American Economic Journal: Economic Policy 2019, 11(4): 350–374
https://doi.org/10.1257/pol.20150024

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

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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. To address these limitations, this paper employs randomly assigned workplace safety inspections to instrument for plant-level risks in a quasi-experimental design. We provide credible causal evidence for the existence of compensating wages for fatality risks and estimate a VSL between \$(2016)8 million and \$(2016)10 million. (*JEL* J17, J28, J31, K32)

This research exploits randomized workplace safety inspections to identify compensating wage differentials for risky working conditions and provides 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 the remarkably large role of VSL estimates in determining benefit-cost ratios for many federal policies (e.g., Ashenfelter and Greenstone 2004; Black, Galdo, and Liu 2003; Cameron 2010; Cropper, Hammitt, and Robinson 2011; Robinson 2007; Environmental

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[†]Go to https://doi.org/10.1257/pol.20150024 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

[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 × 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.

Protection Agency [EPA] 2010; Office of Management and Budget (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 percent of the total benefits across all rules considered are directly attributable to the monetized value of reducing early mortality (OMB 2014). 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 considerable measurement error (Black, Galdo, and Liu 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 Galdo, and Liu 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 to control for unobserved worker characteristics (e.g., Kniesner et al. 2012; Kniesner, Viscusi, and Ziliak 2010). However, identification of the wage/risk premia 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 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—and 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 is used to prevent relapse. Our results show that

[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, Hubbell, and Kramer (2006); and EPA (2010) provide quantitative reviews of the past empirical literature.

[3] Past studies have been particularly sensitive to the 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 interindustry wage differentials (e.g., Hintermann, Alberini, and Markandya 2010; Leigh 1995).

inspections reduce plant-level fatality risks by approximately 45 percent, and these reductions last through the entire study period.[4]

We use a ten-year panel of confidential plant-level fatality, wage, and worksite characteristics data from OSHA and the US Census Bureau. A complete census of US manufacturing plants' employment data is collected by the 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 four-digit standard industrial classification (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 2–3 percent, suggesting a range for the VSL of \$(2016)6 million to \$(2016)8 million. These results are robust to a variety of model and sample-selection choices, which contrasts starkly with panel models that we estimate using commonly employed national average risk rates or even uninstrumented plant-level risk rates. Another unique aspect of our data is that the 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 40 percent of the compensating payments for workplace risks, increasing the VSL point estimates to \$8–\$10 million. 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 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 the OSHA practices for selecting plants for safety inspections that provide the natural experiment we wish to exploit. Section II presents an overview of the data, and Section III presents estimates of the compensating wage differential associated with plant-level safety improvements. Section III also computes a range of point estimates for the VSL and offers several robustness tests for our estimation strategy. Section IV 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 40 years has been devoted to the enforcement of standards through workplace inspections (Fleming 2001, MacLaury 1984, Siskind 1993). The law allows

[4] Scholz and Gray (1993, 1990) and Gray and Mendeloff (2005) also find that OSHA inspections significantly improve workplace safety.

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

states to choose whether they develop and operate their own safety programs or have OSHA's federal program operate within the state. The distribution of federally administered OSHA programs is presented in Figure A1 of the online Appendix. 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 US manufacturing plants, the federal states cover 9.5 million manufacturing workers in over 200,000 plants, representing approximately 57 percent of the US manufacturing workforce.[6]

Our study period is from 1987–1997, which is the only time period in which the federal OSHA program used a random process for assigning "programmed" inspections to plants. Programmed inspections 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. Prior to 1987 and post-1997, OSHA substantially changed its selection criteria for programmed inspections, using a system that targeted plants based on past injury and illness incidence rates (Brooks 1988, OSHA 2004). Specifically, OSHA would randomly select plants to which it sent inspectors, but the inspectors would only complete an inspection after reviewing the plant's injury logs and determining if the plant's reported injury rates were above a predefined threshold that was publicly known to plants. Thus, while plants were still randomly selected to be visited, the actual conduct of an inspection was not random prior to 1987 or post-1997 because plants could manipulate their logs to stay below the threshold.

During the 1987–1997 study period, OSHA focused on 20 high-risk manufacturing industries for inspections (defined at the four-digit SIC level) and randomly selected plants within these industries in each state using a neutral selection criteria. Specifically, 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 (OSHA 1981, 1990, 1995). Thus, programmed inspections during this period were randomly distributed among plants, conditioned on industry, plant size, state, and recent inspection history, according to OSHA policy.

[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 injury accident rates that are 30 percent 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.

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 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. Plants receiving a complaint inspection are not included in the estimation sample since these inspections are not randomly assigned. Information from accident inspections is used to construct fatality rates at the plant level as described further in the next section.

II. Data

Our data are obtained from three sources. Safety inspection and fatality records for every OSHA programmed inspection conducted between 1987–1997 (over 150,000 inspections) are obtained from the publicly available OSHA Integrated Management Information System (IMIS) inspection database.[8] For each inspected plant, IMIS records the plant name, address, 1987 SIC code, 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 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 the 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.

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 that we employ throughout our analysis.[9] 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 (2005), Scholz and Gray (1993, 1990), and Haviland et al. (2010).[10] As a general rule, about

[8] All data are available at https://enforcedata.dol.gov/views/data_summary.php (accessed July 2019).
[9] We also estimate and report models dropping data from the ASM.
[10] Details on the matching process are provided in the online Appendix.

60 percent of 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).[11]

During the study period, there were just over 200,000 manufacturing plants in states that employed the federal OSHA inspection program. After linking the OSHA and census data, there are several restrictions that reduce the number of plants used for estimation (see also online Appendix Figure A2). 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 40 percent. Second, plants that received their first inspection prior to the start of the study period (1987) are dropped from the sample, further reducing the sample by approximately 45 percent.[12] Note, as discussed later, results are robust to this particular exclusion criteria. Third, plants that received complaint or other nonrandom inspections are dropped, and this further reduces the sample by 8 percent. Finally, plants that had a fatality prior to being scheduled for a programmed inspection are dropped since fatalities trigger an OSHA safety inspection equivalent to a programmed inspection.[13] This restriction further reduces the sample by 0.1 percent.

To summarize, the final sample of 65,300 plants is an unbalanced panel that includes all manufacturing plants with more than 10 employees that have never been inspected or received their first inspection after 1987 and appear at least twice in the COM or ASM during the 1987–1997 study period. More details on the sample selection process are provided in the online Appendix.

Table 1 summarizes the census and OSHA data used for estimation. An average of 7,682 plants received an inspection each year, or 11.8 percent of the sample. Of these inspections, 44 percent were randomly assigned programmed inspections. Seventy-three percent of plants that received a programmed inspection received at least 1 violation, and there was an average of 4.6 violations found per inspection.[14] The average annual fatality rate for the high-risk manufacturing industries targeted

[11] 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 2 percent of the control group would have been inspected at some point during the study period, but not identified as such by our matching. This would bias our results away from finding an effect of OSHA inspections on safety and wages. However, as made clear in the results, we find robustly significant impacts.

[12] Plants are dropped for which injury logs were reviewed by OSHA prior to 1987 (regardless of whether or not the plant was actually inspected). As discussed in Section I, 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.

[13] 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. As discussed in Section III, results are robust to the inclusion of these plants.

[14] There may be partial treatment associated with inspections since approximately 27 percent 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).

Table 1—Summary Statistics

Variable name	Description	Mean (SD)
Panel A. OSHA Inspection Program, 1987–1997[a]		
Total Annual Inspections	Average annual inspections	7,682
Programmed Inspections	Average annual programmed inspections	3,365
Programmed Inspections with Violations	Average annual programmed inspections that result in at least one violation found	2,451
Violations	Average number of violations	4.64 (6.98)
Manuf. Fatality Rate (*Fatrate*)[b]	Average annual fatality rate per 10,000 workers in all manufacturing industries	0.65 (21.23)
Fatality Rate in OSHA Targeted Industries[c]	Average annual fatality rate per 10,000 workers for industries targeted by OSHA for randomized inspections	3.05 (51.28)
Panel B. Average Payroll, Employment, and Plant Characteristics, 1987–1997		
Number of Plants	Total number of plants in estimation sample	65,300
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	$12.72 (5.13)
Other Workers' Wages ($1997)	Average hourly wages for non-production workers; computed as the total annual payroll for non-production workers divided by 2,000 hours assuming a 40-hour work week for 50 weeks per year	$21.26 (13.68)
Number of Employees	Average total employees per plant	118 (387)
Number of Production Workers	Average number of production workers (percent of total employees in brackets)	79.2 (207) [67%]
Cost of Materials	Total cost of all materials consumed or put into production for the year, measured in millions ($1997)	$13.6 (84.0)
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)	$166 (558)
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	10.9% (0.29)
Single Unit Plant	Dummy variable equal to 1 for plants that are single-unit establishments and equal to 0 for multiunit establishments	52.6% (0.50)

[a] Inspections data are for states that operate under federal OSHA jurisdiction and were successfully matched to the COM data. The OSHA IMIS database is available online at the following: https://enforcedata.dol.gov/views/data_summary.php (accessed February 2017).
[b] Calculated as the overall plant-level fatality rate for the estimation sample (approximately 65,300 plants).
[c] Reported fatality rate is weighted by four-digit SIC OSHA inspection shares.

by OSHA for programmed inspections was 3.05 deaths per 10,000 workers, nearly five times the fatality rate for all manufacturing industries (0.65 deaths per 10,000 workers). Figure 1 presents annual average plant-level fatality risks for the manufacturing industries targeted by OSHA (solid line) and all US manufacturing (dashed line). As indicated in Figure 1, there was a substantial decline in average risks within the industries OSHA targeted over the study period, while US manufacturing fatality risks were relatively stable.

Panel B of Table 1 reports the sample average annual plant-level wages and employment in 1997 dollars, which is the most recent COM wave used in the

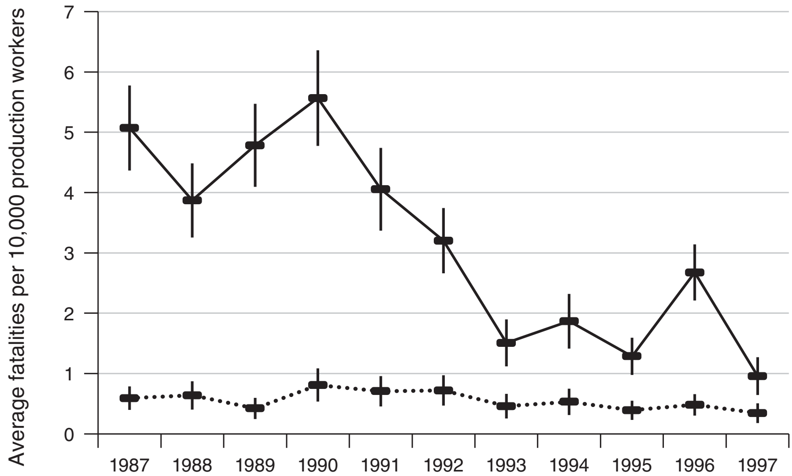

Figure 1. Average Plant-Level Fatality Risks and 95 Percent Confidence Intervals, by Year, for Industries Targeted for Programmed Inspections (solid line) and All US Manufacturing (dotted line)

Note: Average annual fatalities are computed by authors using publicly available OSHA IMIS inspections data reporting fatalities combined with publicly available County Business Patterns data from the census.

analysis. The average hourly wage rate for production workers was \$12.72, or approximately \$(2016)18.[15] There was an average of 118 employees per plant, of which 67 percent 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 (53 percent 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 $\Delta = E\Big[X_t^I \Big| ind_i, state_s - E\Big[X_t^{UI} \Big| ind_i, state_s\Big]\Big]$, 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 11 employees or that 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 1 for a list, beginning with *Number of Employees*

[15] All dollars are reported inflated using the personal consumption expenditures price index, available at https://fred.stlouisfed.org/series/PCEPI (accessed February 2017).

through *Single Unit Plant*). A variable is considered balanced if we fail to reject $\Delta = 0$ at the 10 percent 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 *Number of Employees*, *Number of Production Workers*, *Cost of Materials*, and *Single Unit Plant* classification. In contrast, during the 11 years of the study sample, only the covariate *Single Unit Plant* fails the balance test, and in only two years.[16] 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

Our 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 made 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 but only that wages rise less quickly at plants where safety is improving as compared to plants where safety levels remain unchanged.

Rosen (1974) provides the theoretical framework connecting observed changes in wages to measures of welfare change for workers. The standard hedonic wage model begins with the assumption of competitive labor markets. Workers with heterogeneous preferences over workplace amenities interact in the marketplace with firms that have heterogeneous costs of providing workplace amenities to generate an equilibrium wage-amenity locus. This equilibrium locus is taken as exogenous by both workers and firms and is recovered empirically with just information on wages and job attributes. The equilibrium locus reveals workers' valuation of amenities because workers seeking to maximize utility will choose jobs such that their marginal willingness to pay (in terms of wages foregone) for a small increase in a workplace amenity will just equal the market marginal rate for that amenity.[17] Jones-Lee (1974) extends the Rosen framework to explicitly consider workplace risks or, conversely, workplace safety. The underlying intuition remains the same—workers will choose jobs such that their marginal willingness to pay for increased workplace safety in terms of foregone wages will equal the market marginal price, or marginal

[16] More details on the balance tests and their results are available in the online Appendix.

[17] To recover welfare measures for small changes in risk à la the Rosen framework, empirical models typically assume they are recovering a single, stable equilibrium hedonic wage locus. In a quasi-experimental framework, one must consider whether this condition still holds. If experimental shocks to a labor market result in shifts in the hedonic equilibrium, then one would be capturing differences in wages arising from movement between hedonic equilibria rather than marginal willingness to pay measures that are obtained from a single, stable hedonic wage gradient (Kuminoff and Pope 2014). It is unclear if plant-level OSHA inspections are sufficient in number to shift the market supply of safety (approximately 12 percent of our sample is inspected each year). Regardless of this concern, Kuminoff and Pope (2014) also shows that differences across hedonic price functions identify welfare measures as long as the instrument is randomized, which is clearly the case with OSHA inspections during our study period.

wage change, for increased safety (see Bockstael and McConnell 2007 for an excellent theoretical review).

We begin our empirical analysis by exploring the dynamic impacts of OSHA inspections on plant-level wages and safety through three event studies. First, production workers' real average hourly wages at inspected plants and plants that have never been inspected are compared as follows:

$$wage_{j,t} = a + \sum_{n=-9}^{9} \lambda_n \mathbf{1}\left[PIY_{j,t} = n\right] + I_{j,t} \times S_s \times T_t + P_j + \epsilon_{j,t}, \tag{1}$$

where production worker average real hourly wages (in 1997 dollars) at the jth plant in time t ($wage_{j,t}$) are regressed on indicator variables, $\mathbf{1}\left[PIY_{j,t} = n\right]$, in which $PIY_{j,t}$ is a variable equal to the number of years pre-/post-inspection for the inspected plants and $\mathbf{1}\left[PIY_{j,t} = n\right]$ is equal to 1 if $PIY_{j,t} = n$, and equal to 0 otherwise ($n = -1$ is the omitted category in the model). Plant fixed effects, P_j, are included, 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 that the COM assigns just one SIC code to plants each year that represents the plant's majority production activity in that year. As such, 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 (see the definition of the sample in Table 3, panel B). Figure 2 plots the estimated coefficients and their 95 percent confidence intervals, and two trends emerge. First, there is not a significant difference in wages at the 5 percent 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 percent level in years 2, 4, and 7 post-inspection and are significantly different at the 10 percent level in 7 of the 10 post-inspection years. Focusing on the period beginning 2 years post-inspection, real hourly wages generally remain between \$0.20–\$0.30 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 3 plots the estimated coefficients for each year post-inspection and their 95 percent 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 percent level in 6 of the 10 post-inspection years.

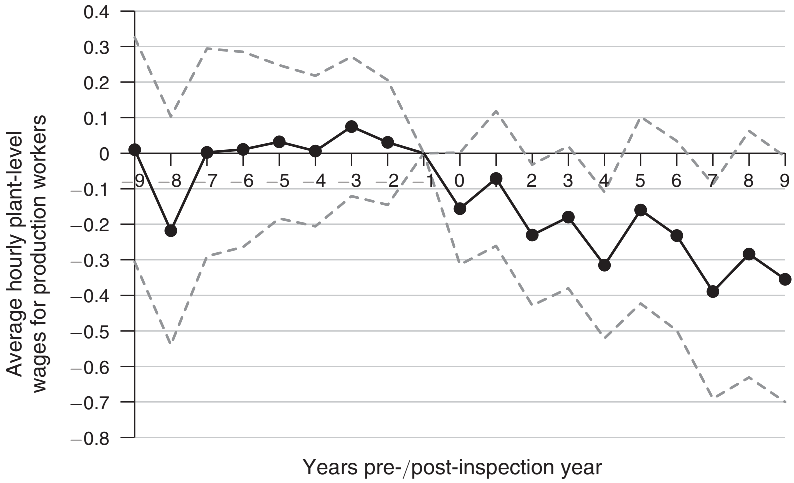

Figure 2. Event Study Analysis of Compensating Wages

Note: Coefficient estimates (dots) and 95 percent confidence intervals (dashed lines) are based on equation (1).

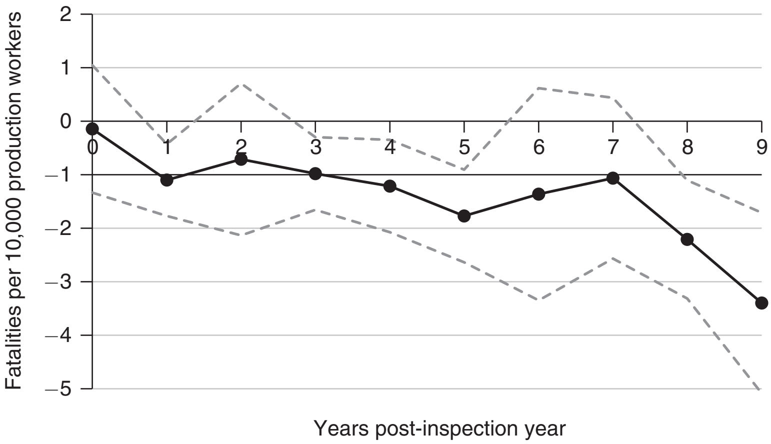

Figure 3. Post-inspection Event Study Analysis of Fatality Rates

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

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:

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

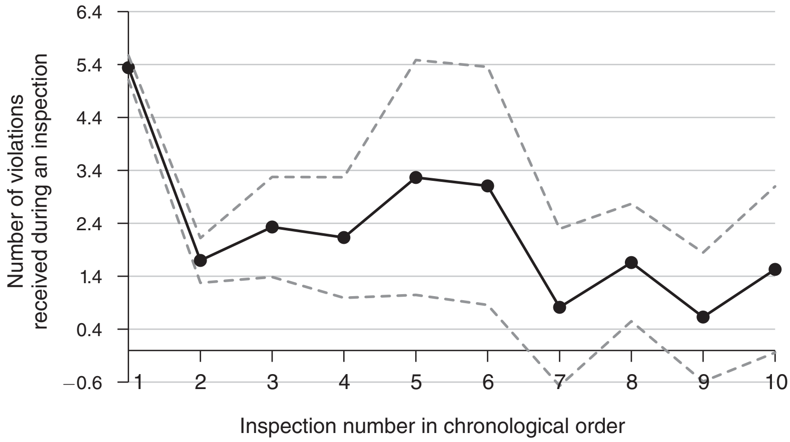

FIGURE 4. TEST FOR TRANSITORY TREATMENT EFFECTS OF OSHA INSPECTIONS ON PLANT SAFETY

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

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 10 for plant j at time t, then plant j received their tenth OSHA inspection at time t). The indicator function $\mathbf{1}\left[IN_{j,t} = n\right]$ is equal to 1 for observations receiving their nth inspection at time t and equal to 0 otherwise. Estimates for β_n are presented in Figure 4, along with their 95 percent confidence intervals, and indicate that the number of violations found during an inspection fall after the first inspection, declining by an average of 50 percent in the second and subsequent inspections.

In sum, the event studies presented in Figures 2–4 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 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

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

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

where the fatality rate ($fatrate_{j,t}$) at plant j in time t in equation (3) is regressed on a treatment indicator variable, $PI_{j,t}$, that is equal to 1 if the plant receives an OSHA programmed inspection in year t and each year after the inspection. Equation (3) also includes a vector of observable time-varying plant characteristics ($PC_{j,t}$) that includes *Turnover*, *PW Productivity*, *Number of Employees*, *Cost of Materials*, *Number of*

Production Workers, and *Single Unit Plant*.[18] The second-stage regression in equation (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 equation (3), and all else is as defined for equation (3). Standard errors are clustered at the plant level for both equations (3) and (4).

The IV model is estimated using the 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 suggested by Figure 2). Models are reported later that explore the sensitivity of the results to the exclusion of these years.

Table 2 presents key coefficient estimates for five specifications of the IV model, and as described below, the models 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. First, wages for production workers are entered either linearly or as their natural log. 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, Viscusi, and Ziliak 2010; Scotton 2013; Scotton and Taylor 2011). However, changes in fringe benefits may also be an important margin of adjustment for establishments, and so we also report 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.[19] Second, 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 the Census Bureau and address the oversampling of large establishments in ASM years. Weighting by the number of production workers addresses heteroskedasticity 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, Haider, and Wooldridge (2015) that corrects for heteroskedasticity in the presence of both a clustered error component (plant identifier) and a group-size error component (number of production workers).[20]

As indicated in Table 2, 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 percent level and stable across model specifications,

[18] See Table 1 for variable descriptions.

[19] 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.

[20] The two-step procedure uses the squared residuals from the unweighted TSLS models in a regression on the inverse of the number of production workers to create weights. As such, the coefficient estimates for the first-stage regression in Table 2 change as weights change in each model presented.

Table 2—IV Estimates of Plant-Level Changes in Risks and Wages in Response to Receiving an OSHA Inspection

Panel A. Select coefficient estimates for first-stage regression (equation (3))[a]					
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[b]	Yes	No	No	No	No
Weighting[c]	None	None	ASM × PW	ASM × PW	ASM × PW
R^2	0.296	0.296	0.294	0.294	0.294
F-statistic[d]	15.70	15.71	20.34	20.32	16.32
Panel B. Select coefficient estimates for second-stage regression (equation (4))[e]					
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[b]	Yes	No	No	No	No
Weighting[c]	None	None	ASM × PW	ASM × PW	ASM × PW
R^2	0.484	0.433	0.547	0.582	0.347

Notes: 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 hundred due to Census Bureau confidentiality requirements.

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

[b] The six plant-level characteristics considered for inclusion are *Number of Employees*, *Number of Production Workers*, *Cost of Materials*, *PW Productivity*, *Single Unit Plant*, and *Turnover* as described in equations (2) and (3) and summarized in Table 1.

[c] Sample weights provided by the Census Bureau for its ASM are combined with transformed inverse production worker weights as described in Section III.

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

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

suggesting that OSHA inspections reduce plant fatality rates by about 1.4 fatalities per 10,000 production workers. Although not reported in Table 2 for succinctness, time-varying plant characteristics included in the model are not statistically significant predictors of fatality rates.[21] Finally, the last row of panel A reports Kleibergen–Paap F-statistics that are all greater than ten, suggesting that receiving an OSHA programmed inspection is indeed a strong instrument (see Bound, Jaeger, and Baker 1995; Kleibergen and Paap 2006; Staiger and Stock 1997).

The average number of fatalities in the OSHA-targeted industries is 3 deaths per 10,000 workers, and thus our empirical models suggest plant-level fatality risks are decreased by approximately 45 percent after plants receive their first programmed OSHA inspection.[22] 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 15–22 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 that 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

[21] The complete set of coefficient estimates for the model presented in column 1 of Table 2 is reported in the online Appendix.

[22] Nonlinear poisson and negative binomial fatality count models were also estimated and suggest that programmed inspections result in a 56 percent reduction in plant-level fatalities, which is similar to the estimates reported for the linear IV models.

noted earlier in Figure 4, the number of violations a plant receives during an inspection declines by nearly 70 percent in the second inspection and remains low in all subsequent inspections, indicating diminishing opportunities for repeated inspections to impact safety.

Panel B in Table 2 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 results from an incremental increase in workplace safety varies from \$0.20 cents per hour for the unweighted model including time-varying plant characteristics to \$0.24 cents per hour for the weighted model. The log-transformed wage model indicates a \$0.23 cents per hour decrease in hourly wages, assuming a mean wage of \$12.72 (all in 1997 dollars). The last column of Table 2 suggests that 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 \$0.34 cents per hour.

The first row of Table 3 presents VSL estimates corresponding to the models in Table 2. The VSL is computed by multiplying the estimated coefficient reported in panel B of Table 2 (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 \$(1997)12.72 for the log-transformed models.[23] All VSL estimates are inflated from 1997 to 2016 dollars using the personal consumption expenditures price index.[24] Focusing on the weighted specifications with wage as the dependent variable, the VSL point estimate is \$(2016)6.7 million. The inclusion of fringe benefits increases the point estimates by approximately 50 percent to \$9.4 million.

The remainder of Table 3 presents VSL estimates based on alternative samples that allow us to evaluate sensitivity to sample selection. Rows 2–4 present results for models that include all observations from all years, including the year that the first inspection occurs (row 2), drop only observations from the inspection year (row 3), drop observations from the inspection year and the subsequent year as in the baseline model, and also drop observations that are from the ASM survey (row 4). The VSL point estimates from these three alternative samples are quite similar to the baseline results presented in the first row of Table 3. Overall, they suggest point estimates for the VSL of \$6–\$7 million when considering wages alone and \$8–\$10 million when considering total worker compensation.

Finally, row 5 increases our sample size by including plants that received an inspection prior to 1987. Recall, OSHA only randomized inspections between 1987–1998. We do not include plants that received an inspection prior to 1987 because of their nonrandom treatment assignment. However, one could consider plants as having some "baseline" safety condition in 1987 that reflects whether or

[23] Production workers in the 20 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, accessed February 2017).

[24] Available at https://fred.stlouisfed.org/series/PCEPI (accessed February 2017).

TABLE 3—VSL ESTIMATES BY ESTIMATION SAMPLE AND MODEL
(REPORTED IN MILLIONS, 2016 DOLLARS)

VSL estimates (95 percent confidence intervals)					
Panel A. Baseline sample used in estimation (year of inspection and following year excluded)					
Observations = 252,800 and Number of Plants = 65,300					
	\$5.69	\$6.21	\$6.74	\$6.38	\$9.39
	(1.78–9.59)	(2.11–10.32)	(2.40–11.09)	(2.22–10.54)	(3.09–15.69)
Panel B. All years (1987–1997) included in the estimation sample					
Observations = 257,600 and Number of Plants = 65,700					
	\$6.02	\$6.52	\$6.49	\$5.99	\$10.00
	(1.53–10.51)	(1.85–11.20)	(0.98–12.01)	(0.95–11.03)	(1.83–18.16)
Panel C. Year of inspection excluded from estimation sample					
Observations = 254,500 and Number of Plants = 65,300					
	\$6.33	\$6.86	\$5.82	\$5.32	\$7.67
	(1.54–11.11)	(1.85–11.86)	(0.83–10.82)	(0.68–9.96)	(1.57–13.78)
Panel D. Baseline sample, excluding ASM observations					
Observations = 133,600 and Number of Plants = 63,300					
	\$6.80	\$7.27	\$6.83	\$6.91	\$7.93
	(1.43–12.17)	(1.65–12.90)	(1.96–11.70)	(2.13–11.70)	(2.20–13.67)
Panel E. Estimation sample includes all years (1987–1997) and plants inspected prior to 1987					
Observations = 499,000 and Number of Plants = 110,000					
	\$6.35	\$6.66	\$7.50	\$7.16	\$9.82
	(2.94–9.77)	(3.15–10.17)	(2.85–12.14)	(2.83–11.50)	(3.43–16.21)
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

Notes: 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. Model variations upon which VSL estimates are based are defined in detail in Table 2. Sample sizes are rounded to the nearest hundred due to Census Bureau confidentiality requirements.

not they had been inspected in the past. Conditional on this baseline, we can then follow all the plants forward in time, examining the impact of programmed inspections from 1987–1997.[25] Taking this approach nearly doubles the sample size to approximately 500,000 observations (110,000 plants). Importantly, as seen in row 5 of Table 3, VSL estimates remain very similar to those reported for the primary samples in the first four rows of Table 3.

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 \$(2016)6 million to \$(2016)7.5 million. 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 \$(2016)1 million to over \$(2016)23 million, although most estimates lie between \$(2016)5.2 million to \$(2016)13 million. This range

[25] Plants inspected in 1986 are dropped since Figure 2 suggests plant wages are still adjusting the year after inspection (but reach a new wage equilibrium in the second year after inspection). Thus, plants inspected in 1985 or earlier could be reasonably assumed to be in their "new equilibrium" when we begin our panel in 1987 and follow them during the decade when OSHA inspections were random.

includes estimates arising from similar worker samples taken during the same time period as our study (e.g., Viscusi 2004 reports estimates of \$9.8 million 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, Viscusi, and Ziliak 2010; Scotton 2013; Scotton and Taylor 2011). While our estimates are generally on the lower end of the current range, 95 percent confidence intervals overlap and the inclusion of fringe benefits increases our point estimates closer to the average from the recent literature.

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 metropolitan statistical area (MSA) by estimating the following model:

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

where all variables are defined as in equation (3) except for the additional covariate *Related MSA* that is equal to 1 for all plants in an MSA after any plant in that MSA receives the first federally programmed OSHA inspection and is equal to 0 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 1 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 0 otherwise.

Key coefficient estimates for equation (5) are presented in the first column of Table 4, while the second column presents the results testing for spillover effects among multiunit 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 0 and not statistically significant. However, as indicated in the first row of Table 4, our main treatment variable, *PI*, is stable in magnitude and highly significant. The last two columns of Table 4 present models identical to the first two columns but use real wages as the

TABLE 4—TESTS FOR GENERAL EQUILIBRIUM EFFECTS AMONG RELATED PLANTS

	Dependent variable			
	Fatality Rate (1)	*Fatality Rate* (2)	*Wages* (3)	*Wages* (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^2	0.295	0.295	0.778	0.778

Notes: 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 hundred due to Census Bureau confidentiality requirements.

dependent variable. Again, results indicate that inspections at plants related through location or company ownership do 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 who 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:

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

The dependent variable, *Other Worker Wages*, is obtained directly from the COM and ASM. The estimated key coefficient, φ, is presented in the first row of Table 5 (model 1). Consistent with our expectations, we find a small (less than 1 percent of average wage) and statistically insignificant 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 5 presents eight additional models that are identical to equation (6) but 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 2–5 present estimates of the impact of OSHA inspections on productivity, capital stock, and cost of materials. Results indicate that inspections reduce production worker productivity (model 2) and total factor 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 \$16.40 or 10 percent (baseline average production worker

Table 5—Select Coefficient Estimates Describing the Impact of OSHA Inspections on Non-production Employee Wages, Factor Productivity Measures, and Employment Levels

Model	Dependent variable	Definition	Coef. estimate for φ in equation (6) (SE)
1	*Other Workers Wages*	Hourly wages of non-production workers (e.g., clerical and managerial), available directly from the COM and ASM surveys[a]	0.167 (0.254)
2	*PW Productivity*	See Table 1 for definition	−16.40 (4.040)
3	*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)
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 1 for definition	−0.0208 (0.0477)
6	*Number 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	*Number of Production Workers*	See Table 1 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 1 for definition	−0.00343 (0.0049)

Notes: 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). 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 hundred due to Census Bureau confidentiality requirements.

[a] Other workers' real wages are originally reported as annual earnings (mean earnings over the sample period is approximately $48,000). Earnings are converted to hourly wages assuming 2,000 hours worked per year.

productivity is $166 per hour) and total factor productivity by approximately 1.6 percent post-inspection, which is an average annual productivity loss of about $165,000 per plant. For comparison, Greenstone, List, and Syverson (2012) estimates that the 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 total factor productivity losses are smaller than Greenstone List, and Syverson (2012), they are still substantial and would outweigh labor cost savings suggested by our models.[26] 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 6–8). Specifically, there is a significant

[26] An OLS regression identical to equation (3) but with *wage* as the dependent variable indicates that OSHA inspections reduce plant production worker wages by approximately $0.34, implying that inspected plants reduce annual payroll expenses by an average of $54,000 or approximately 33 percent of total factor productivity losses ($165,000).

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 production workers (model 8).[27] 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 wage 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 code. 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:

$$\ln\left(wage_{j,t}\right) = \gamma + \delta\, riskrate_{j,t} + PC_{j,t}\beta + P_j + u_{j,t}, \tag{7}$$

where the natural log of real wages at plant j in year t is regressed on one of three risk measures: the two aggregate industry measures noted above and uninstrumented plant-level fatality risk rates. A vector of plant-level characteristics, PC, is included as defined in equation (3), as are plant fixed effects, P_j. We 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 6. In stark contrast to our IV estimates, the results are highly sensitive to model specification and often result in implausibly large (more than \$100 million) or exceptionally small (less than \$50,000) VSL estimates. The very small OLS estimates of the VSL based on plant-level fatality risks are consistent with classical measurement error (i.e., in any given year the observed fatality rate is a draw from the underlying risk distribution), highlighting the need for an IV approach even when data on plant-specific fatalities are available.

[27] 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.

Table 6—VSL Estimates Arising from Alternative and Un-instrumented Risk Rates (reported in millions, 2016 dollars)

	VSL estimate (95% confidence intervals)		
Fatality rates measured as			
Annual average for the industry at the two-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 four-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 (two-digit SIC level)	No	Yes	No
Plant fixed effects	No	No	Yes
Plant characteristics included[a]	Yes	Yes	Yes

Notes: All VSL estimates are based on the sample described in Table 3, panel B. The dependent variable is the natural log of plant wages. All specifications include year fixed effects, plant characteristics as specified in equation (7), and standard errors are clustered at the plant level. All observations (number of plants) are reported rounded to the nearest hundred because of Census Bureau confidentiality requirements.

[a] The six plant-level characteristics are *Number of Employees*, *Number of Production Workers*, *Cost of Materials*, *PW Productivity*, *Single Unit Plant*, and *Turnover* as described in equation (3) and summarized in Table 1.

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 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 2 percent after a comprehensive OSHA inspection is conducted, translating to VSL estimates between \$(2016)6 million and \$(2016)7 million. 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 estimates increase to \$8 million to \$10 million, which is roughly in the midpoint 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 León and Miguel (2017) exploit exogenous changes in transportation risks for commuters and report VSL estimates of approximately \$1 million to \$2 million.[28] Rohlfs, Sullivan, and Kniesner (2015) takes changes in airbag regulations for US motor vehicles to be a quasi-experiment and estimates a median VSL of \$(2016)10 million to \$(2016)12 million, although their estimates are quite imprecise, ranging

[28] Kochi and Taylor (2011) also estimates the VSL based on transportation risks, although not in a quasi-experimental framework, and finds that automotive accident risks are not compensated at all for occupational drivers.

from less than −\$10 million to over \$18 million, and are negative for the lower quartile of their data.[29]

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.[30] Our analysis focuses solely on production workers in manufacturing industries. This more narrow focus may lead to VSL estimates that are inappropriate for the 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, Reed, and Hubbard 1992).[31] DeLeire, Khan, and Timmins (2013) develops 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] Additional quasi-experimental applications estimating the VSL include Greenstone, Ryan, and Yankovich (2012), which estimates a structural model of military reenlistment choices utilizing exogenous variation in reenlistment bonuses to estimate a VSL of \$3–\$4 million for military personnel, and Schnier, Horrace, and Felthoven (2009), which estimates a VSL of about \$(2016)5 million for Alaskan crab fishermen.

[30] Another limitation is that we do not have data on routine plant-level nonfatal accident risks. While industry-specific injury rates are available publicly, they would be absorbed by our industry-by-state-by-year fixed effects.

[31] 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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