OSHA inspections on workplace survival, employment, earnings, and sales.

We determined an establishment to have "died" if it had disappeared from both the NETS and WCIRB databases. Because our dataset extends through 2006 and our one-to-one matching was in the year of the randomized inspection, data on each matched pair of treatment and control were right-censored after the identical number of years. 4.4 percent of the randomly inspected establishments did not survive until 2006, a rate slightly and not statistically significantly lower than the 5.6 percent death rate among the control establishments ($p = 0.423$).

Although treatment status was randomized, there were differences between the treatment and control groups' pre-period sales, employment, and payroll. In Table S7, we present several survival analyses that condition on these characteristics, including a logit, a conditional logit with a fixed effect for each matched pair, and a Cox proportional hazard model with each matched pair its own strata. The logit models (Columns 1 and 2) predict whether the establishment survives until (at least) the end of the sample period (2006) and the Cox model (Column 3) predicts whether the establishment failed to survive (i.e., died) within the sample period. For all models, the survival rate of randomly inspected establishments is not statistically significantly different than that of the controls.

Because company death is relatively rare, we also analyzed whether random inspections affected establishments' creditworthiness, using Dun & Bradstreet's *Composite credit appraisal* and *PAYDEX*. We used ordered logit regression models to predict *Composite credit appraisal* because it is an ordinal dependent variable, and we used OLS with establishment-level fixed effects to predict minimum *PAYDEX* scores, which range from 1 to 100. Table S8 shows that the point estimates on a randomized inspection are positive but very close to zero (that is, random inspections slightly increased creditworthiness) and nowhere near statistically significant. These results are similar to an analysis in Washington State that found no evidence that survival rates were diminished following health and safety enforcement (*32*).

Employment, payroll, and sales

To assess whether random inspections had an impact on firm growth, we estimated fixed-effect OLS models to predict *Log employment*, *Log payroll*, and *Log sales* (Table 2). Randomly inspected establishments did not differ significantly from controls in employment, total earnings, or sales, though the point estimates are each positive (β = 0.027, SE = 0.016 for employment in Column 1; β = 0.005, SE = 0.013 for payroll in Column 2; β = 0.002, SE = 0.044 for sales in Column 3).

Social cost of injuries

Occupational injuries and illnesses in the United States have been estimated to have a total cost to employees and employers of roughly $171 billion in 1992, or roughly 2 percent of GDP (*33*). The authors of that study noted, "These estimates are likely to be low, because they ignore costs associated with pain and suffering as well as those of within-home care provided by family members, and because the numbers of occupational injuries and illnesses are likely to be undercounted" (*33*).

If the cost of occupational injuries and illnesses grew proportionately to GDP, total injury costs would equal $390 billion in 2010. True costs may be higher than this estimate to the extent medical costs have risen more quickly than GDP or lower than this estimate because injury rates in California declined from 1992 to 2010.

If we divide the $390 billion estimated total injury and illness costs by roughly 140 million workers in the United States in 2010, then injury costs average $2800 per employee. Our sample's Average occupational riskiness is about triple California's average. If California is about as risky as the rest of the nation and our sample is representative of high-hazard industries in general, then workplaces in high-hazard industries in California have injury costs that are triple the national average over all industries. Thus, high-hazard industries in California have mean injury costs of roughly $8400 per employee. (This estimate will be biased down slightly as wages in California are above the national average, so lost wages will be a bit higher in California.) Mean employment in our sample is 33, implying a very rough estimate of $276,000 in total injury and illness cost per workplace per year, or $1,380,000 over five years. If injury costs per employer decline 26% (the point estimate implied by the coefficient –0.298 on

Treatment in Table 1, column 3), then five-year injury costs decline by roughly $355,000 at each inspected employer.

This estimate is very rough and will be low because it ignores under-reported injuries (*23*, *24*), safety benefits lasting more than four years, and the reduction in pain and suffering when injury rates decline. In the other direction, the five-year total benefit will decline if we discount future benefits.

Data access

The dataset analyzed in this study includes data from the three sources described below. Table S12 lists the data source of each variable and whether we have permission to share the variable with other researchers. The publicly-accessible portion of the dataset analyzed in this study is available at http://hdl.handle.net/1902.1/17936 in the Murray Research Archive at Harvard University (study identifier: 1902.1/17936). That dataset contains the following variables: a scrambled establishment ID that uniquely identifies each establishment, the establishment's city, industry, year, year of random inspection, treated (has been randomly inspected), sales, employment, PAYDEX score, Composite Credit Appraisal. It does not contain the following variables due to the confidentiality conditions under which they were obtained: establishment name, street address, ZIP code, DUNS number; annual payroll, injury count, injury cost, and average occupational riskiness. Below we describe steps researchers can take to seek access to these data.

We obtained inspection-level data on random inspection dates and establishment names and addresses from U.S. OSHA's Integrated Management Information System (IMIS). These are publicly available data that can be shared with researchers without restrictions.

We obtained annual establishment-level sales, employment, PAYDEX scores, Composite Credit Appraisals, industry (NAICS and SIC Codes), as well as establishment names and addresses, from the National Establishment Time-Series (NETS) database. NETS is a compilation of Dun & Bradstreet data created and maintained by Walls & Associates, a private company that licenses access to NETS. Don Walls, President of Walls & Associates, permitted us to share data on sales, employment, PAYDEX score, and Composite Credit Appraisal, but not the establishment's name, street address, ZIP code, or DUNS number. Researchers seeking to full access NETS data can contact Donald Walls, President, Walls & Associates (tel. +1-510-763-0641, dwalls2@earthlink.net, http://youreconomy.org/nets/index.lasso?region=Walls).

We obtained annual data on the number and costs of workers' compensation claims, occupational riskiness, payroll, and establishment names and addresses from the Workers' Compensation Insurance Rating Bureau of California (WCIRB). WCIRB describes itself as a "nonprofit association comprised of all companies licensed to transact workers' compensation insurance in California," which collects "premium and loss data on every workers' compensation insurance policy." The data obtained from WCIRB includes confidential information regarding individual California businesses that WCIRB collected pursuant to certain statutory authority and is not intended to be publicly available. It was made available to us as a contractor to the Commission on Health and Safety and Workers' Compensation (CHSWC) (a California state agency), pursuant to a memorandum of understanding between the California State Insurance

Commissioner and the CHSWC for a specifically defined research study. As such, we are not permitted to share these data. WCIRB's Chief Actuary Dave Bellusci explained to us that WCIRB would not envision releasing this confidential information to other researchers except in similar circumstances to those under which we obtained the data. Researchers seeking to access WCIRB data may contact Dave Bellusci (tel +1-415-777-0777, dbellusci@wcirbonline.org, www.wcirbonline.org).

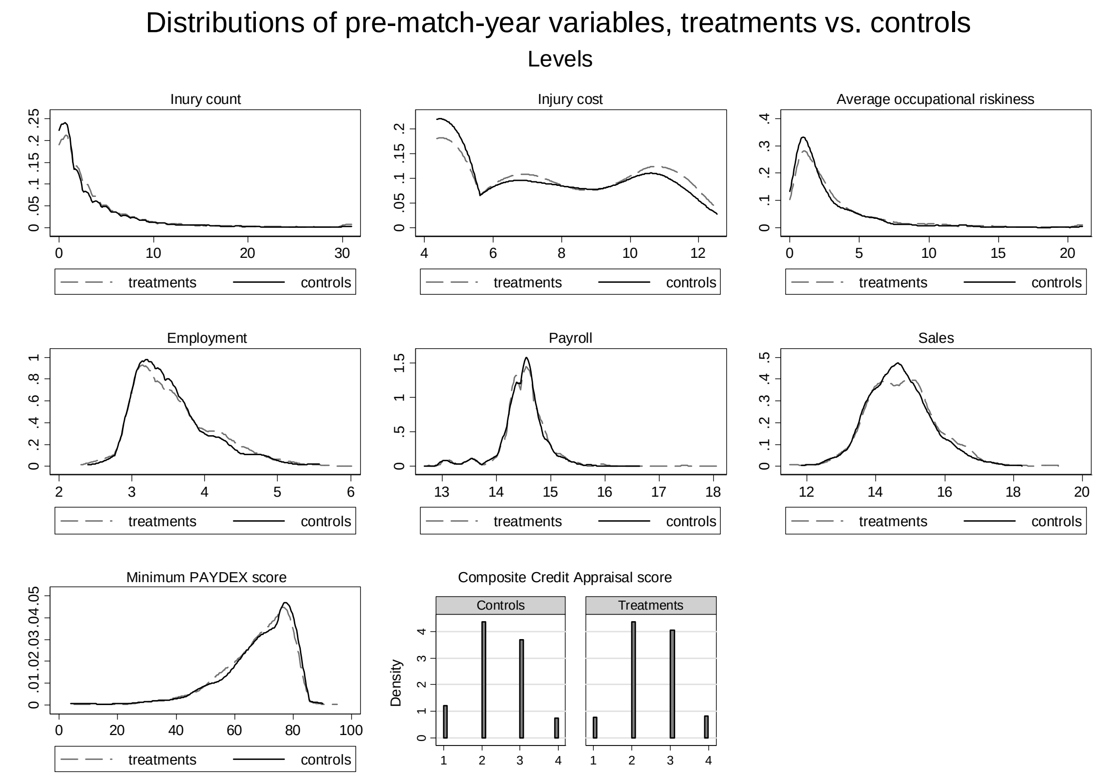

Fig. S1.

Kernel density graphs depicting covariate distributions of matched treatments and controls during the four years before the match year. Average occupational riskiness, Injury count, and Injury cost were top-coded at their 99th percentiles. We took the natural log of Sales, Employment, Payroll, and Injury cost and, as noted in the text, to reduce the effect of very small outliers, we added a small amount to several measures prior to taking logs ($79 to Injury cost, 10 to Employment, and $100,000 to Payroll and to Sales).

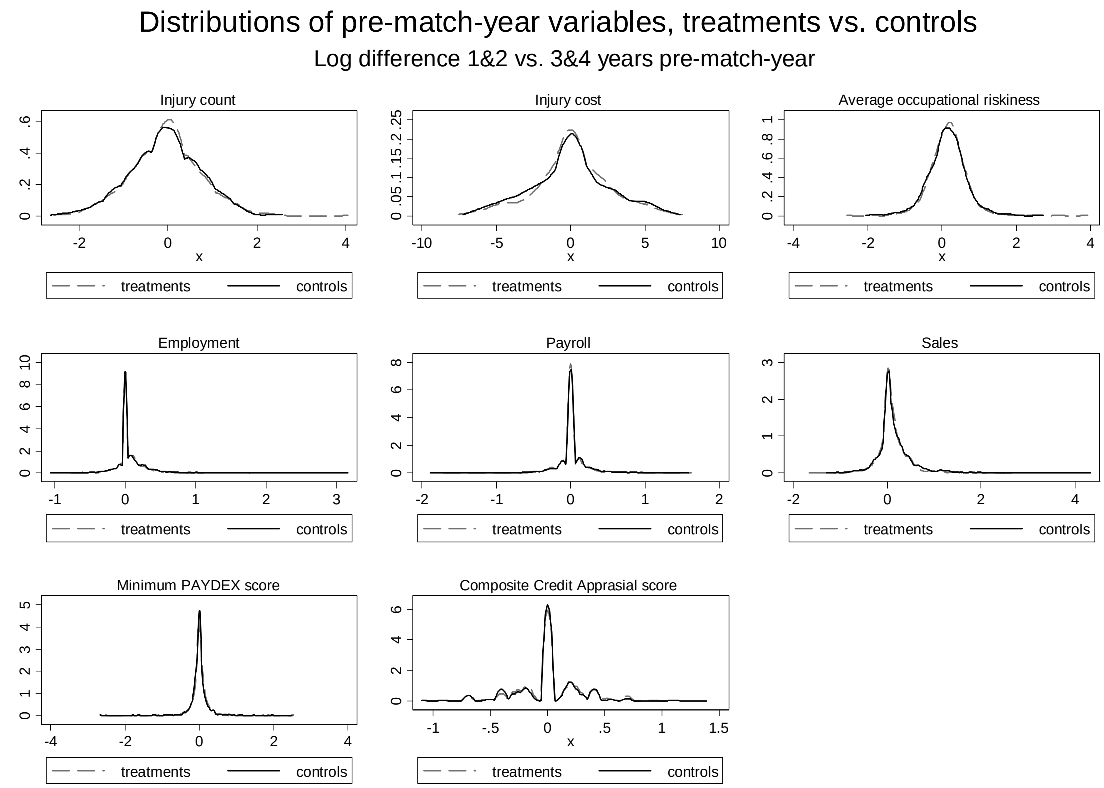

Fig. S2

Kernel density graphs depicting covariate distributions of trends among matched treatments and controls, constructed as the log of the average values three and four years before the match year subtracted from the log of the average values one and two years before the match year. Average occupational riskiness, Injury count, and Injury cost were top-coded at their 99th percentiles.

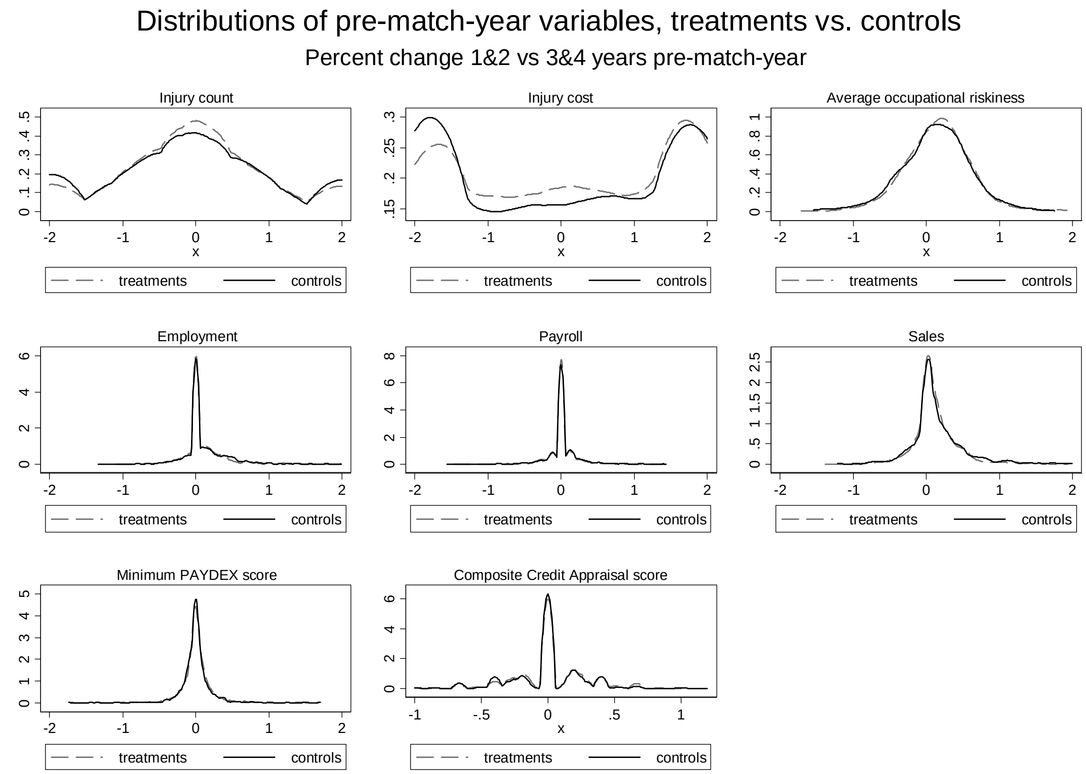

Fig. S3

Kernel density graphs depicting covariate distributions of trends among matched treatments and controls, constructed as the difference in the average value of the three- and four-year lagged values from the average value of the one- and two-year lagged values, divided by half the sum of these values. This ratio approximates "percent change" but is robust to outliers; it ranges from –2 to +2. Average occupational riskiness, Injury count, and Injury cost were top-coded at their 99th percentiles.

Table S1. Pipeline of treatments and potential controls for matched sample

Treatments	Total # treatments	Potential controls (never randomly inspected in IMIS)	Total # potential controls
# of facilities ever randomly inspected in IMIS	1,752		
# of facilities ever randomly inspected linked to NETS	1,392	# of facilities in NETS sharing industry, year, and region with at least one treatment	1,217,893
Of these, those classified as single-establishment firms	952	Of these, those classified as single-establishment firms	1,135,530
with populated employment in NETS in match year	839	with populated employment in NETS in match year	528,141
with at least 10 employees at least once 0–2 years pre-match-year	698	with at least 10 employees at least once 0–2 years pre-match-year	54,676
linked to WCIRB	482	linked to WCIRB	6,600
not sharing a WCIRB ID with an establishment with different address	464	not sharing a WCIRB ID with an establishment with different address	6,377
with average wage >7020 in both 1&2 years pre-match-year	462	with average wage >7020 in both 1&2 years pre-match-year	6,333
with WCIRB data at least once 1–4 years pre-match-year	451	with WCIRB data at least once 1–4 years pre-match-year	6,156
with 0 non-random IMIS inspections 1&2 years pre-match-year	431	with 0 non-random IMIS inspections 1&2 years pre-match-year	6,055
with at least one eligible control (in same industry and region')	410	matched to a treatment	410
Total matched pairs	410		
Drop pair with treatment outside the support of controls' # claims	409		
Total matched pairs for analysis	409		

Table S2. Industry distribution of matched sample

Two-digit SIC code and description		Number of matched pairs of treatment and control establishments
07	Agricultural Services	6
15	General Building Contractors	4
17	Special Trade Contractors	32
20	Food and Kindred Products	49
23	Apparel and Other Textile Products	4
24	Lumber and Wood Products	63
25	Furniture and Fixtures	36
30	Rubber and Miscellaneous Plastics Products	9
32	Stone, Clay, and Glass Products	6
33	Primary Metal Industries	14
34	Fabricated Metal Products	49
35	Industrial Machinery and Equipment	17
36	Electronic and Other Electric Equipment	6
37	Transportation Equipment	15
39	Miscellaneous Manufacturing Industries	8
42	Trucking and Warehousing	5
50	Wholesale Trade—Durable Goods	14
51	Wholesale Trade—Nondurable Goods	18
52	Building Materials and Garden Supplies	5
57	Home Furniture and Furnishings Stores	4
73	Business Services	8
75	Auto Repair, Services, and Parking	3
76	Miscellaneous Repair Services	3
80	Health Services	4
Various	Other industries (with 1–2 matched pairs)	27
	Total :	409

Table S3. Summary statistics

All variables measured in establishment-years, except the two survival variables are at the establishment level. Average occupational riskiness, Injury count, and Injury cost were top-coded at their 99th percentiles. As noted in the text, to reduce the effect of very small outliers, we added a small amount to several measures prior to taking logs ($79 to Injury cost, 10 to Employment, and $100,000 to Payroll and Sales).
In calculating the ratios of injury cost to payroll, we added $100,000 to Payroll (the denominator) to reduce the effect of very small outliers.
Firm survival was based on its presence in the NETS or WCIRB database; disappearance from both indicated firm death.

	N	mean	p50	SD	min	max
Year of random inspection	6245	2001.6	2002.0	3.0	1996	2006
Has been randomly inspected (dummy)	6245	0.24	0.00	0.43	0	1
Sales ($ thousands)	6204	4599.1	2383.4	12942.0	0	411,080
Log sales	6204	14.77	14.73	0.99	11.51	19.83
Employment	6204	34.28	23.00	36.25	0	570
Log employment	6204	3.60	3.50	0.57	2.30	6.36
Minimum PAYDEX score (1 = worst to 100 = best)	5606	67.82	71.00	11.97	4	95
Composite Credit Appraisal (1 = worst to 4 = best)	4794	2.43	2.00	0.75	1	4
Payroll ($ thousands)	5872	2,101.5	1,942.1	1,868.3	134.1	70,333.6
Log payroll	5872	14.50	14.53	0.43	12.36	18.07
Injury count	5880	3.26	1.00	5.41	0	31
Ratio of Injury count to Employment, top-coded at 99th percentile	5821	0.11	0.05	0.22	0	7
Injury cost ($)	5872	25,253	1049	52,674	0	278,961
Log of injury cost	5872	7.41	7.03	2.77	4.36	12.54
Ratio of injury cost to payroll	5872	0.01	0.00	0.05	0	2.11
Ratio of injury cost to payroll, top-coded at 98th percentile	5872	0.01	0.00	0.02	0	0.10
Average occupational riskiness	5872	3.27	1.95	3.83	0.00	21.07
Survived until 2006	816	0.95	1.00	0.22	0	1
Survived three years after match year (among those present at match year)	816	0.98	1.00	0.13	0	1

Table S4. Balancing tests

Average occupational riskiness, Injury count, Injury cost, and Number of violations were top-coded at their 99th percentiles. As noted in the text, to reduce the effect of very small outliers, we added a small amount to several measures prior to taking logs ($79 to Injury cost, 10 to Employment, and $100,000 to Payroll and Sales). Variables subscripted -1&-2 are averages of 1- and 2-year lags, -3&-4 are averages of 3- and 4-year lags, -1to-3 are averages of 1-, 2-, and 3-year lags, and -1to-4 are averages of 1-, 2-, 3-, and 4-year lags. "Percent change" variables are constructed as the difference in the average value of the 3- and 4-year lagged values from the average value of the 1- and 2-year lagged values, divided by half the sum of these values. This ratio approximates "percent change" but is robust to outliers; it ranges from -2 to +2. Standardized difference in means refers to the difference between group means divided by the square root of the average variance across the two groups (Rosenbaum and Rubin 1985). Robust standard errors in brackets, and T: treatment, C: controls. $^{**}p < 0.01$, $^{*}p < 0.05$, $^{+}p < 0.1$.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	N		Mean		T-test		Median		Wilcoxon rank-sum test p value	Kolmogorov-Smirnov equality-of-distributions test p value	Standardized difference in means	Coefficient from conditional logit test if variables collectively predict if establishment is treated
	T	C	T	C	SE	p value	T	C				
Log sales $_{-1to-3}$	409	409	14.82	14.74	0.064	0.25	14.75	14.69	0.36	0.26	8.1	0.066
Percent change in average sales	393	393	0.08	0.12	0.027	0.19	0.04	0.04	0.72	0.51	-9.4	
Log sales $_{-1\&-2}$ – log sales $_{-3\&4}$	393	393	0.08	0.12	0.029	0.22	0.04	0.03	0.62	0.57	-8.8	
Log employment $_{-1to-3}$	409	409	3.6	3.56	0.037	0.33	3.49	3.42	0.26	0.43	6.9	0.393
Percent change in employment	393	393	0.06	0.08	0.021	0.34	0	0	0.60	0.40	-6.8	
Log employment $_{-1\&-2}$ – log employment $_{-3\&4}$	393	393	0.04	0.05	0.015	0.39	0	0	0.60	0.51	-6.1	
Log payroll $_{-1to-4}$	409	409	14.52	14.48	0.027	0.21	14.52	14.51	0.73	0.33	8.8	0.193
Percent change in payroll	362	362	0.01	0	0.016	0.76	0	0	0.60	1.00	2.3	
Log payroll $_{-1\&-2}$ – log payroll $_{-3\&4}$	362	362	0.01	0	0.016	0.80	0	0	0.60	1.00	1.9	
Average minimum PAYDEX score $_{-1to-4}$	356	356	67.45	67.77	0.780	0.68	69	70.25	0.50	0.30	-3.1	-0.021*
Percent change in minimum PAYDEX	311	311	0	-0.01	0.015	0.43	0	0	0.52	0.99	6.4	
Log minimum PAYDEX $_{-1\&-2}$ – log minimum PAYDEX $_{-3\&4}$	311	311	0	-0.01	0.016	0.42	0	0	0.52	0.99	6.5	
Average Composite Credit Appraisal $_{-1to-4}$	296	294	2.47	2.33	0.058	0.02	2.5	2.33	0.04	0.19	20.0	0.434**
Percent change in Composite Credit Appraisal	222	222	0.01	0	0.024	0.55	0	0	0.91	0.77	5.8	
Log Composite Credit Appraisal $_{-1\&-2}$ – log Composite Credit Appraisal $_{-3\&4}$	222	222	0.01	0	0.025	0.55	0	0	0.91	0.77	5.7	
Average occupational riskiness score $_{-1to-4}$	409	409	3.22	2.61	0.220	0.01	2.09	1.69	0.01	0.01	19.5	0.132*
Percent change in occupational riskiness score	362	362	0.14	0.11	0.033	0.38	0.15	0.13	0.69	0.26	6.5	
Log occupational riskiness score $_{-1\&-2}$ – log occupational riskiness score $_{-3\&4}$	362	362	0.15	0.12	0.036	0.40	0.15	0.13	0.69	0.26	6.1	
Injury count $_{-1to-4}$	409	409	3.69	3.05	0.338	0.06	2	1.5	0.05	0.05	13.3	-0.034
Percent change in injury count	301	301	-0.14	-0.12	0.091	0.85	0	0	1.00	0.66	-1.6	
Log injury count $_{-1\&-2}$ – log injury count $_{-3\&4}$	185	185	-0.24	-0.05	0.082	0.02	-0.1	0	0.92	0.18	-24.4	
Log injury cost $_{-1to-4}$	409	409	8.74	8.23	0.158	0.001	9.22	8.78	0.06	0.04	22.6	0.068
Percent change in injury cost	300	299	-0.12	0	0.122	0.33	-0.14	0.04	0.35	0.70	-8.0	
Log injury cost $_{-1\&-2}$ – log injury cost $_{-3\&4}$	185	185	-0.33	0.24	0.228	0.01	-0.13	0.31	0.08	0.07	-26.1	
Conditional logit grouping												Matched pair
Number of pairs of treatment and control establishments												288
Wald test: Sum of coefficients equals 0? (χ^2 statistic)												19.1*
Wald test: Coefficients jointly equals zero? (χ^2 statistic)												18.3*

Table S5. Robustness tests: Injury count and Injury cost

Unlike our primary models reported in Table 1, these models are estimated only on the matched pairs where neither the treatment nor the control were inspected 3 or 4 years prior to the match year. Brackets contain standard errors; $**p < 0.01$, $*p < 0.05$, $+p < 0.10$. The models in Columns 1-2 include establishment-level conditional fixed effects. The models in Columns 3-4 include establishment-level fixed effects. To reduce the effect of very small outliers, we added roughly the first percentile of non-zero values ($79) to Injury cost prior to taking the log. To reduce the effect of large outliers, Injury count, Log Injury cost, and Average occupational riskiness were top-coded at their 99th percentiles. Sample size in Columns 1 and 2 is smaller than in Columns 3 and 4 because the negative binomial specification with conditional fixed effects drops establishments that have no variation in their number of injuries. The bottom six rows report chi-squared statistics in Column 2 and F-statistics in Column 4.

	(1)	(2)	(3)	(4)
Dependent variable	Injury count		Log Injury cost	
Specification	Conditional fixed-effects negative binomial regression		Fixed-effects OLS	
Has been randomly inspected	-0.075+		-0.306**	
(this year or before)	[0.043]		[0.116]	
Year of random inspection		-0.148**		-0.418**
		[0.057]		[0.130]
1 year after random inspection		0.010		-0.202
		[0.059]		[0.150]
2 years after random inspection		0.008		-0.061
		[0.067]		[0.182]
3 years after random inspection		-0.095		-0.538**
		[0.082]		[0.206]
4 years after random inspection		-0.239*		-0.498*
		[0.097]		[0.230]
Year dummies	Included	Included	Included	Included
Observations (establishment-years)	5,009	5,009	5,273	5,273
Number of treatment establishments	349	349	368	368
Number of control establishments	336	336	368	368
Dependent variable sample mean	3.224	3.224	7.323	7.323
Each treatment coefficient is equal to zero (χ^2 or F)		13.89		3.315
{p-value}		{0.016}		{0.006}
Sum of treatment coefficients equals zero (χ^2 or F)		3.846		6.596
{p-value}		{0.049}		{0.010}
All treatment coefficients equal to each other (χ^2 or F)		11.23		2.246
{p-value}		{0.024}		{0.062}

Table S6. Extension: Effects of inspections on minor versus major injuries

Regressions yield evidence that randomized OSHA inspections reduced workplace injury rate and injury cost. Brackets contain standard errors; **p < 0.01, *p < 0.05. Both models include establishment-level conditional fixed effects. To reduce the effect of large outliers, Injury count was top-coded at its 99th percentile. Minor injury claims refers to the annual number of injuries resulting in less than $2000 in workers' compensation. Major injury claims refers to the annual number of injuries resulting in at least $2000 workers' compensation.

	(1)	(2)
Dependent variable	Minor injury claims	Major injury claims
Specification	Conditional fixed-effects negative binomial regression	
Has been randomly inspected	-0.107*	-0.136*
(this year or before)	[0.044]	[0.059]
Year dummies	Included	Included
Observations (establishment-years)	5,211	5,087
Number of establishments	709	690
Number of treatment establishments	368	353
Number of control establishments	341	337
Dependent variable sample mean	2.76	1.00

Table S7. Survival analysis

Brackets contain robust standard errors; **p < 0.01, *p < 0.05, +p < 0.10. Each regression is restricted to pairs in which both the matched treatment and control are "alive" in the match year. To reduce the effect of very small outliers, we added a small amount to several measures prior to taking logs (10 to Employment and $100,000 to Payroll and Sales). Variables subscripted $_{-1to-3}$ refer to averages of each of the three years prior to the match year.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Establishment survival		Establishment survival		Establishment death	
Specification	Logit		Conditional logit		Cox Hazard	
Values reported	coefficients		coefficients		coefficients	
Has been randomly inspected	0.223	0.180	0.236	-0.106	-0.211	0.202
(this year or before)	[0.334]	[0.333]	[0.351]	[0.486]	[0.232]	[0.327]
Log employment $_{-1to-3,}$ mean-centered		-0.277		5.624+		-5.014*
		[0.438]		[3.397]		[2.253]
Log sales $_{-1to-3,}$ mean-centered		0.141		-0.009		0.082
		[0.282]		[0.573]		[0.385]
Log payroll $_{-1to-3,}$ mean-centered		0.558		0.688		-0.553
		[0.418]		[0.878]		[0.633]
Year dummies	Included	Included				
Conditional fixed effects			Matched pair	Matched pair		
Stratified by					Matched pair	Matched pair
Constant	1.924**	2.00**				
	[0.502]	[0.509]				
Observations (establishments)	748	748	68	68		
Observations (establishment-years)					3,980	3,970
Number of treatment establishments	374	374	34	34	409	408
Number of control establishments	374	374	34	34	409	408

Table S8. Creditworthiness regressions

Brackets contain robust standard errors, clustered by establishment in columns 1-2; **p < 0.01, *p < 0.05, +p < 0.10. To reduce the effect of very small outliers, we added $100,000 to Payroll prior to taking the log.

	(1)	(2)	(3)	(4)
Dependent variable	Minimum PAYDEX score		Composite Credit Appraisal score	
Specification	OLS		Ordered Logit	
Has been randomly inspected (this year or before)	0.187	0.026	0.079	0.070
	[0.579]	[0.584]	[0.063]	[0.065]
Log payroll, mean-centered		1.021		-0.016
		[0.763]		[0.064]
Year dummies	Included	Included	Included	Included
Establishment-level fixed effects	Included	Included		
Constant	67.995**	68.171**		
	[0.415]	[0.421]		
Cutpoint 1			-2.230**	-2.238**
			[0.098]	[0.101]
Cutpoint 2			0.140	0.175+
			[0.090]	[0.092]
Cutpoint 3			2.726**	2.772**
			[0.106]	[0.108]
Observations (establishment-years)	5,607	5,302	4,794	4,562
Number of treatment establishments	402	400	386	386
Number of control establishments	392	389	376	370
Dependent variable sample mean	67.81	68.16	2.428	2.442

Table S9. Robustness tests: Injury count and Injury cost

Brackets contain standard errors; **p < 0.01, *p < 0.05, +p < 0.10. The models in Columns 1-2 include establishment-level conditional fixed effects. The models in Columns 3-4 include establishment-level fixed effects. Unlike Table 1, these models were estimated based on Injury count, Average occupational riskiness, and Log Injury cost not being top-coded and on having only $1 added to Injury cost prior to taking the log. Sample size in Columns 1 and 2 is greater than 409 treatments and 409 controls because the negative binomial specification with conditional fixed effects drops establishments that have no variation in their number of injuries.

	(1)	(2)	(3)	(4)
Dependent variable	Injury count		Log Injury cost	
Specification	Conditional fixed-effects negative binomial regression		Fixed-effects OLS	
Has been randomly inspected	-0.103*		-0.524**	
(this year or before)	[0.040]		[0.186]	
Year of random inspection		-0.155**		-0.654**
		[0.054]		[0.208]
1 year after random inspection		-0.030		-0.380
		[0.056]		[0.244]
2 years after random inspection		-0.036		-0.257
		[0.064]		[0.280]
3 years after random inspection		-0.139+		-0.872**
		[0.078]		[0.332]
4 years after random inspection		-0.266**		-0.645+
		[0.093]		[0.377]
Year dummies	Included	Included	Included	Included
Observations (establishment-years)	5,593	5,593	5,872	5,872
Number of treatment establishments	389	389	409	409
Number of control establishments	376	376	409	409
Dependent variable sample mean	3.60	3.60	5.91	5.91
Each treatment coefficient is equal to zero (χ^2 or F)		15.54		2.70
{p-value}		{0.008}		{0.020}
Sum of treatment coefficients equals zero (χ^2 or F)		8.04		6.84
{p-value}		{0.005}		{0.009}
All treatment coefficients equal to each other (χ^2 or F)		9.42		1.32
{p-value}		{0.051}		{0.260}

Table S10. Robustness tests: Employment, Payroll, and Sales

Brackets contain robust standard errors clustered by establishment; **p < 0.01, *p < 0.05, +p < 0.10. Unlike Table 2, these models are estimated on Employment, Payroll, and Sales, where 1 was added to prior to taking logs.

	(1)	(2)	(3)
Dependent variable	Log Employment	Log Payroll	Log Sales
Specification	OLS	OLS	OLS
Has been randomly inspected	0.032	0.006	-0.105
(this year or before)	[0.026]	[0.014]	[0.097]
Year dummies	Included	Included	Included
Establishment-level fixed effects	Included	Included	Included
Observations (establishment-years)	5,278	5,872	3,190
Number of establishments (equals number of fixed effects)	787	818	640
Number of treatment establishments	390	409	329
Number of control establishments	397	409	311
Dependent variable sample mean	3.25	14.44	14.72

Table S11: Prior to inspection, did treatments have higher injury counts and costs than controls conditioning on their size and occupational mix?

Standard errors in brackets; **p < 0.01, *p < 0.05, +p < 0.10. Cross-sectional analysis. All independent and dependent variables are averages for 4 years prior to inspection (designated $_{-1to-4}$). As in our primary analysis, Injury count, Injury cost, Payroll, and Average occupational riskiness were top-coded at 99th percentiles. Expected injury cost = Average occupational riskiness$_{-1to-4}$ * Payroll$_{-1to-4}$.

	(1)	(2)
Dependent variable	10,000 * (Injury count / Expected injury cost)$_{-1\ to\ -4}$	100* (Injury cost / Expected injury cost)$_{-1\ to\ -4}$
Specification	OLS	OLS
Will be randomly inspected	0.008	0.044
	[0.005]	[0.061]
Log (Employment$_{-1\ to\ -4}$ + 10)	0.009	0.138+
	[0.007]	[0.073]
Log (Payroll$_{-1\ to\ -4}$ + $100,000)	0.008	-0.015
	[0.008]	[0.090]
Average occupational riskiness$_{-1\ to\ -4}$	-0.003**	-0.035**
	[0.001]	[0.012]
Dummies for year of match	Included	Included
Observations (establishments)	816	816
Number of treatment establishments	408	408
Number of control establishments	408	408
Dependent variable sample mean	0.0663	0.463

Table S12: Data sources and ability to share with other researchers

Variable	Data Source			Data can be shared with other researchers
	OSHA IMIS	NETS	WCIRB	
Establishment name	•	•	•	No
Scrambled establishment ID created by research team	n/a	n/a	n/a	Yes
Establishment street address	•	•	•	No
Establishment city based on IMIS	•			Yes
Establishment city based on NETS		•		Yes
Establishment city based on WCIRB			•	No
Establishment industry	•	•		Yes
Year	•	•	•	Yes
Year of random inspection	•			Yes
Has been randomly inspected	•			Yes
Sales		•		Yes
Employment		•		Yes
PAYDEX score		•		Yes
Composite Credit Appraisal		•		Yes
Survival		•	•	No
Payroll			•	No
Injury count			•	No
Injury cost			•	No
Average occupational riskiness			•	No

References and Notes

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20. To assess the impact of our handling outliers, we reestimated our models on variables that were neither top-coded to correct for large outliers nor corrected to account for very small outliers (i.e., we added only 1 before taking the log of Injury count, Injury cost, Sales, Employment, and Payroll). The results, presented in tables S9 and S10 in the supplementary materials, continue to indicate that inspections lead to statistically significant reductions in Injury count and Injury cost. The results also continue to yield no evidence that inspections affected Employment, Payroll, or Sales. The magnitude of these estimated effects on injury rates and injury costs exceeded those yielded by our primary model results. This confirms the conservative nature of our primary estimates and suggests the importance of mitigating the influence of outliers.

21. Cal/OSHA would have had some data on injury rates for workplaces they had recently inspected. Because their procedures were to avoid randomized inspections for workplaces with any inspection in the previous 2 years, we dropped potential treatments and controls that had been inspected within 2 years before the match year. Cal/OSHA only had inspected 7% of treatments in the 4 years before the random inspection year; results were unchanged when we dropped treatments or controls with inspections in the prior 4 years.

22. In the supplementary materials, we show that the cost of reported injuries in medical care and lost wages, not counting pain and suffering, is very roughly $8400 per employee in high-hazard industries in California. If an inspection reduces all costs by the same 26% that we estimated for workers' compensation costs (Table 1, column 3) and if there is an average of 33 employees per employer in our sample, then a Cal/OSHA inspection leads to roughly $71,000 in lower medical costs and lost wages per year. If the effect lasts from the inspection year through the next 4 years (as in Table 1, column 4), the total value to society of an inspection is very approximately on the order of $355,000. This estimate is very rough and ignores the underreporting of injuries (*23*, *24*), safety benefits lasting more than 4 years, the reduction in pain and suffering, and (working in the opposite direction) the discounting of future benefits.

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25. The figure of $221,000 is the lower bound of the 95% CI of our estimate on payroll (–0.021) times mean payroll ($2,101,500) times 5 years.

26. A. Smith, *An Inquiry into the Nature and Causes of the Wealth of Nations* (W. Strahan and T. Cadell, London, 1776), vol. 1, chap. 10.

27. The theory that wages are higher for dangerous jobs dates back to Adam Smith's exposition in *The Wealth of Nations* that a blacksmith's hourly wage is much lower than a less-skilled coal miner's because the smith's "work is not quite so dirty, is less dangerous, and is carried on in daylight, and above ground."

28. If there are heterogeneous workers, those who care least about hazard *h* will disproportionately self-select into workplaces with that hazard. In that case, many employees at a workplace with hazard *h* would prefer the higher wages and greater risk to lower wages and remediation of hazard *h* (*26*).

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30. Some share of the nonlinking is due to self-insured firms having no WCIRB record. Among the establishments that successfully linked, we observed a few cases in which multiple NETS establishments shared the same WCIRB identifier. We addressed this by aggregating employment and sales data when the NETS establishments shared the same address and by dropping the establishments when they did not share the same address.

31. Cal/OSHA would have had some data on injury rates for workplaces they had recently inspected, but they avoided randomized inspections for workplaces that had been inspected in the previous 2 years. (Recall that we dropped potential treatments and controls that had been inspected within the 2-year period before the match year.) Cal/OSHA had inspected only 7% of treatments in the previous 4 years.

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