ABSTRACT

LEE, JONATHAN MICHAEL. A Quasi-Experimental Approach to Estimating the Value of Reducing Risk. (Under the direction of Dr. Laura O. Taylor.)

The value of a statistical life (VSL) is the most influential economic point estimate in cost-benefit analyses of regulations designed to reduce mortality risk. A large literature has developed which uses hedonic wage models to estimate the VSL based on the revealed preferences of workers facing a tradeoff between wages and job risks. To date, most hedonic wage studies use cross-sectional individual level data on workers to estimate the marginal willingness to pay for risk reductions. Some studies have made advances using panel data and instrumental variables to control for unobserved confounding variables.

Despite its dominant position in the literature, there remain significant concerns regarding the validity of hedonic wage studies for calculating unbiased estimates of the VSL. Two notable shortcomings of this literature that remain unresolved are error in the measurement of workplace risks and omitted variable bias. While recent studies use measures of occupational risk that vary by broad occupation and industry classifications of the worker, these are national average risk rates. Furthermore, omitted variable bias in hedonic wage studies can result from unobserved worker and/or firm characteristics that are correlated with both wages and risk. Both omitted variables and risk measurement error in hedonic wage studies will produce biased estimates of worker's marginal willingness to pay for risk reduction.

My dissertation research addresses these two shortcomings by employing a unique panel dataset combining Occupational Safety and Health Administration (OSHA) workplace safety and health inspection data with plant-level wage data from the Census of Manufactures (COM). I use randomized OSHA inspections as an exogenous treatment

affecting risks and wages. Randomization in the inspection assignment process provides assurance that OSHA inspections are not correlated with unobserved confounding variables in the hedonic wage equation. The inspections therefore allow compensating wage differentials to be calculated from an exogenous source of risk variation. The COM data covers every manufacturing facility in the United States, and on average, OSHA inspects more than 100,000 plants each year and documents the nature and extent of any found safety and health violations for each inspection. My research is the first to combine OSHA and COM data to test for compensating wage differentials associated with facility-specific workplace risks.

Although I rely on the same theoretical equilibrium assumptions of hedonic wage studies, the empirical strategy that I employ is a unique departure from previous hedonic wage analysis. Furthermore, my estimation strategy offers some distinct advantages over the existing hedonic wage literature. Rather than relying on national average risk rates, I use a finer measure of risk that constructs fatality rates and accident rates for production workers at their actual place of employment. I also employ panel data estimation strategies that allow me to control for time-invariant unobserved plant characteristics. Among other contributions, plant-level fixed effects eliminate concerns of bias arising from confounding inter-industry wage differentials.

Results strongly 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, my point estimates suggest that prior studies may overstate the value workers place on reducing workplace risks.

These results suggest that the empirical challenges inherent in estimating compensating wage differentials via cross-sectional or panel-data hedonic wage models have not yet been fully addressed. Data limitations, especially with regards to the measurement of risk faced by workers at their worksite, and correlated unobservables are likely the key impediments to identification of wage/risk tradeoffs. My results are important for environmental, health and public safety policies that rely on labor market studies to provide measures of the value of reducing fatality risks.

A Quasi-Experimental Approach to Estimating the Value of Reducing Risk

by
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DEDICATION

This dissertation is dedicated to my entire family. In memoriam of my grandmother Rosa who passed away prior to its completion.

BIOGRAPHY

Jonathan Lee was born in Gastonia, North Carolina on June 20, 1981. He played soccer and was president of the interact club at Hunter Huss High School where he graduated valedictorian in 1999.

Jonathan attended the University of North Carolina Chapel Hill in the fall of 1999. He made the Dean's list on numerous occasions, and graduated with a B.A. in Economics in 2003. From 2003-2004, Jonathan completed his M.S. in economics at the University of North Carolina Charlotte writing a thesis on the efficacy of motorcycle helmet legislation.

Jonathan worked in the private sector for two years before returning to school in the fall of 2007 at North Carolina State University in order to pursue a Ph.D. in economics. His fields of specialization are environmental economics and industrial organization, and he will continue research in these fields as an Assistant Professor at East Carolina University beginning in the fall of 2012.

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North Carolina State University has provided me with an excellent graduate education. I would like offer particular thanks to my dissertation committee comprised of Drs. Laura Taylor, Roger von Haefen, Lori Bennear, and Melinda Morrill.

First and foremost, Dr. Laura Taylor has been instrumental in the development of my dissertation topic as well offering continued diligent guidance toward the completion of the dissertation. I first developed the idea for my dissertation while taking Dr. Taylor's empirical environmental economics field course when she noted that there were no quasi-experimental studies estimating compensating wages for risk. I expressed my idea to Dr. Taylor during lecture, and she encouraged me from the beginning to pursue this as my dissertation topic. Dr. Taylor has spent countless hours reviewing my research and writing, helping with fellowship applications, and working together on co-authored work. I am extremely grateful for all of her time and effort devoted to my success, and I will continue to use her example as a successful researcher throughout my career.

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course in environmental economics was instrumental in expanding my knowledge of the field due to its broad coverage of the major research areas of environmental economics.

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Dr. Melinda Morrill was incredibly helpful in providing a labor economist's perspective on my dissertation research. Per her suggestions I also considered other avenues such as worker turnover that may be affected by OSHA inspections. My dissertation research relies on the same theoretical equilibrium assumptions of traditional hedonic wage models, but with Dr. Morrill's suggestions I also tested these equilibrium assumptions to see if they were valid.

Although he is not on my dissertation committee, Dr. Dan Phaneuf has been another key figure who has helped me in my research. Along with all of my committee members, Dr. Phaneuf was willing to give his time and advice while listening to me practice my job market talk in preparation for the economics job market. Dr. Phaneuf is always available to discuss alternative estimation strategies, and his microeconometrics course at N.C. State was by far one of the most helpful classes that I have taken during my graduate studies. Undoubtedly, the estimation tools and techniques that I learned from Dr. Phaneuf will continue to be helpful in my research for years to come.

I have had numerous other instructors who have influenced me over the course of my college career, but four professors in particular deserve mentioning. Drs. Richard Zuber and

John Gandar at UNC Charlotte taught me one of the most important research principles. While serving as my M.S. thesis research advisors they told me to pursue research that I find exciting. This simple advice has made my research enjoyable and personally rewarding. Dr. John Stewart at UNC Chapel Hill helped me to conduct my first econometric analysis of the VSL and was a major influence on my desire to continue doing economic research and pursuing a PhD in economics. Finally, Dr. Kenneth Janken at UNC Chapel Hill challenged me to use the problem solving skills that I had developed as a young economist in order to analyze the Civil Rights Movement. I credit Dr. Janken with helping expand my horizon and develop a better understanding of the universal applications of economics.

My parents, David and Vicki Lee, have provided me with an incredible amount of love, support and guidance while completing this dissertation. I appreciate all of their understanding for the time that I have spent away from home during holidays and family gatherings. They have even gone as far as bringing me leftover Thanksgiving dinner when my research did not permit me time to go home. My mom is an educator, and has given me countless tips on stress management, diet, and exercise while completing my research. My father is my mentor, and has shown a keen interest in my research. When developing a new research idea I still call my pop for insight and advice.

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TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

LIST OF ABBREVIATIONS

ASOII	Annual Survey of Injury and Illness
ATE	Average Treatment Effect
ATT	Average Treatment Effect on the Treated
BLS	Bureau of Labor Statistics
cdf	cumulative distribution function
CFOI	Census of Fatal Occupational Injuries
CIA	Conditional Independence Assumption
COM	Census of Manufactures
DAFWII	Days Away From Work, Injuries and Illness
DART	Days Away From Work, Restricted, or Transferred
DID	Difference in Differences
EPA	Environmental Protection Agency
IMIS	Integrated Management Information System
IV	Instrumental Variables
LATE	Local Average Treatment Effect
LWDII	Lost Workday Injury and Illness
ODI	OSHA Data Initiative
OSHA	Occupational Safety and Health Administration
OLS	Ordinary Least Squares
ME	Matching Estimator
PPN	Permanent Plant Number
RCM	Rubin Causal Model
RDD	Regression Discontinuity Design
RIA	Regulatory Impact Analysis
SST	Site Specific Targeting
SUTVA	Stable Unit Treatment Value Assumption
VSL	Value of a Statistical Life

Chapter 1

Introduction

The value of reducing mortality risk from environmental exposures is a critical input for regulatory impact analyses (RIAs) of environmental regulations.[1] The EPA uses the Value of a Statistical Life (VSL) to monetize the benefits associated with mortality risk reductions, and currently suggests using a mean estimated VSL of $8.2 million (1997$) based on a review of 21 labor market studies and 5 contingent valuation studies (US EPA 2000). The 21 labor market studies are all hedonic wage studies, and as such, it is critical to understand the validity of these estimates.

To see how the VSL is computed, suppose there is a group of 10,000 individuals at risk of death from a certain exposure, and it is estimated that the average willingness to pay is $300 per year to reduce the risk of death by 1/10,000. The VSL in this context is equal to $300 * 10,000, or $3,000,000. The VSL does not measure the value of an identified life, but it instead measures the sum of affected individual's willingness to pay for marginal reductions in risk to the group. The $300 average willingness to pay is the key measurement necessary for constructing the VSL, and is typically estimated from hedonic wage studies.

[1]Recent examples in which mortality reductions comprised a significant portion of total benefits are the EPA's prospective study of the Clean Air Act, the RIA for existing stationary spark ignition, and the RIA for amendments to the standards affecting the Portland cement manufacturing industry. Reductions in mortality risk account for roughly 90%-97% of the total monetized benefits in each of these studies (Sinha et al., 2010; US EPA OAQPS, 2010; US EPA OAR, 1999).

The conceptual basis for the hedonic wage model is described by Rosen (1974), and Jones-Lee (1974) first developed a model explaining compensating wages for dangerous working conditions in a utility theoretic framework. Intuitively, the hedonic wage model recognized explicitly that firms can increase safety in exchange for paying workers lower wages, or decrease safety and pay higher wages in order to attract workers. The hedonic theory of compensating wages for risky working conditions suggests firms and workers are matched together in equilibrium such that the firms willingness to pay higher wages to attract workers to risky jobs is exactly equal to their matched individual's willingness to accept the increased risk (Rosen, 1974).

Empirical applications of the hedonic method seek to estimate the wage premium associated with increased occupational fatality risks. These wage premium estimates are the foundation of VSL estimates that are inputs to RIA's. Typically, simple cross-sectional OLS regressions are estimated wherein the wage a worker earns is regressed on job characteristics, one of which is the risk of death on the job. If isolated properly, the marginal compensation a worker requires to face an additional unit of risk is directly obtained from the OLS regression and this MWTP forms the basis of a VSL estimate. For example, if job risks are measured in deaths per 10,000 workers, and one estimates that workers require $300 per year to increase risk of death on the job by 1/10,000, then the VSL in this context is simply the $300 average yearly willingness to pay scaled by 10,000, or $3,000,000. The VSL does not represent the value of avoiding a certain or "identified" death, but rather can be interpreted as the value that a group of 10,000 individuals would collectively place on reducing the chance that one among them dies. Beginning with Thaler and Rosen (1976), there exists an

extensive empirical literature attempting to isolate compensating wages for risk. This literature is reviewed in Mrozek & Taylor (2002), Viscusi & Aldy (2003), Kochi, et al. (2006), and U.S. EPA NCEE (2010).

There are a host of issues in the literature on estimating the VSL using labor markets. Key among these challenges are endogenous regressors arising from omitted variables and the inability to accurately measure the risk a worker faces at their actual place of employment. First, unobserved worker characteristics and job characteristics may be correlated with job risks and wages and therefore bias regression estimates estimates for risk in an unknown direction (see, for example, Black et al. ,2003, and Scotton and Taylor, 2011).

The second issue prevalent in past studies is the mismeasurement of workplace risks. Studies prior to 2000 are based on risk data known to be deeply flawed (see Drudi, 1997) and studies based on newer improved risk data still use national average risk rates, not risks measured at the actual place of employment. These crude risk measures are still subject to considerable measurement error, and the coarse aggregation techniques fail to control for any idiosyncratic differences in risk at the plant level.

To date, there remains considerable debate on the validity of hedonic wage models for estimating the value of reducing risks. In this dissertation research, I provide evidence on the value of reducing job-related risk which addresses the two aforementioned shortcomings of existing hedonic wage studies. I employ the first quasi-experimental design within a labor-market setting to provide causal evidence on the existence and magnitude of compensating wages for risk. A unique database is constructed that combines confidential U.S. Census microdata on plant-level wages, fringe benefits, and worksite characteristics

over a thirty-year period with plant-level injury and fatality data over the same period obtained from the Occupational Safety and Health Administration (OSHA). I take advantage of OSHA's federal programmed inspections that randomly select plants for inspection (conditioned on known and measured plant characteristics) to instrument for plant-level risks.

Due to the known OSHA inspection assignment rules, the empirical analysis used in this dissertation falls within the Rubin Causal Model (RCM) theoretical framework for explaining causal relationships; and as such, all of the estimators are presented within the confines of the RCM (Rubin, 1974). An OSHA inspection ultimately results in one of two possible outcomes: plants are found to be in violation of OSHA standards for safety and health and by law are required to correct the found violations, or plants are found to be in compliance with OSHA standards and are not required to make any safety improvements. The two key features of the OSHA inspection process that are exploited are the randomized selection process for inspections and the highly visible nature of the inspection process to workers that results in subsequent safety improvements. Within this quasi-experimental framework, compensating wage differentials for job-site risks are identified in two ways.

The first empirical strategy employs difference-in-differences (DID) estimators that compare the change in wages of production workers at inspected plants to changes in wages at uninspected plants. Here, the treatment is simply receiving an OSHA inspection and wages at plants that are inspected are expected to be lower post-inspection than at uninspected plants due to the required improvements in safety conditions post-inspection. While this analysis relies on OSHA randomized inspections to instrument for plant-level safety and thus provides a clean identification strategy, estimated compensating differentials

are confounded because the control group (uninspected plants) includes both safe and risky plants in unknown proportions.

The second analysis focuses only on inspected plants so that safe and risky plants can be distinguished, allowing more precise estimates of compensating wage magnitudes. A difference-in-differences (DID) estimator is again used to estimate wage differentials between inspected plants with found safety violations ("known violators") and inspected plants with no found violations ("known compliers"). In this analysis, the randomization inherent in the OSHA inspection process is lost because although inspections are randomly assigned, violations are not. However, the ability to identify plants known to be safe from those known to have less safe working conditions, combined with information on plant-level accident and fatality rates for all inspected plants, allows computation of the VSL by directly linking wage differentials post-inspection to known changes in plant-level risks post-inspection.

Results from the comparison of known violators to known compliers indicates that on average workers' wages are reduced by 2.5 to 3.3 percent post-inspection, or approximately $1,010 to $1,287 annually (2011$). Furthermore, average workers' fringe benefits are reduced by an additional $612 to $855 annually (2011$). The plausible range of point estimates for the VSL estimated over the entire manufacturing sector is between $5.0 million and $7.1 million with a central point estimate of $6.4 million, all measured in 2011 dollars (95% confidence interval of 3.0 million to 9.8 million). The U.S. EPA's central estimate of $8.2 million for the VSL is slightly higher than my range of estimates, but $8.2 million is still within the estimated 95% confidence interval.

Past hedonic wage studies typically only have information on workers' wage rate, and are therefore unable to control for impacts of job risks on the fringe benefits offered by employers. Importantly, approximately 40% of the estimated VSL can be attributed to reductions in fringe benefits following an OSHA violation. Using only the change in wages following an OSHA violation, I estimate a central VSL of $3.8 million, which is similar to the VSL reported by Kochi (2011) in a panel-data hedonic wage study, but is substantially lower than most other recent VSL estimates provided by hedonic wage studies. For example, Viscusi (2004) and Kneisner et al. (2012) both use labor market data on workers' wages from the same time period as what my estimate is based upon and report VSLs of between $6.6 to $15 million (2011$).

While there are no other quasi-experimental labor-market studies examining compensating wages for risky working conditions to which my estimates can be compared, Ashenfelter and Greenstone (2004) estimate the VSL in a quasi-experimental framework based on a 1987 change in the federally mandated national maximum speed limit law. Ashenfelter and Greenstone first estimate the amount of travel time saved in states adopting increased speed limits, and then estimate the impact of increased speed limits on the states rural interstate fatality rates. They report an upper-bound point estimate for the VSL of $2.2 million (2011$), which is substantially less than my estimates. It is not surprising that Ashenfelter and Greenstone's estimates are lower given the evidence that VSL estimates based on traffic fatality risks are typically at the lower end of the VSL range reported in the literature (see, for example, Viscusi, 1992, de Blaeij et al., 2003, and Kochi and Taylor, 2011).

An outline of this dissertation is as follows. Chapter 2 provides a brief overview of the theory of hedonic wages and reviews the existing empirical hedonic wage literature, its shortcomings, and presents the alternative empirical framework that is employed in this research to isolate compensating wages for risk. As described in Chapter 2, the empirical techniques employed in this dissertation are quite different from the techniques typically used to estimate the hedonic wage function, however, the analysis still relies on the same theoretical justification of compensating wages for risk in equilibrium. Chapter 3 provides an overview of the OSHA inspection process, with a focus on the features of these inspections which form the basis of the natural experiment that is exploited and presents an overview of the data used for estimation.

Chapter 4, describes the main estimation strategies used in this dissertation, and provides the results of those estimations. Chapter 5 discusses alternative estimation strategies to the DID estimator. Several variations of matching estimators (ME's) and regression discontinuity design (RDD) are employed in Chapter 5 to estimate the causal effects of OSHA inspections on plant-level wages. These estimators are also presented in the confines of the RCM framework in order to clearly state the underlying assumptions necessary for estimating the causal effects of plant-level risks on wages. Finally, Chapter 6 concludes this dissertation.

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Chapter 2

Theoretical Model and Empirical Framework

The conceptual basis for the hedonic wage model is described by Rosen (1974), and Jones-Lee (1974) first developed a model explaining compensating wages for dangerous working conditions in a utility theoretic framework. This dissertation presents alternative estimation techniques to traditional empirical hedonic wage methods, but it still relies on the same underlying theoretical justification of the existence of compensating wages for risk. Intuitively firms can increase safety in exchange for paying workers lower wages, or decrease safety and pay higher wages in order to attract workers. In this chapter, I first present the hedonic theory of compensating wages for risk and review how this model has been implemented empirically to estimate the VSL. Section 2.2 presents the alternative empirical approach, the Rubin Causal Model (RCM), that is used in this research. Finally, Section 2.3 presents an overview of the estimation strategies available within the RCM framework.

2.1 Hedonic Wage Theory

Following Rosen (1974) and Jones-Lee (1974), first assume that workers receive increasing utility in wages, and that they have a typically shaped concave utility with properties:

$$\frac{\partial U(w)}{\partial w} > 0, \tag{2.1}$$

$$\frac{\partial^2 U(w)}{\partial w^2} < 0, \tag{2.2}$$

where U represents utility as a function of wages, w. Equations (2.1) and (2.2) imply that utility is increasing in wages at a decreasing rate. Furthermore, it is assumed that an individual can occupy one of two mutually exclusive health states: safe (S), and dead (D). An individual has a probability ρ of being involved in a fatal job accident. The expected utility from a job with wage rate w, and risk of death ρ can be written as:

$$E(U) = (1-\rho)U_S(w) + \rho U_D(\hat{w}(w)), \tag{2.3}$$

where $\hat{w}$ is a function of the wage rate w that expresses the expected earnings that are bequeathed to surviving relatives in the event that a worker dies on the job.[2]

From equation (2.3), it is clear that the expected utility is decreasing in ρ provided that the utility in the safe state is greater than the worker's utility in the dead state:

$$U_S(w) > U_D(\hat{w}(w)). \tag{2.4}$$

If equation (2.4) holds for any wage rate, w it is possible to analyze the effects of an increase in probability of death on the wages that workers must receive in order to maintain a constant amount of utility. If the probability of death is increased to $\bar{\rho} > \rho$, then combination of equations (2.1) and (2.3) yields the following result:

$$E(U) = (1-\bar{\rho})U_S(w+V) + \bar{\rho}U_D(\hat{w}(w+V)). \tag{2.5}$$

In other words, in order to hold expected utility constant at $E(U)$, it is necessary to supplement the wage (w) by some positive amount (V) if there is an increase in probability of death. V is therefore a measure of the compensating wage differential that is required to

[2] In most states in the US, if you are killed on the job family members will receive a settlement that is actuarially based on your wage rate suggesting that $\hat{w}$ should be increasing in w. In the simplest case with no workers compensation laws, $\hat{w}$ represents only the savings that is bequeathed to heirs, which should also be increasing in w.

make an individual worker indifferent when choosing among two alternative jobs with differing probabilities of death given by $\bar{\rho}$ and ρ. Taking the total derivative of equation (2.5) with respect to $\bar{\rho}$ yields the following result:

$$\frac{\partial V}{\partial \bar{\rho}} = \frac{U_S(w+V) - U_D\left(\hat{w}(w+V)\right)]}{(1-\bar{\rho})U_S'(w+V) + \bar{\rho}U_D'\left(\hat{w}(w+V)\right)} > 0 \tag{2.6}$$

where $U'(\cdot)$ indicates $\frac{\partial U(\cdot)}{\partial V}$, the numerator is positive due to equation (2.4), and the denominator is positive under the assumption of increasing utility of wage given by equation (2.1). The results provided in equation (2.6) are all that is necessary to establish the existence of compensating wages for increased job risk. Furthermore, assuming that the following inequality holds for all levels of risk:

$$U_S'(w+V) > U_D'\left(\hat{w}(w+V)\right), \tag{2.7}$$

it is possible to differentiate equation (2.6) with respect to $\bar{\rho}$ in order to obtain the following result:

$$\frac{\partial^2 V}{\partial \bar{\rho}^2} > 0. \tag{2.8}$$

Proof of equation (2.8) relies on the concavity assumptions along with the requirement that equations (2.4) and (2.7) are satisfied for all wage rates (see Jones-Lee, 1974 for the proof). The implications of equations (2.6) and (2.8) are that individuals must be paid higher wages in order to take a job with increased risk of death, and these compensating wages are increasing at an increasing rate in risk. Figure 2.1 shows a series of such loci holding expected utility, U_y^z, constant. The subscripts on utility (y) represent individuals with heterogeneous preferences for risk. As Figure 2.1 illustrates, the slope of the constant expected utility loci for individual 1 is greater than the slope for individual 2 holding fatality

risk constant at ρ_0, which suggests that individual 1 has a stronger aversion to job-risks than individual 2. The superscripts on utility (z) are ordered such that $z_1 > z_2$ implies $U^{z_1} > U^{z_2}$. The figure illustrates that expected utility is increasing for all individuals with movements in the northwest direction, i.e. increases in the wage rate coupled with reductions in job risks.

2.1.1 Workers Utility Maximization

Suppose there exists an equilibrium hedonic wage function, $w(\rho)$, which the workers take as given and choose the level of fatality risk ρ faced on the job in order to maximize expected utility:

$$max\ E(U) = \max_{\rho}(1-\rho)U_S\big(w(\rho)\big) + \rho U_D(\hat{w}(w(\rho))). \tag{2.9}$$

The FOC of equation (2.9) is given by:

$$\frac{\partial w}{\partial \rho} = \frac{U_S(w) - U_D\big(\hat{w}(w)\big)}{(1-\rho)U_S'(w) + \rho U_D'\big(\hat{w}(w)\big)}. \tag{2.10}$$

Comparing equation (2.10) with equation (2.6), indicates that in equilibrium, the utility maximizing worker chooses a level of workplace risk, ρ, and associated wage, $w(\rho)$, such that the slope of the hedonic wage function is exactly equal to the slope of the worker's indifference curve for wages and risk given by the function $V(w, \rho)$. Furthermore, market clearing requires that willingness to accept increases in risk must be equal to the hedonic wage function in equilibrium:

$$V(w, \rho) = w(\rho). \tag{2.11}$$

This equilibrium is depicted in Figure 2.2, where individual 1 with expected utility U_1 chooses the wage, w_1, and risk level, ρ_1 in equilibrium.

2.1.2 Firms

Firms must choose the amount of workers to hire along with the safety level of the firm in order to maximize their profits given by:

$$\max \pi = \underset{\rho, L}{max} P * f(L) - w(\rho) * L - c(\rho), \tag{2.12}$$

where profit (π) is a function of the market price (P) of the good, the production function ($f(L)$), and the cost to the firm for providing a marginal increase in safety, $c(\rho)$.

The FOC's of equation (2.12) are given by:

$$\frac{\partial \pi}{\partial \rho} = -L * \frac{\partial w}{\partial \rho} - c_\rho = 0, \tag{2.13}$$

$$\frac{\partial \pi}{\partial L} = P * f_L - w = 0. \tag{2.14}$$

According to equation 2.14, the amount of labor is chosen optimally such that the marginal revenue product ($P*f_L$) is equal to the wage rate. Rearranging equation (2.13) reveals the following result:

$$\frac{\partial w}{\partial \rho} = \frac{-c_\rho}{L}, \tag{2.15}$$

where the left-hand side of equation (2.15) is equal to the derivative of the hedonic wage function with respect to risk of death, and the right hand side of the equations is equal to the slope of an isoprofit curve for the firm holding everything but w and ρ constant.

Assuming that the cost of safety $c(\rho)$ is increasing at an increasing rate in plant-level safety:

$$\frac{dc(\rho)}{d\rho} < 0, \tag{2.16}$$

$$\frac{d^2c(\rho)}{d\rho^2} > 0, \tag{2.17}$$

the slope of the isoprofit curve will be positive, and increasing at a decreasing rate. Equation (2.15) implies that in equilibrium firms will sort themselves along the wage risk continuum such that the slope of the firm's isoprofit curve is the same as the slope of the hedonic wage equation. Furthermore, market clearing conditions require that in equilibrium the firms willingness to pay for increases in risk given by its isoprofit functions must be equal to what it actually has to pay given by the hedonic wage function. This equilibrium is depicted in Figure 2.3 for two firms labeled 1 and 2 with corresponding isoprofit curves π_1 and π_2.

Equations (2.15) and (2.10), and Figure 2.3 illustrate that the hedonic wage function simultaneously envelopes a family of indifference curves for workers and isoprofit curves for firms leading to an equilibrium where individuals and firms are matched together such that the firms' willingness to pay for increased risk is exactly equal to their matched workers' willingness to accept the increased risk (Rosen, 1974).

2.1.3 Empirical Implementation of the Hedonic Wage Theory

Empirical applications of the hedonic method seek to estimate the equilibrium hedonic wage function, $w(\rho)$, depicted in Figure 2.3. Note that the theoretical model provides a simplistic representation of the equilibrium hedonic wage function, $w(\rho)$, as a function of job risks. Empirical applications recognize that there may be other sources of heterogeneity in wages and attempt to control for relevant job and worker characteristics that are correlated with the equilibrium wage rate and risks. A simple cross-sectional OLS specification of a hedonic wage equation is given by the following form:

$$wage_k = \alpha + WC_k\gamma + J_k\theta + \beta * fatality\_risk_k + \partial * injury\_risk_k + \epsilon_k, \quad (2.18)$$

where the hourly wage rate of worker k is a function of a vector of worker characteristics (WC_k) including age, education, race, and gender; a vector of job characteristics (J_k) such as union status, occupation, and industry classification; the worker's on-the-job fatality risk measured in deaths per 10,000 workers, injury risk measured in lost workday injuries per 10,000 workers, and a random error term, ϵ_k. The estimated coefficient β provides a measure of the average willingness to pay for a marginal reduction in the risk of death on the job. For example, if β is estimated to be \$0.15 then workers would be willing to pay \$300 per year in order to reduce their probability of death on the job by 1/10,000.[3] The VSL in this context is simply the \$300 average yearly willingness to pay scaled by 10,000 in order to calculate the value of one statistical life, which equals \$3.0 million. The VSL is therefore a linear extrapolation of the marginal willingness to pay for small reductions in risk. Over 45 studies to date have used this method to estimate the VSL, and are reviewed extensively in Mrozek & Taylor (2002), Viscusi & Aldy (2003), Kochi, et al. (2006), and U.S. EPA NCEE (2010).

There are a host of unique problems facing the reduced form estimation of the hedonic wage function as specified in equation (2.18). Key among these challenges are endogenous regressors arising from omitted variables and the inability to accurately measure the risk a worker faces at their actual place of employment.

First, unobserved worker characteristics and job characteristics may be correlated with job risks and wages and therefore bias the coefficient estimates for risk in an unknown

[3]The \$300 average yearly willingness to pay is calculated by multiplying \$0.15 by 2,000 under the assumption that the average worker works 40 hours per week and 50 weeks per year.

direction (see Garen, 1988, Moore and Viscusi, 1989, Viscusi and Hersch, 2001, Black et al., 2003, Scotton and Taylor, 2011, and Kochi, 2011 for a discussion). Past researchers have employed two approaches to address the problem of omitted variable bias in hedonic wage studies: instrumental variables (IVs) (see, for example, Garen, 1988, Seibert and Wei, 1994, and Arabsheibani and Marin, 2001), and panel data models (see, for example, Black et al., 2003, Kniesner et al., 2012, and Kochi, 2011). Empirical application of IV techniques has been plagued by weak and invalid instruments. Garen (1988) for example, uses marital status as an instrument for risk, but marital status is most often used as a proxy for unobserved human capital which is also correlated with the wage rate (McConnell & Bockstael, 2007).

Panel data estimation techniques can provide causal estimates of the impact of job risks on workers' wages if the unobserved variables that are correlated with risks are time invariant for individual workers. If these unobserved variables are time varying, however, panel data methods will still produce biased estimates for the coefficients on risk. Moreover, the identification of risk parameters in panel data applications is likely attributed to job changers whose unobserved job characteristics are certainly time varying (Kniesner et al., 2012).

The problem of measurement error of risk is the second key issue involved with estimating wage premiums for risk in the hedonic wage context. The problems of measurement error are interrelated with the problems of omitted variable bias because there is evidence that panel data methods may exacerbate the attenuation bias associated with measurement error in risk (Black et al., 2003). Studies published prior to 2000 relied on

workplace fatality statistics which were generally only available coded by industry of the worker who died. This resulted in crude assignment of the same risk of death to workers across all occupations within a specific industry. Furthermore, it is now known that the raw data upon which these risk measures were based were deeply flawed (Drudi, 1997).

Most newer hedonic studies of compensating wages have been repetitions of the older methods with improved risk data from the Census of Fatal Occupational Injuries (CFOI). The CFOI began in 1992, and has collected much more detailed information on workplace fatalities that is verified by three independent sources (thus correcting the inaccurate reporting of deaths prevalent in earlier data). Most importantly, the CFOI data has allowed the construction of risk measures that vary across occupations and industries.

Hedonic wage studies using the CFOI data construct a risk measure based on a count of the number of fatal accidents for a particular occupational grouping in a particular industrial sector, aggregated across all US workers. On-the-job fatality risk is then constructed as the total number of deaths in an occupation and industry divided by the total number of workers in the same occupation and industry. This creates a national average risk rate for an occupation within an industry, and it is usually averaged over several years. Injury risks are constructed similarly and are also national average rates. The occupation and industry categories used to categorize risk are coarsely aggregated as well. For example, Viscusi (2004) aggregates risks to 8 broad occupations in 72 industries, and Scotton and Taylor (2011) aggregate to 22 occupations in 23 industries. These crude risk measures are still subject to considerable measurement error, and the coarse aggregation techniques fail to control for any idiosyncratic differences in risk at the plant level (Black et al. (2003) and

Black and Kniesner (2003) offer detailed discussions of the nature of measurement error for job risks).

The following section describes the Rubin Causal Model (RCM) as an alternative empirical framework for estimating compensating wages for risk. While based on the same theoretical equilibrium model described in the previous section, the RCM approach offers several alternative estimators capable of dealing with the issues of measurement error and omitted variable bias that plague traditional empirical hedonic wage applications.

2.2 Rubin Causal Model

Quasi-experimental estimation techniques are concerned with estimating the causal effects of a known treatment. In this dissertation, the treatment analyzed is OSHA enforcement of safety regulations, and the causal effects of interest are the impacts of OSHA enforcement on plant-level risks, wages, and fringe benefits. The maintained hypothesis is that OSHA enforcement of safety regulations results in plant-level safety improvements. Hedonic wage theory suggests plants that are forced to improve safety in order to comply with OSHA standards are subsequently able to offer lower wages. Likewise, workers receiving more pleasant (safer) working conditions also require less wages, all else equal. Thus OSHA enforcement of safety regulations provides an opportunity to estimate compensating wage differentials for risk. In the program evaluation literature, there are a variety of statistical estimators available for identifying the causal effects of treatment. In order to analyze the various estimators and the assumptions that these estimators rely on, it is necessary to construct a theoretical framework for treatment effects based on causal analysis. In this section an overview of the RCM framework for identifying population averaged

treatment effects is presented.[4] For ease of exposition, I begin with the general example of an individual receiving treatment.

2.2.1 Individual treatment effects

In evaluating the causal effects of a treatment on an individual, the ideal technique would involve observing the same individual at the same point in time both with and without the treatment. This is the fundamental starting point of the RCM framework. Let y_i^1 denote the outcome for individual i who has received treatment, and y_i^0 denote the outcome for the same individual in the untreated state. The causal effect of treatment for individual i is given by the difference in potential outcomes for that individual:

$$\Delta_i = y_i^1 - y_i^0. \tag{2.19}$$

The observed outcome for individual i, y_i, can be expressed as:

$$y_i = D_i * y_i^1 + (1 - D_i) * y_i^0, \tag{2.20}$$

where D_i is an indicator variable that equals one if an individual receives the treatment, and equals zero if the individual does not receive treatment.

Equations (2.19) and (2.20) are enough to fully classify the impact of treatment under the Stable Unit Treatment Value Assumption (SUTVA).[5] SUTVA assumes that the treatment effect is constant for individuals, and is independent of the treatments received by others or the mechanism by which treatment was delivered (Rubin, 1986). The first part of

[4] The RCM is also commonly referred to as the potential outcome approach. The approach is derived from the writings of Neyman (1923), and Fisher (1925) with their applications to randomized experiments and the extensions by Rubin (1974) to nonrandomized data.
[5] SUTVA is a term coined by Rubin (1980, 1986). In the economics framework, this can be interpreted as the absence of general equilibrium effects (see, for example, Meyer, 1995, Heckman and Vytlacil, 2005, and Heckman, 2005).

SUTVA is violated if there are interactions among individuals such that y_i^1 or y_i^0 depends on D_j for any individual j≠i. This part of the assumption is clearly violated in the presence of any general equilibrium effects of treatment. The second part of the SUTVA is violated if there are heterogeneous treatments that are not directly modeled. Violation of the SUTVA clearly violates the assumptions of ceteris paribus change needed for estimation of causal effects.

2.2.2 Population Effects

When measuring the population effects of a treatment program, the average treatment effect (ATE) and average treatment effect on the treated (ATT) are the two commonly used measurements in the literature. Denote Y^1 and Y^0 as the population variables for outcomes under the treatment and control state, respectively.[6] Any individual observations of y_i^1 or y_i^0 are simply one realization from the population vectors Y^1 and Y^0. Likewise, D without the i subscript now represents the population variable for assignment to either treatment or control group. As before, any individual observation of D_i that is equal to either one or zero is merely one realization from the population vector D. Using the same notation throughout, the ATE and ATT are given by:

$$ATE=E(\Delta)=E(Y^1-Y^0)=E(Y^1)-E(Y^0), \tag{2.21}$$

$$ATT=E(\Delta|D=1)=E((Y^1-Y^0)|D=1). \tag{2.22}$$

[6] See Rubin (1978), Frölich (2003), Caliendo (2006), and Athey and Imbens (2006) for examples using similar notation. In the main analysis presented in Chapter 4, the outcomes (Y) analyzed include wages, total annual compensation, fatality rates, and accident rates. The two specific treatments modeled include OSHA inspections and OSHA violations.

Equation (2.21) indicates that the ATE is just the expected value of the treatment for the population. This expectation is taken across individuals who are observed to be treated (their unobserved potential outcome is the outcome in the untreated state), and individuals who are observed to be untreated (their potential outcome is treatment).[7] The ATE measures the expected effect of treatment on a random individual chosen from the population for treatment. Equation (2.22) on the other hand, says that the ATT is the expected value of the treatment taken across only the individuals who are actually observed to be treated (their potential outcome is untreated). The ATT is the typical treatment effect calculated in empirical research using observational data. It can be interpreted as the effect of treatment on a random individual chosen from a subpopulation of individuals eligible for treatment.

2.3 Estimation Strategies for Observational Data

If one could observe the same individual in both the treatment and control states, the calculation of the causal individual treatment effects in equation (2.19) would be straightforward. Once the individual treatment effects were calculated across the population, the ATE is easily calculated from the average of all the individual effects. Likewise, the ATT is calculated from the average of all the individual effects across the population of treated. But, the fact that no individual can be observed in both states at the same point in time is the "fundamental problem of causal inference" (Holland, 1986). Stated alternatively, only the population outcome vector, Y, is fully observable, for which Y is equal to Y^1 over the range of treated individuals (D=1) and Y is equal to Y^0 over the range of untreated individuals

[7] Equations (2.21) and (2.22) have dropped the i subscripts, and are now using the uppercase letter Y to indicate that these expectations are taken over the population.

(D=0). In order to solve this problem, it is necessary to construct valid estimates for the unobserved potential outcomes, y_i^1 for $D_i = 0$ and y_i^0 for $D_i = 1$ in equation (2.20) (Heckman, LaLonde, & Smith, 1999). All of the estimators that will be introduced in this section necessarily rely on some identifying assumptions, which, if true, allow for valid causal inference. The strengths and weaknesses of these estimators along with necessary data requirements will be evaluated in turn.

2.3.1 Randomization as a Solution for Causal Inference[8]

In keeping with the same notation that has been employed up to this point, I further assume that an experimenter randomly selects individuals from the population to participate in an experiment. Once those individuals are randomly selected for participation, they are randomly assigned to either the treatment or control group. Once again, y_i denotes the observed outcome for individual i, D_i is an indicator variable that is equal to one if individual i is randomly selected for treatment and equal to zero if individual i is randomly selected into the control group. Furthermore, it is helpful to introduce a new variable R_i that is equal to one if individual i is randomly chosen for participation in the program and equal to zero otherwise.[9] The observed outcome for any individual in the population is now given by:

$$y_i = R_i * \left(D_i * y_i^1 + \left(1 - D_i \right) * y_i^0 \right) + \left(1 - R_i \right) * y_i^0. \qquad (2.23)$$

[8] The theoretical model for this section is a more stringent model based on the one created by Heckman, Ichimura, and Todd (1997), who assume conditional independence rather than the general independence to treatment assumption. The conditional independence assumption will be introduced when specific estimators are discussed in later sections.

[9] Note that participation in the program does not ensure treatment. It only means that you have a random chance of receiving treatment.

In other words equation (2.23) can be interpreted as saying that individuals are not systematically chosen for selection into the program based on their potential outcomes, and once selected into the program, individuals are not assigned to treatment or control groups based on their potential outcomes. Randomization at the selection and assignment stages necessarily implies the general independence of treatment and selection with potential outcomes:

$$Y^1, Y^0 \perp D, R.^{10} \tag{2.24}$$

Under this generalized construction, randomization allows calculation of the sample analogs of the ATE, and the ATT which are distinguished from equations (2.21) and (2.22) with the ^ symbol.

$$\widehat{ATE} = \frac{1}{n}\sum_{i=1}^{n}((y_i^1 - y_i^0)|R_i=1), \tag{2.25}$$

$$\widehat{ATT} = \frac{1}{j}\sum_{i=1}^{j}((y_i^1 - y_i^0)|R_i=1, D_i=1). \tag{2.26}$$

Using Kolmogorov's second strong law of large numbers, it is easily shown that equations (2.25) and (2.26) converge almost surely to equations (2.21) and (2.22), respectively (Kolmogorov, 1956). Furthermore, the general independence assumption of equation (2.24) allows comparison of the average of the observed outcomes for individuals receiving treatment, $E(Y|D=1,R=1)$, with the average of the observed outcomes for untreated individuals, $E(Y|D=0,R=1)$, in order to estimate the ATE and ATT and surmount the fundamental problem of causal inference.

[10] The notation ⊥ stands for independence. The independence result is redundant given our construct of randomization in the model given by equation (2.23). The notation will be helpful, however when examining situations where assignment is not random and some form of independence is necessarily assumed rather than implied.

An important conclusion to be drawn from the analysis on randomization as a solution to the fundamental problem of causal inference is the idea that randomization at both the selection and assignment stages of the program is one possible solution to ensure unbiased estimates of average treatment effects from researcher designed experimental data on treatment and control groups (a result from random treatment assignment in the second stage), and ensure the generalization of the estimated causal effects to the population at large (a result from random program selection in the first stage).

Randomized social experiments that are useful to economist are often difficult to employ due to high cost of implementation or possible moral concerns (Rivlin, 1974). As a result, researchers are left to analyze situations using observational data where treatment assignment is not purely random. There are two broad categories of estimators that can be used to deal with observational data when assignment to treatment is non-random:

1. Estimators relying on selection on observables where any covariates that affect both treatment assignment and the dependent variable of interest can be observed and controlled for (Heckman & Hotz, 1989).
2. Estimators assuming selection on unobservables, or the idea that some covariates that affect both treatment assignment and the dependent variable of interest are unobservable to the researcher.

All of the methods that are examined rely on some version of a conditional independence assumption in order to deduce the causal effects of treatment. In its most basic framework, the conditional independence assumption can be stated as follows:

$$Y^1, Y^0 \perp D | X. \tag{2.27}$$

Equation (2.27) says that treatment assignment mimics a randomized assignment process conditional on X.[11]

2.3.2 Selection on Observables: Matching Estimator and Ordinary Least Squares[12]

If researchers have access to individual level cross-sectional data then it may still be possible to estimate causal effects of treatment by controlling for all the confounding variables X that simultaneously affect treatment and the outcome variable of interest Y. The conditional independence assumption resulting from successfully controlling for all confounding variables which are assumed to be observable can be written as:

$$Y^1, Y^0 \perp D | X. \tag{2.28}$$

If equation (2.28) holds, then a matching estimator (ME) can be used to estimate treatment effects by matching individuals who receive treatment with a control group receiving no treatment on the basis of the confounding variables (X). Equation (2.28) implies the following equalities:

$$E(Y^0|D = 1, X) = E(Y^0|D = 0, X) = E(Y^0|X), \tag{2.29}$$

$$E(Y^1|D = 1, X) = E(Y^1|D = 0, X) = E(Y^1|X). \tag{2.30}$$

[11] The terms ignorability (Rosenbaum & Rubin, 1983), unconfoundedness (Rubin, 1990), and conditional independence assumption (CIA) are used frequently in the literature on matching estimators, where conditioning on observable covariates X is assumed to remove bias from estimated treatment effects. While conditional independence is a necessary assumption for matching, I will be using this term in its more general distributional independence interpretation postulated by Dawid (1979) and Florens and Mouchart (1982). All of the estimators that will be presented rely on some form of conditional independence, and the simplifying assumptions of the estimators will be presented in this framework.

[12] Caliendo (2006) provides a detailed analysis of all the matching estimators. This section follows his exposition closely except where otherwise noted.

From equations (2.29) and (2.30), it is possible to obtain unbiased estimates of the ATT using the observable difference in expected values:

$$ATT = E(Y|D = 1, X) - E(Y|D = 0, X). \quad (2.31)$$

The ATT given in equation (2.31) can be estimated using a non-parametric matching estimator (ME) or ordinary least squares (OLS). OLS estimation of the ATT requires the additional assumption of correct specification of the functional relationship between *X* and the outcome variable *Y*.

Non-parametric MEs have the benefit of not relying on functional form for estimation, but they do not come without costs. MEs require the additional assumption of common support, i.e., that there is a positive probability of treatment over all observations of *X* (Heckman, LaLonde, & Smith, 1999). The common support assumption is likely to be violated if individuals with certain realizations of X_i are systematically denied treatment or receive treatment with certainty.[13] The common support requirements of the ME are not necessarily a weakness when comparing matching with OLS because matching simply ignores observations for which there is no common support on X for constructing counterfactuals. Therefore, when the assumption of common support fails, the ATT produced by matching is only a valid estimate of the ATT over the region for which there exists common support. OLS on the other hand keeps these observations by projecting a linear approximation even over areas where there is no common support. As indicated by Smith (2004, p. 300): "Put differently, the regression identifies the untreated outcome model

[13]If the researcher knows this assignment rule, however it may still be possible to obtain unbiased estimates of treatment effects with regression discontinuity designs that will be explained in greater detail later.

in the region of the data where the untreated observations lie, and then projects it out into the region of the data where the treated units lie, thereby implicitly estimating the counterfactual."

If the functional form assumptions made by OLS are correct, then OLS is likely to be more effective, and not require observations over the treated portion of support. On the other hand misspecification of functional form can lead to even greater bias in situations where common support fails. The importance of functional form in OLS analysis is analyzed by Smith (2000), who found that OLS results closely approximate ME results with the inclusion of polynomial and interaction terms. Abadie and Imbens (2011) further reconcile the differences between matching and OLS by developing a bias corrected ME that simultaneously adjusts regression coefficients for misspecification and removes bias from simple MEs.

There are several MEs available to researchers including nearest-neighbor, radius, caliper, and kernel matching. All of the ME techniques can be applied to matching directly on the confounding variables (X) or matching on the propensity score (the probability of receiving treatment given X). Intuitively, it seems preferable to match on the confounding variables, because these variables are assumed to remove any bias from estimated treatment effects. The difficulty of matching on confounding variables arises if there are several confounding variables, and in this case, the ME suffers from the same curse of dimensionality that plagues many dynamic programming problems (Bellman, 1957). Assuming that the confounding variables are all discrete in nature, matching on k variables across n individuals will require n outer loops and a nest of k inner loops. If the inner loops

were all computed individually, the computational effort would require evaluating n*k possible matches for each individual and will be increasing exponentially in the number of individuals.

Matching on the propensity score significantly reduces the computational burden of the ME. Rosenbaum and Rubin (1983) prove the following conditional independence assumption:

$$Y^0, Y^1 \perp D|X \Longrightarrow Y^0, Y^1 \perp D|P(X), \tag{2.32}$$

where *P(X)* is a propensity score that indicates the probability of treatment conditional on *X*. Equation (2.32) justifies use of the propensity score for reducing the computational burden of MEs in situations where there are several confounding variables. It is worth noting however, that the propensity score used in empirical studies is generally estimated using probit or logit. As a result, the propensity score ME is a semi-parametric approach that is not free from assumptions on functional form in the initial estimation of the propensity score. To date there has been no formal proof that propensity score matching with flexible functional form provides less biased estimates than OLS with flexible functional form.

The critical assumption for the ME and OLS is that any confounding variables that jointly impact the treatment decision and outcome variable of interest are observed. Several matching algorithms were mentioned in this section, and there is no clear choice on best estimator for all data sets. Estimators such as weighting on propensity score, kernel matching, and local linear matching that use the full set of observations for constructing counterfactuals typically produce more biased estimates resulting from poorer matches, but they are more efficient. Estimators such as nearest-neighbor and caliper matching on the

other hand tend to reduce bias in exchange for reduced efficiency (Smith & Todd, 2005). All of the MEs should converge as the number of observations approaches infinity. As a result, if the estimators are all relatively close, then the sample size may be large enough to ignore any finite sample bias issues.

2.3.3 Selection on Unobservables: Difference-in-Differences Estimator

The MEs presented in section 2.3.2 provide unbiased estimates of the ATT when all confounders, X, can be observed and controlled for. In many situations, it is expected that there is some unobserved heterogeneity that is correlated with treatment and outcomes. In these instances it is necessary to have a new set of estimators that are robust to various forms of unobserved heterogeneity. If longitudinal data is available for both participants and non-participants before and after treatment assignment, then the difference-in-differences (DID) estimator can be used to eliminate possible bias from unobserved time-varying factors. It is easiest to assume two time periods (t=0,1), where no individuals receive treatment at time t=0, and some of the individuals in the sample receive treatment at time t=1 and others do not. The conditional independence assumption of the DID estimator is given by:

$$((Y_t^0|t=1)-(Y_t^0|t=0)) \perp D, \tag{2.33}$$

where Y^0 is now given the subscript t to indicate that potential outcomes are taken at different points in time, and the change in Y^0 from time t=0 to t=1 is assumed to be independent of treatment assignment. Equation (2.33) implies the following equality:

$$E[Y_t^0|t=1, D=1] - E[Y_t^0|t=0, D=1] =$$

$$E[Y_t^0|t=1, D=0] - E[Y_t^0|t=0, D=0].^{14} \quad (2.34)$$

In other words, the DID estimator accommodates unobserved confounding factors under the assumption of a common time trend for the treatment and control groups in the absence of treatment. Assuming that equation (2.34) holds, the DID estimator for the ATT is given by:

$$ATT = E[(Y_t|t=1, D=1) - (Y_t|t=0, D=1)] -$$

$$E[(Y_t|t=1, D=0) - (Y_t|t=0, D=0)]. \quad (2.35)$$

The *ATT* given in equation (2.35) can be estimated using an OLS regression of the following form:

$$y_{i,t} = a + \beta_1 D_i + \beta_2 T_t + \beta_3 D_i * T_t + \epsilon_{i,t}, \quad (2.36)$$

where the observed outcome variable (*y*) of individual i at time t is a function of a dummy variable (D_i) that is equal to one for individuals receiving treatment, a dummy variable (T_t) that is equal to one for all observations during the treatment period (t=1), and an interaction of the treatment and treatment period dummies (D_i*T_t). Finally, $\epsilon_{i,t}$ is a random error term that is uncorrelated with the explanatory variables. Following the expectation ordering presented in equation (2.35), the ATT is equal to:

$$ATT = [(a + \beta_1 + \beta_2 + \beta_3) - (a + \beta_1)] - [(a + \beta_2) - (a)]. \quad (2.37)$$

Estimates of the coefficient β_3 in equation (2.36) provide the OLS variant of the ATT.

The shortcomings of the DID estimator are that it cannot control for all types of unobserved variation. If there are omitted time varying covariates that are correlated with

[14] Derivation of equation (2.34) from equation (2.33) requires the following rule for expected values: For any two random variables X and Y, E(X-Y) = E(X) – E(Y).

both *Y* and *D*, then the estimated ATT provided by a DID estimator will be biased. The obvious way to correct this problem would be to control for all the relevant time varying confounding variables during estimation.[15] This is synonymous with the idea of selection on observables that was discussed using cross-sectional data in Section 2.3.2. In situations where there are additional time varying confounding variables they can be included in the OLS estimation equation, or a matching variant of the DID estimator may be employed.[16]

2.3.4 Selection on Unobservables: Instrumental Variables

In the case where there are unobserved covariates ($X_{i,t}$) that vary over time and individuals and are correlated with the treatment decision, the DID estimator in any parametric, or non-parametric form is no longer capable of producing unbiased estimates of the ATT. An alternative to dealing with this type of situation is to employ instruments (Z) that are strongly correlated with the endogeneous treatment indicator (*D*), but have no direct correlation with the potential outcomes (Y^0, Y^1). In the remaining analysis, assume there exists a binary instrument *Z* such that *Z=1* for individuals who are randomly given an incentive to participate in a treatment program, and *Z=0* otherwise (Angrist, Imbens, & Rubin, 1996). As an example, the incentive could be an advertisement of the treatment program that is mailed to a random sample of the population. Assume that the incentive

[15] Controlling for time and individual varying covariates is tantamount to conditioning on observables within a DID framework. It requires the less restrictive conditional independence assumption: $((Y_t^0|t=1)-(Y_t^0|t=0)) \perp D|X$.

[16] See (Heckman, Ichimura, & Todd, 1997) for semiparametric DID estimators in cases of longitudinal data. (Abadie, 2005) extends these methods to cases of repeated cross-sections and develops a simpler weighting scheme based on propensity score. (Athey & Imbens, 2006) relax the assumptions of the traditional DID estimation methods even further using nonparametric techniques to estimate cdf's of the unobserved potential outcomes for the treatment group and construct ATT from the cdf's.

satisfies the strong instrument requirement such that individuals randomly receiving the incentive are more likely to participate in the treatment program. Further assume if individuals receive the incentive and choose not to participate in the program then they would not have participated even without receiving the incentive.[17]

The conditional independence assumption of the instrumental variables (IV) estimator can be written as:

$$Y^{00}, Y^{11}, D^{1}, D^{0} \perp Z. \tag{2.38}$$

The notation in equation in (2.38) is slightly different from that used in previous sections. Y^{00} now denotes the population vector of outcome variables for individuals receiving no treatment ($Y^{0(\cdot)}$) and no incentive ($Y^{(\cdot)0}$). Likewise, Y^{11} denotes the population vector of outcome variables for individuals who receive treatment and the incentive. D^1 and D^0 are the treatment indicator variables for individuals receiving the incentive and individuals receiving no incentive, respectively. Finally, Z=1 indicates the individual received the incentive and Z=0 indicates the individual did not receive the incentive.

The IV estimator satisfying equation (2.38) removes the necessity of identifying all relevant covariates X_{it}, because the incentive (Z) is randomly assigned and can only affect the outcome variable through the impact on treatment status. In the case of the binary instrument, the IV estimator can be implemented with fairly innocuous assumptions on functional form. In the presence of homogenous treatment effects, the estimable ATE in the IV application is given by:

[17] This is equivalent to the monotonicity assumption of Angrist, Imbens, and Rubin (1996), which is formally given by: $D_i|Z=1 \geq D_i|Z=0$.

$$ATE = E(Y^{11} - Y^{00}|D^1 - D^0 = 1) = \frac{E(Y|Z=1)-E(Y|Z=0)}{E(D|Z=1)-E(D|Z=0)}, \quad (2.39)$$

where the second equality in equation (2.39) is the standard Wald estimator. Notice from the first equality in equation (2.39) that the estimated treatment effect is coming only from individuals opting to receive treatment based on receipt of the random incentive. In the likely scenario that there are heterogeneous treatment effects across individuals, then the treatment effect estimated in equation (2.39) is interpreted as a local average treatment effect (LATE) that is valid only for the individuals who are incentivized to receive treatment by the instrument (*Z*) (Angrist, Imbens, & Rubin, 1996).

The primary issue with the instrumental variables estimator is finding a relevant and valid instrument that is strongly correlated with the treatment decision and uncorrelated with the outcomes of interest. Traditionally, applied empiricists have paid particularly close attention to the exogeneity requirements of a valid instrument and very little attention to the requirements of strong correlation with the endogenous regressor. However, Bound, Jaeger and Baker (1995) examined the finite sample properties of IV in the presence of weak instruments, and note that even in large samples, the bias of IV estimates approaches that of OLS when instruments are systematically reduced in strength. Furthermore, the addition of many weak instruments holding the total predictive quality of the instrument set constant only serves to exacerbate the bias of weak IV estimates.[18]

[18] Bound, Jaeger and Baker (1995) are examining IV strictly in the application to two-stage least squares with weak instruments. Chao and Swanson (2005) examine the asymptotic properties of various estimators when the number of instruments approaches infinity as well as sample size approaching infinity. They find that the two-stage least squares estimator is inconsistent unless the concentration parameter (a measure of the first stage goodness of fit for the instruments in predicting the endogenous regressor) grows faster than the number of

2.3.5 Regression Discontinuity Design[19]

Regression Discontinuity Design (RDD) exploits a known assignment rule for determining treatment based on the observable variable, S. In many ways there is a great deal of overlap between methods of RDD, selection on observables, and IV. Classification of RDD in the selection on unobservables category is therefore not entirely correct as it is a more generalized model capable of handling both observable and unobservable factors in a very specific context. The very nature of RDD is much more structural in method, because the assignment mechanism for treatment is known and can be modeled directly in the estimation.[20] This is a particular advantage for RDD as compared to the other quasi-experimental estimators, because the resulting conditional independence assumption can be stated as:

$$Y^0, Y^1 \perp D | S = S_T, \tag{2.40}$$

where S is simply the variable on which the assignment mechanism is based, and S_T is a known assignment threshold such that there is a discrete jump in the probability of treatment for $S \geq S_T$. ME and OLS require that all observable covariates are controlled for and there are

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instruments. Other estimators such as limited information maximum likelihood and biased two stage least squares do not require as stringent assumptions on the growth of the concentration parameter when using many weak instruments, but the necessary conditions for consistent estimation in the presence of many weak instruments are still fairly stringent (Staiger & Stock, 1997; Chao & Swanson, 2005).

[19] Hahn, Todd, and Van Der Klaauw (2001) and van der Klaauw (2002) develop a general formulation for RDD techniques expressing the connections of RDD with selection on observables, and IV(including traditional IV estimators such as two-stage least squares). The empirical explanations of this section will follow their formulation.

[20] With sharp RDD the assignment mechanism is modeled without error, while fuzzy RDD estimates the assignment mechanism based on some a priori knowledge of the assignment rule.

no relevant unobserved covariates, DID requires no individual specific time varying confounders, and IV requires valid and relevant instruments. All of these assumptions are likely to be sources of controversy in the implementation of any of the aforementioned estimators. With RDD, however the assignment mechanism is known, thereby satisfying the necessary conditional independence assumption in a trivial manner provided that individuals are not able to systematically alter their assigning variable, *S*, to avoid or achieve treatment (Lee & Lemieux, 2010).

RDD are generally distinguished in the literature as either sharp RDD or fuzzy RDD (Trochim, 1984; Campbell, 1969). The distinction is based on the deterministic nature of the sharp RDD assignment rule in comparison to the stochastic nature of the fuzzy RDD assignment rule. Both fuzzy and sharp RDD exploit a discrete jump in probability of treatment based on a known treatment assignment rule. For sharp RDD, treatment assignment is based on the following deterministic assignment rule:

$$D_i = 1, if\ S_i \geq S_T,$$
$$D_i = 0, if\ S_i < S_T. \tag{2.41}$$

Individuals with a realization of the assigning variable (S_i) that is greater than or equal to the cutoff threshold (S_T) receive treatment with certainty, while individuals below the cutoff threshold receive no treatment. For fuzzy RDD, there is still a discrete jump in the probability of treatment at the cutoff threshold, but the jump in probability is less than 1. The assignment rule for fuzzy RDD is given by:

$$P(D_i = 1) > P(D_i = 0), if\ S_i \geq S_T, \tag{2.42}$$

where the probability of treatment, $P(D_i=1)$, is greater for individuals with realizations of S_i above the cutoff threshold.

In the presence of homogenous treatment effects across individuals, it is possible to estimate the ATE in RDD using the following Wald estimator:

$$ATE = \frac{E(Y^+)-E(Y^-)}{E(D^+)-E(D^-)}, \tag{2.43}$$

where $E(Y^+)$ and $E(Y^-)$ are the averages of the outcome variable directly above and below the cutoff threshold, respectively. Similarly, $E(D^+)$ and $E(D^-)$ are the average probabilities of treatment directly above and below the cutoff threshold. If treatment effects are heterogeneous, however, the estimated treatment effects from RDD can be interpreted as a local average treatment effect (LATE) valid only for those individuals in close proximity to the cutoff threshold S_T (Angrist, Imbens, & Rubin, 1996). The only difference between sharp RDD and fuzzy RDD is the denominator of equation (2.43). In sharp RDD, the probability of treatment jumps from zero to one for individuals above the cutoff threshold, and the denominator is therefore equal to one. In fuzzy RDD the probability of treatment jumps by a finite amount that is less than one at the cutoff threshold. The denominator in equation (2.43) can therefore be viewed as a correction factor that adjusts the estimated treatment effect for misclassification in cases where the cutoff threshold is not a purely deterministic indicator of treatment assignment.

2.4 Conclusion

In sum, this chapter presents the hedonic equilibrium theoretical framework for the existence of compensating wage differentials for risk. Empirical applications of the hedonic wage approach are thought to produce biased estimates of the wage/risk tradeoff due to

measurement error in risk and omitted variable bias. As a result, the alternative RCM empirical framework is presented as a possible alternative, and several quasi-experimental estimators and their identifying assumptions are then introduced within the RCM framework for estimating causal treatment effects.

The following chapter introduces the OSHA inspection process focusing on the characteristics of inspection assignment that plausibly satisfy the conditional independence assumptions necessary for estimating causal treatment effects. The data available to explore these assumptions and estimate the effects of OSHA inspections on wages is also presented, in turn.

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2.6 Tables and Figures

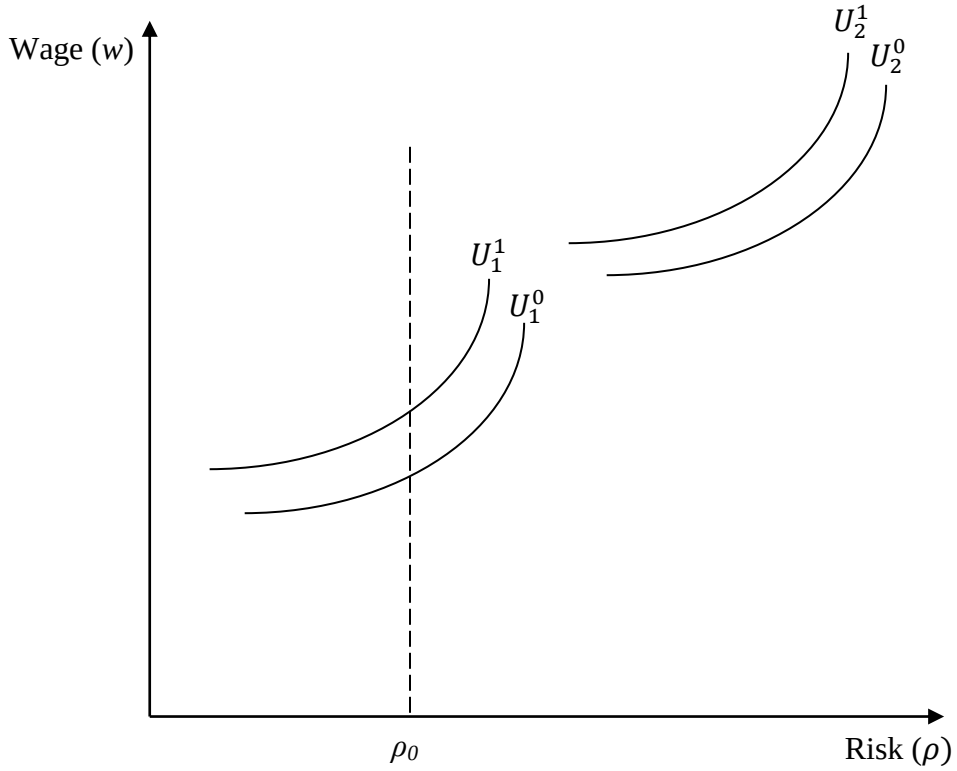

Figure 2.1: Indifference Curves for Workers with Heterogeneous Preferences for Risk

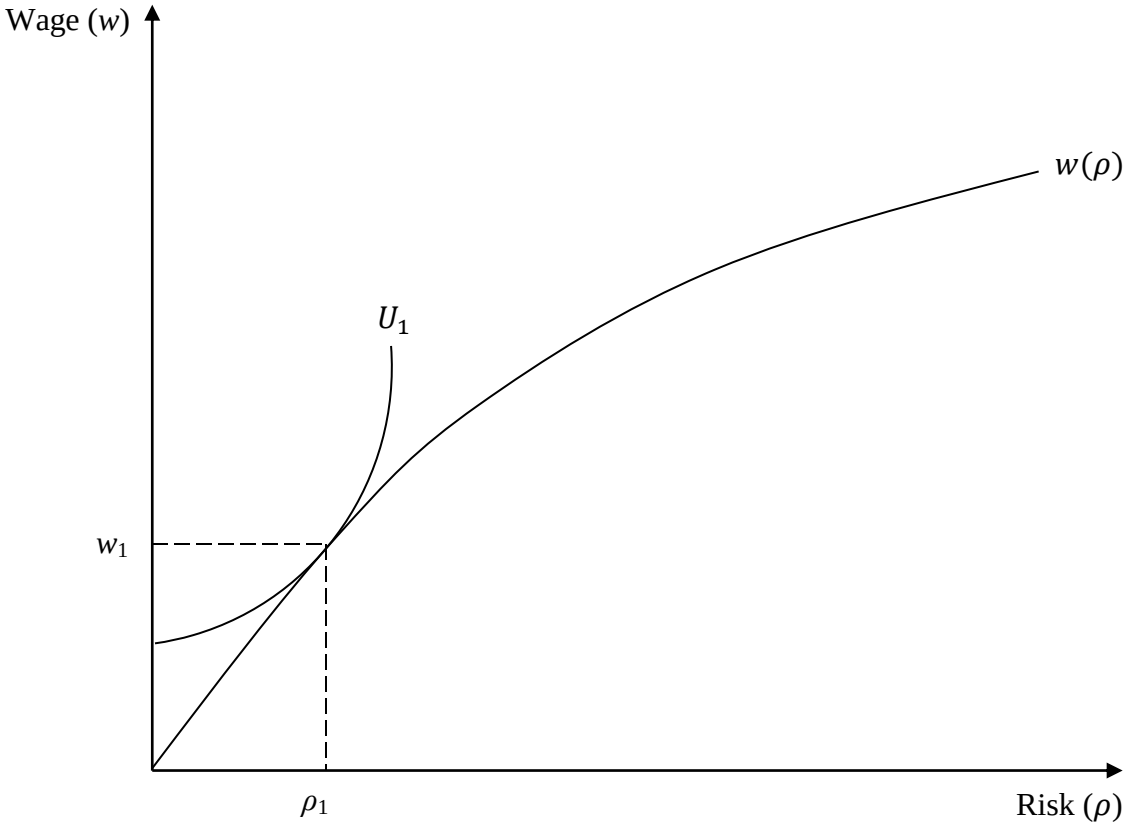

Figure 2.2: Equilibrium Sorting of Workers

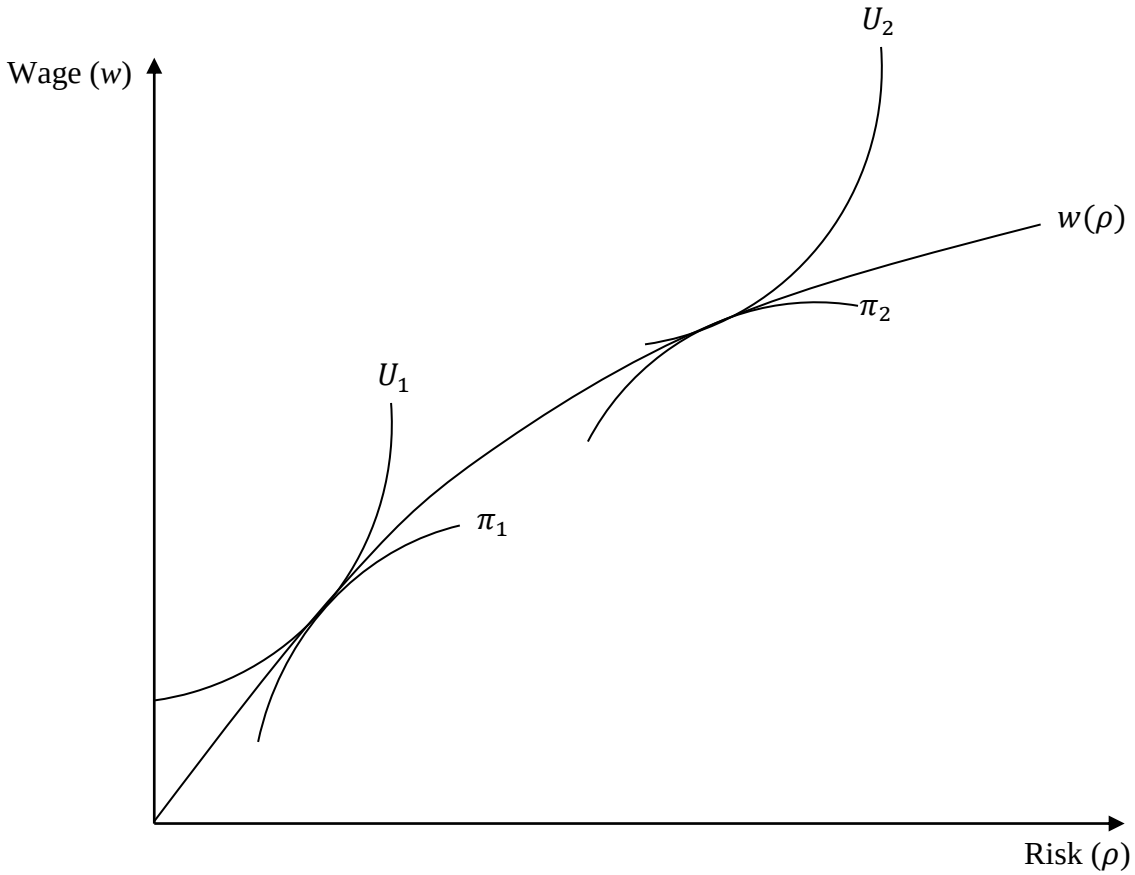

Figure 2.3: Hedonic Wage Equilibrium

Chapter 3

OSHA Inspection Process and Data

OSHA was established under the Occupational Safety and Health Act (OSH Act) on December 29, 1970. Since inception, OSHA has focused mostly on setting and enforcing workplace safety and health standards, with the majority of its funding devoted to enforcement of standards through workplace inspections (Fleming, 2001; Siskind, 1993; MacLaury, 1984). The OSHA inspection process can be separated into two basic stages. First plants are chosen to receive inspections based on various criteria. Selected plants then receive unannounced (surprise) inspections to ensure that OSHA safety and health standards are being enforced at the plant.

In order for OSHA inspections to be a valid and relevant instrument for plant-level risks, the inspections must be uncorrelated with any unobserved determinants of the wage rate and significantly correlated with plant-level risk rates. In Section 3.1, the selection process that OSHA uses to target plants for safety and health inspections is described. Importantly, Section 3.1 also highlights the 1978 to 1998 time period during which a particular class of OSHA inspections were randomly assigned. The neutral selection criterion offers assurance that OSHA inspections are a valid instrument for risk.

The relevant instrument requirement is addressed through the OSHA enforcement process, which is described in detail Section 3.2. In sum, plants found in violation of OSHA standards for safety and health are required by law to comply with the standards, and compliance must generally occur within 30 days of violation citation (OSHA 1994).

Following inspection, OSHA continues to monitor plants found in violation until the violations are corrected.

Section 3.3 presents an overview of the data available from OSHA that is used to test whether OSHA inspections statistically satisfy the requirements for a valid and relevant instrument. In order to estimate the causal impacts of job risk on wages, the OSHA inspection data is combined with confidential wage data from the Census Bureau's census of manufactures (COM). The COM data is described in detail in Section 3.4, and Section 3.5 concludes this chapter.

3.1 OSHA Selection Process for Inspections

The initial process to select a plant for inspection varies considerably in terms of methodology. Inspections are broadly classified as "programmed" and "unprogrammed" based on alternative selection criteria, and each are discussed in turn below.

3.1.1 Programmed Inspections

Programmed inspections are those which are targeted at plants or industries that are believed to be the most dangerous and pose the largest health and safety hazards. Table 3.1 summarizes the scheduling system for Federal OSHA programmed inspections 1978 to 1998. During the initial years of operation from 1972 to 1977, OSHA had no formal targeting procedure for selecting plants to receive programmed inspections and OSHA was often criticized for its "haphazard" targeting system (Mintz, 1984). Beginning in 1978, OSHA implemented a scheduling system for programmed safety inspections that targeted high-hazard industries, but randomly selected the firms within these industries using a neutral selection criterion. In 1979, a similar scheduling system was adopted for programmed health

inspections (Gray & Jones, 1991).[21] Plants with fewer than 11 employees and plants that received comprehensive inspections in the recent past were exempted.[22] Thus, conditioned on industry, firm size, state and recent inspection history, programmed inspections were randomly distributed among plants.

Programmed inspections followed this selection criterion from 1978 to October, 1981, at which point a "records–review–only" inspection program was established for manufacturing facilities. This program allowed inspectors to view a plant's injury/illness records upon arrival at the plant, and if the plant's rates were below the national average injury/illness rate for manufacturing industries, then the OSHA officer would not conduct a comprehensive safety or heath inspection. There was a substantial decline in records-review-only inspections beginning in 1984 due to increased criticism that the program created

[21] OSHA's inspection scheduling system was developed in response to the 1978 U.S. Supreme Court case Marshall v. Barlow's Inc., which established that OSHA had to obtain a warrant if refused entry to a plant and that probable cause for a warrant could be justified with a documented employee complaint or demonstration that the plant was chosen for inspection through a process of neutral selection (Marshall v. Barlow's Inc. 436 U.S. 307, 321, 1978).

[22]The definition of "recent past" for inspection varies across time. In 1981, OSHA directive CPL 2.25B states that plants receiving a comprehensive safety inspection within the past year are exempted (Mintz, 1984). This is presumed in force from 1978 until 1984 (and as discussed later, the data appear to support this supposition). From 1990-1994, plants receiving a comprehensive safety inspection within the past two years and comprehensive health inspections within past three years are exempted (Source: CPL 2.25H) and from 1995-1998, plants receiving a comprehensive safety or health inspection within the past five years were exempted (Source: CPL 2.25I). During the period 1984 -1990, direct documentation of the exemption period for recently inspected plants could not be found. Based on the stated procedures in the years prior and after this time-frame, and data analysis examining plants receiving multiple inspections during 1984-1990, it was assumed that plants receiving a comprehensive safety or health inspection within the past two years were exempt from inspection.

incentives for firms to purposely underreport their injury and illness rates.[23] OSHA responded to the criticism by reducing aggregate numbers of records-review-only inspections as well as adopting a policy of randomly inspecting every 10th plant regardless of their injury/illness rate (Brooks, 1988). By 1987, records-review-only inspections were only 4% of total inspections, falling to 2% and 1% in 1988 and 1989, respectively. OSHA discontinued this program completely in 1990, returning to the previous scheduling system that randomly selects firms conditional on industry, state and inspection history.

OSHA's random scheduling system for programmed inspections continued from 1990 until 1998, at which point OSHA substantially changed its selection criteria, moving back to a system that targeted plants based on past injury and illness incidence rates. For the primary analysis, therefore, the focus is on the programmed inspections during the period 1978 to 1998, excepting the five-year period between 1982 and 1987.

Beginning in 1999, OSHA adopted the site-specific targeting (SST) plan, which targeted plants for inspection whose reported lost workday and injury and illness (LWDII) rates in the prior year exceeded a threshold set by OSHA (OSHA, 2004). Programmed inspections conducted under the SST plan are not randomly assigned, and are therefore not part of the main analysis of this dissertation. In the supplementary analysis of Chapter 5 Section 5.3 the causal effects of SST programmed inspections are explored. Table 5.4 provides a detailed overview of the SST plans employed by OSHA from 1999 to present.

[23] (Subcommittee of the Committee on Government Operations House of Representatives, 1987)

It should be noted that under the Occupational Safety and Health Act of 1970, States are able to develop and operate their own OSHA programs, in which case Federal OSHA programming is not implemented. I focus on 28 states and the District of Columbia that operate under the Federal OSHA inspection programs since a common scheduling system is implemented across each of these states. A map of the states that are covered by Federal OSHA is provided in Figure 3.1. As the map reveals, nearly half of the state plan OSHA states are clustered in the Western region. The remaining state plan states are distributed across the Midwest and South. Vermont is the only state in the Northeast that operates its own OSHA plan. Section 3.4 provides an examination of the differences in demography and manufacturing activities between Federal and state OSHA.

3.1.2 Unprogrammed Inspections

Although not a focus of my analysis, OSHA also conducts a substantial number of "unprogrammed" inspections per year. Unprogrammed inspections are those which occur in response to an event at the workplace such as an accident, a complaint, a follow-up from previous inspections, a referral, monitoring, and unprogrammed related inspections. Plant-level accidents that result in a worker fatality or multiple injuries generally trigger an OSHA accident inspection. The purpose of accident inspections is to collect detailed information about the accident to determine the cause, and whether any violations contributed to the accident in order to help guide the formation of safety and health standards.

Complaint inspections are initiated based on formal reports of dangerous working conditions at a plant that are made by employees of the plant. Follow-up inspections are conducted to insure that abatement of hazardous conditions cited in previous inspections has

occurred. Referral inspections are similar to complaint inspections save the fact that they result from a media report, or referral from some individual who is not an employee of the cited plant. Monitoring inspections are conducted to check the progress of abatement activities resultant from previous inspections (these inspections are conducted before the required abatement date, thereby distinguishing themselves from follow-up inspections). Finally, unprogrammed related inspections occur when multiple employers at a single worksite are inspected due to a complaint against one of the employers. An example would be a complaint about an electrical company at a construction worksite that also results in the inspection of a plumbing company at the same worksite.

Unprogrammed inspections are not random, but are used two ways in the analysis. First, when considering the impact of programmed inspections on plants, I must carefully consider whether or not (and when) a plant had an unprogrammed inspection in the recent past. While having an unprogrammed inspection does not exempt a plant from being included in the programmed inspection selection process, and thus does not affect the probability of receiving a programmed inspection, it could result in plant-level changes in safety that pre-date the programmed inspection and hence confound the estimated impacts of programmed inspections on safety and wage rates. As such, they are important to catalogue and consider in the econometric analysis. Second, data from unprogrammed accident inspection reports are used to construct injury and fatality rates at the plant level for a sample of firms. This data is used to determine whether OSHA inspections do in fact result in changes in plant-level injury and fatality rates.

3.2 OSHA Enforcement Process for Found Violations

Although the initial selection process varies considerably by inspection classification, the enforcement process outlined below is generic and applies to all establishments receiving an inspection.[24] Inspections are conducted without advance notice to employers except under rare circumstances. When a plant is chosen for inspection, OSHA compliance officers first show credentials, then meet with the employer to discuss the nature and scope of the inspection (safety inspection, health inspection, or both). The employer chooses an "employer representative" to accompany the inspector throughout the inspection process. An "employee representative" is also usually chosen to accompany the compliance officer during inspection. 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 of the inspection.

If any violations are found during the inspection process they are reported to the Area Director of the OSHA jurisdiction to decide on citations and penalties. OSHA citations based on violated standards can be classified into five broad categories.

1. An Other-Than-Serious Violation is an infraction that "has a direct relationship to job safety and health, but probably would not cause death or serious physical harm"(OSHA, 2002). These violations carry a maximum penalty of $1,000 as of 2002.

[24] Information on the inspection process is summarized in OSHA (2002) except where otherwise noted.

2. A Serious Violation is an infraction "where there is a substantial probability that death or serious physical harm could result" (OSHA, 2002), and carry a maximum penalty of $7,000 as of 2002.
3. A Willful Violation is an offense that is intentionally and knowingly committed by the employer. The maximum penalty associated with a willful violation is $70,000 as of 2002. Furthermore, if a willful violation is found to have resulted in the death of an employee then employers could be subject to a larger fine and/or imprisonment.
4. A Repeated Violation is an offense that is found upon re-inspection that is similar to an original infraction, and carries a maximum fine of $70,000 as of 2002.
5. A Failure-to-Abate Violation is a violation resulting from an establishment not correcting prior infractions within the allotted timeframe. It carries a maximum civil penalty of $7,000 for every day that the infraction continues past the allotted timeframe.

Plants that are found to be in violation ("known violators") are required by law to correct the violations in order to comply with OSHA standards for safety and health. In comparison, plants that are in compliance with OSHA standards ("known compliers") are not required to make any safety improvements. For known violators, correction of violations must generally occur within 30 days of citation (OSHA 1994).

This dissertation focuses on the plant-level safety impacts of the initial citation issuance for plants found to be in violation of OSHA standards. Plants never receive a repeat or failure-to-abate violation during their first inspection. Willful violations are rarely issued, and for the plant's receiving their first citations nearly all receive at least one serious

violation. As a result, the focus is not on the heterogeneous impacts of different types of OSHA violations. A plant is simply classified as a known violator if they receive a violation of any classification (#'s 1 to 5 above). Otherwise if the plant receives no violation citations it is classified as a known complier.

3.3 OSHA Data

A census of every OSHA inspection conducted in the manufacturing sector between 1972 and 1998 is combined with information on plant-level wages and establishment characteristics from the Census Bureau's confidential Census of Manufactures (COM) for the 20 year period between 1978 and 1998. Data on every OSHA inspection conducted during OSHA's first 27 years of operation is obtained from the publicly available OSHA Integrated Management Information System (IMIS) inspection database.[25] For each inspected plant, IMIS records the plant name, address, 1987 SIC Classification, date of inspection, type of inspection, found violations, fines levied, plant injury rates for the prior year as recorded in the plant's OSHA-required log reports, and the number of employees at the plant. Note, the injury rates are not made publicly available. However, as discussed in Section 3.2, "serious injury" rates are computed for each plant based on unprogrammed inspection reports.

There are just over 69,000 inspections conducted by OSHA each year during the twenty-year period. Of these, an average of 24,038 each year were in the manufacturing sector. Table 3.2 presents summary statistics for manufacturing inspections. Programmed inspections account for the majority of inspections each year, comprising 57% of the total

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

inspections. There is an increase in inspections during the first decade (1978-1987) and a decline over time thereafter.

OSHA policy of reviewing injury logs and only inspecting if the rate is above the national average began in October, 1981. As a result, there are 2.7% of programmed inspections in the period 1978 through 1981 that were records-review-only. Although not shown in Table 3.2, from January 1978 to September 1981, there were no records-review-only inspections. Records-review-only inspections are 26.6% of total programmed inspections during the period 1982 to 1987, indicating that 73.4% of plants had injury rates above the national average for all manufacturing and received a comprehensive on-site inspection. It is not surprising that records-review-only inspections are a minority of inspections given programmed inspections target high-risk manufacturing industries and the cut-off for receiving a comprehensive inspection is the national average injury rate for all manufacturing industries. As indicated earlier, the summary statistics show a significant decline in the number of programmed inspections that were records-review-only in 1988 and 1989, and the process was discontinued entirely in 1990.

Table 3.2 also reports the average number of times a violation is found after an inspection (programmed or unprogrammed) and the average number of violations per inspection. Across time periods, between 50% and 66% of inspections result in a violation, and there are between six and eight violations found per inspection on average.

The OSHA inspection data for each plant is matched to COM data for each plant (described in the next section) using an iterative process that is based on a matching algorithm developed by Fellegi and Sunter (1969). A probability matching procedure is

implemented which assigns a matching score to each possible record combination in the COM and OSHA inspection data sets. The matching score is a ratio of the probability that a particular sequence of fields are matched given they are true matches divided by the probability that a sequence of fields are matched given they are *not* true matches. In addition to plant name and address, the comparison space is augmented to include whether or not the two records are a match using the DQMatch function in SAS.[26] A threshold for the matching score is then chosen so that plants above the threshold are considered matches, and plants below the threshold are not. A lower-bound threshold is also established to eliminate records that are almost certainly not matches. The matching is conducted on an industry by industry basis at the 2-digit SIC classification. As a general rule, about one-half of plants have matching scores above the threshold indicating they are a match. The remaining records are manually checked if their matching score is above the lower-bound threshold. The manual checks are time-intensive, with generally between 100,000 and 250,000 records to inspect. As such the analysis is limited to industries that have relatively high fatality rates as well as having relatively high initial match rates. Focusing on relatively dangerous industries may reduce the external validity of this analysis, especially in the presence of negative equilibrium labor market sorting between risk and risk aversion.

Table 3.3 reports the average fatality and injury rates by industry for the five year period 1992 to 1996. Lumber and wood products (SIC 24) for example, was excluded from the carefully matched industries despite having the highest fatality and injury rates because it had the lowest initial match rate of all the manufacturing industries. The seven industries

[26] *SAS 9.1.2 Data Quality Server: Reference*, Cary, NC: SAS Institute Inc., 2004

focused upon are food and kindred products (SIC 20), paper and allied products (SIC 26), printing and publishing (SIC 27), chemicals and allied products (SIC 28), stone, clay and glass products (SIC 32), primary metal industries (SIC 33), and fabricated metal products (SIC 34).

Measured in terms of industry fatality rates stone, clay and glass (SIC 32), and primary metal industries (SIC 33) are the most dangerous industries with average fatality rates of 12.3 per 100,000 workers. Printing and publishing is the safest industry with an average fatality rate of 4.3 per 100,000 workers. Table 3.3 also indicates that there is considerable heterogeneity between industry rankings in terms of fatality rates versus injury rates. Chemicals and allied products (SIC 28) for example has the lowest injury rates of the seven industries focused upon, but it ranks third highest in terms of fatalities.

The general trend in inspection history among the seven industries closely follows the trend for all manufacturing industries as presented in Table 3.2. The seven focus industries account for roughly 50% of all manufacturing establishments and 49% of all OSHA inspections conducted in the manufacturing sector. There were a total of 246,573 inspections in these seven SICs over the twenty-year study period. Of these, 58% or 141,854 were programmed inspections. Table 3.4 presents summary statistics on average annual inspections for each industry over the study period. As indicated in Panel A and C in Table 3.4, there are an average of 14,311 inspections per year in the seven industries from 1978 to 1981, and this number declines by about 50% to 7,168 inspections per year on average during 1988 to 1998. Among the seven industries, fabricated metals receives the most total inspections on average (programmed or unprogrammed), followed by food products, primary

metals and then chemical products. It should be noted that the number of inspections in each industry is related to the number of plants in each industry, with fabricated metals having the most plants among the seven SICs. Printing has the second most plants followed by food products, but printing generally receives the fewest number of OSHA inspections because it is the safest relative industry included in the analysis.

Across all industries, about half of the inspections result in at least one safety violation being found. Printing consistently has the lowest average number of violations per inspection further reflecting the relative safety of the printing industry in comparison to the other six industries considered. Although not reported in Table 3.4, inspections occur at approximately 10% of plants in the seven SIC industries on average each year. This rate offers a reasonable number of inspections for analysis (an average of 17,000 per year) as well as a substantial number of control group observations for comparison (approximately 170,000 plants per year).

3.4 COM Data

The COM is conducted every five years from 1967 to present and is a complete census of all manufacturing plants in the U.S.. Importantly, the COM allows for construction of a panel of plant-level data based on each plant's unique longitudinal identifier assigned by the Census Bureau, the Permanent Plant Number (PPN). The census collects detailed information on plant-level revenue, expenditures, employment and payroll information. Confidential plant-level records for five COM waves from 1977 to 1997 are accessed through special approval at the Triangle Census Research Data Center.

Every COM questionnaire collects employment and payroll information for the total number of workers and the total number of production workers separately. The total number of hours worked in total and for production workers only is also collected. These two pieces of information allow computation of the average hourly wage for production workers at each plant. Being able to distinguish production workers' wages at the plant-level is important as these workers' wages are expected to be more tightly linked to plant-level safety since they are the workers routinely exposed to risks. Importantly, the COM also collects detailed data on employer's costs for fringe benefits. This will allow separate identification of the compensating tradeoffs between risk, wages, and other monetary fringe benefits.

In addition to payroll information, the COM collects information on the total cost of materials, total revenues, and quarterly number of employees by type of worker. The quarterly employment levels allows me to construct a rough measure of worker turnover by calculating the average decrease in total production workers on payroll between quarters across a year as a percent of the average number of production workers employed that year. Table 3.5 presents the variable definitions for data available or constructed from the COM.

As stated in Section 3.1, the focus is on states operating under federal OSHA jurisdiction. Columns one and two of Table 3.6 compare federal and state plan OSHA states. On average the manufacturing activity in state plan jurisdictions is slightly greater than in federal jurisdictions as reflected by the difference in total value of shipments and cost of materials. The difference in average manufacturing activity is due to the fact that California operates its own state OSHA plan. Although not reported in Table 3.6, excluding California, the remaining state plan OSHA states average total value of shipments and cost of materials

are significantly lower than the Federal OSHA average. Wages are slightly higher in state plan OSHA states, but production worker productivity is slightly higher in federal states. State OSHA has more employees and more production workers on average, but the percentage of total employees who are production workers is 71% in both state and federal OSHA.

Table 3.6 also compares state-level risk rates for manufacturing workers. State plan OSHA has significantly higher injury rates, but fatality rates are comparable between state and federal OSHA. Finally, Table 3.6 reports differences in population demographics between federal OSHA and state OSHA, which may be sources of heterogeneous preferences for risk among the two groups. In general the average age, gender, and income characteristics are very similar between federal and state OSHA. White and black racial groups make up the largest and second largest share of the population for both federal and state OSHA. Although not reported in Table 3.6, there is a larger degree of heterogeneity in the racial profile of state OSHA in comparison to federal OSHA. State plan OSHA states have more Hispanic, Asian, American Indian, and Pacific Islander persons than their Federal OSHA counterparts.

The last two columns of Table 3.6 compare all manufacturing industries with the seven carefully matched industries. As reflected by the differences in total value of shipments and cost of materials; on average industry output is larger in the seven industry sample. Industries in the seven industry sample are also larger with approximately 900,000 total employees per industry compared to 800,000 total employees across all manufacturing

industries. The seven industry sample also has more production workers, and as a result there are more hours worked by production workers in the seven industry sample.

The average hourly wage rate in the seven industry sample is $0.23 higher than the average for all manufacturing industries, and they face a higher overall level of job risks (a 1/100,000 higher probability of death and a 2,432/100,000 higher probability of injury). It is not surprising that the fatality rates and injury rates are higher in the seven industry sample because this sample specifically targeted the most dangerous industries with the highest initial match rates.

Table 3.7 presents summary statistics from the 1997 COM wave for the seven industries. Fabricated metal products is the largest industry with 1.8 million workers in 1997, of which 72% are production workers. The second largest industry, food and kindred products, has 1.5 million workers and 73% are production workers. The remaining industries had fewer than one million workers, although in each industry production workers are the majority of the workforce. The last row in Table 3.7 presents my constructed wages for production workers. Chemical, paper, and primary metal industries had the highest average wage for production workers in 1997, and food and kindred products had the lowest. Interestingly, as the next to last row in Table 3.7 indicates, the food industry had a relatively high productivity measure for production workers (defined as the ratio of the total value of shipments to the total hours worked by production workers), yet this industry has the lowest average wage among the seven considered.

3.5 Conclusion

In sum, the key features of the OSHA inspection process that I exploit in this dissertation are the random inspection assignment for programmed inspections, the plant-level accident and fatality counts from unprogrammed accident inspections, and OSHA's heterogeneous treatment of known violators and known compliers. Table 3.1 summarizes the scheduling system for Federal OSHA programmed inspections 1978 to 1998. During this 20-year period, Federal OSHA inspections are considered to be a plausibly exogenous instrument for plant-level risk, with the exception of the period between 1982 and 1987. In order for OSHA inspections to be a relevant instrument however, they must affect plant-level fatality rates and injury rates in addition to being exogenous. To test the relevance of the OSHA inspection instrument, the fact that known violators are required by law to make the necessary safety improvements required for complying with OSHA standards is utilized. Therefore, the effects of the safety improvements following an OSHA violation on plant-level fatality and accident rates is estimated in order to measure the efficacy of OSHA enforcement. Complying plants that are already meeting OSHA safety standards serve as a control group in this analysis.

In the following chapter, the empirical evidence on the exogeneity of the OSHA inspection process is established, as well as the impact on fatality and injury rates. Given evidence that OSHA inspections are a valid and relevant instrument, I then implement and present the DID models described in Chapter 2 Section 2.3.3 to test for compensating wages for risk and estimate the VSL.

3.6 References

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3.7 Tables and Figures

Table 3.1: Summary of OSHA Inspection Process 1978 - 1998.

Time Period	OSHA Selection Process for Programmed Inspections
1972 – 1977	Indeterminate.
1978 – 1981	High-hazard industries are targeted. Plants within these industries are randomly selected within each state; plants with fewer than 11 employees or that have received a comprehensive review within the recent past are exempted.[1]
1982 – 1987	"Records-review-only" inspections are conducted. Upon arrival at plant, inspector reviews injury/illness logs and only inspects if reported rates are above the national average (across all industries).
1988 – 1989	"Records-review-only" inspections are being phased out, representing only 1-2% of total inspections.
1990 – 1998	Selection criteria established for 1978-1981 are again in force.
1999 - present	Plants selected based on previous year's injury/illness rates. Reported rates above a threshold trigger inspection.

[1] See footnote 21 for the time frames which OSHA chose for "recent past" when exempting plants that have been inspected recently.

Table 3.2: Average Annual OSHA Inspections Summary for the Manufacturing Industries.

	Annual *Averages*				
Time Period	**Programmed Inspections [% records review only]**	**Unprogrammed Inspections**	**Total Inspections**	**Inspections Resulting in a Violation (% of Total)**	**Violations per Inspection**
Total Period (1978 – 1998)	13,674 [8.9%]	10,364	24,038	12,369 [54.0%]	6.95
1978 – 1981	10,353 [2.7%]	17,672	28,025	14,548 [52.0%]	6.21
1982 – 1987	27,726 [26.6%]	9,968	37,694	16,777 [45.8%]	5.98
1988 – 1989	9,606 [4.7%]	10,552	20,158	13,233 [65.7%]	7.69
1990 – 1998	6,686 [0.8%]	7,339	14,025	8,271 [57.9%]	7.77

[1]All data compiled from the OSHA IMIS data, available as downloadable files from: www.ogesdw.dol.gov/raw_data_summmary.php (last accessed April, 2011).

Table 3.3: Average Industry Fatality Rates and Injury Rates 1992-1997.[1]

Industry Name	SIC	Injury Rate[2]	DAFWI[3]	Fatality Rate[4]
Food and kindred products	20	19,788	5,640	7.5
Tobacco products	21	9,179	3,063	0
Textile mill products	22	9,399	1,961	2.3
Apparel and other textile products	23	7,235	1,961	1.0
Lumber and wood products	24	17,859	6,045	34.9
Furniture and fixtures	25	15,330	4,022	2.9
Paper and allied products	26	12,168	3,378	5.1
Printing and publishing	27	10,389	3,363	4.3
Chemicals and allied products	28	10,679	2,806	7.4
Petroleum and coal products	29	9,633	2,701	19.1
Rubber and misc. plastics products	30	15,044	4,179	3.7
Leather and leather products	31	12,718	3,362	0.8
Stone, clay, and glass products	32	17,250	6,196	12.3
Primary metal industries	33	20,634	6,435	12.3
Fabricated metal products	34	19,068	5,834	4.5
Industrial machinery and equipment	35	16,969	4,877	4.2
Electronic and other electric equipment	36	10,275	3,006	2.0
Transportation equipment	37	23,851	6,670	4.9
Instruments and related products	38	8,344	2,497	1.9
Misc. manufacturing industries	39	11,376	3,596	4.1

[1]The highlighted fields are for the seven carefully matched industries that have high fatality rates and high initial match rates.

[2]Injury rates are derived from the Bureau of Labor Statistics (BLS) Annual Survey of Injuries and Illness (ASOII), and are scaled to represent injuries per 100,000 employees.

[3]Days away from work injury rates (DAFWI) are also obtained from the Annual Survey of Injuries and Illness.

[4]Fatality rates are calculated by dividing the total number of workplace fatalities as recorded in the BLS Census of Fatal Occupational Injuries (CFOI) by the total number of employees within the industry as reported in the BLS National Industry-specific Occupational Employment and Wage Estimates. Fatality rates are scaled to represent fatalities per 100,000 employees.

Table 3.4: Average Annual OSHA Inspections by Two-Digit SIC Code.[1]

Time Period	Annual Averages (% of Total) Programmed Inspections [% records review only]	Un-programmed Inspections	Total Inspections	Inspections Resulting in a Violation	Violations per Inspection
		Panel A: 1978-1981			
Total	5,542 [2.7%]	8,769	14,311	7,476	6.2
Food Products	1,556 [5.0%] (28.1%)	1,460 (16.6%)	3,016 (21.1%)	1,602 (21.4%)	5.3
Paper Products	469 [2.0%] (8.5%)	694 (7.9%)	1,163 (8.1%)	631 (8.4%)	6.3
Printing	237 [2.3%] (4.3%)	500 (5.7%)	737 (5.1%)	379 (5.1%)	4.7
Chemical Products	591 [1.0%] (10.7%)	1,082 (12.3%)	1,673 (11.7%)	797 (10.7%)	5.6
Stone, Clay and Glass	459 [3.3%] (8.3%)	741 (8.5%)	1,200 (8.5%)	634 (8.5%)	6.2
Primary Metals	664 [2.6%] (12.0%)	1,579 (18.0%)	2,243 (15.7%)	1,140 (15.2%)	7.7
Fabricated Metals	1,566 [2.8%] (28.3%)	2,713 (30.9%)	4,279 (29.9%)	2,293 (30.7%)	6.5
		Panel B: 1982-1987			
Total	13,689 [28.0%]	4,717	18,406	8,192	6.1
Food Products	3,003 [31.1%] (21.9%)	765 (16.2%)	3,768 (20.5%)	1,662 (20.3%)	5.3
Paper Products	884 [35.2%] (6.5%)	359 (7.6%)	1,243 (6.8%)	566 (6.9%)	5.9
Printing	993 [36.5%] (7.3%)	388 (8.2%)	1,381 (7.5%)	493 (6.0%)	4.7
Chemical Products	1,223 [24.0%] (8.9%)	558 (11.8%)	1,781 (9.7%)	712 (8.7%)	5.2
Stone, Clay and Glass	1,626 [27.0%] (11.9%)	482 (10.2%)	2,108 (11.5%)	929 (11.3%)	6.1
Primary Metals	1,416 [17.2%] (10.3%)	719 (15.2%)	2,135 (11.6%)	1,089 (13.3%)	7.2
Fabricated Metals	4,544 [27.4%] (33.2%)	1,446 (30.7%)	5,990 (32.5%)	2,741 (33.5%)	6.6

Table 3.4 (continued)

Time Period	Annual Averages (% of Total) Programmed Inspections [% records review only]	Un-programmed Inspections	Total Inspections	Inspections Resulting in a Violation	Violations per Inspection
		Panel C: 1988-1998			
Total	3,409 [1.9%]	3,759	7,168	4,307	8.0
Food Products	707 [2.9%] (20.7%)	610 (16.2%)	1,317 (18.4%)	778 (18.1%)	7.1
Paper Products	207 [1.7%] (6.1%)	312 (8.3%)	519 (7.2%)	335 (7.8%)	7.9
Printing	155 [2.2%] (4.5%)	317 (8.4%)	472 (6.6%)	307 (7.1%)	6.4
Chemical Products	191 [3.0%] (5.6%)	474 (12.6%)	665 (9.3%)	401 (9.3%)	7.7
Stone, Clay and Glass	416 [1.5%] (12.2%)	357 (9.5%)	773 (10.8%)	454 (10.5%)	7.7
Primary Metals	379 [1.8%] (11.1%)	568 (15.1%)	947 (13.2%)	583 (13.5%)	8.8
Fabricated Metals	1,354 [1.3%] (39.7%)	1,121 (29.8%)	2,475 (34.5%)	1,449 (33.6%)	8.7

[1]All data compiled from the OSHA IMIS data, available as downloadable files from: www.ogesdw.dol.gov/raw_data_summmary.php (last accessed April, 2011).

Table 3.5: Variable Description for Census of Manufactures (COM) Data.

Variable Name	Description
Total Value Shipments	Total value of all products shipped by a plant, in millions.
Production Workers Payroll	Total annual payroll for production workers, in millions.
Other Employees Payroll	Total annual payroll for workers not classified as production workers, in millions.
Average # Production Workers	Average of the quarterly number of production workers.
Total # Employees	Average annual production workers plus all other employees.
Other # Employees	Total employees minus average annual production workers. Includes everyone not classified as a production worker excepting proprietors and partners.
Hours Worked By Production Workers	Total annual hours worked by production workers, in millions.
Cost of Materials	Total cost of all materials consumed or put into production for the year, in millions.
Single Unit	Dummy variable equal to 1 for plants that are single unit establishment and equal to zero for multi unit establishments
Turnover	The average decrease in production workers on payroll between quarters across a year as a percent of the average number of production workers employed that year.
Productivity	Total value of shipments divided by production workers' total hours worked.
Other Employees Salary	Average annual salary of workers not classified as production workers computed as the total annual payroll for other employees divided by the total number of other employees.
Wages	Average hourly wages of production workers computed as the total payroll for production workers in a plant divided by total hours worked by the production workers.
Total Compensation	Total average yearly compensation for production workers computed as the yearly average salary plus average yearly fringe benefits.

Table 3.6: Comparison of Federal versus State OSHA and Seven Industries versus All Manufacturing (1997).[1]

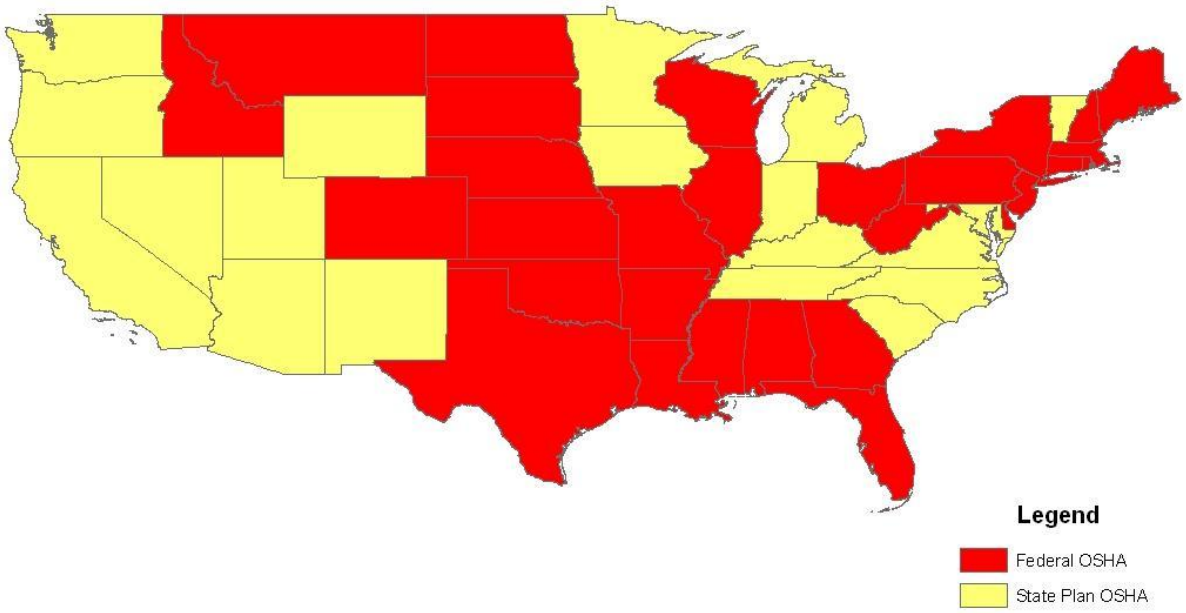
Variable Name	Federal OSHA[2]	State OSHA	Seven Industries	All Manufacturing Industries
Total Value of Shipments[3]	136,000	179,000	226,075	191,735
Production Workers' Payroll	11,500	15,500	20,118	16,913
Total # Employees	0.6	0.8	0.9	0.8
Average # Production Workers (% of total)	0.4 (71%)	0.6 (71%)	0.7 (73%)	0.6 (71%)
Hours Worked By Production Workers	823	1,114	1,414	1,209
Cost of Materials	72,100	90,100	116,661	100,771
Single Unit	----	----	71%	75%
Productivity (in dollars)	163.03	161.89	159.84	158.57
Wages (in dollars)	13.81	13.95	14.22	13.99
Fatality Rate (per 100,000 workers)[4]	4.2	4.5	7.6	6.5
Injury Rate (per 100,000 workers)	9,927	13,117	14,565	12,133
Population[5]	11.1	14.0	----	----
Working Age Population	6.5	8.3		
Working Age 20 to 44 Years Old	62%	63%	----	----
Working Age 45 to 64 Years and Over	38%	37%	----	----
Income per capita (in dollars)	26,992.23	27,198.44		
Female Population	51%	51%	----	----
White Population	74%	70%	----	----
Black Population	14%	11%	----	----

[1] All data are reported in millions, unless otherwise noted.
[2] Average values for federal and state OSHA are weighted by population.
[3] Data are from publicly available COM files for the selected manufacturing industries for the year 1997. The data is publicly downloadable via http://factfinder.census.gov/home/saff/main.html?_lang=en (last accessed Nov. 2011).
[4] Data on fatality rates and injury rates are derived from the BLS CFOI and ASOII, respectively. The data is publicly downloadable via http://www.bls.gov/iif/data.htm (last accessed May 2012).
[5] Population characteristics are available from the Census Bureau for the year 2000. The data is publicly downloadable via http://www2.census.gov/census_2000/datasets/demographic_profile/ (last accessed May 2012).

Table 3.7: Characteristics by SIC Classification (1997).[1]

Variable Name	SIC 20: Food and Kindred Products	SIC 26: Paper and Allied Products	SIC 27: Printing and Publishing	SIC 28: Chemicals and Allied Products	SIC 32: Stone, Clay, and Glass Products	SIC 33 Primary Metal Industries	SIC 34 Fabricated Metal Products
Total Value of Shipments	421,737	150,296	97,485	415,617	86,465	168,117	242,813
Production Workers' Payroll	25,185	15,187	16,336	19,192	11,319	17,355	36,252
Total # Employees	1.5	0.6	0.8	0.9	0.5	0.6	1.8
Average # Production Workers (% of total)	1.1 (73%)	0.4 (67%)	0.6 (73%)	0.5 (56%)	0.4 (80%)	0.5 (83%)	1.3 (72%)
Hours Worked By Production Workers	2,234	936	1,164	1,069	797	1,025	2,675
Cost of Materials	258,537	80,189	39,220	192,200	37,072	99,343	110,068
Single Unit	75%	53%	92%	59%	64%	63%	88%
Productivity (in dollars)	188.76	160.61	83.75	388.80	108.39	164.07	90.77
Wages (in dollars)	11.27	16.23	14.03	17.95	14.19	16.94	13.55

[1] All data are reported in millions, unless otherwise noted. All data are from publicly available COM files for the selected manufacturing industries for the year 1997. The data is publicly downloadable via http://factfinder.census.gov/home/saff/main.html?_lang=en (last accessed Nov. 2011).



*Note: Alaska and Hawaii are both State Plan OSHA States

Figure 3.1: Distribution of Federal OSHA States.

Chapter 4

Results

As described in Section 3.1 of Chapter 3, the period 1978 to 1998 is the period where the rules governing inspections make OSHA programmed inspections a plausibly exogenous instrument affecting plant-level risks. In order to test this assumption empirically, a non-parametric matching estimator is used to test whether OSHA is truly randomly choosing plants for inspection. Section 4.1 presents the estimator and results suggest that OSHA inspections are randomly assigned within high-risk industries during the period 1978 to 1998, except during the records-review-only period from 1982-1987.

In addition to being a valid instrument, OSHA inspections must be significantly correlated with plant-level risks in order to be considered a relevant instrument. Section 4.2 uses difference-in-differences (DID) estimators to provide evidence that plants receiving OSHA violations experience significant declines in their fatality rates and serious accident rates post-violation citation. F-tests of the null hypothesis that receiving an OSHA violation has no significant impact on plant-level safety are soundly rejected in all specifications. As a result, OSHA inspections appear to be a valid and relevant instrument for jobsite risks.

In order to test for compensating wage differentials for job-site risk, Section 4.3 presents DID estimators comparing wages of inspected plants to uninspected plants. The treatment effect estimated in this analysis is the causal average effect of OSHA inspections on plant-level wages, and provides strong causal evidence on the existence of compensating wages for risks. However, it should be noted that the treatment group contains both violators

whose safety improves after inspection, and compliers whose safety is unchanged following an OSHA inspection. To better isolate the impact of safety (risk) changes post OSHA inspection on wages, Section 4.3 reports results from a DID estimator comparing wages of violators and compliers post-violation citation. In this analysis, the randomization is lost because random inspection assignment does not imply random assignment of violations. However, common trend tests find no evidence of differential changes in wages between violators and compliers prior to receiving an OSHA violation, therefore suggesting DID estimators are sufficiently controlling for relevant confounding variables.

Finally, in Section 4.4, the compensating wage differentials associated with an OSHA violation (Section 4.3) are combined with the improvements in plant-level safety following an OSHA violation (Section 4.2) to estimate the value of a statistical life (VSL) and the value of a statistical serious accident (VSSA) which are compared to existing estimates.

4.1 OSHA Inspections: A Valid Instrument for Plant-Level Risk

During the period 1978 to 1998, OSHA programmed inspections are considered as a plausibly exogenous instrument affecting plant-level risks. To determine if the data support this assumption, it is necessary to test whether OSHA followed stated procedures and the assignment of inspections to plants was indeed random, conditional on the state, industry, and recent inspection history of each plant. Following OSHA guidelines regarding what type of plants would not be considered for inspection, plants with fewer than eleven employees or which received a comprehensive inspection in the recent past are deleted from the analysis. The timeframe defining "recent past is given by OSHA's Scheduling System for Programmed Inspections (OSHA, 1981, 1990, and 1995, see also Ch. 3, Section 3.1.1).

The following nonparametric matching estimator is used to test for statistically significant differences between the covariates of inspected and uninspected plants:

$$\Delta = E[(X_t^I | industry_i, state_s) - E(X_t^{UI} | industry_i, state_s)], \quad (4.1)$$

where, covariate *X* in time *t* for an inspected plant (*I*) in industry *i* in state *s* is compared to the average value of covariate *X* in time *t* for all uninspected plants (*UI*) in industry *i* and state *s*. The test statistic, Δ, is the mean difference among differences. The covariates are considered balanced at time *t* if the test statistic is not significantly different than zero at the 10% level.

All observed establishment characteristics listed in Table 3.5 are tested for balance between inspected and non-inspected groups. Inspections are tracked on an annual basis, while the COM occurs once every five years. As a result, balance is tested in the COM variables for inspections occurring in each of the five years prior to the subsequent COM wave. These variables are total value shipments, production workers payroll, average # production workers, total # employees, other # employees, hours worked by production workers, cost of materials, single unit, turnover, productivity, other employees salary, wages, and total compensation. For instance, if the COM data was collected in the 1992 wave, balance tests are conducted on the 1992 COM data assigning plants to inspected or uninspected groups based on whether or not they received a programmed inspection in 1992. Balance is also tested in the 1992 COM data assigning plants to inspected or uninspected groups based on whether or not they received a programmed inspection in 1993, 1994, 1995, or 1996. Thus, each wave of the COM data is tested for balance five times using inspection status in each of five years.

The results of these balance tests are presented in Table 4.1. Covariates for which the null hypothesis is rejected at the 10% level are listed directly in the table. Table 4.1 highlights the impact of the "records-review-only" policy at OSHA from 1982-1987. In these years, many covariates fail to balance. It is worth noting that during 1982 to 1985, the fewest variables achieve balance. This is also the time period associated with the greatest number of records-review-only inspections. Prior to 1982 and post 1987, however, the vast majority of covariates balance, supporting the randomization assumption during these time periods. Overall, the results strongly suggest that federally programmed OSHA inspections are randomly assigned during the 20 year period of focus, except 1982-1987 when the records-review-only scheduling was in force.

4.2 OSHA Inspections: A Relevant Instrument for Plant-Level Risk

In addition to being exogenous, OSHA inspections must affect plant-level injury rates to be a relevant instrument. This is expected to be the case because post-inspection, all found violations must be corrected and plants are often re-inspected to ensure corrections have been made. OSHA's assignment of violations is therefore the impetus for plant-level safety improvements following inspection. This supposition is borne out by a number of studies that document plant-level safety improvements after inspection (e.g. Scholz & Gray, 1990; Scholz & Gray, 1993; Gray & Mendeloff, 2004).[27] To test for this empirically, a difference-in-differences (DID) estimator is employed that compares fatality and serious accident rates between inspected plants that receive an OSHA violation and inspected plants that are found

[27] In a similar vein Viscusi (1986) provides early evidence that OSHA inspections reduce fatality rates at the industry level, although the effect at the aggregated industry level is much smaller in magnitude.

to be in compliance with OSHA standards (i.e., receive no violations).[28] The analysis focuses only on inspected plants because fatality and serious accident rates are not observable for uninspected plants. Fatality and serious accident rates are expected to fall post OSHA violation citation if violators are indeed coming into compliance with OSHA standards and improving safety at their jobsite.

The analysis is limited to only plants receiving randomly assigned programmed inspections and/or unprogrammed accident inspections. The analysis focuses on programmed inspections because these are the inspections that were established as a valid exogenous instrument in the previous section. Unprogrammed accident inspections are included in the analysis because these inspections contain the counts of worker fatalities and serious accidents that are necessary for constructing plant-level fatality rates and serious accident rates. However, a plant may have a serious accident, but have no violations upon inspection after the accident, and this is the case approximately 35% of the time.

The DID estimator is given by:

$$fatrate_{j,t} = a + PC_{j,t}\beta + T_t + P_j + \varphi V_{j,t} + \epsilon_{j,t}, \quad (4.2)$$

$$accrate_{j,t} = a + PC_{j,t}\beta + T_t + P_j + \pi V_{j,t} + \epsilon_{j,t}, \quad (4.3)$$

where, the fatality rate (*fatrate*) or serious accident rate (*accrate*) at plant j in time t is a function of a vector of observable time-varying plant characteristics ($PC_{j,t}$), time and plant

[28] Recall from Chapter 3 that OSHA defines a serious accident as one that results in a worker fatality or multiple worker hospitalizations. Serious accidents automatically trigger an OSHA accident inspection in which compliance safety and health officers determine the cause of the accident and whether the plant was in violation of any OSHA standards contributing to the accident's occurrence.

fixed-effects, T_t and P_j, respectively, and an indicator variable ($V_{j,t}$) equal to one if the plant has received a violation in year *t* or any year prior in the panel.

Equations (4.2) and (4.3) are estimated using data pooled from three consecutive waves of COM data, supplemented by data from the Annual Survey of Manufactures (ASM) for the years 1987 to 1997. The ASM supplements the COM and collects the same core data in off-COM years for large firms with greater than 1,000 employees and a probability sample of remaining firms based on firm size and contribution to total industry value of shipments.[29] The ASM data are included in the analysis because fatalities and serious accidents are inherently rare events. Therefore, failure to include the ASM data results in a considerable loss of information on annual fatality rates from 1987 to 1997.

Figure 4.1 exhibits the adjustment process for plant-level fatality rates and accident rates following an OSHA inspection and highlights the average treatment effect on the treated (ATT) that is estimated by the coefficients φ and π in equations (4.2) and (4.3). Violators are presumed to have a higher fatality rate prior to any inspection, and these rates are presumed to be declining in general for both violators and compliers over time, as has been the case since OSHA's inception (Kniesner & Leeth, 1995). In order to precisely estimate the ATT for plants receiving an OSHA violation observations of known violators during the transition year that violation citations are conferred are dropped. The constructed

[29] Approximately 1/7 of all manufacturing establishments are surveyed in the ASM. The Census Bureau does not fully disclose the sampling method for including firms in the ASM. Additional details are available at:
http://www.census.gov/manufacturing/asm/how_the_data_are_collected/index.html

panel therefore consists only of plants that receive their first OSHA violation during the time period 1988-1996. This results in a maximum of 51,447 observations available for analysis.

Results from the DID estimates are presented in Table 4.2. The first three columns contain estimation results for the seven carefully matched industries, and the last three columns contain results for all industries, including those industries that were not manually checked and thus include only plants for which the matching algorithm matched COM and OSHA entries for the plant. Table 4.2 also presents three alternative estimation strategies for estimating the effects of OSHA violations on plant-level fatality and serious accident rates. The first is an un-weighted regression as presented in equations (4.2) and (4.3). The second weights each observation in the regression by the square root of the number of production workers at the plant in order to adjust for the fact that fatality rates are more precisely measured at worksites with a larger number of production workers. The underlying assumption of the weighted regression models is that the variance of the error term ($\epsilon_{j,t}$) in equations (4.2) and (4.3) is inversely proportional to the number of production workers over which the fatality and accident rates are constructed. If this assumption is satisfied the weighted regression estimates that correct for the production worker dependent heteroskedasticity are more efficient than the un-weighted regression estimates (see, for example, King, 1997, and Lewis and Linzer, 2005). Finally, as an alternative to the weighted regressions, the un-weighted regressions are estimated on a subsample of the data dropping observations at plants with fewer than 15 production workers.[30]

[30] Similar regressions were estimated on samples dropping plants with between 1 and 25 production workers. The estimated impact of OSHA inspections on plant-level fatality rates

As indicated in Table 4.2, there is consistent evidence of reductions in plant-level serious accident and fatality rates after plants receive a violation citation by OSHA (see coefficient for "Violations Found", highlighted in bold type). This effect is significant across all samples and estimation strategies. Note that, there is a larger estimated reduction in both fatality and serious accident rates in the seven industry sample as compared to the sample containing all manufacturing industries. Recall from the previous chapter that the seven carefully matched industries are those which have relatively higher fatality rates, so we would expect to see the larger impact of OSHA violations in these industries. Finally, the estimated coefficients from the weighted regressions and the samples dropping 15 or fewer production workers are very similar, and they are both smaller than the un-weighted results from the sample that includes plants with 15 or fewer production workers. The 95% confidence intervals for each comparable coefficient do not overlap for the fatality rate regressions, but they do overlap in the accident rate regressions (i.e., the 95% confidence intervals for the 7 industries weighted, un-weighted, and >15 workers models all overlap in the accident rate regressions but do not overlap in the fatality rate regressions).

Focusing on the results from the samples dropping 15 or fewer production workers, the coefficient estimates indicate that receiving an OSHA violation citation reduces the number of workers involved in a serious accident by 5.3 in 10,000 workers across all manufacturing industries. This effect is 41% larger for the seven industry sample, with an OSHA violation citation reducing the number of workers involved in a serious accident by

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and serious accident rates decreases sharply when plants with fewer than 3 or 4 production workers are included in the regression. The estimated ATT then remains stable over the models estimated that include plants with between 5 and 25 production workers.

7.5 workers in 10,000, post-violation citation. On average, there were 15.2 workers per 10,000 involved in a serious accident in the sample containing all industries, and 17.3 workers per 10,000 involved in a serious accident in the seven industry sample. Thus, these estimates indicate a substantial reduction in the accident rates of between 35% and 43%.[31]

Considering only fatalities, Panel B indicates that receiving an OSHA violation reduces the number of fatalities by 3.3 in 10,000 workers in the panel covering all industries. Similar to the serious accident models, the impact of an OSHA violation citation is larger in the seven industry panel, and is estimated to reduce the number of fatalities by 4.4 workers in 10,000. On average, there are 5.0 workers per 10,000 involved in a fatality in the sample covering all manufacturing industries, and 6.1 workers per 10,000 involved in a fatality in the seven industry sample. Thus, the estimates indicate a reduction in the fatality rates, on average, of between 66% and 72%. Aside from the unobserved plant and time fixed effects, there are generally no other factors that explain changes in a plant's fatality or serious accident rates other than receiving an OSHA violation citation.

Finally, Table 4.2 reports F-statistics that compare the unrestricted models reported in Table 4.2 with restricted models that omit the violation citation indicator variable. Staiger and Stock (1997) suggest a cutoff threshold of ten for the F-statistic as a rule of thumb for determining a strong instrument (see also Bound et al., 1995). Overall, the F-tests suggest

[31] Note, the average serious accident rate of 17.3 workers per 10,000 is based on publicly available OSHA data for the seven SIC industries using only the inspections that were conducted from 1987 to 1997. During an inspection OSHA records the number of employees at an establishment along with a count of serious accidents. A more precise measure of accident rates for my sample of plants that includes plants inspected prior to 1987 who have never received an OSHA violation will be available upon confidentiality disclosure review when this work is submitted for journal publication.

the null hypothesis that receiving a violation citation has no significant effect on plant-level accident and fatality rates can be rejected. Furthermore, the F-statistics in the fatality and accident rate equations are nearly all greater than ten, suggesting receiving an OSHA violation is a valid, strong instrument.

4.3 Compensating Wage Differentials for Risky Working Conditions

The key features of the OSHA inspection process that are exploited in this research are the randomized selection process for inspections and the highly visible nature of the inspection process to workers that result in subsequent safety improvements. Given these elements, it is possible to explore the existence and magnitude of compensating wages for dangerous working conditions using two approaches. First, in Section 4.3.1, wages at plants receiving programmed inspections in year *t* are compared to wages at plants that have never been inspected as of *t*. Here, the treatment is simply receiving an OSHA inspection and wages at plants that are inspected are expected to be lower than at uninspected plants due to the required improvements in safety conditions post-inspection. While this analysis relies on OSHA randomized inspections to instrument for plant-level safety and thus provides a clear identification strategy, the estimated compensating differentials are confounded by the treatment group containing both violators and compliers. Thus, in Section 4.3.2, a second analysis is employed that focuses on only inspected plants that allows separation of risky and safe plants to more precisely estimate the magnitude of compensating wages for riskier working conditions.

4.3.1 Comparison of Wages at Inspected and Uninspected Plants

This analysis focuses on the randomization of inspections 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. Evidence by Scholz and Gray (1990 and 1993) and Gray and Mendeloff (2004) and the analysis in Section 4.2 suggests this is a valid assumption. The control group to which inspected plants are compared is all uninspected plants that contain both unsafe plants (violators) and safe plants (compliers) in unknown proportions. 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 be lower than a control group that consists of both violators and compliers. While the relative frequencies of violators and compliers in the control group cannot be known with certainty, it can be estimated by the observed relative frequency of violators and compliers in the treatment group. Given an estimate of this frequency, an estimate of the compensating wage differential between violating and complying plants can be derived.

Figure 4.2 illustrates the wage transition dynamics for plants receiving an OSHA inspection, and highlights the ATT in this application, which is the compensating wage differential for risk. Inspected and uninspected plants are presumed to have similar average wage rates prior to the inspection date and nominal wages are rising over time for both the inspected and uninspected plants. Figure 4.2 also illustrates that post-inspection, the wages of inspected plants increase less than the wages of uninspected plants, and the difference in wages post-inspection is the compensating wage differential for risk. Implicitly, figure 4.2

assumes that labor markets are in equilibrium prior to inspection and workers are aware of the risk levels at their place of employment. It is possible that workers are not fully aware of their job risks prior to inspection in which case an OSHA inspection may reveal new safety information to workers and create frictions in the wage adjustment process. The empirical analysis that follows addresses this threat to validity by dropping observations during the year of inspection (see footnote 33). Recall from chapter 3 that OSHA violations are required to be corrected within 30 days of violation assignment, so dropping observations during the year of inspection should allow sufficient time for wage adjustments to occur. In addition, 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 DID models are given as follows:

$$wage_{j,t} = a + PC_{j,t}\beta + T_t + P_j + \delta_1 FPI_{j,t} + \delta_2 AI_{j,t} + \delta_3 CI_{j,t} + \delta_5 OI_{j,t} + \epsilon_{j,t}, \quad (4.4)$$

$$total\ compensation_{j,t} = a + PC_{j,t}\beta + T_t + P_j + \delta_1 FPI_{j,t} + \delta_2 AI_{j,t} + \delta_3 CI_{j,t} + \delta_5 OI_{j,t} + \epsilon_{j,t}, \quad (4.5)$$

where the average hourly wage rate (*wage*) and the average yearly total compensation (*total compensation*) of production workers at plant j at time t are a function of a vector of five observable time varying plant characteristics ($PC_{j,t}$) including *Turnover*, *Productivity, Total # Employees*, *Cost of Materials*, *Avg. # Production Workers*, and *Single Unit* (see Table 3.5 for definitions); year (T_t) and plant-level (P_j) fixed effects. Total average annual

compensation is the average yearly salary plus fringe benefits. $FPI_{j,t}$ is a dummy variable equal to one for plants receiving their first federal programmed inspection in year *t* and each year subsequent to *t*. As such, δ_1 is the key coefficient of interest that identifies the impact of receiving a federal programmed inspection on wages or total compensation as represented in Figure 4.2 by the ATT. In other words, δ_1 is the estimate of the compensating differential associated with plant-level safety changes resulting from OSHA inspections. It is expected that δ_1 will be negative, indicating that improvements in safety at inspected plants lead to reductions in compensation.

Also included in equations (4.4) and (4.5) are dummy variables indicating whether or not the plant has also received any other type of inspection at time *t* including an accident inspection (*AI*), complaint inspection (*CI*), or "other" inspection as classified by OSHA (*OI*).[32] These inspection dummy variables equal one in the year the plant receives its first inspection and every subsequent year. The inspections that are not federally programmed are not randomly assigned, but they are important to include in the analysis because they likely result in plant-level safety improvements and do not preclude a programmed inspection from occurring. Finally, $\epsilon_{j,t}$ is a random error term clustered at the plant-level to allow for inter-temporal correlation within clusters.

Equations (4.4) and (4.5) are estimated using panels of identical length to those used for estimating equations (4.2) and (4.3) and thus include three consecutive COM waves from 1987 to 1997. As before, the COM data are supplemented by the ASM, which collects wage

[32] Other Inspections include programmed related, unprogrammed related, follow-up and monitoring inspections to ensure safety improvements have taken place after a violation citation is issued.

and plant-level data annually. Results are robust to inclusion or exclusion of the ASM data. These 11-year increments allow sufficient time for safety improvements and compensation adjustments to occur post-inspection, but are also narrow enough to only cover the time frame when OSHA was randomly selecting plants for programmed inspections. This gives more confidence that the plant-level fixed effects are effective in capturing time-invariant unobservable plant characteristics. Finally, the models are only estimated using the seven industries that are carefully matched. This yields 68,443 observations available for analysis.

Results from the estimation of equations (4.4) and (4.5) are presented in Table 4.3 for the 1987-1997. The estimated coefficient on federal programmed inspections is negative as expected, and is statistically significant at the 99% confidence level in both models. As indicated in Table 4.3, for plants receiving their first inspection during 1987-1997, average hourly wages at inspected plants were $0.28 lower post-inspection, or $560 per year assuming workers work an average of 2,000 hours per year.[33] This represents a 2.8% reduction in the average 1997 wages for the seven industries analyzed. The total yearly compensation paid to production workers, including fringe benefits (see Table 3.5), at inspected plants was $944 lower post-inspection.

Comparing these two models, it appears that 60% of the change in total yearly compensation after an OSHA inspection is due to reductions in the wage rate, and the other

[33] In order to allow sufficient time for wage adjustments to occur I drop transitional observations in which the census year is equal to the year of inspection. This is the same approach that was used to estimate the fatality rate and serious accident rate equations (4.2) and (4.3). As a result, observations at establishments receiving an OSHA inspection in 1987 or 1997 are dropped from the sample.

40% can attributed to reductions in employer contributions to production workers' fringe benefits.

The inclusion of fringe benefits in this analysis is an important contribution relative to the existing VSL literature. Due to data limitations, previous hedonic wage studies have generally not included fringe benefits in the analysis, although a few authors have controlled for the effects of workers' compensation on the equilibrium wage rate (see, for example, Moore and Viscusi, 1990, Kniesner and Leeth, 1991, and Viscusi, 2004). The average total yearly compensation variable used in equation (4.5) includes employers' legally required fringe benefits such as workers compensation insurance payments, as well as employers' voluntary fringe benefits such as 401k contributions. Additional results not reported in Table 4.3, suggest the reductions in production workers' fringe benefits following an OSHA inspection are entirely driven by reductions in the voluntary fringe benefit contributions by employers.[34] The results here clearly indicate that changes in the fringe benefit packages offered by employers account for a significant portion of the total estimated compensating differential for job risk.

The estimated sign and significance for the other explanatory variables reported in Table 4.3 generally follow the same trend in both the wage and total compensation equations.

[34] Up until the year 1997, but not including 1997, the COM collected data on voluntary and legally required fringe benefit contributions separately. From 1997 onward the COM only collects data for the total fringe benefit contributions (legally required plus voluntary). All observations from the year 1997 were therefore dropped from the analysis for the purpose of estimating the impacts of OSHA inspections on legally mandated and voluntary fringe benefit contributions separately. These results are not reported, but they indicate a statistically significant negative impact of OSHA inspections on voluntary contributions and find no significant impact on mandatory contributions.

Aside from complaint inspections, none of the other inspection classifications have a significant impact on wages or fringe benefits. Complaint inspections have a negative impact, and the effect is similar in magnitude to the estimated impact of federal programmed inspections. Higher production worker turnover rates are associated with higher wages and total compensation. As one would expect, the coefficient on worker productivity is positive indicating that more productive workers receive higher wages, but the effect is only significant in the wage equation at the 10% level.

The variables controlling for plant size have differing impacts on production workers' total compensation. An increase of 1,000 total employees is associated with a $2,897.40 reduction in annual compensation. On the other hand, a $1 million increase in cost of materials is associated with a $147.30 increase in annual compensation. The estimated coefficient for production workers is significant and negative in both the wage equation and the total compensation equation. An increase of 1,000 production workers is associated with a $2.26 reduction in average wages, and a $8,960.20 reduction in total yearly compensation respectively. Interestingly, the negative estimated coefficients on total employees and average number of production workers seem to contradict traditional empirical findings that suggest large firms pay a wage premium in comparison to small firms (see Oi and Idson, 1999 for a detailed review). This peculiarity may be due to the fact that the estimation strategy employed in equations (4.4) and (4.5) makes use of panel data and controls for plant-level fixed effects. All identification is therefore coming from within unit changes in the number of employees. The results are therefore consistent with classical theory of labor market demand where workers are paid their marginal revenue product of labor. Intuitively,

marginal productivity declines with the employment of additional labor, and thus, in the converse case of reduced employment at the plant we expect higher marginal productivity which would be associated with higher wages.

The estimated impact of an inspection on wages is only consistent if wages of inspected plants followed a common trend with the wages of non-inspected plants prior to inspection. More formally, let D_T be an indicator variable that is equal to one if a plant is inspected in time *T*, and equal to zero otherwise. Assuming a simple two period model where some plants are inspected at time *T*, and no plants are inspected prior to *T*. Then, the common trend assumption can be written as:

$$E(wage_T^{IP}|D_T = 0) - E(wage_{T-1}^{IP}|D_{T-1} = 0) =$$
$$E(wage_T^{NIP}|D_T = 0) - E(wage_{T-1}^{NIP}|D_{T-1} = 0), \quad (4.6)$$

where D_T = 1 for inspected plants, post-inspection and equals zero otherwise; $E(wage_T^{IP}|D_T = 0)$ is the unobserved counterfactual expected wage rate of the inspected plant if the inspected plant were actually not inspected, and $E(wage_{T-1}^{IP}|D_{T-1} = 0)$ is the expected wage rate of the inspected plant prior to receiving an OSHA inspection in period *T-1*. Likewise, $E(wage_T^{NIP}|D_T = 0)$ is the expected wage rate of the non-inspected plant during the post-inspection period, and $E(wage_{T-1}^{NIP}|D_{T-1} = 0)$ is the expected wage rate of the non-inspected plant during the pre-inspection time period.

To explore the validity of the common trend assumption the following equations are estimated:

$$wage_{j,t} = a + PC_{j,t}\beta + T_t + P_j + \delta_1' FPI_{j,t+10}$$
$$+ \delta_2' AI_{j,t+10} + \delta_3' CI_{j,t+10} + \delta_5' OPI_{j,t+10} + \epsilon_{j,t}, \quad (4.7)$$

$$total\ compensation_{j,t} = a + PC_{j,t}\beta + T_t + P_j + \delta_1' FPI_{j,t+10}$$
$$+ \delta_2' AI_{j,t+10} + \delta_3' CI_{j,t+10} + \delta_5' OPI_{j,t+10} + \epsilon_{j,t}, \quad (4.8)$$

where all variables are as defined for equations (4.4) and (4.5), but now wages of plant j in time t are regressed on inspection status of the plant in time t+10. A plant's inspection status at time t+10 is used as a "pseudo" treatment variable in a comparison of wages. Thus, for example, the 1977-1987 panel of wages is regressed on plant characteristics in 1977-1987 and on inspection status as defined by the 1987-1997 panel. If there is a common trend in wages among inspected and uninspected plants, we expect δ_1' to be statistically indistinguishable from zero.

Results from the estimation of equations (4.7) and (4.8) are presented in Table 4.4. The "pseudo" treatment variable for federally programmed OSHA inspections is not statistically significant at the 55% level in either equation, failing to reject the common trend assumption for inspections occurring between 1987 and 1997. Also, the other covariates in Table 4.4 generally have the same sign as the estimated coefficients from equations (4.4) and (4.5), but the magnitudes and statistical significance are slightly different. Worker productivity and single unit classification, for example, have a much larger highly significant impact on wages and total compensation during the 1977-1987 period as opposed to the 1987-1997 period.

In addition to adjustments in production workers' pay it is also possible that companies may make adjustments along other margins in response to an OSHA inspection. Appendix A provides a detailed overview of the effects of an OSHA inspection when labor markets are perfectly competitive and when plants have some degree of market power. In

general, if a plant's demand for labor is negatively impacted by an OSHA inspection and the plant has some market power in the labor market, then the estimated impact of an OSHA inspection on plant-level wages will *over-state* the true compensating wage differential for risks. Given this possibility, Appendix A also includes empirical tests for the impact of an OSHA inspection on the number of production workers, other employees, and production worker productivity. As reported in Table A.1 of Appendix A, OSHA inspections have no statistically significant impact on plant-level employment levels or production worker productivity. Although insignificant, the estimated signs on the coefficients for number of production workers and other employees are both positive, which is consistent with a scenario where labor demand is not negatively impacted by an OSHA inspection. Furthermore, recall that the number of employees and production worker productivity are directly controlled for in the wage and total compensation equations (4.4) and (4.5). As a result, it is not likely that the estimated compensating differentials for risk following an OSHA inspection that were estimated in equations (4.4) and (4.5) are biased.

In sum, the quasi-experimental results taken as whole appear to provide strong causal evidence for the existence of compensating wages for riskier working conditions. However, as noted earlier, the change in production worker compensation following an OSHA inspection cannot directly be linked to a change in plant-level risks since fatality and serious accident counts are only available for inspected plants. As such the estimated compensating wage differential cannot be linked to an average risk change and thus cannot be used to compute VSL estimates. In addition, the treatment group of inspected plants contains both violators (whose safety are improving) and compliers (whose safety are unchanged). As a

result, the estimated coefficients δ_1 in equations (4.4) and (4.5) are unbiased estimates of the causal effect of an OSHA inspection, but they are downward biased estimates of the more interesting compensating wage differential for working at a riskier plant (as evidenced by being in violation of OSHA standards) as opposed to a plant in compliance with OSHA standards. The next analysis addresses both these issues by focusing only on inspected plants for whom violation status is known and risk data is available allowing for the computation of a VSL.

4.3.2 Comparison of Wages at "Known Violators" and "Known Compliers"

In this section, a more precise measure of compensating wages for risk is estimated by only focusing on comparisons among the manufacturing plants that are inspected by OSHA. Specifically, wage differentials between plants with found safety violations (referred to as "known violators") and plants with no found violations ("known compliers") are estimated. In this analysis, the randomization inherent in the OSHA inspection process is lost because violations are not randomly assigned. However, the tradeoff is that we gain the ability to identify plants known to be safe from those known to have less safe working conditions. In addition, post-inspection, information on plant-level accident and fatality rates are known, which allows me to directly link wage differentials to known changes in risks post-inspection and thus compute VSL measures.

The DID estimator employed compares wages at known violators to wages at known compliers before their inspection dates (i.e. before any safety interventions from OSHA) and after their inspection dates (i.e. after violators are assumed to improve their plant's safety level) in order to estimate the magnitude of the compensating wage differentials for working

at the dangerous plants. The compensating wage differential is labeled as the average treatment effect on the treated (ATT) in Figure 4.3. Nominal wages are assumed to be rising over time for violators and compliers in the graph (an assumption supported by the data). Violators are assumed to have higher wages than compliers prior to OSHA inspection and any mandated safety improvements. As opposed to the inspected/uninspected comparison in Figure 4.2, the compensating wage differential between violators and compliers is estimated based on pre-treatment pay differentials between the two groups. The wages at violating and complying plants are presumed to share a common trend prior to inspection, which is a necessary requirement for unbiased estimates from the DID estimator.

Specifically, the DID models are given by:

$$wage_{j,t} = a + PC_{j,t}\beta + T_t + P_j + \rho V_{j,t} + \epsilon_{j,t}, \quad (4.9)$$

$$total\ compensation_{j,t} = a + PC_{j,t}\beta + T_t + P_j + \rho V_{j,t} + \epsilon_{j,t}, \quad (4.10)$$

where $V_{j,t}$ is a dummy variable equal to one if the plant has received a violation in year *t* or any year prior in the panel, and all other variables are as described for equations 4.4 and 4.5. Estimates of the coefficient ρ in equations 4.9 and 4.10 provide point estimates of the ATT, the average impact of being found in violation of an OSHA standard on wages and total compensation.

Panels are once again limited to a 10-year increment from 1987 to 1997 containing three consecutive COM waves supplemented by ASM data for the non-census years.[35] Observations for violators who received their first violation prior to the first year of each

[35] For this comparison, removing the ASM observations and estimating a first difference model yields similar estimates of the impact of an OSHA violation.

panel are also dropped since safety improvements would have occurred prior to the first wave of the data. Results are presented in Table 4.5 for the same samples used to estimate the effects of OSHA violations on plant-level fatality and serious accident rates in Section 4.2. The first two columns in Table 4.5 are for the seven carefully matched industries. Column one reports results for the sample covering all establishments, and column two reports results for the sample that only contains establishments with more than 15 production workers. Columns three and four follow the same pattern, but report results for a sample covering all manufacturing industries.

Results indicate a negative and statistically significant impact of receiving an OSHA violation on the plant-level wage rate and total compensation in the samples containing the seven carefully matched industries as well as the full sample of all manufacturing industries. The impacts of receiving an OSHA violation are robust to the exclusion of small plants with fewer than 15 production workers. During the period 1987-1997, receiving an OSHA violation is associated with a $0.25 to $0.30 (1987$) reduction in the wage rate post-violation, or 2.5% to 3.0% of average hourly wages in 1997 for plants in the seven industry sample.[36] A similar impact is found in the full sample where receiving an OSHA violation is associated with a $0.31 to $0.33 reduction in the wage rate post-violation (see Panel A, Table 4.5). Importantly, receiving an OSHA violation is associated with an $819 to $954 and $1,002 to $1,082 reduction in total compensation in the seven industry and all manufacturing samples, respectively. Similar to the results from the previous section comparing inspected

[36] On average across all samples, the wage premium associated with working at risky plants is $0.30 per hour (1987$), or 3.0% of the average 1997 hourly wage of production workers.

to uninspected plants changes in the wage rate account for roughly 60 - 63% of the total reduction in total compensation, with the remaining 37-40% due to reductions in employers' fringe benefit contributions. As before, a reduction in total compensation post-violation citation is expected since plants are required to make safety-improvements after receiving a violation citation. Thus, one can alternatively state the results as indicating that workers receive a $819 to $1,082 increase in total yearly compensation in exchange for working at plants with riskier working conditions as proxied by having OSHA safety violations present at the location.

The DID analysis in this Section Again relies on the common trend assumption. Similar to the previous section comparing inspected and uninspected plants, tests of the common trend assumption for the violator/complier comparison are conducted using DID models with pseudo-treatment assignment as follows:

$$wage_{j,t} = a + PC_{j,t}\beta + T_t + P_j + \rho' V_{j,t+10} + \epsilon_{j,t}, \quad (4.11)$$

$$total\ compensation_{j,t} = a + PC_{j,t}\beta + T_t + P_j + \rho' V_{j,t+10} + \epsilon_{j,t}, \quad (4.12)$$

where all variables are as defined for equations (4.7) and (4.8), but now wages of plant j in time t are regressed on violation status at time t+10 ($V_{j,t+10}$), which is the pseudo treatment variable.

Results from the estimation of equations (4.11) and (4.12) are provided in Table 4.6 where the common trend tests are conducted separately for the seven carefully matched

industries and sample containing all manufacturing industries.[37] Similar to the results in Section 4.3.1, the "pseudo" treatment variable for OSHA violations is not statistically significant at the 16% level and 25% level in the wage and total compensation equations, respectively. As a result, there is again a failure to reject the common trend assumption for inspections occurring between 1987 and 1997. Also, the other covariates in Table 4.6 generally have the same sign and magnitude as the estimates reported in Table 4.5. Worker productivity has a larger impact on plant-level wages and compensation during the 1977-1987 period. Plant size, as proxied by the total number of employees, has a positive impact during 1977 to 1987 as opposed to the negative estimated impact in Table 4.5.

4.4 Measuring the Value of Reducing Workplace Risks

In the comparison of known violators and compliers just presented, the analysis relies on risk changes at the plant level thereby addressing measurement error concerns of traditional hedonic wage applications that rely on national averages. Furthermore, the tests for common trend prior to receiving a violation offers evidence that any relevant differences between violators and compliers prior to inspection for the years 1987-1997 are accounted for through plant fixed effects and time-varying covariates included in the model. The decline in total compensation of violators in comparison to total compensation of compliers subsequent to inspection therefore is taken to be a consistent estimate of the magnitude of compensating payments required for increased risk of injury and death due to working at a violating plant as compared to a complying plant. The estimated compensating payments are

[37] A test for common trend was also conducted on the samples of plants with more than 15 production workers. The results are similar to those presented in Table 4.6, and also support the hypothesis of a common trend prior to receiving an OSHA violation.

clear measures of willingness-to-pay resulting from mandated improvements in job safety (see Viscusi and Huber (2012) for a discussion of the divergence between willingness-to-pay and willingness-to-accept). This compensating pay differential can be combined with the point estimates of the changes in plant-level fatality and accident rates resulting from receiving an OSHA violation citation to calculate the VSL as well as the value of avoiding a serious accident, or the "value of statistical serious accident" (VSSA).

Specifically, the average change in total compensation associated with safety improvements as estimated by ρ in equation (4.10) is combined with the average change in plant-level fatality and accident rates as estimated by φ and π in equations (4.2) and (4.3), respectively, as follows:

$$VSL = {}^{\rho * 10{,}000}/_{\varphi}\,, \tag{4.13}$$

$$VSSA = {}^{\rho * 10{,}000}/_{\pi}\,, \tag{4.14}$$

where the yearly average willingness to pay (ρ) is scaled by 10,000 because the accident and fatality rates are standardized per 10,000 workers.

Table 4.7 presents the estimated VSL and VSSA for the 1987 to 1997 time period. Note, to distinguish between models that employ wages only as the dependent variable versus those employing total compensation as the depending variable, Table 4.7 reports coefficients subscripted as either ρ_{wages} or $\rho_{\text{totalcomp}}$. Accordingly, Table 4.7 also reports VSL estimates that are distinguished by whether they are estimated on models including wages

only (VSL^{wage}) or models including total compensation ($VSL^{totalcomp}$).[38] Although it is theoretically correct to include all sources of compensation for facing increased risk in the workplace when computing the VSL, estimates of VSL^{wage} are reported for ease of comparison to the extant hedonic wage literature.

Focusing first on the VSL estimates based on wages, the central VSL estimates reported in Table 4.7 come from the samples dropping plants with 15 or fewer production workers. The results from the sample covering all manufacturing industries indicate a VSL^{wage} estimate of $3.8 million measured in 2011 dollars (95% confidence interval of 1.5 million to 6.2 million). The range of VSL estimates from post 2000 hedonic wage research using CFOI fatality rates that vary by workers' industry and occupation is between $2.0 million in Kochi (2011) and $15.0 million in Kniesner et al. (2012) all measured in 2011 dollars. Excepting the $2.0 million estimate from Kochi (2011), the remaining range of post 2000 VSL estimates is much higher. For example, Viscusi (2004) and Kneisner et al. (2012) both use labor market data from the same time period as what my estimate is based upon and report VSLs between $6.6 to $15 million (2011$). Viscusi (2004) reports a $9.9 million VSL estimate for "blue collar" workers providing an estimate that is comparable to the VSL for production workers. With the exception of Kochi (2011), the VSL estimate of $3.8 million reported in Table 4.7 is lower than the range of post 2000 estimates, and is dramatically lower than the $9.9 million estimated by Viscusi (2004).

[38] The formula for VSL^{wage} is slightly different from the formula for $VSL^{totalcomp}$ given in equation (4.13). When calculating VSL^{wage} it is necessary to scale the estimated ρ from equation (4.9) by 2,000 in order to derive the average yearly willingness to pay assuming that workers on average work 2,000 hours per year.

Turning now to VSL estimates based on total compensation, the central estimate of the VSL for these years based on a sample that drops plants with 15 or fewer production workers is $3.7 million measured in 2011 dollars for the seven carefully matched industries (95% confidence interval of 0.7 million to 6.8 million). Likewise, the central VSL estimate for the sample of all manufacturing industries is $6.4 million measured in 2011 dollars (95% confidence interval of 3.0 million to 9.8 million).[39] Other estimates from the weighted regressions and alternative samples dropping plants with 5, 10 and 20 production workers results in a plausible estimated range of the VSL between $3.6 million and $5.0 million in the seven industry sample. Using the same methodology, the range of VSL estimates in the sample containing all manufacturing industries is between $5.0 million and $7.1 million. The VSL estimates based on total compensation are more similar to the existing hedonic wage literature and are somewhat lower than the $8.2 million (2011$) central point estimate of the VSL currently suggested by the U.S. Environmental Protection Agency (2010). The EPA's point estimate does, however, lie within the 95% confidence interval of the estimates reported here. Importantly, as indicated above, roughly 60% of the estimated VSL is attributed to changes in production workers' wages, and the other 40% is due to changes in fringe benefit payments.

The point estimates for the VSSA vary from $0.8 to $4.8 million in 2011 dollars. Typical estimates for the value of a statistical injury (VSI) have ranged between $25,000 and

[39] The VSL and VSSA are ratios of two OLS estimates. Instrumental variables estimates were used to calculate the robust standard errors necessary for constructing the 95% confidence intervals for the Wald estimators of the VSL and VSSA given in equations 4.13 and 4.14 respectively.

$93,000 (e.g. Viscusi and Hersch, 1990, Viscusi and Aldy, 2003, and Kuhn and Ruf, 2008). The VSSA estimates based on wages reported in Table 4.7 are considerably higher than the VSI range reported in the literature. This is not surprising, however because the VSSA computed in this research is based upon serious accidents wherein a worker was killed or multiple workers were hospitalized. The VSIs reported in the earlier literature are based on any type of injury, including minor injuries, where a worker loses a workday and thus reflect fundamentally different risks.

4.5 Conclusion

In this chapter, DID models were used to empirically explore the existence and magnitude of compensating wages for risky working conditions. First, OSHA inspections were empirically established as a valid and relevant instrument for risk. Non-parametric MEs indicate that OSHA is randomly choosing plants for inspection within high risk industries. Results from DID estimators relating fatality and serious accident rates to the receipt of an OSHA violation indicated that post-violation citation, plant-level safety improves.

Given the relevance of OSHA inspections as an instrument for plant-level risks, wages at inspected and uninspected plants are compared to test the theory of compensating wages for risk. Results indicate that wages of inspected plants fall in comparison to their uninspected counterparts post-inspection. However, the estimated impact of OSHA inspections on wages is confounded by the inclusion of compliers whose plant safety is unchanged in the group of inspected plants. Furthermore, OSHA only collects fatality and serious accident data for inspected plants making it impossible to compare the compensating

wage differential estimated from an OSHA inspection to a measurable change in plant-level safety.

The above two shortcomings are addressed in the second analysis which focuses only on inspected plants and compares violators to compliers. This comparison once again finds strong evidence of compensating wages for risky working conditions, and the estimated wage changes can now be connected to changes in plant-level fatality and accident rates to compute VSL and VSSA measures. Results indicate VSL point estimates of approximately $6.4 million when considering the total compensation received by workers. The VSL and VSSA estimates reported herein are placed in context of the existing literature, and the existing policy use of the VSL, in the concluding chapter of this dissertation.

The next chapter extends the models reported here to alternative estimating strategies for measuring compensating wages for risk that include non-parametric matching, propensity score matching, and regression discontinuity.

4.6 References

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4.7 Tables and Figures

Table 4.1: Randomization Tests for Federal Programmed Inspections.

COM Wave	Year of Inspection	Variables Failing to Balance[1]
1977	1977	*0*[2]
	1978	*0*
	1979	*0*
	1980	*Total # Employees, Average # Production Workers*
	1981	*0*
1982	1982	*Total # Employees, Cost of Materials, Single Unit, Productivity, Average # Production Workers*
	1983	*Single Unit*
	1984	*Total # Employees, Single Unit, Average # Production Workers*
	1985	*Total # Employees, Single Unit, Average # Production Workers*
	1986	*0*
1987	1987	*Single Unit*
	1988	*0*
	1989	*0*
	1990	*0*
	1991	*0*
1992	1992	*0*
	1993	*0*
	1994	*0*
	1995	*Single Unit*
	1996	*0*
1997	1997	*0*

[1] If a variable name is listed, there was a statistically significant difference between the means of inspected and non-inspected plants at the 10% level.
[2] A "0" implies all variables were balanced between the two samples in this year.

Table 4.2: DID Estimates of Plant-level Changes in Risks in Response to Receiving an OSHA Violation.[a]

Variable Name	***Seven Industries (Un-weighted)*** **Estimated Coefficient (Std. Err.)**	***Seven Industries (Weighted)*** **Estimated Coefficient (Std. Err.)**	***Seven Industries (>15 Workers)*** **Estimated Coefficient (Std. Err.)**	***Full Sample (Un-weighted)*** **Estimated Coefficient (Std. Err.)**	***Full Sample (Weighted)*** **Estimated Coefficient (Std. Err.)**	***Full Sample (>15 Workers)*** **Estimated Coefficient (Std. Err.)**
Panel A: Dependent Variable is Annual Plant-level Serious Accident Rate per 10,000 employees[b]						
Turnover	8.536	1.305	3.608*	5.533	0.382	2.011
	(8.821)	(1.889)	(1.992)	(4.796)	(0.931)	(1.273)
Productivity	-8.199	-2.118	-0.124	-5.284	-1.307	0.034
	(16.941)	(2.717)	(2.328)	(11.153)	(1.569)	(1.905)
Total # Employees	-1.191	-1.356**	-2.945	-0.997	-0.462	-1.995**
	(2.054)	(0.573)	(2.113)	(0.998)	(0.287)	(0.971)
Cost of Materials	0.187	-0.003	0.036	0.032	0.0004	-0.005
	(0.208)	(0.044)	(0.089)	(0.045)	(0.005)	(0.018)
Avg. # Produc. Workers	0.191	0.613	3.061	-0.825	0.275	1.014
	(4.831)	(0.842)	(2.936)	(1.936)	(0.365)	(1.243)
Single Unit	-9.908	1.643	4.327	-5.280	-0.455	2.657
	(8.941)	(2.883)	(4.733)	(4.594)	(1.817)	(2.567)
Violations Found	-15.853***	-6.311***	-7.485***	-8.575***	-4.142***	-5.306***
	(4.254)	(1.248)	(2.431)	(2.883)	(0.744)	(1.384)
Number of obs.	26,884	26,884	19,291	51,147	51,147	36,676
R-squared	0.003	0.269	0.002	0.002	0.240	0.001
Number of plants	5,124	5,124	3,102	10,257	10,257	6,260
F-statistic[c]	13.81	25.57	9.420	8.830	30.10	14.60

Continued, next page.

Table 4.2 (continued)

Variable Name	***Seven Industries*** **Estimated Coefficient (Std. Err.)**	***Seven Industries (Weighted)*** **Estimated Coefficient (Std. Err.)**	***Seven Industries (>15 Workers)*** **Estimated Coefficient (Std. Err.)**	***Full Sample*** **Estimated Coefficient (Std. Err.)**	***Full Sample (Weighted)*** **Estimated Coefficient (Std. Err.)**	***Full Sample (>15 Workers)*** **Estimated Coefficient (Std. Err.)**
Panel B: Dependent Variable is Annual Plant-level Fatality Rate per 10,000 employees[b]						
Turnover	8.590	0.124	0.736	4.927	-0.135	0.483
	(7.782)	(0.814)	(1.134)	(4.184)	(0.378)	(0.638)
Productivity	-10.90	-0.936	1.797	-7.478	-0.838	0.638
	(15.866)	(1.631)	(1.721)	(10.44)	(0.891)	(1.375)
Total # Employees	0.601	-0.449	-0.672	0.245	-0.110	-0.399
	(1.073)	(0.310)	(0.795)	(0.548)	(0.123)	(0.328)
Cost of Materials	0.144	0.008	-0.032	0.031	0.001	-0.008
	(0.185)	(0.011)	(0.035)	(0.040)	(0.003)	(0.007)
Avg. # Produc. Workers	-2.993	0.442	0.714	-1.462	0.055	0.192
	(4.085)	(0.379)	(1.087)	(1.590)	(0.151)	(0.440)
Single Unit	-8.365	-0.472	0.186	-4.879	-0.784	0.210
	(6.927)	(1.379)	(1.858)	(3.689)	(0.839)	(1.242)
Violations Found	-14.15***	-4.374***	-4.364***	-8.990***	-3.018***	-3.346***
	(3.483)	(0.687)	(1.236)	(2.134)	(0.379)	(0.734)
Number of obs.	26,884	26,884	19,291	51,147	51,147	36,676
R-squared	0.004	0.284	0.004	0.003	0.302	0.003
Number of plants	5,124	5,124	3,102	10,257	10,257	6,260
F-statistic[c]	16.42	40.54	12.32	17.70	63.41	20.58

[a]Statistical significance at the 1%, 5%, and 10% level are represented by ***, **, and *, respectively. Although not reported, each model presented also includes a full set of plant and year fixed effects as indicated in equations (4.2) and (4.3).

[b]Serious accidents are defined by OSHA as workplace accidents resulting in a worker fatality or hospitalization of three or more workers.

[c]The reported F-statistics test the restriction that *Violations Found* has no effect on plant-level accident/fatality rates.

Table 4.3: DID Estimates Comparing Inspected and Uninspected Plant Wages and Total Compensation.[a]

	Dependent Variable is Wages	**Dependent Variable is Total Compensation**
Variable Name	**Estimated Coefficient (Std. Err.)**	**Estimated Coefficient (Std. Err.)**
Turnover	0.166**	605.7***
	(0.080)	(215.6)
Productivity	0.897*	1,757.1
	(0.482)	(1,124.5)
Total # Employees	-0.683	-2,897.4*
	(0.642)	(1,731.7)
Cost of Materials	0.031	147.3***
	(0.021)	(52.07)
Avg. # Production Workers	-2.255***	-8,960.2***
	(0.791)	(2,127.8)
Single Unit	0.068	261.6
	(0.122)	(291.1)
Federal Programmed Insp. (FPI)	-0.282***	-944.1***
	(0.079)	(193.107)
Accident Inspection (AI)	-0.071	37.25
	(0.298)	(751.0)
Complaint Inspection (CI)	-0.213**	-751.7***
	(0.100)	(247.3)
Other Programmed Insp. (OPI)	0.141	526.1
	(0.218)	(553.8)
Number of obs.	68,443	68,443
R-squared	0.208	0.249
Number of Plants	15,500	15,500

[a]Statistical significance at the 10%, 5%, and 1% level are represented by *, **, and ***, respectively. The dependent variables in columns 2 and 3 are the wage rate of production workers and the total yearly compensation of production workers, respectively. Although not reported, each model presented also includes a full set of plant and year fixed effects as indicated in equations (4.4) and (4.5).

Table 4.4: DID Estimates Comparing Inspected and Uninspected Plant Wages and Total Compensation Prior to the Inspection Year.[a]

Variable Name[b]	Wages: 1977-1987 Inspections: 1987-1997	Total Compensation: 1977-1987 Inspections: 1987-1997
Turnover	0.056	200.9
	(0.053)	(149.156)
Productivity	4.543***	9,586.7***
	(0.565)	(1,251.8)
Total # Employees	-0.751	-2,449.8
	(0.727)	(1,766.4)
Cost of Materials	-0.005	52.48
	(0.026)	(58.90)
Avg. # Production Workers	-0.431	-3,555.9
	(0.982)	(2,480.0)
Single Unit	0.298***	755.3***
	(0.107)	(256.0)
Federal Programmed Insp. (FPI)	-0.016	132.4
	(0.094)	(223.4)
Accident Inspection (AI)	0.336	662.4
	(0.272)	(603.8)
Complaint Inspection (CI)	-0.018	-54.11
	(0.114)	(273.8)
Other Programmed Insp. (OPI)	0.187	487.2
	(0.217)	(510.3)
Number of obs.	45,936	45,936
R-squared	0.435	0.480
Number of Plants	10,679	10,679

[a]Statistical significance at the 10%, 5%, and 1% level are represented by *, **, and ***, respectively. The dependent variables in columns 2 and 3 are the average hourly wage rate of production workers and the total yearly compensation of production workers respectively. Although not reported, each model presented also includes a full set of plant and year fixed effects as indicated in equations (4.7) and (4.8).

[b] Plant-specific characteristics are for the same year as the wage data indicated at the top of each column; inspection-related variables are as indicated in the top of each column heading.

Table 4.5: DID Estimates Comparing Wages and Total Compensation of Known Violators and Known Compliers.[a]

Variable Name	***Seven Industries*** **Estimated Coefficient (Std. Err.)**	***Seven Industries (>15 Workers)*** **Estimated Coefficient (Std. Err.)**	***Full Sample*** **Estimated Coefficient (Std. Err.)**	***Full Sample (>15 Workers)*** **Estimated Coefficient (Std. Err.)**
Panel A: Dependent variable is wages				
Turnover	-0.198	-0.142	-0.110	-0.102
	(0.139)	(0.163)	(0.101)	(0.114)
Productivity	3.371***	4.280***	3.384***	4.891***
	(0.491)	(0.752)	(0.480)	(0.697)
Total # Employees	-0.122	-0.067	-0.400	-0.372
	(0.550)	(0.544)	(0.356)	(0.347)
Cost of Materials	0.016	0.004	-0.001	-0.006
	(0.016)	(0.016)	(0.005)	(0.005)
Avg. # Produc. Workers	-1.470**	-1.199*	-0.602	-0.394
	(0.619)	(0.624)	(0.466)	(0.453)
Single Unit	0.381*	0.378	0.516***	0.393***
	(0.212)	(0.236)	(0.138)	(0.146)
Violations Found	-0.301***	-0.255***	-0.307***	-0.325***
	(0.084)	(0.098)	(0.060)	(0.070)
Number of obs.	26,884	19,291	51,147	36,676
R-squared	0.257	0.287	0.243	0.268
Number of plants	5,124	3,102	10,257	6,260

Continued, next page.

Table 4.5 (continued)

Variable Name	***Seven Industries*** **Estimated Coefficient (Std. Err.)**	***Seven Industries (>15 Workers)*** **Estimated Coefficient (Std. Err.)**	***Full Sample*** **Estimated Coefficient (Std. Err.)**	***Full Sample (>15 Workers)*** **Estimated Coefficient (Std. Err.)**
Panel B: Dependent Variable is total compensation				
Turnover	-606.6	-416.8	-435.6	-327.4
	(397.3)	(446.3)	(272.7)	(295.3)
Productivity	6,928.9***	8,400.4***	6,974.3***	9,649.1***
	(1,101.1)	(1,690.1)	(1,052.8)	(1,500.1)
Total # Employees	-1,062.1	-947.9	-1,195.1	-1,117.2
	(1,489.4)	(1,477.6)	(1,002.6)	(973.2)
Cost of Materials	100.5**	77.33*	17.84	6.652
	(43.93)	(42.494)	(14.56)	(13.87)
Avg. # Produc. Workers	-5,986.1***	-5,355.4***	-3,014.4**	-2,559.5*
	(1,598.2)	(1,598.8)	(1,362.3)	(1,310.6)
Single Unit	912.0*	1,051.7*	1,263.8***	1,112.9***
	(507.8)	(571.395)	(327.8)	(351.4)
Violations Found	-953.8***	-818.8***	-1,001.8***	-1,082.0***
	(209.6)	(250.3)	(146.8)	(172.9)
Number of obs.	26,884	19,291	51,147	36,676
R-squared	0.297	0.339	0.280	0.318
Number of plants	5,124	3,102	10,257	6,260

[a]Statistical significance at the 1%, 5%, and 10% level are represented by ***, **, and *, respectively. Although not reported, each model presented also includes a full set of plant and year fixed effects as indicated in equations (4.9) and (4.10).

Table 4.6: DID Estimates Comparing Wages and Total Compensation of Known Violators and Known Compliers Prior to the Inspection Year.[a]

Variable Name	***Seven Industries*** **Estimated Coefficient (Std. Err.)**	***Full Sample*** **Estimated Coefficient (Std. Err.)**
Panel A: Dependent variable is wages		
Turnover	-0.139*	-0.092
	(0.083)	(0.058)
Productivity	5.484***	4.859***
	(0.847)	(0.592)
Total # Employees	0.779*	0.245**
	(0.467)	(0.099)
Cost of Materials	0.0002	-0.015***
	(0.013)	(0.005)
Avg. # Produc. Workers	-1.339**	-0.666***
	(0.541)	(0.171)
Single Unit	0.384***	0.227**
	(0.131)	(0.091)
Violations Found	-0.106	-0.081
	(0.081)	(0.058)
Number of obs.	21,725	41,188
R-squared	0.505	0.469
Number of plants	4,184	8,255

Continued, next page.

Table 4.6 (continued)

Variable Name	***Seven Industries*** **Estimated Coefficient (Std. Err.)**	***Full Sample*** **Estimated Coefficient (Std. Err.)**
Panel B: Dependent Variable is total compensation		
Turnover	-452.9**	-406.4***
	(203.9)	(149.0)
Productivity	12,096.5***	10,868.4***
	(1,857.2)	(1,341.9)
Total # Employees	1,302.0	996.5***
	(1,088.0)	(258.5)
Cost of Materials	-2.658	-25.46**
	(26.64)	(10.04)
Avg. # Produc. Workers	-4,553.3***	-3,177.7***
	(1,228.7)	(509.4)
Single Unit	989.7***	657.8***
	(312.9)	(212.0)
Violations Found	-223.2	-120.0
	(195.0)	(136.1)
Number of obs.	21,725	41,188
R-squared	0.554	0.524
Number of plants	4,184	8,255

[a]Statistical significance at the 1%, 5%, and 10% level are represented by ***, **, and *, respectively. Although not reported, each model presented also includes a full set of plant and year fixed effects as indicated in equations (4.11) and (4.12).

Table 4.7: Estimates of the VSL and VSSA.[a]

Variable Name	***Seven Industries* Estimated Coefficient**	***Seven Industries (Weighted)* Estimated Coefficient**	***Seven Industries (>15 Workers)* Estimated Coefficient**	***Full Sample* Estimated Coefficient**	***Full Sample (Weighted)* Estimated Coefficient**	***Full Sample (>15 Workers)* Estimated Coefficient**
ρ_{wages} *point estimate (Table 4.5, Panel A)*[b]	-602.0***	-----	-510.0***	-614.0***	-----	-650.0***
$\rho_{totalcomp}$ *point estimate (Table 4.5, Panel B)*	-953.8***	-----	-818.8***	-1,001.8***	-----	-1,082.0***
φ *point estimate (Table 4.2, Panel B)*	-14.15***	-4.374***	-4.364***	-8.990***	-3.018***	-3.346***
π *point estimate (Table 4.2, Panel A)*	-15.853***	-6.311***	-7.485***	-8.575***	-4.142***	-5.306***
VSL^{wages} *(2011$)*	$842,356	$2,725,237	$2,314,044	$1,352,369	$4,028,428	$3,846,572
$VSL^{totalcomp}$ *(2011$)*	$1,334,710	$4,317,825	$3,715,176	$2,206,519	$6,572,767	$6,403,063
$VSSA^{wages}$ *(2011$)*	$751,920	$1,888,795	$1,349,165	$1,417,819	$2,935,246	$2,425,674
$VSSA^{totalcomp}$ *(2011$)*	$1,191,331	$2,992,578	$2,166,070	$2,313,308	$4,789,137	$4,037,815

[a]Statistical significance of the original coefficient estimates at the 10%, 5%, and 1% level are represented by *, **, and ***, respectively. See equations (4.13) and (4.14) for the formulas and description of how the VSL and VSSA are computed.
[b]The estimated coefficient ρ_{wages} is reported as average yearly willingness to pay by multiplying the estimates from Table 4.5 by 2,000 under the assumption that individuals work an average of 2,000 hours per year.

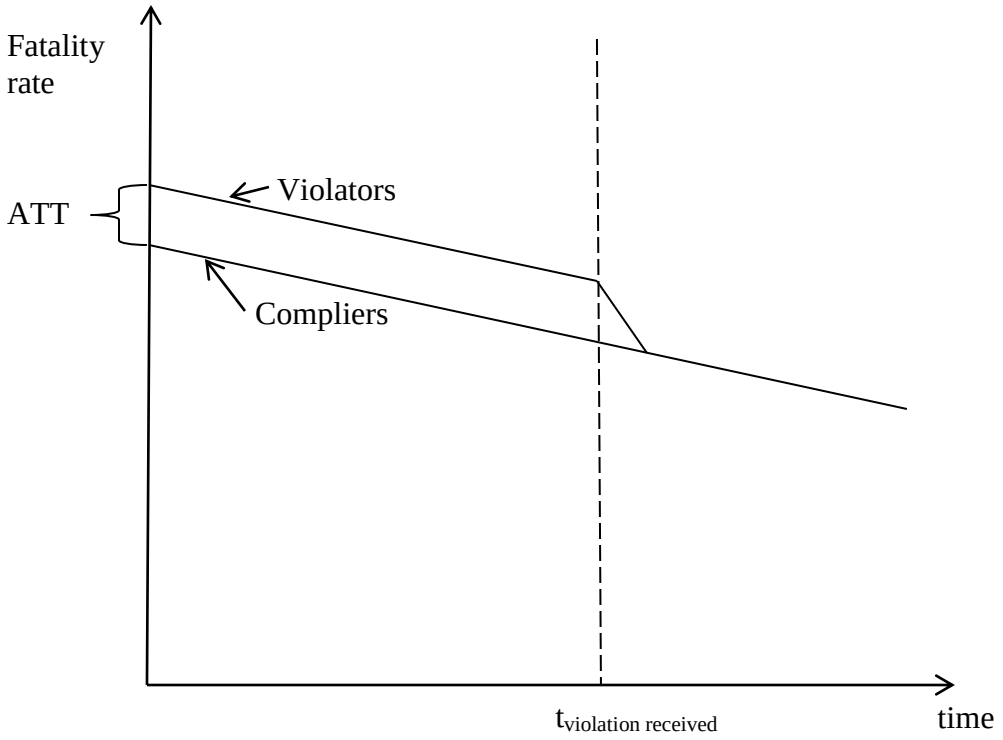

Figure 4.1: Adjustments in Fatality Rates After Receiving an OSHA Violation.

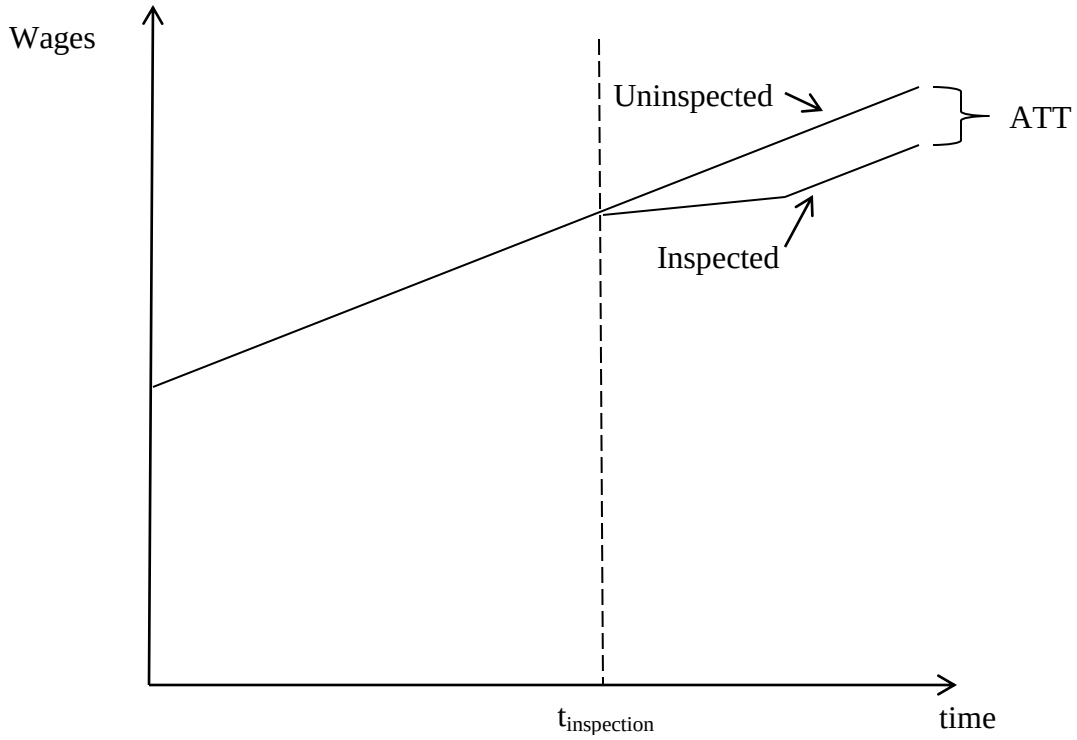

Figure 4.2 Adjustments in Wages Following an OSHA Inspection.

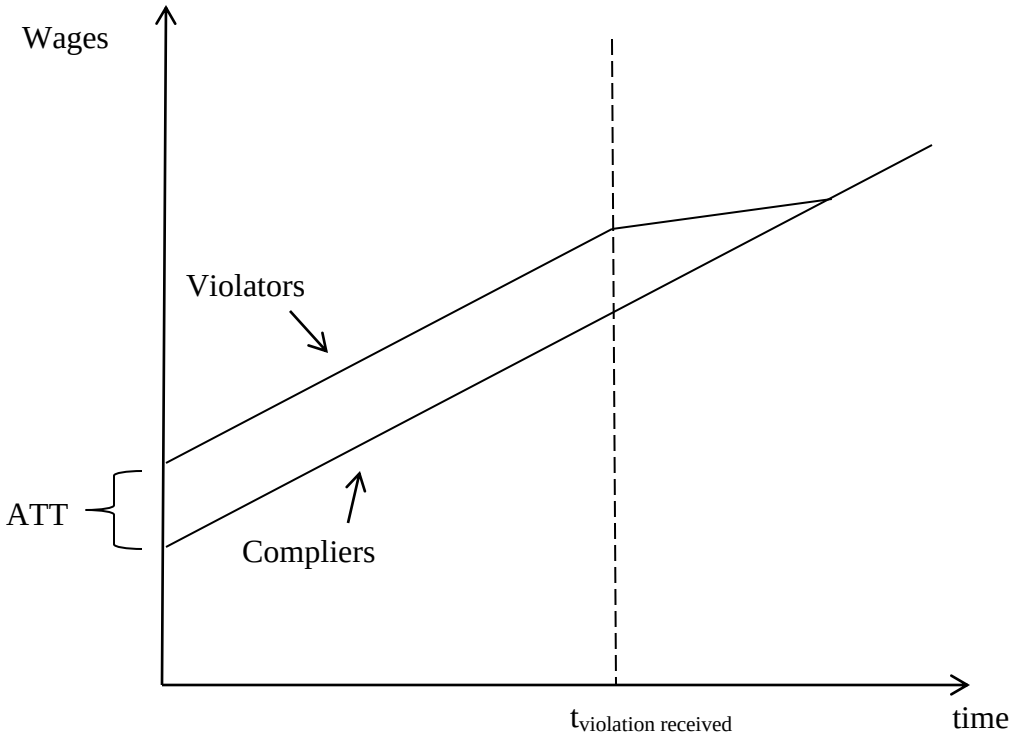

Figure 4.3 Adjustments in Wages Following an OSHA Violation.

Chapter 5

Ancillary Analyses

In Chapter 4, the analysis relied primarily on OSHA inspections from 1978 to 1998; the period during which inspections were randomly assigned based on known selection criteria. The main analyses presented in Chapter 4 used DID estimators applied in two key applications. First, wages at all inspected plants were compared to those at all uninspected plants pre- and post-OSHA inspection. The second application focused only on inspected plants and compared the wages of violators and compliers pre- and post-inspection.

In this chapter, first an alternative to the DID estimators applied in Chapter 4 is presented. Specifically, non-parametric and semi-parametric matching estimators (MEs) are computed for the plants analyzed in Chapter 4 during the period 1978-1998. The non-parametric ME has the advantage of not making any assumptions regarding functional form. The key weakness of the MEs is the requirement that all confounding variables correlated with wages and the probability of receiving an OSHA inspection or violation are observed and controlled for in the model. As indicated in sections 5.1 and 5.2, the MEs are generally not statistically significant. Furthermore, the semi-parametric ME is particularly sensitive to small specification changes when estimating the propensity scores.

The second analysis presented in this chapter employs inspection and COM data from the period after 1998 when OSHA changed its inspection rules to a non-random process. In particular, recall from Chapter 3 that in 1999, OSHA began the site-specific targeting (SST) plan that targets individual plants for inspection based on their reported injury rates for the

previous year. Section 5.3 describes this process in detail, and highlights an important feature of the SST inspections that can be exploited within a regression discontinuity framework in order to further test for evidence of compensating wages for risk. Namely, the SST plan assigns inspections to plants with injury rates above a cutoff threshold. The cutoff threshold is unknown to plants prior to inspection, and it is frequently changed by OSHA in an unpredictable fashion.

The RDD estimator is presented in Section 5.3.2 along with a sample of the results. As indicated in this section, these models were not capable of detecting a significant impact of OSHA inspections on plant-level wages. Importantly, however, the RDD estimators also fail to predict a significant impact of injury rate threshold proximity on the probability of receiving an OSHA inspection. As a result, the underlying assumptions of the RDD are violated in this application and thus the model is not pursued further.

5.1 Non-parametric Matching Estimator

Recall from Chapter 3, excepting the years 1982 to 1987, the assignment process for inspections from 1978 to 1998 consists of targeting high risk industries within a state and randomly selecting plants within those industries for inspection who have not received a comprehensive inspection in the recent past. The random nature of treatment assignment and the finite number of industry by state combinations allows a non-parametric matching estimator (ME) to be used to compare plants receiving random inspections with those that do not receive inspections in a given year (Rubin, 1977).[40] When a plant is inspected, it is either

[40] OSHA randomly chooses plants for inspection within a four-digit SIC classification. Within the seven carefully matched two-digit SIC industries there are a total of 197 four-digit

found to have no violations (and is therefore "safe") or it has violations which must be corrected.

Based on the above, a non-parametric ME to determine the impacts of safer working conditions on wages can be given as follows. Assume wages are measured in time t and inspections occur at time j<t. Wages of all inspected plants are compared to the average wages of all uninspected plants in the same state and industry in order to estimate the ATT. In this application the ATT is the compensating wage differential for receiving an OSHA inspection ($\Delta wages_t$), given as follows:

$$\Delta wages_t = E\left[\left(wages_t^{I_j}\middle|industry_i, state_s\right) - E\left(wages_t^{UI_j}\middle|industry_i, state_s\right)\right], \quad (5.1)$$

where, wages in time *t* for a plant inspected (*I*) at time j<t in industry *i* in state *s* are compared to the average wages in time *t* for all uninspected plants (*UI*) in industry *i* and state *s*.

Equation (5.1) is estimated for every COM panel from 1978 to 1998. While safety violations are expected to be corrected very quickly, it is recognized that wages may adjust more slowly. Thus for each COM year t, equation (5.1) is estimated considering j=t-1, t-2, t-3, and t-4. For illustrative purposes, results for the 1998 panel are presented in Table 5.1, considering j=t-1. As indicated in Table 5.1, the estimated compensating wage differential for the safety improvements following an OSHA inspection is equal to $0.02, but this estimate is not statistically significant at the 10% level.

SIC classifications. There are a total of 28 Federal OSHA states making the total number of industry/state combinations equal to 5,516.

In general the results from the non-parametric ME are insignificant every year except during the time period 1982 to 1987 when OSHA's records-review only inspection policy was implemented. During the records-review only time period there are several years in which inspected plants have significantly higher wages than their matched controls post-inspection. This result is counterintuitive, and appears to be driven by the nonrandom selection process highlighted in Table 4.1 of Chapter 4.

From 1988 to 1998 the estimated compensating wage differentials are normal in sign, albeit statistically insignificant. The insignificant results post 1987 appear to be due to small samples of inspected plants. When the treatment is simply receiving a federal programmed inspection in a given year regardless of past inspection history, there are 1,900 inspections per year on average in the data. Recall from Chapter 4, however, the DID estimators focus on plants receiving their first OSHA inspection. Limiting the treatment group to only include plants receiving their first inspection should make the non-parametric ME results from Table 5.1 comparable to the DID results from Table 4.4. However, limiting the treatment group in the manner described results in a significant loss of observations. Focusing only on plants receiving their first OSHA inspections results in a treatment group containing only 834 observations on average each year, and the number of observations in the treatment group is decreasing over time. Power tests indicate sample sizes of 5,000 observations for both treatment and control groups are needed in order to detect the $0.28 wage differential that was estimated in section 4.3.1. The U.S. Census Bureau (2001) reports roughly 363,000 total manufacturing establishments operating in the U.S. during 1997. Assuming all of these manufacturing establishments are available to serve as controls for the 834 average annual

observations in the treatment group, there is only a 34% probability of rejecting the null hypothesis of no significant wage differential when the true wage differential is $0.28.

5.2 Propensity Score Matching Estimator

The second main analysis in Chapter 4 focuses only on inspected plants, and compares the average production workers' wages at plants that are violators to the average wages at compliers. A ME can once again be employed in a similar fashion as described in the previous section. Here, wages at plants that are violators are compared to wages at plants that are compliers before their inspection dates (i.e. before any safety interventions from OSHA) in order to estimate the magnitude of the compensating wage differential for working at dangerous plants.

This comparison of violators to compliers is complicated by the fact that random assignment of inspections does not imply random assignment of safety violations. If there are systematic differences between plants that receive citations and those that do not, and these factors affect both the probability of receiving a violation and wages, then I have an important confound that must be controlled in order to attain unbiased estimates of the impact of risk on wages. As described in Chapter 4, intuition suggests plant characteristics such as plant size, worker turnover, and worker productivity may be correlated with both safety and wages.[41] In situations where there are multiple confounding variables, but the confounds are all observable, a propensity score ME can be used. Rosenbaum and Rubin (1983) suggest matching on the propensity score in order to reduce dimensionality of confounders to a manageable size and still attain unbiased estimates of treatment effects.

[41] Data on these variables is available for analyses from the COM.

The propensity score in this application is the conditional probability of receiving an OSHA violation given the confounding variables that are correlated with both wages and safety, and can be estimated using logit or probit. Under the logit specification, the estimated propensity score, P_n, is given by:

$$P_n = \frac{e^{\alpha+PC_n\beta}}{e^{\alpha+PC_n\beta}+1}, \tag{5.2}$$

where a is an alternative specific constant, and PC_n is a vector of observable plant characteristics for plant n that are correlated with both wages and plant-level safety. The logit model is estimated using maximum likelihood by finding the values of α and β that maximize the log-likelihood function given by:

$$\text{LL}(\alpha, \beta)=\sum_n d_n \ln(P_n). \tag{5.3}$$

Here, d_n is a binary variable that is equal to 1 for plants that are known violators of OSHA standards, and is equal to zero otherwise.

Once the set of control plants (compliers) with similar confounding characteristics are identified through the estimated propensity scores, the ATT ($\Delta wages_t$) for working at a risky plant is computed, which measures the average willingness to pay (AWTP) for the reduction in probability of death and injury at a complier in comparison to a violator. Formally, a generalized propensity score ME is given by:

$$\Delta wages_t = E\left[\left(wages_t^{V_j}\right) - \sum_{i=1}^{N} W_i \left(wages_t^{C_j}\right)\right], \tag{5.4}$$

where t denotes the year wages are observed for plants found in violation (V) or compliance (C) at time j>t. The expected value of the wage differential between violators and compliers is calculated over the total number of violators in the sample in order to estimate $\Delta wages_t$.

W_i is an individual weight assigned to each complier in the control group based on estimated propensity scores. The primary difference among propensity score matching algorithms is the method used to match C_j to V_j, i.e., the choice of W_i for each observation in the complier group.

There are several propensity score matching algorithms available to explore when estimating the ATT given in equation (5.4). Some of the more popular matching algorithms include nearest-neighbor, caliper, radius, and kernel matching.[42] In the nearest-neighbor matching algorithm W_i is set equal to one and each violator is matched with one complier observation with the closest estimated propensity score. More formally, the matched complier (e) for a given violator (d) is chosen to satisfy the following:

$$\min_{e} \left|\left|P_d - P_e\right|\right|, (d \in V, e \in C), \tag{5.5}$$

such that the absolute difference in the estimated propensity scores for the violator (P_d) and complier (P_e) are minimized. The full set violators and compliers available for analysis are given as V and C, respectively.

Caliper matching is closely related to nearest-neighbor matching because it continues to set W_i equal 1 and chooses one complier per violator such that equation (5.5) is satisfied (Cochran & Rubin, 1973). The difference between caliper matching and nearest-neighbor matching is that caliper matching attempts to reduce the bias associated with poor matches by setting a maximum allowable threshold (r) and choosing the matched complier to satisfy the following:

[42] A more detailed discussion of the available matching algorithms can be found in Smith & Todd (2005), Imbens (2004), and Caliendo & Kopeinig (2008).

$$\min_{e} \left| |P_d - P_e| \right| \leq r, (d \in V, e \in C). \quad (5.6)$$

All of the variables in equation (5.6) are defined the same as (5.5) with the additional requirement that the absolute difference in propensity scores must be less than or equal to the caliper threshold (r) that defines the maximum allowable difference.

Radius matching is another closely related matching algorithm to nearest-neighbor and caliper matching (Dehejia & Wahba, 2002). Similar to caliper matching, the radius matching algorithm defines a caliper threshold (r) defining the maximum allowable difference in propensity scores for two observations to be considered matches. For radius matching, each violator is matched to the entire group of compliers whose difference in propensity scores is less than the caliper threshold as follows:

$$\left| |P_d - P_e| \right| \leq r, (d \in V, e \in C). \quad (5.7)$$

For a given violator, any complier satisfying equation (5.7) serves as counterfactual control group member. If there are M compliers satisfying equation (5.7), the weighting scheme in radius matching assigns each complier an equal weight of 1/M.

As a final alternative, the kernel matching algorithm incorporates the distance between propensity scores directly into the weighting scheme such that compliers who are better matches (i.e. smaller distance between propensity scores) are given a larger weight (Heckman, Ichimura, & Todd, 1997). The formula for the individual weight (W_i) is given by:

$$\frac{G\left(\frac{P_d - P_e}{a_b}\right)}{\sum_{k \in C} G\left(\frac{P_d - P_k}{a_b}\right)} \quad (5.8)$$

where $G(\cdot)$ is a kernel function, a_b is a bandwidth parameter, and C is the full set of compliers. There are a wide variety of kernel functions available for researchers including,

but not limited to the Uniform, Epanechnikov, and Traingular kernels. In essence, kernel matching can be viewed as an extension to radius matching that allows varying weights. Kernels such as the Uniform, Epanechnikov, and Triangular only take on non-zero positive values over the interval (-1,1). Due to the fact that the estimated propensity scores are already constrained to lie in the interval (-1,1), the use of a bandwidth parameter a_b that lies between zero and one makes it possible to set a caliper threshold such that compliers are given zero weights if $\left|\frac{P_d - P_e}{a_b}\right| > 1$. In other words, the bandwidth parameter (a_b) is the caliper threshold in equation (5.7).

The first-stage propensity scores are estimated using the logit specification given in equation (5.2) and an alternative probit specification. In general, the results are similar for either choice of probit or logit. Several plant characteristics are considered when estimating the first-stage propensity scores. The simplest specification includes covariates controlling for worker turnover, productivity, total number of employees, cost of materials, average number of production workers, single unit classification, and union status. Alternative specifications considered include industry, state, and regional fixed effects; and higher order polynomials for total number of employees, cost of materials, and average number of production workers. Table 5.2 presents the results from the first-stage propensity score estimation using the most basic logit specification without polynomials or fixed effects. A cross-section of plant characteristics in 1997 (t=1997) is employed for plants that were inspected by OSHA in 1998 (j=1998). The results indicate that single unit establishments and unionized plants have a higher probability of receiving an OSHA violation. None of the

other covariates considered in Table 5.2 are statistically significant predictors of violation citations.

The second-stage propensity score ME given in equation (5.4) is estimated for every COM panel from 1978 to 1998. For each COM year t, equation (5.4) is estimated considering j=t+1, t+2, t+3, and t+4. A variety of matching algorithms are considered, including nearest-neighbor, caliper, radius, and kernel matching. The kernel and radius matching algorithms appear to be better suited for this application because the control group of compliers is typically much smaller than treated group of violators. When implementing kernel matching the Uniform, Triangular, and Epanechnikov kernels are all considered. Kernel choice appears to have little influence on the estimated effect of receiving an OSHA violation on wages. Estimates are quite sensitive, however, to the choice of bandwidth parameter. Panel A of Table 5.3 presents the results from the propensity score ME given in equation (5.4) using the same cross-section of plant characteristics in 1997 (t=1997) for plants that were inspected by OSHA in 1998 (j=1998). A kernel matching algorithm was employed using the Epanechnikov kernel with a bandwidth of 0.06. As indicated in panel A of Table 5.3, violators are estimated to have a $.30 lower wage rate prior to inspection, but this counterintuitive result is not statistically significant. Overall, results across all model choices were not statistically significant and usually of the wrong sign.

Panel B presents the results of balance tests performed on the vector of plant characteristics (PC_n). Rosenbaum and Rubin (1983) show that if the treatment outcome is independent of violation assignment conditional on the observed vector PC_n, then it is also independent of violation assignment conditional on the propensity scores. Recall, however

that the propensity scores (P_n) from equation (5.2) are estimated propensity scores based on a parametric logistic specification. It is therefore necessary to test for balance among the covariates to insure that the estimated propensity scores produce equivalent conditional distributions of the plant characteristics (PC_n) across the groups of violators and their matched compliers. Results indicate that all the covariates achieve balance in this specification.

In general, the results of the propensity score MEs comparing violators to compliers prior to inspection are not statistically significant. The estimated sign for the difference in wages between violators and compliers, however, is typically negative which is counterintuitive. The main problem using propensity score matching to compare the wages of violators and compliers is that the MEs generally fail to achieve balance for covariates measuring plant size and worker productivity. Furthermore, the balance issues are exacerbated when using specifications that include industry, state, or regional fixed effects. On average across all inspections in the sample, there are 3,800 violators each year. Limiting the analysis to include only plants receiving their first violation so that the results are comparable to the difference in difference models of Chapter 4 Section 4.3.2 provides the expected positive estimated wage differentials between violators and compliers prior to inspection. The estimated wage differentials remain statistically insignificant, however, likely due to the fact that there are only 957 violations per year on average when the sample only includes first time violators.

5.3 Regression Discontinuity Design

Recall from Chapter 3 that from 1978 until 1998 OSHA randomly selected plants for inspection within high hazard industries. Beginning in 1999, however, there was a critical change in the inspection assignment process. OSHA began targeting individual plants for inspection based on plant-level injury and illness rates. Plants with injury rates above a known cutoff threshold were slated to receive comprehensive safety and health inspections. This selection criteria allows me to use regression discontinuity design (RDD) to estimate the impact of OSHA inspections on plant-level wages for plants with injury rates in direct proximity of the cutoff threshold.

5.3.1 Programmed Inspections: 1999 and Later

In 1996, OSHA began the OSHA Data Initiative (ODI) which is an annual survey of roughly 80,000 manufacturing facilities collecting the following information: average number of employees, total hours worked by employees, and injury and illness data from the plant (OSHA, 2004). Standard 1904 requires every employer to record data on work related fatalities, illnesses and injuries regardless of who was at fault, or whether the injured individuals are eligible for workers compensation.[43] Plant-level injury and illness data come from the log reports that OSHA requires all employers to maintain, and the rates are measured in terms of Lost Workday Injury and Illness (LWDII) rates, Days Away From Work Injury and Illness (DAFWII) rates, and Days Away From Work, Restricted, or Transferred (DART) rates. LWDII, DAFWII, and DART rates are calculated on an annual

[43] Exceptions include companies with 10 or fewer employees and low risk industries that are specified in the appendix to standard 1904(29 C.F.R. XVII. 1904, 2003).

basis, and are normalized by the total employment levels of the firm so the rates are comparable across firms of different sizes.[44] Typically these log reports are received by OSHA upon commencing an inspection, but the ODI began collecting the reports via a survey of firms that are not necessarily inspected. Figures 5.1 and 5.2 present a copy of the OSHA 200 log report and subsequent 300 log report which replaced the 200 form in 2002.

Once the ODI was established and operating effectively, OSHA began using the data collected in the ODI to target specific plants for inspection beginning in 1999. The site-specific targeting (SST) plan for 1999 set a Lost Workday Injury and Illness (LWDII) rate threshold at 16. Any plant in a Federal OSHA state that had a LWDII rate at or above 16 was placed on a primary targeting list for inspection. State OSHA agencies are then charged with inspecting each plant. If states complete inspections of all plants on the primary list, they are given a secondary list of plants to inspect that had an LWDII rate between 10 and 16. For context, the average LWDII rate for private firms in 1997 was 3.3, so the SST plans are clearly targeting those plants with much higher than average LWDII rates (U.S. Department of Labor, 1997).

The thresholds used to determine which plants are inspected have changed in six of the 11 years that SST plans have been in effect, but the primary mechanics of the plan have remained constant. OSHA reviews the ODI from the previous year, sets a threshold based on the number of plants that are feasible for compliance safety and health officers to inspect in a given year, and inspects the plants on the primary list followed by the plants on the

[44] An LWDII rate of 16 for example can be interpreted as 16 cases involving a lost workday due to injury and illness per 100 full-time employees in a given year.

secondary list if possible (OSHA, 2004). Beginning in 2003 SST thresholds were set using both LWDII and DAFWII rates. In 2004 the thresholds were changed again based on DAFWII and DART rates, which remains the current SST policy today. The changes in the SST thresholds are not announced until after the ODI survey has been conducted to ensure that respondents are unaware of whether or not they will be targeted for inspection. A summary of major changes in the inspection process is given in Table 5.4. The programmed inspections in this later time period document the same plant-level violation data as the programmed inspections occurring in the years 1972-1998.

5.3.2 RDD Application

For inspections occurring after 1998, it is possible to use a regression discontinuity design (RDD), which makes use of the known cutoff thresholds for inspection assignment in order to estimate average treatment effects of OSHA inspections on plant-level production worker wages. Although the assignment mechanisms are clearly specified for each year in Table 5.4, it is not possible to use a sharp RDD in this application due to the fact that assignment is made on more than one assigning variable. A plant may be assigned to the inspection list based on their DART rate and not on their DAFWII rate or vice versa. Furthermore, there is a possibility that a plant assigned to be inspected may not actually be inspected if the local OSHA office does not have enough time to finish all of its SST inspections in a given year. There is also a possibility that a local OSHA office can finish all of its required SST inspections on the primary list in which case they will be given a secondary list of plants to inspect with injury rates below the threshold. Given these particular features, a fuzzy RDD can be used. The necessary requirement for implementing

fuzzy RDD is that the only changes correlated with injury risks and wages around the cutoff threshold is a discrete jump in probability of inspection at the assignment cutoff threshold (see Chapter 2, Section 2.3.5 for additional details).

The estimating equations for a RDD in this context are given by:

$$D_i = f(IR_i, \theta) + \gamma T_i + \varepsilon_i, \tag{5.10}$$

$$\widehat{D}_i = f\left(IR_i, \hat{\theta}\right) + \hat{\gamma} T_i, \tag{5.11}$$

$$Wage_i = \alpha + \beta \widehat{D}_i * + k(IR_i, \partial) + u_i, \tag{5.12}$$

where $\widehat{D}_i$ is the expected probability of inspection for plant *i* conditioned on a continuous function expressing the relationship between injury rates of plant i (IR_i) and inspection probability, $f(IR_i, \theta)$, and a dummy variable, T_i, that is equal to one for observations with injury rates at or above the cutoff threshold for inspection and equal to zero otherwise. Equation (5.12), then uses the estimated probabilities of inspection and $k(IR_i, \partial)$, the control function representing the continuous relationship between wages and injury rates, in order to estimate the effect of an OSHA inspection on the wage rate.[45] The random error component, u_i, is assumed to be independent of the probability of inspection and injury rates. A flexible functional form with higher order polynomials on injury rates is suggested by van der Klaauw (2002) for approximating both $f(IR_i, \theta)$ and $k(IR_i, \partial)$ in the above equations.[46] The estimated coefficient β provides a measure of the local average treatment effect (LATE) of OSHA inspections on plant-level wages for plants with injury rates that are in close

[45] IR_i could be measured with the LWDII, DART, or DAFWII depending on the year in question.

[46] It is also possible to use semi-parametric and non-parametric estimators to approximate these control functions.

proximity to the cutoff threshold, and is therefore not directly comparable to the ATT estimated in Chapter 4, Section 4.3.1. Note that for the linear regression case, if $f(IR_i, \theta)$ and $k(IR_i, \partial)$ include the same polynomial terms the procedure is equivalent to a two-stage least squares estimator, where the known cutoff thresholds serve as an instrument for inspection assignment.

The key underlying assumption of the RDD method is that the control functions expressing the relationship between wages and injury rates ($k(IR_i, \partial)$), and inspection probability and injury rates ($f(IR_i, \theta)$) are correctly specified. If β from equation (5.12) is estimated over the full set of inspected and uninspected plants it will produce unbiased estimates of the LATE provided that the aforementioned control functions have the correct specification.

One method for relaxing the importance of functional form choice in the control function specification is to limit the observations used in analysis to be contained in calipers of different sizes around the cutoff assignment threshold. If the estimated LATE is stable over different caliper sizes the evidence would suggest that specification bias is not of particular concern. Another specification robustness test involves using weighted least squares in a manner that gives greater importance to observations that are closer to the cutoff threshold.

For sensitivity analysis, I employ a non-parametric RDD that is much more flexible in estimating the functional relationship between the wage rate, probability of inspection, and the injury rate. Similar to Hahn et al. (2001), the weighting and caliper approaches discussed

above are combined using local linear regression to estimate δ and γ in order to solve the following minimization:

$$\min_{\delta,\gamma} \sum_{i=1}^{n} (wages_i - \delta - \gamma(IR_i - IR_T))^2 G\left(\frac{IR_i - IR_T}{a_b}\right) d_i. \quad (5.13)$$

Equation (5.13) minimizes the sum of squared errors from a weighted regression of the wage rate ($wages_i$) on the variable created by the difference between plant i's injury rates (IR_i) and the injury rate cutoff threshold for inspection (IR_T) and an intercept (δ). Similar to the kernel propensity score matching algorithm from Section 5.2 of this chapter, $G(\cdot)$ is a kernel function and a_b is a bandwidth parameter defining the caliper threshold. The definition of the indicator variable (d_i) varies when estimating average wages above the cutoff threshold ($wages^+$) versus estimating average wages below the cutoff threshold ($wages^-$). When estimating $wages^+$, d_i is equal to 1 if $IR_i \geq IR_T$ and equal to zero otherwise. Alternatively, when estimating $wages^-$, d_i is equal to 1 if $IR_i < IR_T$ and equal to zero otherwise. Algebraically, the estimation of δ and γ satisfying equation (5.13) is equivalent to a weighted least squares regression of an intercept and the variable ($IR_i - IR_T$) on $wages_i$, with weights given by $\sqrt{G\left(\frac{IR_i - IR_T}{a_b}\right)}$. The estimated intercept δ is equal to E($wages \mid IR_i = IR_T$), which is equivalent to $wages^+$ when estimating above the cutoff threshold, or $wages^-$ when estimating below the threshold.

I estimate local linear regressions satisfying equation (5.13) on each side of the inspection assignment threshold using a bandwidth that is optimally chosen according to the methods given in Imbens and Kalyanaraman (forthcoming). The LATE is then constructed

as the local wald estimator, which is a ratio of the change in average wage rates divided by the change in average probability of inspection:

$$LATE = \frac{wages^{+} - wages^{-}}{D^{+} - D^{-}}. \quad (5.14)$$

The LATE calculated in equation (5.14) is the same by definition as the LATE estimated by β in equation (5.12). The key difference between equation (5.12) and the non-parametric approach is that equation (5.12) is more sensitive to correct specification of the functional form between wages, probability of inspection, and injury rates. Correct specification of the functional form allows one to extrapolate away from the cutoff threshold and use all observations in the dataset, even those that are far away from the cutoff threshold for estimating the LATE. The non-parametric approach on the other hand estimates the functional form in a much more flexible manner, and only uses the observations that are located within the optimal bandwidth on each side of the cutoff threshold for estimating the LATE.

The results from the RDD estimates of the LATE of OSHA inspections on plant-level wages are given in Table 5.5. Wages are observed in year 2002 for plants reporting their injury rates in the 1998 ODI. The results for four separate estimation methods are reported:

1. The most simplistic specification uses instrumental variables (IV) with a linear specification in injury rates and a common slope on both sides of the cutoff threshold.
2. A more flexible specification uses IV with a linear specification in injury rates and differential slopes on both sides of the cutoff threshold.

3. In the third specification I estimate a more flexible functional form using IV with a second-degree polynomial term for injury rates with varying slopes above and below the cutoff threshold.
4. The final specification uses the non-parametric local linear regression estimates from equation 5.13 to relax the assumptions on functional form even further and narrow the caliper to contain observations in direct proximity of the cutoff threshold.

For the year 2002, a statistically significant positive impact of OSHA inspections on plant-level wages post-inspection is estimated in the simplistic IV linear specification at the 10% level. The other three specifications find no statistically significant impact of OSHA inspections. This is however, not necessarily due to an ineffectiveness of OSHA inspections. Close examination of the results reveals that having injury rates above the cutoff threshold for inspection has a statistically insignificant impact on the probability of receiving an OSHA inspection when using the more flexible estimation strategies in models three and four. Overall very few plants in the sample receive ODI surveys, and even fewer have injury rates above the cutoff threshold. Of those receiving ODI surveys approximately 10% have injury rates above the cutoff threshold for inspection. Furthermore, when examining the data closely, I find numerous instances of plants with injury rates above the cutoff threshold that were not inspected in all of the data cross-sections. Without a statistically significant discrete jump in the probability of inspection at the cutoff threshold, RDD is unable to provide a consistent estimate of the effects of inspection, and thus was not pursued further.

5.4 Conclusion

This chapter presented 3 alternative approaches to the DID estimators in Chapter 4 to estimate the compensating wage differential for risk. Overall, all of the alternative estimation strategies considered suffer from small sample size and find insignificant impacts of OSHA inspections on plant-level wages. In addition to small sample size, the propensity score ME used in Section 5.2 suffers with balance issues and sensitivity to choice of bandwidth. The RDD employed in Section 5.3 fails to find a significant discrete jump in the probability of receiving an OSHA inspection at the documented cutoff threshold. As a result, the cutoff threshold does satisfy the strong instrument requirements, and thereby renders RDD inappropriate in this application.

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5.6 Tables and Figures

Table 5.1: Non-parametric Matching ($wages_{inspected} - wages_{uninspected}$).[a]

Sample	Mean $Wages_{Inspected}$	Mean $Wages_{Uninspected}$	Estimated ATT (Std. Error)
Unmatched Sample	14.28	15.76	-1.483
			(1.432)
Matched Sample	14.40	14.42	-0.023
			(0.377)
Number of Obs.	13,083	423	13,506

[a]Statistical significance of the original coefficient estimates at the 10%, 5%, and 1% level are represented by *, **, and ***, respectively.

Table 5.2: First Stage Logistic Estimation of Propensity Scores. Dependent Variable is Violation Assignment.[a]

Variable Name[b]	**Estimated Coefficient (Std. Error)**
Turnover	-0.277
	(0.152)
Productivity	-0.225
	(0.150)
Total # Employees	-0.284
	(0.202)
Cost of Materials	-0.0001
	(0.0002)
Avg. # Production Workers	0.111
	(0.285)
Single Unit	0.161***
	(0.059)
Union	0.091*
	(0.061)
Number of obs.	2,320
pseudo R-squared	0.015

[a]Statistical significance at the 10%, 5%, and 1% level are represented by *, **, and ***, respectively.

[b] Plant-specific characteristics are for the same year as the wage data (1997). Violation classification occurs in 1998.

Table 5.3: Propensity Score Matching ($wages_{violators} - wages_{compliers}$).[a]

Sample	**Mean $Wages_{violators}$**	**Mean $Wages_{compliers}$**	**Estimates ATT (Std. Error)**
	Panel A: Propensity Score ME Results		
Unmatched Sample	12.91	13.52	-0.610
			(0.196)
Matched Sample	12.91	13.21	-0.296
			(0.211)
Number of Obs.	1,498	822	2,320
Variable Name	**Mean (Violators)**	**Mean (Compliers)**	**Difference (Std. Error)**
	Panel B: Tests for Balance		
Turnover	0.076	0.076	0.0004
			(0.005)
Productivity	0.134	0.131	0.003
			(0.006)
Total # Employees	0.184	0.192	-0.008
			(0.014)
Cost of Materials	25.08	24.22	0.860
			(4.778)
Avg. # Production Workers	0.141	0.147	-0.006
			(0.011)
Single Unit	0.524	0.514	0.010
			(0.018)
Union	0.310	0.299	0.012
			(0.017)

[a]Statistical significance of the original coefficient estimates at the 10%, 5%, and 1% level are represented by *, **, and ***, respectively.

Table 5.4: SST Plans 1999-2009.

SST Plan Date	Primary Inspection Threshold[1]	Secondary Inspection Threshold	Other Significant Changes
1999	LWDII ≥16	10≤LWDII<16	N/A
2000	LWDII ≥14	8≤LWDII<14	N/A
2001	Same as 2000	Same as 2000	N/A
2002	Same as 2000	Same as 2000	N/A
2003	LWDII≥14, or DAFWII≥9	8≤LWDII<14, or 4≤DAFWII<9	-Randomly select 200 low rate plants from high rate industries for comprehensive health and safety inspections.
2004	DART≥15, or DAFWII≥10	8≤DART<15, or 4≤DAFWII<10	Add non-respondents from 2003 ODI to Primary Inspection List.
2005	DART≥12, or DAFWII≥9	7≤DART<12, or 5≤DAFWII<9	-Number of low-rate plants increased from 200 to 400. -Provision of tertiary list for offices completing both their primary and secondary lists.
2006	Same as 2005	Same as 2005	Number of low-rate plants reduced from 400 to 175.
2007	DART≥11, or DAFWII≥9	7≤DART<11, or 4≤DAFWII<9	Number of low-rate plants reduced from 175 to 100.
2008	Same as 2007	Same as 2007	Number of low-rate plants increased from 100 to 175.
2009	DART≥8, or DAFWII≥6	6≤DART<8, or 4≤DAFWII<6	Randomly selected low-rate plants no longer part of SST. Now handled under new Recordkeeping National Emphasis Program.

[1]Prior to 2004, Lost Workday Injury and Illness (LWDII) rates were used for establishing thresholds. LWDII is defined as (N/EH)x(200,000), where N is the number of lost work day injuries and illnesses, EH is the number of hours worked by all workers during the year, and 200,000 is the number of hours worked by 100 people. Days Away from Work Injuries and Illness (DAFWII) and Days Away from Work, Restricted, or Transferred (DART) rates are calculated using the same formula as for LWDII. The only difference in the rates is what is included in N. DAFWII involves only cases where at least one day of work is missed. LWDII involves all the cases included in DAFWII as well as cases where at least one day of restricted work activity occurs. DART is similar in nature to the LWDII rates, but the usage of DART rates coincides with the adoption of the OSHA 300 form where the definitions of recordable injuries/illnesses was changed. DART rates are generally lower than LWDII due to the classification of many musculoskeletal disorders as recurring rather than new cases (U.S. Government Accountability Office, 2009).

Table 5.5: Regression Discontinuity Design $(wages^{+} - wages^{-})/(D^{+} - D^{-})$.[a]

Estimation Method	$\boldsymbol{D^{+} - D^{-}}$ **(Std. Error)**	***LATE*** **(Std. Error)**
1. Simple IV	0.131**	16.14*
	(0.060)	(9.376)
2. IV with variable slopes	0.398***	-2.144
	(0.134)	(3.552)
3. IV with polynomials	0.154	22.13
	(0.305)	(49.07)
4. Local linear regression	0.017	78.41
	(0.116)	(520.05)

[a]Statistical significance of the original coefficient estimates at the 10%, 5%, and 1% level are represented by *, **, and ***, respectively.

OSHA Form 200

U.S. Department of Labor

For Calendar Year 19___ Page ___ of ___

Company Name

Establishment Name

Establishment Address

Form Approved O.M.B. No. 1220-0029

Extent of and Outcome of INJURY

Fatalities | Nonfatal injuries

Type, Extent of, and Outcome of ILLNESS

Type of Illness

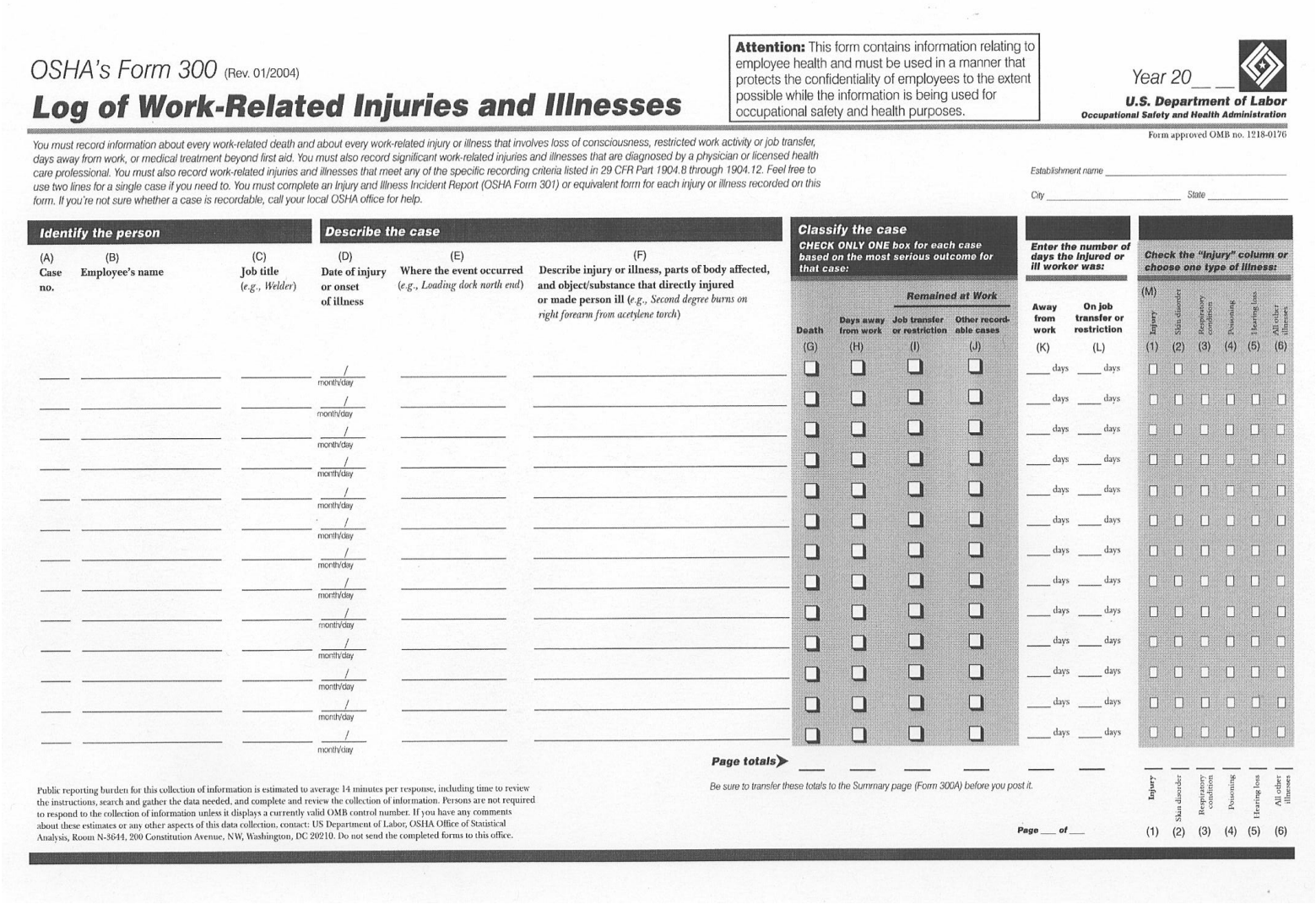
Fatalities | Nonfatal Illness

Injury Related	Injuries With Lost Workdays				Injuries Without Lost Workdays	CHECK Only One Column for Each Illness (*See other side of form for terminations or permanent transfers.*)							Illness Related	Illnesses With Lost Workdays				Illnesses Without Lost Workdays
Enter DATE of death. Mo./day/yr.	Enter a CHECK if injury involves days away from work, or days of restricted work activity, or both.	Enter a CHECK if injury involves days away from work.	Enter number of DAYS away *from* work.	Enter number of DAYS of *restricted work activity.*	Enter a CHECK if no entry was made in columns 1 or 2 but the injury is recordable as defined above.	Occupational skin diseases or disorders	Dust diseases of the lungs	Respiratory conditions due to toxic agents	Poisoning (systemic effects of toxic materials	Disorders due to physical agents	Disorders associated with repeated trauma	All other occupational illnesses	Enter DATE of death. Mo./day/yr.	Enter a CHECK if illness involves days away from work, or days of restricted work activity, or both.	Enter a CHECK if illness involved days away from work.	Enter number of DAYS away *from* work.	Enter number of DAYS of *restricted work activity.*	Enter a CHECK if no entry was made in columns 8 or 9.
(1)	(2)	(3)	(4)	(5)	(6)	(7) (a)	(b)	(c)	(d)	(e)	(f)	(g)	(8)	(9)	(10)	(11)	(12)	(13)

Certification of Annual Summary Totals By _____ Title _____ Date _____

OSHA No. 200 POST ONLY THIS PORTION OF THE LAST PAGE NO LATER THAN FEBRUARY 1.

Figure 5.1: OSHA Form 200.



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

Log of Work-Related Injuries and Illnesses

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

Year 20__ __

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

Form approved OMB no. 1218-0176

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

Establishment name _____

City _____ State _____

Identify the person

(A) Case no.	(B) Employee's name	(C) Job title (*e.g., Welder*)

Describe the case

(D) Date of injury or onset of illness	(E) Where the event occurred (*e.g., Loading dock north end*)	(F) Describe injury or illness, parts of body affected, and object/substance that directly injured or made person ill (*e.g., Second degree burns on right forearm from acetylene torch*)
/ month/day		

Classify the case

CHECK ONLY ONE box for each case based on the most serious outcome for that case:

Death (G)	Days away from work (H)	Remained at Work: Job transfer or restriction (I)	Remained at Work: Other recordable cases (J)
☐	☐	☐	☐

Enter the number of days the injured or ill worker was:

Away from work (K)	On job transfer or restriction (L)
____ days	____ days

Check the "injury" column or choose one type of illness:

(M) Injury (1)	Skin disorder (2)	Respiratory condition (3)	Poisoning (4)	Hearing loss (5)	All other illnesses (6)
☐	☐	☐	☐	☐	☐

Page totals ➤ ____ ____ ____ ____ ____ ____

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

Injury (1)	Skin disorder (2)	Respiratory condition (3)	Poisoning (4)	Hearing loss (5)	All other illnesses (6)
____	____	____	____	____	____

Page ___ of ___

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

Figure 5.2: OSHA Form 300.

Chapter 6

Conclusion

This research provides a quasi-experimental approach to estimating compensating wages as an alternative to traditional hedonic wage applications. Notably, the research uses plant-level risk measures, which are a much-improved alternative to the national-average risk rates that are typically used in hedonic wage applications. Furthermore, concerns regarding omitted variable bias in hedonic wage models are alleviated by the use of randomly assigned OSHA inspections as an exogenous instrument affecting plant level safety.

Federal programmed OSHA safety inspections are established as a valid instrument for plant-level safety (risk) by formally testing the hypothesis of random inspection assignment. Evidence strongly indicates that OSHA inspections are indeed randomly assigned within state and 4-digit industry classification for the time periods 1978 to 1981 and 1988 to 1997, and that these inspections lower plant-level serious accident and fatality rates. For example, during the period 1987 to 1997, estimates indicate that OSHA violation citations result in five fewer serious accidents and three fewer fatalities per 10,000 workers annually across all manufacturing industries. OSHA inspections are then used as an instrument for plant-level risks to test for compensating differentials by comparing average wages and fringe benefits at inspected plants before and after inspection to average wages and fringe benefits at non-inspected plants. For the period where COM panel data wholly overlap with the time periods in which OSHA policy dictates that it neutrally select plants

that are inspected, strong evidence of negative compensating pay differentials associated with improved workplace safety are found, as theory would predict.

A second analysis is conducted that focuses only on inspected plants and compares wages and fringe benefits of inspected plants with found safety violations and inspected plants with no found violations. Again, strong evidence for compensating pay differentials is found in the periods when OSHA inspections are randomized. Results indicate that on average, workers' wages are reduced by 2.5 to 3.3 percent post-inspection, or approximately $1,010 to $1,287 annually in 2011 dollars. As reported in table 6.1, these estimates translate to a central estimate for the VSL based only on changes in the wage rate of $3.8 million measured in 2011dollars for the sample covering all manufacturing industries (95% confidence interval of 1.5 million to 6.2 million). The central estimate is substantially lower than most recent estimates of the VSL from hedonic wage studies. For example, Viscusi (2004) and Kneisner et al. (2012) both use labor market data from the time period 1992 to 2001 and report VSLs between $6.6 to $15 million in 2011 dollars. The $6.6 million estimate from the low end of their range is beyond the values contained within the 95% confidence interval around my $3.8 million central estimate of the VSL.

As reported in column six, row two of table 6.1, accounting for both changes in wages and fringe benefits following and OSHA violation results in a central estimate of the VSL of $6.4 million in 2011 dollars for the sample of all manufacturing industries (95% confidence interval of 3.0 million to 9.8 million). The results indicate that differentials in fringe benefits offered by employers play an important role accounting for approximately 40% of the estimated VSL. This estimate is still slightly lower than most recent VSL

estimates from hedonic wage studies as well as the $8.2 million point estimate currently used by the EPA. However, the 95% confidence interval around the central estimate of $6.4 million does overlap the EPA's estimate as well as the range of estimates from recent hedonic wage studies.

While there are no other quasi-experimental labor-market studies examining compensating wages for risky working conditions to which my estimates can be compared, Ashenfelter and Greenstone (2004) estimate the VSL in a quasi-experimental framework that first estimates the amount of saved travel time in states adopting increased speed limits and then estimates the impact of increased speed limits on rural interstate fatality rates.[47] They report an upper-bound point estimate for the VSL of $2.2 million (2011$), approximately 66% lower than the $6.4 million central estimate reported herein. However, this discrepancy is consistent with evidence that VSL estimates based on traffic fatality risks are typically at the lower end of the VSL range reported in the literature (see, for example, Kochi and Taylor, 2011, de Blaeij et al., 2003, and Viscusi, 1992).

Taken as a whole, 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, the results of this research also suggests that the empirical challenges inherent in estimating the VSL via cross-sectional or panel-data

[47] Moore and Viscusi (1989) use national average risk rates varying by state and industry of worker to estimate a model that is conceptually similar my approach. First, they estimate the effects of state workers' compensation benefit caps laws on risk and then in a second stage, they estimate the effect of the benefit caps on workers' wage rates. Moore and Viscusi note, however, that the benefit caps are not exogenous so their estimation strategy relies on correctly instrumenting for a worker's probability of earning a wage higher than the benefit cap. Their resulting point-estimate of a VSL is 12.0 million (2011$).

hedonic wage models have not yet been fully addressed. Data limitations, especially with regards to the measurement of risk faced by workers at their worksite, correlated unobservables, and accounting for all forms of compensation including fringe benefit payments are likely the key impediments to identification of unbiased compensating wage differentials.

Of course, my approach is not without data limitations as well. Wages are aggregated at the plant-level and OSHA targets the most dangerous industries resulting in a non-representative data sample. Continued research that carefully identifies exogenous variations in risk that are linked to observable monetary tradeoffs is clearly needed.

Future research directly related to OSHA includes modeling the impacts of OSHA inspections on minor injury rates, testing for firm-level spillover effects of OSHA inspections, and combining data on individual workers with the COM and OSHA data. First, data on minor plant-level injuries that do not result in a worker hospitalization or fatality is collected by OSHA during inspections, but is not made publicly available. A freedom of information act request has been filed with OSHA in order to obtain the data on minor injuries.

Second, the current research in this dissertation focuses on direct plant-level impacts of OSHA inspections, but there may be safety spillover effects from firm-level changes in safety policy in response to an inspection at one establishment of a multi-unit firm. Assuming these spillover effects are significant, they will provide another instrument for identifying the causal impacts of OSHA inspections on plant-level risks.

Finally, the Census Bureau's Longitudinal Employer Household Dynamics (LEHD) data collects individual level data on worker characteristics and place of employment, making it possible to match individual worker data to the job-site data from the COM and OSHA's IMIS. Combination of the LEHD, COM, and IMIS datasets will make it possible to follow individual workers overtime and observe their employment decisions in response to OSHA inspections.

6.1 References

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6.2 Tables and Figures

Table 6.1: Estimates of the VSL and VSSA.[a]

Variable Name	***Seven Industries* Estimated Coefficient**	***Seven Industries (Weighted)* Estimated Coefficient**	***Seven Industries (>15 Workers)* Estimated Coefficient**	***Full Sample* Estimated Coefficient**	***Full Sample (Weighted)* Estimated Coefficient**	***Full Sample (>15 Workers)* Estimated Coefficient**
VSL^{wages} *(2011$)*	$0.8	$2.7	$2.3	$1.4	$4.0	$3.8
$VSL^{totalcomp}$ *(2011$)*	$1.3	$4.3	$3.7	$2.2	$6.6	$6.4
$VSSA^{wages}$ *(2011$)*	$0.8	$1.9	$1.3	$1.4	$2.9	$2.4
$VSSA^{totalcomp}$ *(2011$)*	$1.2	$3.0	$2.2	$2.3	$4.8	$4.0

[a]All estimates reported in millions.

APPENDIX

Appendix A

Additional Robustness Tests

The simple theoretical hedonic wage model presented in Chapter 2 implies that firms take the equilibrium wage rate as given by the hedonic wage function, and there is no direct effect of plant-level safety on labor demand. Stated differently, any changes in labor demand in response to changes in jobsite safety are due to movements along the demand curve for labor rather than shifts in labor demand. It is, however, possible that worker productivity is affected by job risks and/or firm's cost of providing safety depends on the amount of labor employed. This chapter analyzes impacts of relaxing the assumption that safety costs are independent of employment levels. Notably, under the assumptions of perfectly competitive labor markets shifts in labor demand in response to safety changes do not distort the measures of compensating wage differentials. Finally, the chapter provides empirical tests for the impact of OSHA inspections on plant-level employment decisions in order to test for shifts in labor demand.

A.1 Perfectly Competitive Markets, Cost of Safety Independent of Labor

In Chapter 2, Section 2.1.2 the profit maximization decision of a firm operating in a perfectly competitive labor market was explicitly modeled. Firms are assumed to maximize the following profit equation:

$$\max \pi = \underset{\rho, L}{max} P * f(L) - w(\rho) * L - c(\rho). \quad \text{(A.1)}$$

where profit (π) is a function of the market price (P) of the good, the production function ($f(L)$), and the cost to the firm for providing a marginal increase in safety, $c(\rho)$. An

interesting implication of the first order conditions for the maximization of equation (A.1) is that individual firms choose the optimal amount of workers to hire by setting the marginal revenue product of labor ($P*f_L$) equal to the equilibrium wage rate ($w(\rho)$), but mortality risks (ρ) do not shift firms demand for labor. Figure A.1 illustrates an individual firm's profit maximizing choice of labor (L^*) in a perfectly competitive market. Following an OSHA inspection violating firms are required to make safety improvements in order to comply with OSHA standards. Under the assumption of diminishing marginal returns to labor, firms receiving OSHA violations will increase the quantity of labor employed as the equilibrium wage rate falls due to reductions in mortality risks, but this is a movement along the demand curve for labor. Figure A.2 illustrates the change in labor following an OSHA violation as the movement along the labor demand curve from L^* to L^{**}.

A.2 Perfectly Competitive Markets, Cost of Safety Dependent on Labor

If firm's cost of safety ($c(\cdot)$) is a function of mortality risk (ρ) and labor (L), then the profit maximizing decision of an individual firm is given by:

$$\max \pi = \max_{\rho, L} P * f(L) - w(\rho) * L - c(\rho, L). \quad \text{(A.2)}$$

Firms now choose the optimal amount of labor to satisfy the following first order condition:

$$\frac{\partial \pi}{\partial L} = P * f_L - w(\rho) - c_L = 0. \quad \text{(A.3)}$$

In this scenario the demand of an individual firm is now a function of mortality risks due to the inclusion of c_L in the FOC of equation (A.3). It is easy to imagine a scenario where firms have to provide each individual worker safety equipment such as respirators, in which case the cost of safety would be increasing as labor increases. Assuming that the cost of safety is

increasing in labor (c_L>0), and the cross-partial derivative of the cost function ($c_{L,\rho}$) is negative, the firm's demand curve for labor will shift left as ρ decreases.[48] Figure A.3 illustrates the impacts of an OSHA violation in situations where firm's cost of safety is increasing in labor. As the graph illustrates, the quantity of labor demanded may increase or decrease in response to an OSHA violation when the cost of safety is increasing in labor. Fortunately, however, the shifts in demand do not affect the equilibrium wage rate, and the change in the equilibrium wage rate at the firm still reflects the compensating wage differential for risks.

A.3 Market Power, Cost of Safety Independent of Labor

I now relax the assumption of perfectly competitive labor markets, and assume that an individual firm has some influence on the wage rate through their decisions of how many workers to employ. The individual firm now faces an upward sloping supply curve for labor. Assuming cost of safety is not a function of labor yields the new first order condition for equation (A.1) with respect to labor:

$$\frac{\partial \pi}{\partial L} = P * f_L - w(\rho) - L * \frac{dw}{dL} = 0. \quad \text{(A.4)}$$

Equation (A.4) implies that firms maximize profits by choosing the amount of labor satisfying:

$$P * f_L - L * \frac{dw}{dL} = w(\rho), \quad \text{(A.5)}$$

where the fraction $\frac{dw}{dL}$ is determined by the slope of the labor supply curve. Figure A.4 illustrates the profit maximizing choice of labor and the equilibrium wage rate under the

[48] It is common to assume constant marginal safety costs of labor using the following specification $c(\rho,L) = c(\rho)*L$ where $c(\rho)$ is as defined in Chapter 2.

market power scenario. Using DID to estimate the average change in individual firm's wages following an OSHA violation, it is still possible to estimate a compensating wage differential when labor markets are not perfectly competitive. This possibility is due to the fact that mortality risk does not enter into the firm's profit maximizing choice of labor given in equation (A.5) except through the wage rate. Figure A.5 illustrates the compensating wage differential estimated from a change in wages following an OSHA violation. The profit maximizing amount of labor employed increases following an OSHA violation.

A.4 Market Power, Cost of Safety Dependent on Labor

If firms are not operating in a perfectly competitive market, and the cost of safety is increasing in labor as in equation (A.2), then the firm maximizes profits by choosing the amount of labor satisfying the following equation:

$$P * f_L - L * \frac{dw}{dL} - c_L = w(\rho). \quad \text{(A.6)}$$

Similar to Section A.2, the profit maximizing choice of labor is now dependent upon mortality risk. As Figure A.6 illustrates, the profit maximizing quantity of labor employed may increase or decrease following an OSHA inspection. More importantly, Figure A.6 illustrates that when firms have market power and safety costs are increasing in labor the change in wages following an OSHA violation does not represent the compensating wage differential. In this scenario it is not possible to employ DID to attain unbiased estimates of compensating wages for risk.

A.5 Empirical Tests for Labor Market Structure and Equilibrium

Section A.4 illustrates that when cost of safety is increasing in labor and firms have market power in the labor market, the wage differentials estimated from the DID models in

Chapter 4 will produce upwardly biased estimates of the compensating wages for risk. Importantly, assuming productivity is unaffected by plant safety, sections A.2 and A.4 also illustrate that reductions in labor demanded following an OSHA violation are only expected if the cost of safety is increasing in labor. Therefore a DID estimator is used to test for changes in labor hired using the following specifications:

$$Production\ workers_{j,t} = a + PC_{j,t}\beta + T_t + P_j + \delta_1 FPI_{j,t} + \delta_2 AI_{j,t} + \delta_3 CI_{j,t} + \delta_5 OI_{j,t} + \epsilon_{j,t}, \quad \text{(A.7)}$$

$$Other\ Employees_{j,t} = a + PC_{j,t}\beta + T_t + P_j + \delta_1 FPI_{j,t} + \delta_2 AI_{j,t} + \delta_3 CI_{j,t} + \delta_5 OI_{j,t} + \epsilon_{j,t}, \quad \text{(A.8)}$$

$$Productivity_{j,t} = a + PC_{j,t}\beta + T_t + P_j + \delta_1 FPI_{j,t} + \delta_2 AI_{j,t} + \delta_3 CI_{j,t} + \delta_5 OI_{j,t} + \epsilon_{j,t}, \quad \text{(A.9)}$$

where the average number of production workers (A.7), or other employees (A.8), and the average productivity of production workers (A.9) at plant j and time t are a function of a vector of observable time-varying plant characteristics ($PC_{j,t}$), and time and plant fixed-effects, T_t and P_j, respectively. $FPI_{j,t}$ is a dummy variable equal to one for plants receiving their first randomly assigned federal programmed inspection in year *t* and each year subsequent to *t*. As such, δ_1 is the key coefficient of interest that identifies the impact of receiving a federal programmed inspection on the dependent variables of interest. Also included in equations (A.7), (A.8), and (A.9) are a series of indicator variables equal to one for plants receiving accident inspections ($AI_{j,t}$), complaint inspections ($CI_{j,t}$), and "other" inspections ($OI_{j,t}$) as classified by OSHA.

The estimated impact of a federally programmed OSHA inspection on the number of production workers and other employees is expected to be positive under the market and cost structures specified in sections A.1 and A.3. A positive estimate of δ_1 in equations (A.7) and (A.8) does not rule out the possibility of market power and increasing cost of safety, but a negative estimate certainly calls to question whether or not the DID estimates from Chapter 4 are unbiased estimates of the compensating wage differential.

The first two columns in Table A.1 report the results from the estimation of equation (A.7) and (A.8) analyzing the impacts of OSHA inspections on employment levels. The results indicate that federally programmed OSHA inspections result in one additional production worker and one additional other employee being hired post-inspection. The positive results are consistent with the scenarios presented in sections A.1 and A.3, where labor demand does not shift in response to safety changes. However, the estimated positive impacts are statistically insignificant.

Assuming that a plant's production function is concave, I would expect average worker productivity at a plant to decrease as additional workers are hired. However, worker productivity may also be correlated with mortality risks where the exact nature of the correlation is unknown (see Garen, 1988, Hwang, Reed, and Hubbard, 1992, and Viscusi and Hersch, 2001). The effect of OSHA inspections on worker productivity is therefore theoretically uncertain. Column three in Table A.1 reports the results from the estimation of equation (A.9). The estimated impact of a federally programmed OSHA inspection is positive but insignificant.

As a final robustness test, I also explore the potential impact of inspections on worker turnover rates. Another key assumption from the theoretical hedonic wage model presented in Chapter 2 is that compensating wages are observed in equilibrium in the labor market. Higher turnover rates at plants with increased mortality risks may indicate frictions and disequilibrium in the labor market. I therefore test for the impact of OSHA violations on plant-level turnover rates for production workers using the following DID estimator:

$$Turnover_{j,t} = a + PC_{j,t}\beta + T_t + P_j + \delta_1 FPI_{j,t} + \delta_2 AI_{j,t} + \delta_3 CI_{j,t} + \delta_5 OI_{j,t} + \epsilon_{j,t}, \quad \text{(A.10)}$$

where all variables are defined as above, but the dependent variable is now production worker turnover rates at plant j and time t.

The results from the estimation of equation (A.10) are presented in column 4 of Table A.1. Results indicate that OSHA inspections have no significant impact on plant-level turnover rates.

A.6 References

Garen, J. (1988). Compensating Wage Differentials and the Endogeneity of Job Riskiness. *Review of Economics and Statistics*, Vol. 70, 9-16.

Hwang, H., Reed, W., & Hubbard, C. (1992). Compensating Wage Differentials and Unobserved Productivity. *Journal of Political Economy*, Vol. 100, 835-858.

Viscusi, W. K., & Hersch, J. (2001). Cigarette Smokers As Job Risk Takers. *Review of Economics and Statistics*, Vol. 83, 269-280.

A.7 Tables and Figures

Table A.1: Tests for Labor Market Structure and Equilibrium.[a]

	Dependent Variable:			
	Average # Production Workers	**Average # Other Employees**	**Productivity**	**Turnover Rates**
Variable Name	**Estimated Coefficient (Std. Err.)**	**Estimated Coefficient (Std. Err.)**	**Estimated Coefficient (Std. Err.)**	**Estimated Coefficient (Std. Err.)**
Turnover	-0.010***	-0.001*	0.020	----
	(0.001)	(0.001)	(0.013)	
Productivity	-0.013**	0.003**	----	0.021
	(0.005)	(0.001)		(0.014)
Total # Employees	----		0.248***	-0.015
			(0.072)	(0.037)
Cost of Materials	0.009***	0.001**	0.030***	-0.006***
	(0.002)	(0.001)	(0.008)	(0.002)
Avg. # Produc. Workers	----		-0.731***	-0.367***
			(0.110)	(0.068)
Single Unit	-0.003*	-0.002**	0.001	-0.044***
	(0.002)	(0.001)	(0.006)	(0.009)
Federal Programmed Insp. (FPI)	0.001	0.001	0.0004	-0.001
	(0.001)	(0.001)	(0.004)	(0.006)
Accident Inspection (AI)	0.030**	0.009*	-0.006	-0.045
	(0.012)	(0.004)	(0.017)	(0.029)
Complaint Inspection (CI)	0.011***	0.002	-0.0002	0.010
	(0.003)	(0.001)	(0.006)	(0.008)
Other Programmed Insp. (OPI)	0.005	0.002	0.007	-0.007
	(0.006)	(0.002)	(0.009)	(0.015)
Number of obs.	68,443	68,443	68,443	68,443
R-squared	0.095	0.007	0.024	0.012
Number of Plants	15,500	15,500	15,500	15,500

[a]Statistical significance at the 10%, 5%, and 1% level are represented by *, **, and ***, respectively. The dependent variables in columns 2 and 3 are the average hourly wage rate of production workers and the total yearly compensation of production workers respectively. Although not reported, each model presented also includes a full set of plant and year fixed effects as indicated in equations (A.7), (A.8), (A.9), and (A.10).

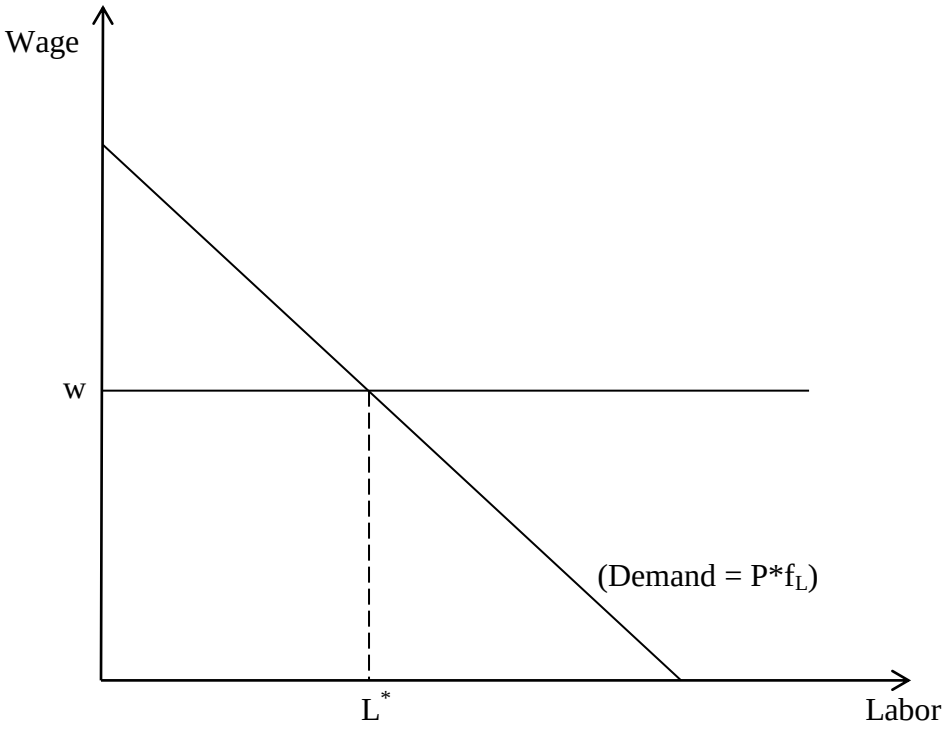

Figure A.1: Perfectly Competitive Labor Market.

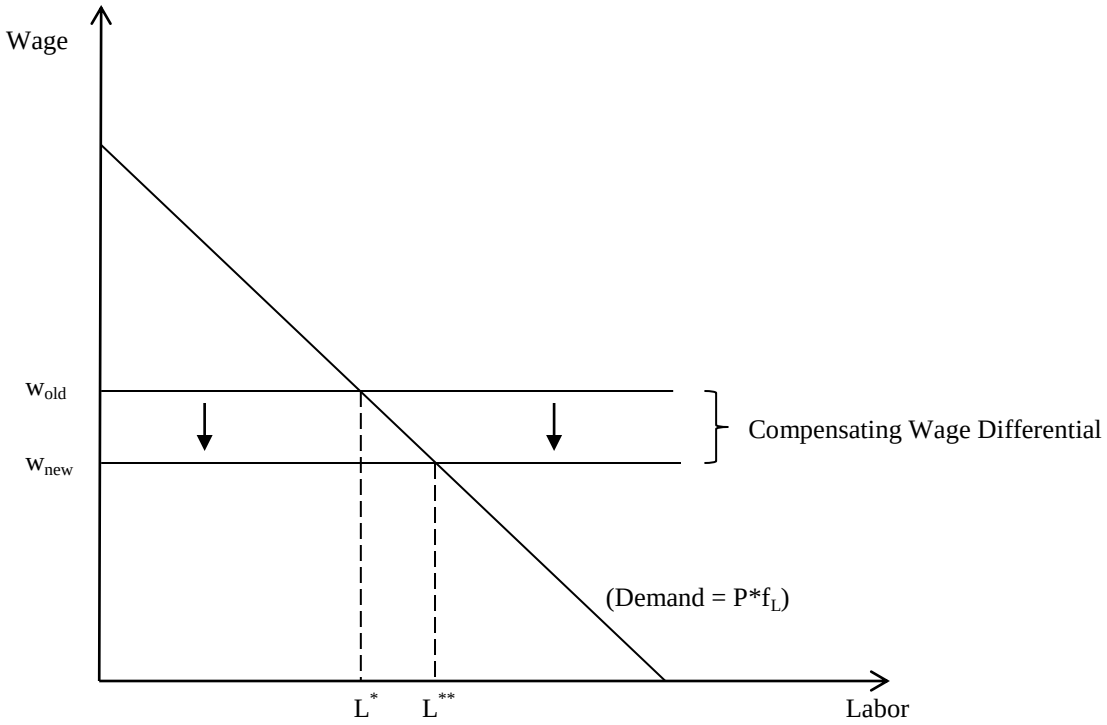

Figure A.2: Perfectly Competitive Labor Market, $c(\rho)$.

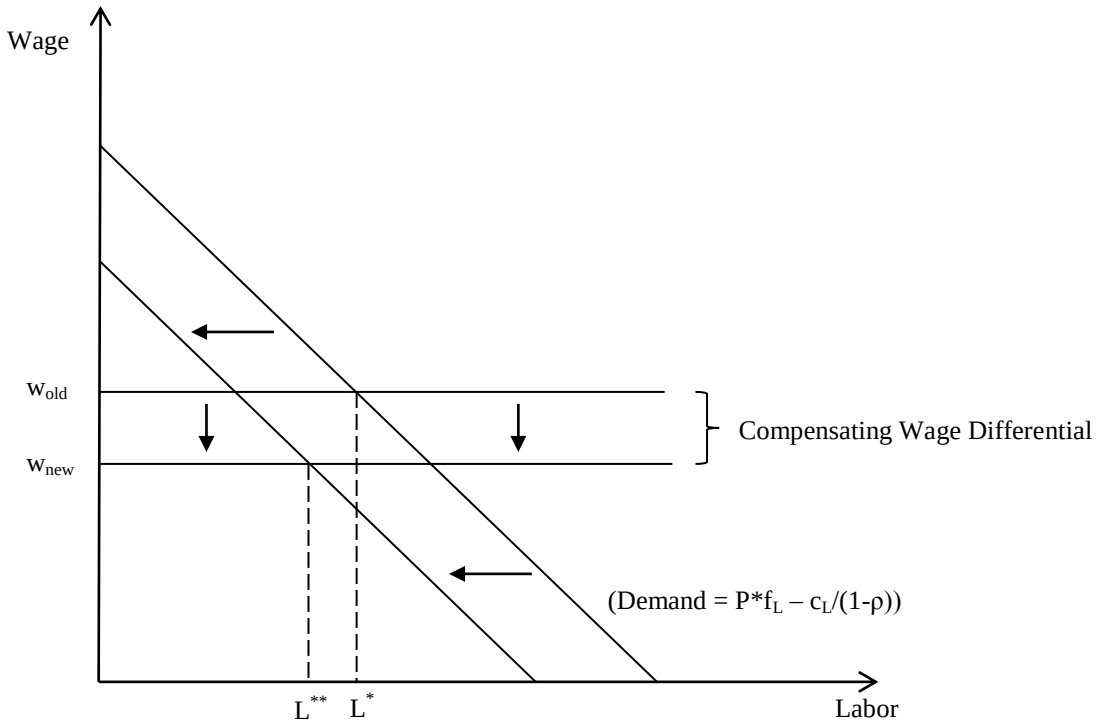

Figure A.3: Perfectly Competitive Labor Market, $c(\rho,L)$.

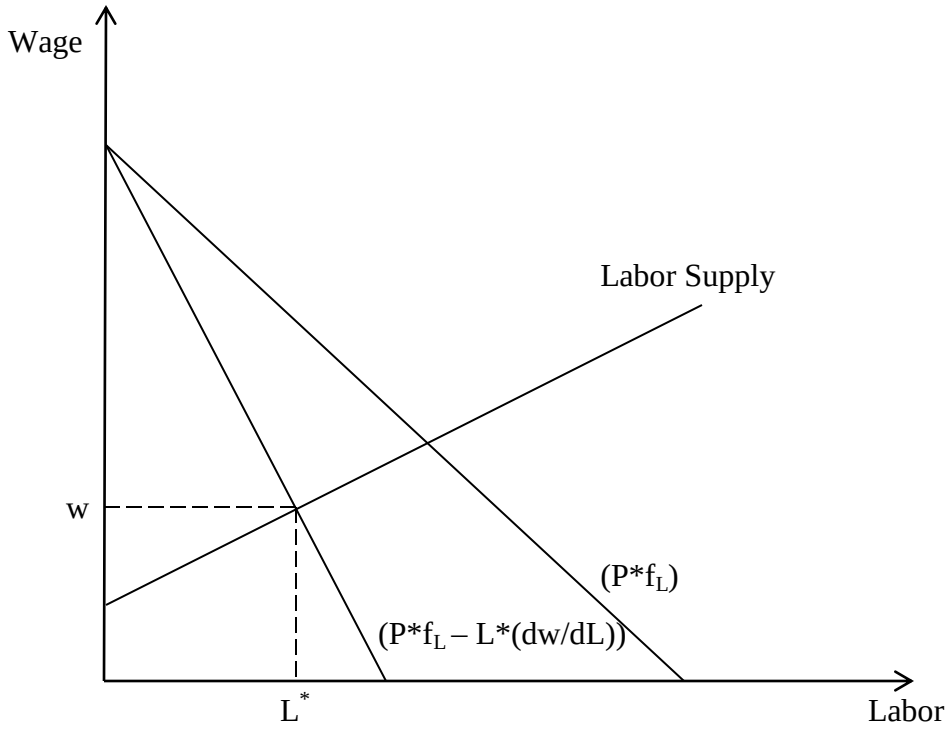

Figure A.4: Labor Market with Market Power.

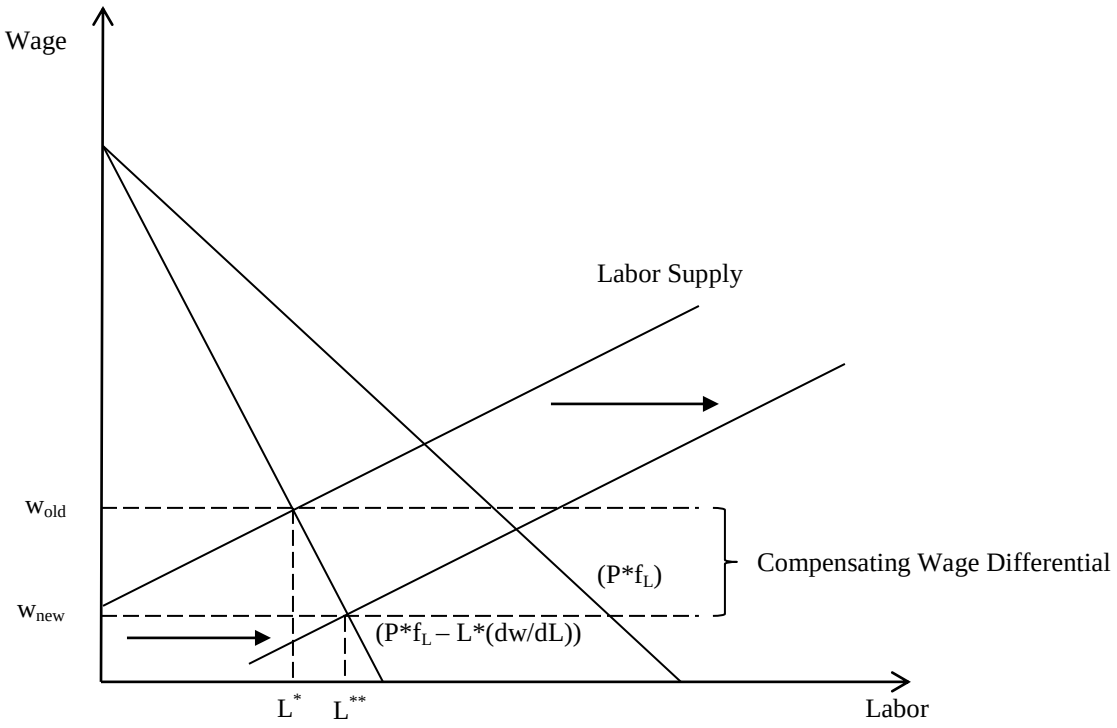

Figure A.5: Labor Market with Market Power, $c(\rho)$.

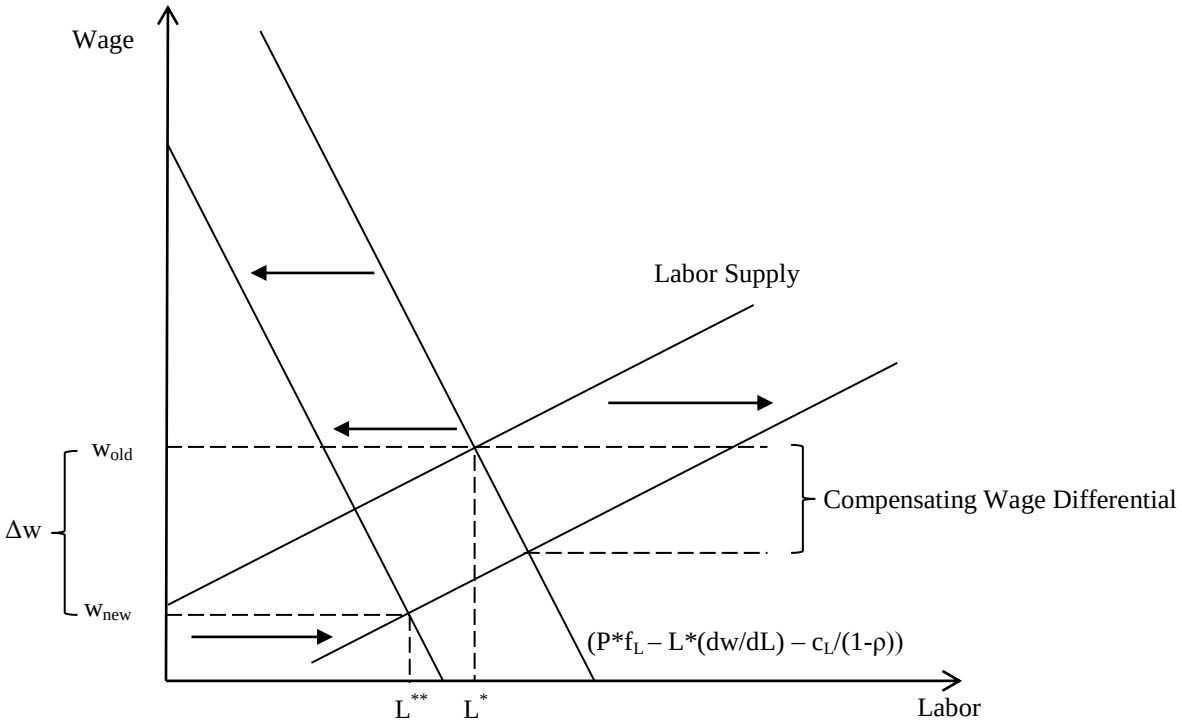

Figure A.6: Labor Market with Market Power, $c(\rho,L)$.