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Jonathan M. Lee, Laura O. Taylor

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Randomized Safety Inspections and Risk Exposure on the Job: Quasi-experimental Estimates of the Value of a Statistical Life

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By JONATHAN M. LEE AND LAURA O. TAYLOR*

The value of a statistical life (VSL) is a critical driver of estimated benefits for federal policies designed to improve human health, safety, and environmental exposures. The vast majority of empirical evidence on the magnitude of the VSL arises from hedonic wage models that have been plagued by measurement error and omitted variables. This paper employs randomly assigned workplace safety inspections to instrument for plant-level risks in a quasi-experimental design to address these limitations. We provide credible causal evidence for the existence of compensating wages for fatality risks and estimate a VSL between $8 and $10 million ($2016).

*Lee: East Carolina University, Brewster A439, 10th Street, Greenville, NC 27858 (e-mail: leejo@ecu.edu). Taylor: North Carolina State University, Box 8109 NCSU, Raleigh, NC 27695 (e-mail: laura_taylor@ncsu.edu). The research reported herein was conducted while the authors were Special Sworn Status researchers of the U.S. Census Bureau at the Triangle Census Research Data Center. Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed. For comments on earlier drafts, the authors thank Michael Anderson, Lori Bennear, Paul Ferraro, Michael Greenstone, Edward Miguel, Melinda Morrill, Daniel Phaneuf, Steven Sexton, Roger von Haefen, and seminar participants and discussants at the American Economic Association Annual Meetings, Cornell University, Georgia State University, Georgia Institute of Technology, the University of South Carolina, the University of Maryland, the National Bureau of Economic Research Summer Institute, and the Southern Economic Association Annual Meetings.

This research exploits randomized workplace safety inspections to identify compensating wage differentials for risky working conditions and provide credibly identified estimates of the "value of a statistical life" (VSL). The VSL is an aggregate measure of individual marginal willingness to pay for risk reductions and it is most commonly estimated with hedonic wage models.[1] Despite decades of empirical research, the credibility of VSL estimates obtained from hedonic wage models continues to be the subject of considerable debate, due in part to its remarkably large role in determining benefit-cost ratios for many federal policies (e.g., Ashenfelter and Greenstone, 2004, Black et al., 2003, Cameron, 2010, Cropper et al., 2011, Robinson, 2007, U.S. EPA 2010, U.S. OMB 2003). For example, a recent review of the benefits and costs of 115 major federal regulations promulgated over the past decade, including health, transportation and environmental regulations, indicates that up to 70% of the total benefits across all rules considered are directly attributable to the monetized value of reducing early mortality (U.S. OMB 2013). These benefits are computed by multiplying the estimated number of lives saved as a result of the regulation by an agency's preferred point-estimate for the VSL.

The extant hedonic wage literature employs cross-sectional or panel data models, usually on national samples of workers, to estimate compensating wage differentials associated with increased occupational mortality risk and compute VSL estimates.[2] However, endogenous regressors and an inability to measure risks at the place of employment have plagued this literature. Occupational-risk measures have only been available as national averages that are aggregated by coarsely defined industry and occupation groups, and are thus subject to

[1] To see how the VSL is computed, suppose there is a group of 100,000 individuals at risk of death from a particular exposure, and it is estimated that the average willingness to pay is $30 per year to reduce the risk of death by 1/100,000. The VSL in this context is equal to $30 x 100,000, or $3,000,000. The VSL does not measure the value of an identified life, but is instead an aggregate of the affected individuals' marginal willingness to pay for marginal reductions in risk.

[2] Viscusi (1992) and Bockstael and McConnell (2007) provide a review of the theory and empirical approaches in the hedonic wage literature focused on estimating the VSL. Mrozek and Taylor (2002), Viscusi and Aldy (2003), Kochi et al. (2006), and U.S. EPA (2010) provide quantitative reviews of the past empirical literature.

considerable measurement error (Black, et al., 2003, Black and Kniesner, 2003, Scotton, 2013).[3] In addition, unobserved worker and job characteristics are likely correlated with job risks and wages, biasing compensating wage estimates in an unknown direction (Black, et al., 2003, Garen, 1988, Scotton and Taylor, 2011, Viscusi and Hersch, 2001). To address this latter point, panel models following workers over time have been employed that control for unobserved worker characteristics (e.g., Kniesner et al., 2012, Kniesner et al., 2010). However, identification of the wage/risk premia relies in these panel models relies on individuals who change jobs to a different occupation and/or industry in order to change the associated job risks, thus not alleviating potential unobserved job characteristic confounders. VSL estimates from this literature vary from as little as $1 million to over $23 million (e.g., Kniesner, et al., 2012, Kochi, 2011).

We address important shortcomings of the existing empirical literature by employing a quasi-experimental design within a general labor-market context that closely mimics the data and framework of the traditional hedonic wage literature, but which credibly controls for endogeneity and reduces noise in the measurement of workplace risk. We overcome endogeneity and measurement error concerns by exploiting conditionally random manufacturing safety inspections conducted by the federal Occupational Safety and Health Administration (OSHA) to instrument for plant-level production worker risks (the workers most exposed). These surprise inspections are thorough, often taking multiple days, are highly visible to employees, and plants are required to correct safety violations within 30 days. Follow-up inspections are conducted to ensure compliance and a tiered penalty structure imposing larger fines for repeat violations are used to prevent relapse. Our results show that inspections reduce plant-level fatality risks by

[3] Past studies have been particularly sensitive to inclusion of industry and occupation indicator variables leading some authors to question the existence of compensating wages for fatality risks on the basis that they are conflated with inter-industry wage differentials (e.g., Hintermann et al., 2010, Leigh, 1995).

approximately 50 percent and that these reductions last through the entire study period.[4]

Our data consists of a 10 year panel of confidential plant-level fatality, wage, and worksite characteristics data from OSHA and the U.S. Census Bureau. A complete census of U.S. manufacturing plants' employment data is collected by U.S. Census Bureau every five years and this data is coupled with a complete census of workplace inspections data and occupational fatality data maintained by OSHA. The panel nature of the data allows individual plants to be tracked over time in order to control for time-invariant worksite characteristics with the inclusion of plant-level fixed effects. Our instrumental variables (IV) estimators also include industry-specific time trends at the most disaggregated 4-digit SIC level thereby eliminating concerns of conflating inter-industry wage differentials with compensating wages for risk (Dorman and Hagstrom, 1998, Leigh, 1995).

Results from our IV models indicate that post-inspection, production workers' wages are reduced by an average of two-to-three percent, suggesting a range for the VSL of $6 to $8 million. These results are robust to a variety of model and sample selection choices, which contrasts starkly with panel models we estimate using commonly employed national average risk rates or even uninstrumented plant-level risk rates. Another unique aspect of our data is that Census collects information on fringe benefit payments to production workers. We are thus able to estimate both direct and indirect payments to employees in exchange for increased job risks. Results suggest that fringe benefit payments account for as much as 35 percent of the compensating payments for workplace risks, increasing the VSL point-estimates to $8 to $10 million when included. Overall, our results reaffirm the existence of compensating wages for occupational risks as suggested by theory and explored empirically in the hedonic wage literature for over 40

[4] Scholz and Gray (1993, 1990), and Gray and Mendeloff (2004) also find that OSHA inspections significantly improve workplace safety.

years, and they support the use of $9 million as a reasonable point-estimate for the VSL in regulatory analysis.[5]

The remainder of this paper is as follows. In the next section we provide a brief overview of OSHA practices for selecting plants for safety inspections that provides the natural experiment we wish to exploit. Section 2 presents an overview of the data and Section 3 presents estimates of the compensating wage differential associated with plant-level safety improvements. Section 3 also computes a range of point estimates for the VSL and offers several robustness tests for our estimation strategy. Section 4 offers conclusions.

I. The OSHA Inspection Process

OSHA was established under the Occupational Safety and Health Act of 1970 to set and enforce workplace safety standards. The majority of OSHA's funding over the past forty years has been devoted to enforcement of standards through workplace inspections (Fleming, 2001, MacLaury, 1984, Siskind, 1993). The law allows states to choose whether they develop and operate their own safety programs or have OSHA's federal program operate within the state. This research focuses on the 28 states and the District of Columbia that operate under the Federal OSHA inspection program since a common and transparent scheduling system for conducting inspections is available for these states. Federal OSHA program states are concentrated in the midwest, south and northeastern census regions, and although not a complete census of U.S. manufacturing plants, the federal states cover 9.5 million manufacturing workers in over 200,000 plants, representing approximately 57% of the U.S. manufacturing workforce.[6]

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[5] Guidance documents for the U.S. Environmental Protection Agency (2000) and the U.S. Department of Transportation (2013) indicate a VSL of approximately $9 million (2016$) is to be used for their agency regulatory impact analyses.

[6] There is not a statistically significant difference in the mean manufacturing employment, wages, and fatality rates among states with federally-administered programs and those with state-administered programs. However, states with Federal OSHA inspection programs have mean in-

Our study period is from 1987 to 1997 during which OSHA conducted an average of 14,000 inspections per year in the manufacturing industries. Approximately half of these are randomly assigned "programmed" inspections, which are unannounced (surprise) inspections that involve a comprehensive and highly visible (to workers) inspection of all aspects of a plant's physical operations that relate to safety or health.[7] Safety violations are documented during an inspection, penalties are established, and OSHA then continues to monitor the plants until all violations are corrected. Most violations are required to be corrected within 30 days, and follow-up inspections are conducted to ensure compliance.

During the study period, OSHA focused on twenty high-risk manufacturing industries for inspections (defined at the 4-digit SIC level), but randomly selected plants within these industries in each state using a neutral selection criteria. Plants with fewer than 11 employees and plants that received comprehensive inspections in the recent past were exempted. Although the definition of "recent past" for inspection varied between one and three years over our study period, the rules are well-documented for each year (1981, 1990, 1995). Thus, programmed inspections during this period were to be randomly distributed among plants, conditioned on industry, plant size, state and recent inspection history according to OSHA policy.[8]

In addition to programmed inspections, OSHA also conducts inspections that occur in response to events such as a complaint by an employee, a follow-up from

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jury accident rates that are 30% lower than states with state-administered programs. While we recognize that the sample is selected, it is not possible to incorporate state-administered OSHA programs because they are not required to disclose their scheduling processes publicly.

[7] During a programmed inspection, an OSHA compliance officer reviews on-site plant-level records of historical injuries and illnesses, meets with employees, and inspects all aspects of a plant's physical operations that relate to safety or health. Upon completion of the inspection process, the compliance officer holds a closing conference with the employer, employees and/or the employees' representative to discuss any findings.

[8] In years outside our study period, OSHA substantially changed its selection criteria, moving to a system that targeted plants based on past injury and illness incidence rates (Brooks1988, OSHA 2004). Specifically, OSHA would randomly select plants to which it sent inspectors, but the inspectors would only decide on whether a full-inspection was conducted after reviewing the plant's injury logs. If the plant's reported injury rates were below the national average for all manufacturing, no inspection was conducted and plants knew this rule. Thus, while plants were still randomly selected to be visited, the decision to conduct an actual inspection was not random.

a previous inspection to ensure compliance, or in response to a serious accident at a plant that results in either a fatality or the hospitalization of three or more workers. While these types of inspections are not the focus of the analysis here, information from accident inspections are used to construct fatality rates at the plant level. Plants receiving an inspection for any of the reasons just noted are not included in the estimation sample since these inspections are not randomly assigned. This results in approximately 7% of plants being deleted from the sample.

II. Data

Our data are obtained from three sources. Safety inspection and fatality records for every OSHA programmed inspection conducted between 1987 and 1997 (>150,000 inspections) are obtained from the publicly available OSHA Integrated Management Information System (IMIS) inspection database.[9] For each inspected plant, IMIS records the plant name, address, 1987 SIC Classification, date of inspection, type of inspection, violations found, fines levied and the number of employees at the plant. A census of workplace fatality and serious injuries requiring hospitalization of at least three workers is also available through IMIS because these automatically trigger an OSHA inspection and report.

Confidential plant-level revenue, expenditures, employment and payroll data are obtained from the U.S. Census Bureau's *Census of Manufactures* (COM) through special approval at the Triangle Census Research Data Center. The COM is a census of all manufacturing plants conducted every five years. Plants are tracked over time using the Permanent Plant Number (PPN), a unique longitudinal identifier assigned by Census. For the purposes of this research, plant-level data are pooled from the 1987, 1992, and 1997 waves of the COM. Plants may open or close during the 11 year study period and still be included in the final sample.

[9] All data are available at http://ogesdw.dol.gov/raw_data_summmary.php (last accessed April, 2011).

To augment the number of observations for each plant, we also employ data from the Census Bureau's *Annual Survey of Manufactures* (ASM). The ASM is a yearly survey conducted in-between COM years, and it collects exactly the same information as the COM for all plants with greater than 1,000 employees and a probability sample of remaining plants based on plant size and contribution to total industry value of shipments. Approximately 14 percent of plants with less than 1,000 employees are surveyed in each ASM year, and although the Census Bureau does not fully disclose their sampling method, they do provide sample weights which we employ throughout our analysis.[10] Data from the ASM are merged into the COM data using each plant's PPN, and we refer to the merged data as simply the Census data.

Plants in the OSHA data are matched by name, address and two-digit SIC Code to the Census data using an iterative record-linkage algorithm first developed by Fellegi and Sunter (1969) and implemented by Gray and Mendeloff (2004), Scholz and Gray (1993, 1990), and Haviland et al. (2010).[11] As a general rule, about half the OSHA records are successfully matched to the Census records, which is similar to the matching rate in other applications (e.g., Haviland, et al., 2010, Walker, 2013). There are a number of reasons for imperfect matches including ownership (name) changes, misreported or miscoded addresses, and plants using multiple addresses (e.g., the same plant may have an on-site delivery address, but use an off-site business office address for correspondence).[12]

During the study period, there were just over 200,000 manufacturing plants in states that employed the federal OSHA inspection program. After linking the

[10] We also estimate and report models dropping data from the ASM.
[11] Details on the matching process are provided in the online appendix.
[12] A less than perfect match rate between OSHA and Census data implies that the control group (uninspected plants) includes some inspected plants. We estimate that less than two percent of the control group would have been inspected at some point during the study period, but not identified as such by our matching. While this would bias our results away from finding an effect of OSHA inspections on safety and wages, as made clear in the results, we find robustly significant impacts. To explore this point further, we manually matched plants in seven industries with high risk rates and high initial match rates to decrease the potential for contamination of the control group. Results remain qualitatively the same for this restricted sample as for the full sample.

OSHA and Census data, there are two restrictions that reduce the number of plants used for estimation. First, plants with fewer than 11 employees are dropped from the sample since these plants are exempt from the OSHA programmed inspection process as just described. This reduces the sample by approximately 50 percent. Second, plants that received their first inspection prior to the start of the study period (1987) are dropped from the sample, as are plants that had a fatality prior to being scheduled for a programmed inspection since fatalities trigger an OSHA safety inspection equivalent to a programmed inspection.[13] This restriction further reduces the sample by approximately 40 percent. To summarize, the final sample (65,700 plants) is an unbalanced panel that includes all manufacturing plants with more than 10 employees, and which have never been inspected or received their first inspection after 1987, and which appear at least twice in the *Census of Manufactures* or *Annual Survey of Manufactures* during the study period. More details on the sample selection process are provided in the online appendix.

Table I summarizes the Census and OSHA data used for estimation.[14] An average of 13,508 plants received an inspection each year, or approximately six percent of all manufacturing plants. Of these inspections, 46% were randomly assigned programmed inspections. Almost 60% of plants that receive a programmed inspection received at least one violation, and there were an average of 8.5 violations found per inspection. Although not reported in Table I, the proportion of inspections resulting in a violation increases to over 75% when

[13] Plants are dropped whose injury logs were reviewed by OSHA prior to 1987 (regardless of whether or not the plant was actually inspected). As discussed in footnote 8, injury log reviews were randomly conducted by OSHA prior to 1987, thus ensuring randomization of the plants that are dropped from the sample based on their prior inspection history. Plants that receive a fatality-related inspection prior to being scheduled for a programmed inspection are dropped because inclusion of these plants in the treatment group may overstate the impact of programmed inspections on fatality rates due to regression to the mean.

[14] *NOTE TO REVIEWERS*: The data reported in Table I are from publicly available OSHA and Census data and do not match our estimation samples because of confidentiality review requirements by the Census Bureau. We have been advised by Census that releasing summary statistics for preliminary samples can jeopardize our ability to release summary statistics for the samples upon which our final models are based. We will substitute the data that matches our estimation sample in the final version of the manuscript.

considering only first time inspections.[15] The average annual fatality rate for the high-risk manufacturing industries targeted by OSHA for programmed inspections was 27.7 deaths per 100,000 workers, nearly five times the fatality rate for all manufacturing industries (6.1 deaths per 100,000 workers).

Panel B of Table I reports average annual plant-level wages and employment in 1997, which is the most recent COM wave used in the analysis. The average hourly wage rate for production workers was approximately \$14 in 1997, or approximately \$19 in 2016 dollars.[16] There was an average of 45 employees per plant, of which 72% were production workers. In addition to payroll and employment information, plant characteristics used in the analysis include total cost of materials, a measure of worker productivity calculated as the value of products shipped divided by production workers' total hours worked, and a proxy for worker turnover rates (see Table 1, Panel B). Also included is an indicator variable for whether or not a plant is a stand-alone firm or part of larger entity (82% are single-unit establishments).

Before presenting the empirical model, it is useful to consider whether or not the data support the assumption that OSHA randomized inspections during the study period. To explore this question, we test for balance in the observable characteristics between inspected and uninspected plants over time, beginning nine years before OSHA enacted its randomization policy and continuing through each year of the study period. Specifically, for all inspected (I) and uninspected (UI) plants in year t, we compute the mean difference among differences in covariate X as $\Delta = E\left[X_t^I | ind_i state_s - E[X_t^{UI} | ind_i state_s]\right]$, conditioning on the plant's industry (ind_i) and state ($state_s$). To be consistent with OSHA's randomization policy, we exclude plants with fewer than eleven employees or

[15] There may be partial treatment associated with inspections since approximately 25% of plants receiving their first inspection receive no violation and violations are the mechanism by which safety conditions are expected to occur. We expect that partial treatment will not bias the VSL since it attenuates the impact of OSHA inspections on both wages and fatality risks by the same amount (assuming that inspections resulting in no violations do not impact wages or fatality risks).
[16] All dollars are reported inflated using the Personal Consumption Expenditures Price Index, available at https://fred.stlouisfed.org/series/PCEPI (last accessed February 2017).

which received a comprehensive inspection within a time period specified by OSHA guidelines.

All available plant-level employment and characteristic variables are tested for balance in each year (see Table I for a list, beginning with *Number of Employees* through *Turnover*). A variable is considered balanced if we fail to reject $\Delta = 0$ at the 10% level. In the years when OSHA did not have a randomization policy (prior to our sample period), we find that many covariates fail to balance in most years including the number of employees, number of production workers, cost of materials, and single-unit plant classification. In contrast, during the eleven years of the study sample, only the covariate *Single Unit Plant* fails the balance test and in only two years. Taken together, these results are suggestive that OSHA did indeed follow its stated policy and randomized inspections during the study period.

III. Compensating Wage Differentials for Risky Working Conditions

The analysis focuses on randomized inspections as a treatment and assumes that post-inspection, all inspected plants are in compliance with safety rules because they were in compliance to begin with or because they make the changes required by law. The control group to which inspected plants are compared is all uninspected plants. On average, if there are compensating wages for dangerous working conditions as theory would suggest, then one would expect average wages of inspected plants to fall relative to the control group post inspection. It is not necessary for real wages to fall to identify the wage/risk tradeoff; only that wages rise less quickly at plants whose safety is improving as compared to plants whose safety levels remain unchanged.

The dynamic impacts of an OSHA programmed inspection on plant-level wages and safety are initially explored by constructing three event studies. First, production worker average real hourly wages at inspected plants and plants that have never been inspected are compared as follows:

$$(1) \qquad wage_{j,t} = a + \sum_{n=-9}^{9} \lambda_n I[PIY_{j,t} = n] + I_{j,t} \times S_s \times T_t + P_j + \epsilon_{j,t},$$

where production worker average real hourly wages (1997$) at the j^{th} plant in time t ($wage_{j,t}$) are regressed on indicator variables, $I\left[PIY_{j,t} = n\right]$, in which $PIY_{j,t}$, is a variable equal to the number of years pre/post inspection for the inspected plants, and $I\left[PIY_{j,t} = n\right]$ is equal to one if $PIY_{j,t} = n$ and equal to zero otherwise (n = -1 omitted category in the model). Plant fixed effects are included, P_j, as are industry-state-year fixed effects, $I_{j,t} \times S_s \times T_t$, since OSHA randomizes its inspections within each industry in a state and year. Note, it is possible for a plant to change industrial classification over time as its production mix changes. Standard errors are clustered at the plant level.

The coefficient, λ_n, represents the mean wage difference for production workers in inspected and uninspected plants n years pre- or post-OSHA inspection, relative to the omitted year immediately preceding inspection. Equation (1) is estimated using the full sample of 65,700 plants and 257,600 observations between the years 1987 and 1997. Note, all reported sample sizes are rounded to the nearest one-hundredth due to U.S. Census Bureau confidentiality requirements.

Figure 1 plots the estimated coefficients and their 95% confidence intervals. Two trends emerge in Figure 1. First, there is not a significant difference in wages at the 5% level between inspected and uninspected plants prior to a plant being inspected. This result again supports the assumption that OSHA is randomly selecting plants for programmed inspections conditioned on industry, state and year. Second, real production worker wages at inspected plants decline relative to uninspected plants after a programmed inspection. There is some evidence that wages begin to adjust quickly post-inspection. Wage differentials between inspected and uninspected plants are statistically significant at the 5% level in years 2, 4, and 7 post-inspection and are significantly different at the 10% level in seven of the ten post-inspection years. Focusing on the period beginning two years post-inspection, real hourly wages generally remain between 20 and 30 cents lower in inspected plants.

To explore the effects of OSHA inspections on plant safety, equation (1) is re-estimated substituting plant-level annual fatality rates for the dependent variable. By construction, all plants have zero fatalities prior to inspections because fatal workplace events automatically trigger a comprehensive OSHA inspection. Figure 2 plots the estimated coefficients for each year post-inspection and their 95% confidence intervals. Similar to the event study for wages, we find a decline in fatalities post-inspection relative to uninspected plants, and the fatality changes are statistically significant at the 5% level in 6 of the 10 post inspection years.

Another way to explore safety impacts post-inspection is to examine the changes in the number of times a plant is found to be in violation of OSHA rules during an inspection. We take the number of violations found to be an indicator of safety conditions at a plant, and since found violations must be corrected, they are the mechanism by which changes in workplace safety conditions are expected to occur. The following model is estimated to explore how violations evolve over multiple inspections:

$$(2) \qquad violations_{j,t} = a + \sum_{n=1}^{10} \beta_n I[IN_{j,t} = n] + I_{j,t} \times S_s \times T_t + P_j + \epsilon_{j,t},$$

where all variables are as defined in equation (1) except the dependent variable of interest is a count of the total number of violations found at plant j at time t. The count variable $IN_{j,t} = n$ is equal to the inspection number for plants inspected in year t (i.e. if $IN_{j,t}$ is equal to ten for plant j at time t, then plant j received their tenth OSHA inspection at time t). The indicator function $I[IN_{j,t} = n]$ is equal to one for observations receiving their nth inspection at time t and equal to zero otherwise. Estimates for β_n are presented in Figure 3, along with their 95% confidence intervals and indicate that the number of violations found during an inspection fall after the first inspection, declining by an average of 50% in the second and subsequent inspections.

In sum, the event studies presented in Figures 1 through 3 suggest that inspected plants share a common trend in wages with uninspected plants prior to the inspection year and that there is a significant non-transitory reduction in both wages and workplace risks post-inspection. The event-studies lead us to adapt the fixed effects estimation strategies presented in equations (1) and (2) to an instrumental variables (IV) model that directly links compensating wages to fatality risks and compute a local average treatment effect for the industries targeted by OSHA. Specifically, we estimate:

(3) $$fatrate_{j,t} = a + \varphi PI_{j,t} + PC_{j,t}\beta + I_{j,t} \times S_s \times T_t + P_j + \epsilon_{j,t},$$

(4) $$wage_{j,t} = \gamma + \delta \widehat{fatrate}_{j,t} + PC_{j,t}\beta + I_{j,t} \times S_s \times T_t + P_j + u_{j,t},$$

where the fatality rate ($fatrate_{j,t}$) at plant j in time t in equation (3) is regressed on an indicator variable, $PI_{j,t}$, that is equal to one if the plant receives a programmed OSHA inspection in year *t* or any year thereafter, and a vector of observable time-varying plant characteristics ($PC_{j,t}$) that includes *Turnover*, *Productivity*, *Number of Employees*, *Cost of Materials*, *Number of Production Workers*, and *Single Unit*.[17] The second stage regression in (4) regresses real average production worker wages at plant j in time t ($wage_{j,t}$) on predicted fatality rates, $\widehat{fatrate}_{j,t}$, from (3) and all else is defined as for (3). Standard errors are clustered at the plant level for both (3) and (4).

The IV model is estimated using a sample of 65,300 plants (252,800 observations). Observations in the year of inspection and the year immediately following an inspection are dropped to allow sufficient time for wage adjustments to occur post-inspection (as is also suggested by figure 1). Models are reported later that explore the sensitivity of the results to the exclusion of these years.

[17] See Table I for variable descriptions.

Table II presents key coefficient estimates for five specifications of the IV model that vary by *i)* the measure of wages or compensation used as the dependent variable in the second stage regression, *ii)* whether or not time-varying plant characteristics are included in the model, and *iii)* whether or not the models are weighted. Wages for production workers are entered either linearly or as their natural log. Also reported is a model in which the dependent variable is the total compensation for production workers measured as the sum of hourly wages and the estimated fringe compensation for production workers at each plant.[18] Our main focus is on models including wages only as the dependent variable, which is consistent with the vast majority of the literature estimating the VSL with labor market data (see, for example, Evans and Schaur, 2010, Evans and Smith, 2008, Kniesner, et al., 2012, Kniesner, et al., 2010, Scotton, 2013, Scotton and Taylor, 2011). However, changes in fringe benefits may be an important margin of adjustment for establishments that has not yet been explored and something we are able to do with our data.

Time-varying plant characteristics are excluded in some models out of concern that inspections may influence these variables as well, rendering these covariates "bad controls" (Angrist and Pischke, 2009). Lastly, we present models that are either unweighted or weighted by both the ASM survey weights and the number of production workers at a plant. The ASM weights are provided directly by U.S. Census Bureau, and address the oversampling of large establishments in ASM years. Weighting by the number of production workers addresses heteroscedasticity that may arise from using plant-level average wages and fatality rates. Production worker weights are computed using the two-step procedure outlined in Dickens (1990) and Solon et al. (2015) that corrects for heteroscedasticity in the presence of both a clustered error component (plant identifier) and a group-size error component (number of production workers).

[18] Total fringe compensation for all workers at a plant is reported in the COM. To estimate the total fringe compensation for production workers, we multiply total fringe compensation by the ratio of production worker wage payments to wage payments for all employees at each plant.

As indicated in Table II, Panel A, there is consistent evidence across models that OSHA programmed inspections reduce plant-level fatality rates. The coefficient estimates are all significant at the 1% level and stable across model specifications, suggesting that OSHA inspections reduce plant fatality rates by about 1.4 fatalities per 10,000 production workers. Although not reported in Table II for succinctness, time-varying plant characteristics included in the model are not statistically significant predictors of fatality rates.[19] Finally, the last row of Panel A reports Kleibergen-Paap F-statistics and are all greater than ten, suggesting that receiving an OSHA programmed inspection is indeed a strong instrument (see Bound et al., 1995, Kleibergen and Paap, 2006, Staiger and Stock, 1997).

The average number of fatalities in the OSHA targeted industries is 2.8 deaths per 10,000 workers and thus our empirical models suggest plant-level fatality risks are decreased by approximately 50% after plants receive their first programmed OSHA inspection.[20] While there are no other estimates to which we can directly compare our results, Scholz and Gray (1993, 1990) find that OSHA inspections reduce nonfatal injury rates by 15 to 22 percent, roughly a half to a third of the impact suggested by our models. The divergence in our estimates may in part be driven by the fact that Scholz and Gray's estimates average over all OSHA inspections a plant has ever received, while our estimation strategy focuses on first-time inspections in which the most dangerous violations are likely to be found and corrected. As noted earlier in Figure 3, the number of violations a plant receives during an inspection declines by nearly 70% in the second inspection, and remains low in all subsequent inspections indicating diminishing opportunities for repeated inspections to impact safety.

[19] The complete set of coefficient estimates for the model presented in column 1 of Table II are reported in the online appendix.
[20] Nonlinear poisson and negative binomial fatality count models were also estimated and suggest that programmed inspections result in a 56% reduction in plant-level fatalities, which is similar to the estimates reported for the linear IV models.

Panel B in Table II presents results from the second-stage wage regression (equation 4). Similar to the first-stage results, the key coefficient measuring the impact of an incremental increase in fatality risks on production worker wages is robust to changes in model specification. The estimated reduction in hourly wages that result from an incremental increase in workplace safety varies from 20 cents per hour for the unweighted model including time-varying plant characteristics to 25 cents per hour for the log-transformed wage model, assuming a mean wage of $13.91 (all in 1997 dollars). The last column of Table II suggests fringe payments are an important margin for adjustment. Including fringe benefits as part of total compensation increases the compensating differential for workplace risks by nearly 40 percent to 34 cents per hour.

The first row of Table III presents VSL estimates corresponding to the models in Table II. The VSL is computed by multiplying the estimated coefficient reported in Panel B of Table II (δ in equation 4) by the number of hours worked per year divided by the marginal risk change (1/10,000). We assume an average of 2,000 hours worked per year and a mean wage of $13.91 in 1997$ for the log-transformed models.[21] All VSL estimates are inflated from 1997 to 2016 dollars using the Personal Consumption Expenditures Price Index.[22] Focusing on the specifications with wages as the dependent variable, the VSL point estimates range from $5.7 to $7.0 million in 2016 dollars. The inclusion of fringe benefits increases the point estimates by approximately 50 percent to $9.4 million.

The remainder of Table III presents additional VSL estimates based on alternative samples that allow us to evaluate sensitivity to potential frictions in the wage adjustment process following an OSHA inspection and the inclusion of ASM data in our estimation sample. The second through fourth rows present results for models that: *i)* include all observations from all years, including the year that the first inspection occurs, *ii)* drops only observations from the

[21] Production workers in the twenty industries studied worked an average of 2,003 hours in 1997 according to publicly available COM data (available at https://www.census.gov/prod/www/economic_census.html, last accessed February 2017).
[22] Available at https://fred.stlouisfed.org/series/PCEPI (last accessed February 2017)

inspection year, *iii)* drops observations from the inspection year and the subsequent year as in the baseline model and also drops observations that are from the ASM survey. The VSL point estimates from alternative samples are quite similar to the baseline results presented in the first row of Table III. Overall, Table III suggests point estimates for the VSL of $6 to $7 million when considering wages alone, and this range increases to $8 to $10 million when considering total compensation to the worker that includes both wages and fringe benefits.

To compare our VSL results to the extant hedonic wage literature, we focus on specifications using wage as the dependent variable and that weight by production workers and the ASM survey weights. The VSL point estimates range from just under $6 million to $7.5 million (2016 dollars). As noted earlier, VSL point estimates based on best available national average risk rates that vary by occupation within an industry vary from as little as $1 million to over $23 million, although most estimates lie between $5.2 to $13.0 million in 2016 dollars. This range includes estimates arising from similar worker samples and taken during the same time period as our study (e.g., Viscusi, 2004 reports estimates of $9.8 to $11.8 million for his comparable sample of workers); risk rates that vary only by industry (e.g., Evans and Schaur 2010, Evans and Smith 2008); and risk rates that vary by occupation within industries (e.g., Kniesner, et al., 2012, Kniesner, et al., 2010, Scotton, 2013, Scotton and Taylor, 2011). While our estimates are generally on the lower end of the current range, 95% confidence intervals overlap and the inclusion of fringe benefits increases the our point estimates closer to the average from the recent literature.

A. Robustness and Additional Analysis

In this section, we explore potential threats to the validity of our estimation strategy by testing for general equilibrium effects of OSHA inspections on non-inspected plants. We also present falsification tests using non-production worker wages as our outcome variable; explore the impact of OSHA inspections on factor productivity and employment levels; and estimate traditional hedonic wage models that mimic the approach in the extant VSL literature to which we compare our IV estimates.

To test for general equilibrium effects of OSHA inspections on plant-level risks, we examine spillovers among closely related plants using two approaches. First, we define a related plant geographically and explore whether an OSHA inspection at one plant has spillover effects on the safety at other plants in the same by metropolitan statistical area (MSA) by estimating the following model:

$$(5) \qquad fatrate_{j,t} = a + \varphi PI_{j,t} + \gamma Related\ MSA_{j,t} + I_{j,t} \times S_s \times T_t + P_j + \epsilon_{j,t},$$

where all variables are defined as in equation (3) except for the additional covariate *Related MSA* that is equal to one for all plants in an MSA after any plant in that MSA receives the first federally programmed OSHA inspection, and is equal to zero otherwise. The programmed inspection indicator variable, $PI_{j,t}$, is fully nested within the related inspection indicator, *Related* $MSA_{j,t}$, and indicates any additional direct effects of OSHA inspections net of spillover effects from related plants.

A second way to consider spillover effects from OSHA inspections is to define related plants by their company ownership. Equation (5) is re-estimated replacing *Related* $MSA_{j,t}$ with an indicator variable, *Related* $Firm_{j,t}$ that is equal to one for all plants owned by the same parent company each year after the first plant owned by the parent company receives a federally programmed OSHA inspection, and equal to zero otherwise.

Key coefficient estimates for equation (5) are presented in the first column of Table IV, while the second column presents the results testing for spillover effects among multi-unit plants of the same company. Results indicate that related plants in the same MSA or plants owned by the same parent company do not experience a change in fatality risks after a related plant receives an OSHA inspection. The coefficient estimates for *Related MSA* or *Related Firm* are near zero and not statistically significant. However, as indicated in the first row of Table IV, our main treatment variable, *PI*, is stable in magnitude and highly significant. The last two columns of Table IV present models identical to the first two columns, but which use real wages as the dependent variable. Again, results indicate that an inspection at plants related through location or company ownership does not create spillover effects on wages at uninspected plants.

Next, we present a falsification test that estimates the impact of OSHA inspections on wages of employees that are not production workers (e.g., clerical and management positions). Because non-production employees are less likely to be impacted by OSHA safety rules, we expect the wages of non-production workers to be unaffected by inspections. To test this assumption, the following model is estimated:

$$(6) \qquad Other\ Worker\ Wages_{j,t} = a + \varphi PI_{j,t} + I_{\mathrm{j,t}} \times S_s \times T_t + P_j + \epsilon_{j,t}.$$

The dependent variable, *Other Worker Wages*, is obtained directly from the Census of Manufacturers and the Annual Survey of Manufacturers. The estimated key coefficient, φ, is presented in the first row of Table V (Model 1). Consistent with our expectations, we do not find a statistically significant impact of OSHA inspections on the wages of non-production workers.

Our IV estimator assumes that inspections result in costly safety improvements that lower plant wages, all else constant. To better understand the potential mechanisms underlying this assumption, we explore how plant employment levels, turnover rates and productivity are impacted by inspections. Table V also presents eight additional models that are identical to equation (6), but which use one of eight different dependent variables describing an outcome of interest. The definitions of each dependent variable and the coefficient estimate for the dummy variable indicating a plant has received a programmed inspection (φ in equation 6) are presented in the table.

Models (2) through (5) present estimates of the impact of OSHA inspections on productivity, cost of materials, and capital stock. Results indicate that inspections reduce total factor productivity (model 2) and production worker productivity (model 3) suggesting that inspections raise plant production costs, in addition to any fines that may be levied. Specifically, inspections are estimated to reduce average production worker productivity by approximately 10 percent and total factor productivity by approximately 1.6 percent post inspection, translating to an average annual productivity loss of about $165,000 per plant. For comparison, Greenstone et al. (2012a) estimate that the US Clean Air Act reduced regulated manufacturing plants total factor productivity by an average of 2.6 percent over the 1972-1993 time period. While our estimated impacts are smaller than Greenstone et al., they are still substantial and would outweigh labor cost savings suggested by our IV model.[23] Given OSHA-induced safety improvements are relatively costly to plants, it is not surprising that we find no evidence of general equilibrium effects from inspections as previously highlighted.

Changes in total factor productivity appear to be driven by labor productivity changes rather than capital adjustments since we find no significant impact of inspections on capital stock or cost of materials (models 4 and 5, respectively) but do find a significant impact on employment (models 6 through 8). Specifically, there is a significant increase in non-production workers post-inspection (model 6), which is consistent with additions of safety and process managers as one mechanism to reduce worker risks following OSHA inspections. There is also a proportionally similar increase in production workers post-inspection (model 7) and thus no significant change in the share of total employees classified as pro-

[23] Wald estimators indicate that OSHA inspections reduce plant wages by approximately $0.34, implying that inspected plants reduce annual payroll expenses by an average of $22,000.

duction workers (model 8).[24] Finally, we find no significant impact of OSHA inspections on plant turnover rates (model 9), although our turnover measure is noisy since we only have total production worker counts each quarter and are only able to detect turnover when employees leave and are replaced in the same quarter.

Finally, we compare our IV estimates to standard hedonic wages models that employ commonly used national-average industry-specific risk rates (e.g., Evans and Schaur, 2010, Kniesner, et al., 2012, Scotton, 2013, Scotton and Taylor, 2011, Viscusi, 2004). Two traditional measures of industry-level fatality risks are constructed by aggregating production worker fatality and employee counts to either a two-digit SIC level representing industrial sectors or a more finely partitioned four-digit SIC industry classification. Both these measures are directly analogous to the national average risk rates typically employed in the extant hedonic wage literature. Although past studies typically pool across all types of workers, and so risks are also varied by broad occupational classes within each industry, one of these occupational classes is production workers and so our risk rates are directly analogous within the context of our specific sample of workers.

After constructing the more traditional fatality risk rates, the following hedonic wage equation is estimated using OLS:

$$(7) \qquad lnwage_{j,t} = \gamma + \delta riskrate_{j,t} + PC_{j,t}\beta + P_j + u_{j,t}.$$

where the natural log of real wages at plant *j* in year *t* are regressed on one of three risk rate measures: the two aggregate industry measures noted above, and uninstrumented plant-level fatality risk rates. A vector of plant-level characteristics, *PC*, are included as defined in equation (3), as are plant fixed effects, P_j. We

[24] In the simplest profit maximization formulation with convex safety provision costs and no workers' compensation requirements, employment would be expected to increase as safety improves. However, as Kniesner and Leeth (1991) and Bockstael and McConnell (2007) illustrate, when the theoretical model is slightly modified to accommodate workers' compensation, the relationship between plant employment levels and safety provision is theoretically ambiguous.

also consider models in which plant-level fixed effects are replaced with industry-sector fixed effects created at the two-digit SIC level.

Resulting VSL estimates from the estimation of equation (7) are presented in Table VI. In stark contrast to our IV estimates, the results are highly sensitive to model specification and often result in implausibly large (> $100 million) or exceptionally small (< $50,000) VSL estimates. The very small OLS estimates of the VSL based on plant-level fatality risks are consistent with classical measurement error highlighting the need for an IV approach even when data on plant-specific fatalities are available.

IV. Conclusions

This research provides quasi-experimental estimates of the VSL within a labor-market context as an alternative to traditional hedonic wage applications. Notably, the research uses exogenous changes in risks at the place of employment that are an improved alternative to nationally aggregated risk measures that are typically used in hedonic wage applications. We are able to ameliorate concerns regarding omitted variable bias by employing randomly assigned OSHA inspections as an exogenous instrument affecting plant level safety. Our IV models suggest that workers' wages are reduced by approximately two percent after a comprehensive OSHA inspection is conducted, translating to VSL estimates between $6 and $8 million in 2016 dollars.[25] These results are robust to a variety of samples and model specifications. When considering responses in both wage and fringe benefits offered by the employer post-inspection, VSL

[25] Recently, Kuminoff and Pope (2013) highlight that quasi-experimental applications in hedonic property value studies often estimate capitalization rates (differences in prices arising from movement between hedonic equilibria) under certain conditions rather than identifying marginal willingness to pay (MWTP) measures that are obtained from a single, stable hedonic rent gradient à la Rosen (1974). Capitalization rates are equal to MWTP under the assumption that changes in the policy variable do not shift the hedonic price function. We expect plant-level OSHA inspections would not shift the labor market hedonic equilibrium given only six to seven percent of plants are inspected each year. Regardless of whether the hedonic wage function shifts, capitalization rates identify MWTP as long as the instrument is randomized, which is clearly the case with OSHA inspections during our study period.

estimates increase to $8 to $10 million, which is roughly in the mid-point of conventional cross-sectional or panel-data based hedonic wage models.

There are few other quasi-experimental studies to which we can compare our results. Within a transportation choice context, Ashenfelter and Greenstone (2004) and Leon and Miguel (2017) exploit exogenous changes in transportation risks for commuters and report VSL estimates of approximately $1 to $2 million.[26] Rohlfs et al. (2015) take changes in air bag regulations for U.S. motor vehicles to be a quasi-experiment and estimate a median VSL of $10 to $12 million (2016$), although their estimates are quite imprecise ranging from less than -$10 million to over $18 million, and are negative for the lower quartile of their data.[27]

Taken as a whole, our results suggest that compensating wage differentials for risky working conditions do indeed exist as suggested by theory and explored empirically in the hedonic wage literature for over 40 years. However, our results also suggest that the empirical challenges inherent in estimating the VSL via cross-sectional or panel-data hedonic wage models have not yet been fully addressed. We estimate models that mimic the existing hedonic wage literature and although we are able to recover VSL estimates that are consistent with the existing literature based on similar samples of workers (e.g., Viscusi, 2004), our estimates are highly unstable and often result in implausible VSL estimates.

Of course, our approach is not without limitations as well, especially in regards to transferability of our results to a more general population.[28] Our analysis focuses solely on production workers in the manufacturing industries. This more

[26] Kochi and Taylor (2011) also estimate the VSL based on transportation risks, although not in a quasi-experimental framework, and find that automotive accident risks are not compensated at all for occupational drivers.
[27] Additional quasi-experimental applications estimating the VSL include Greenstone et al. (2012b) who estimate a structural model of military re-enlistment choices utilizing exogenous variation in re-enlistment bonuses to estimate a VSL of $3.0 to $4.0 million for military personnel, and Schnier et al. (2009) who estimate a VSL of about $5 million 2016 dollars for Alaskan crab fishermen.
[28] Another limitation is that we do not have data on routine plant level non-fatal accident risks. While industry-specific injury rates are available publicly, they would be absorbed by our industry-by-state-by -year fixed effects.

narrow focus may lead to VSL estimates that are inappropriate for general population as targeted by environmental and transportation safety regulations. This is also a limitation of many hedonic wage samples focused mostly on male blue collar workers (e.g., Viscusi, 2004). In addition, endogenous sorting of more productive workers into safer jobs will likely bias VSL estimates downward (Garen, 1988, Hwang et al., 1992).[29] DeLeire et al. (2013) develop a generalized endogenous sorting empirical model to address the notion that workers who are more productive in safer workplace conditions will sort into safer jobs. Using data on individual workers, they find VSL estimates increase by roughly $9 million within their application when endogenous sorting is directly incorporated into the modeling strategy. However, without detailed data on worker characteristics we are unable to explore endogenous sorting in our application. Continued research that carefully identifies exogenous variations in workplace risk that are linked to observable monetary tradeoffs is clearly needed.

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[29] Alternatively, endogenous sorting could be based on skill at avoiding risks in which case the unobserved productivity bias will bias the VSL in the opposite direction (Garen, 1988, Shogren and Stamland, 2002).

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Table I. Summary Statistics

Variable Name	Description	Mean
	Panel A: OSHA Inspection Program, 1987 – 1997 [1]	
Total Annual Inspections	Average annual inspections.	13,508
Programmed Inspections	Average annual programmed inspections.	6,248
Programmed Inspections w/ Violations	Average annual programmed inspections that result in at least one violation found.	3,682
Avg. # Violations	Average number of violations among plants with at least one violation.	8.5
Manuf. Fatality Rate [2]	Average annual fatality rate per 100,000 workers in all manufacturing industries.	6.1
Fatality Rate in OSHA targeted Industries [3]	Average annual fatality rate per 100,000 workers for industries targeted by OSHA for randomized inspections.	27.7
	Panel B: Average Payroll, Employment, and Plant Characteristics (1997). [4]	
# Plants	Total number of plants in estimation sample.	65,700
Hourly Wage ($1997)	Average hourly wages of production workers in a plant; computed as the total annual payroll for production workers in a plant divided by total hours worked by production workers.	$13.91
Number of Employees	Average total employees per plant.	44.96
No. Production Workers	Average number of production workers [% of total employees].	32.23 [72%]
Cost of Materials	Total cost of all materials consumed or put into production for the year, measured in $1997 millions.	$5.5
PW Productivity	Total value of all products shipped by a plant each year divided by total hours worked by production workers (PW) in that year ($1997).	$159.76
Single Unit Plant	Dummy variable equal to 1 for plants that are single unit establishment, and equal to 0 for multi-unit establishments.	82%
Turnover	Average decrease in production workers on payroll between quarters across the year as a percent of the average number of production workers employed that year.	0.3%

[1] Inspections data are for states that operate under Federal OSHA jurisdiction, and are available from the OSHA IMIS database (available at https://enforcedata.dol.gov/views/data_summary.php last accessed February 2017).

[2] Fatality rate is constructed by the authors using data from the BLS CFOI data reporting industry average fatality rates. See https://www.bls.gov/iif/oshcfoi1.htm (last accessed February 2017).

[3] Fatality rate for OSHA targeted industries is computed by the authors from the OSHA IMIS database reporting the number of deaths and workers in each industry (available at https://enforcedata.dol.gov/views/data_summary.php last accessed February 2017).

[4] All data are from publicly available COM files for 211,567 plants in states that operate under Federal OSHA jurisdiction in the year 1997, and thus do not strictly match the restricted-access sample upon which the empirical analysis is based. The public data is downloadable via https://www.census.gov/prod/www/economic_census.html (last accessed February 2017). *Note to reviewers: Summary statistics that match the estimation sample will be requested for release once models are final*.

Table II. IV estimates of plant-level changes in risks and wages in response to receiving an OSHA inspection.[a]

	Panel A				
	Select Coefficient Estimates for First Stage Regression (Equation 3)[b]				
Programmed Inspections (*PI* = 1 in year of inspection and each year thereafter; =0 otherwise)	-1.338*** (0.338)	-1.339*** (0.338)	-1.398*** (0.309)	-1.394*** (0.309)	-1.270*** (0.314)
Model Variations					
Plant Characteristics Included:[c]	Yes	No	No	No	No
Weighting:[d]	None	None	ASM*PW	ASM*PW	ASM*PW
R-squared	0.296	0.296	0.294	0.294	0.294
F-statistic[e]	15.70	15.71	20.34	20.32	16.32
	Panel B				
	Select Coefficient Estimates for Second-Stage Regression (Equation 4)[f]				
Fatality Rate ($\widehat{fatrate}_{j,t}$)	0.204*** (0.0715)	0.223*** (0.0751)	0.242*** (0.0795)	0.0180*** (0.00599)	0.337*** (0.115)
Model Variations					
Dependent Variable:	Wages	Wages	Wages	Ln(Wages)	Wages + Fringe
Plant Characteristics Included:[c]	Yes	No	No	No	No
Weighting:[d]	None	None	ASM*PW	ASM*PW	ASM*PW
R-squared	0.484	0.433	0.547	0.582	0.347

[a]Statistical significance at the 1%, 5%, and 10% level are represented by ***, **, and *, respectively. All models are based on 252,800 observations (65,300 plants) and include plant and industry-by-state-by-year fixed effects as specified in equations (3) and (4). Standard errors clustered at the plant level are in parentheses. Sample size is rounded to the nearest hundredth due to U.S. Census Bureau confidentiality requirements.

[b] Dependent variable is annual plant-level fatality rate measured in deaths per 10,000 workers.

[c] The six plant-level characteristics considered for inclusion are *Number of Employees*, *Number of Production Workers*, *Cost of Materials*, *Productivity*, *Single Unit Plant*, and *Turnover* as described in equations (2) and (3) and summarized in Table I.

[d] Sample weights provided by the US Census Bureau for its Annual Survey of Manufacturers (ASM) are combined with transformed inverse production worker weights as described in Section III.

[e] The Kleibergen-Paap F-statistics test the restriction that *Programmed Inspections* have no effect on plant-level fatality rates.

[f] Dependent variable is real plant-level average production worker wages in 1997$.

Table III. Value of Statistical Life Estimates by Estimation Sample and Model (reported in millions, 2016$).[a]

	Value of Statistical Life Estimate (95% Confidence Intervals)				
Sample Used in Estimation:					
1. Baseline Sample (Year of Inspection & Following Year Excluded)	$5.69	$6.21	$6.74	$6.98	$9.39
	(1.78 – 9.59)	(2.11 – 10.32)	(2.40 – 11.09)	(2.43 – 11.53)	(3.09 – 15.69)
No. Obs. = 252,800					
No. Plants = 65,300					
2. All Years	$6.02	$6.52	$6.49	$6.55	$10.00
No. Obs. = 257,600	(1.53 – 10.51)	(1.85 – 11.20)	(0.98 – 12.01)	(1.04 – 12.06)	(1.83 – 18.16)
No. Plants = 65,700					
3. Year of Inspection Excluded	$6.33	$6.86	$5.82	$5.81	$7.67
No. Obs. = 254,500	(1.54 – 11.11)	(1.85 – 11.86)	(0.83 – 10.82)	(0.74 – 10.89)	(1.57 – 13.78)
No. Plants = 65,300					
4. Baseline Sample & ASM Observations Excluded	$6.80	$7.27	$6.83	$7.56	$7.93
	(1.43 – 12.17)	(1.65 – 12.90)	(1.96 – 11.70)	(2.32 – 12.79)	(2.20 – 13.67)
No. Obs. = 133,600					
No. Plants = 63,300					
Model Variations:					
Dependent Variable:	Wages	Wages	Wages	Ln(Wages)	Wages + Fringe
Plant Characteristics Included?	Yes	No	No	No	No
Weighting method:	None	None	ASM*PW	ASM*PW	ASM*PW

[a] All VSL estimates are based on IV models that include plant and industry-by-state-by-year fixed effects as specified in equations (3) and (4), and standard errors clustered at the plant level. Models variations upon which VSL estimates are based are defined in detail in Table II. Sample sizes are rounded to the nearest hundredth due to U.S. Census Bureau confidentiality requirements.

Table IV. Tests for General Equilibrium Effects Among Related Plants.[a]

	Dependent Variable			
	Fatality Rate	**Fatality Rate**	**Wages**	**Wages**
	(1)	(2)	(3)	(4)
Programmed Inspection (PI)	-1.370***	-1.451***	-0.345***	-0.353***
	(0.310)	(0.308)	(0.0824)	(0.0962)
Related MSA	-0.00869	----	0.0127	----
	(0.111)	----	(0.0439)	----
Related Firm	----	0.0896	----	0.0134
	----	(0.0823)	----	(0.0615)
R-squared	0.295	0.295	0.778	0.778

[a] Statistical significance at 1%, 5%, and 10% level are represented by ***, **, and *, respectively. All models are based on 252,800 observations (65,300 plants) and include plant and industry-by-state-by-year fixed effects as specified in equation (5). Standard errors are clustered at the plant level. Sample size is rounded to the nearest hundredth due to U.S. Census Bureau confidentiality requirements.

Table V. Select coefficient estimates describing the impact of OSHA inspections on non-production employee wages, factor productivity measures, and employment levels.[a]

Model	Dependent Variable	Definition	Coef. Estimate for φ in Equation 6. (std. error)
(1)	*Other Workers Wages*	Wages of non-production workers (e.g., clerical and managerial), available directly from the COM and ASM surveys.	0.334 (0.507)
(2)	*Ln(TFP)*	Log total factor productivity calculated by Foster et al. (2016) as the residual amount of plant output that is not explained by differences in capital, labor, or materials using an input index method.	-0.0165** (0.0077)
(3)	*PW Productivity*	See Table I for definition.	-16.40*** (4.040)
(4)	*Capital Stock*	Real plant-level capital stock calculated by Foster, et al. (2016) using perpetual inventory method.	-470.2 (659.1)
(5)	*Cost of Materials*	See Table I for definition.	-0.0208 (0.0477)
(6)	*No. Other Employees*	Number of total employees per plant (available directly from the COM and ASM surveys) minus number of production workers.	2.850* (1.490)
(7)	*No. Production Workers*	See Table I for definition.	7.940*** (1.670)
(8)	*Share Production Workers*	Percent of total employees who are production workers.	0.00219 (0.0029)
(9)	*Turnover*	See Table I for definition.	-0.00343 (0.0049)

[a] Results for nine models are presented that are based on equation (6) and only vary by the dependent variable used in estimation. The coefficient estimates reported are for φ in equation (6). Statistical significance at 1%, 5%, and 10% level are represented by ***, **, and *, respectively. All models use 252,800 observations (65,300 plants) and include plant and industry-by-state-by-year fixed effects. Standard errors are clustered at the plant level. Sample size is rounded to the nearest hundredth due to U.S. Census Bureau confidentiality requirements.

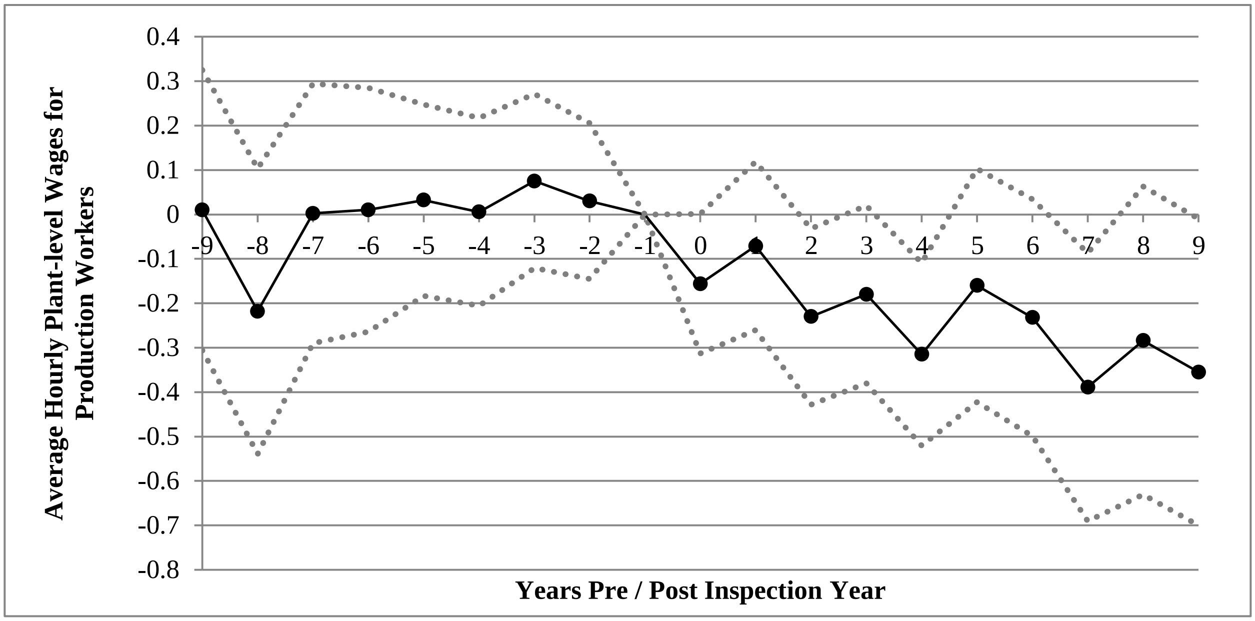
Table VI. Value of Statistical Life Estimates Arising from Alternative and Un-instrumented Risk Rates (reported in millions, 2016$).[a]

	Value of Statistical Life Estimate (95% Confidence Intervals)		
<u>Fatality Rates Measured as:</u>			
Annual average for the industry at the 2-digit SIC-level.	$107.4 (99.5 – 115.3)	$10.82 (3.61 – 18.03)	$1.48 (-4.26 – 7.22)
Annual average for the industry at the 4-digit SIC-level.	$13.80 (12.10 – 15.50)	$5.16 (3.74 – 6.58)	$0.050 (-0.80 – 0.90)
Annual average for each plant	$0.046 (0.015 – 0.077)	$0.030 (0.005 – 0.055)	$0.013 (-0.013 – 0.039)
Model Variations:			
Industrial Sector Fixed Effects: (2-digit SIC-level)	No	Yes	No
Plant Fixed Effects:	No	No	Yes
Plant Characteristics Included:[b]	Yes	Yes	Yes

[a] All VSL estimates are based on 257,600 observations (65,700 plants). The dependent variable is the natural log of plant wages, and all specifications include year fixed effects and plant characteristics as specified in equation (7), and standard errors clustered at the plant level. All observations (number of plants) are reported rounded to the nearest 100 because of U.S. Census Bureau confidentiality requirements.

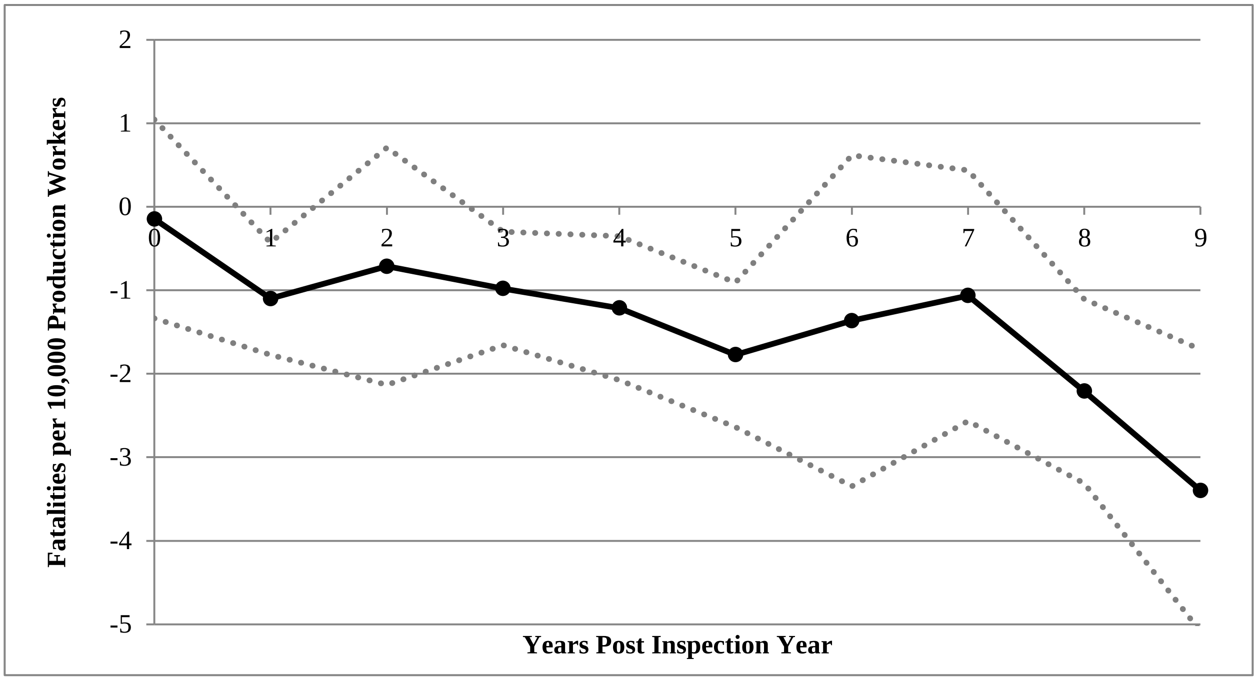
[b] The six plant-level characteristics considered for inclusion are *Number of Employees*, *Number of Production Workers*, *Cost of Materials*, *Productivity*, *Single Unit Plant*, and *Turnover* as described in equations (2) and (3) and summarized in Table I.

Figure 1: Event Study Analysis of Compensating Wages.[a]



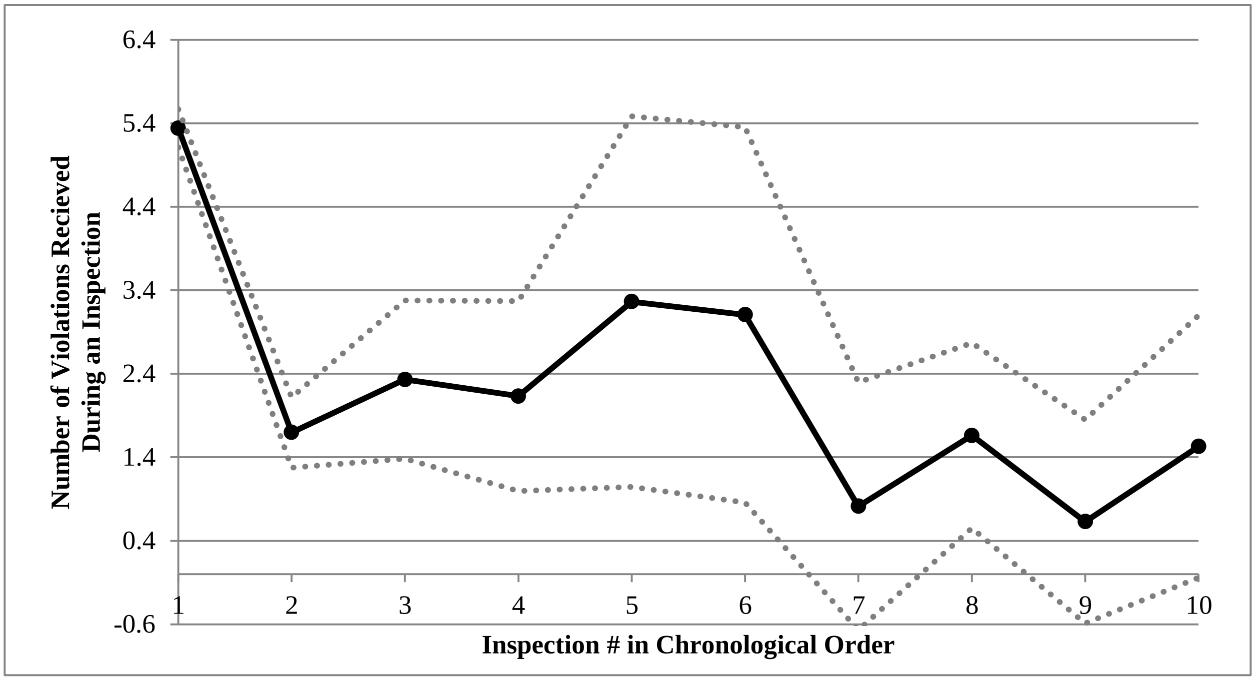
[a] Coefficient estimates (dots) and 95% confidence intervals (dashed lines) are based on equation (1).

Figure 2: Post-Inspection Event Study Analysis of Fatality Rates.[a]



[a] Coefficient estimates (dots) and 95% confidence intervals (dashed lines) are based on a post-inspection event-study for fatality rates.

Figure 3: Test for Transitory Treatment Effects of OSHA Inspections on Plant Safety.[a]



[a] Coefficient estimates (dots) and 95% confidence intervals (dashed lines) are based on equation (2). The sample is the full sample of all manufacturing plants between 1987 and 1997 that have never been inspected prior to 1987.