Using Income Tax Changes to Identify the Value of a Statistical Life

David Powell*
MIT Economics Department

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Abstract

A vast literature has focused on estimating the value of a statistical life (VSL) by looking at the tradeoff between wages and occupational risk. Many authors have recognized the inherent identification issues with this empirical strategy, likely biasing the estimates downward. This paper recognizes that compensating differentials are a function of the income tax rate and uses this observation to introduce a new methodology for estimating compensating differentials with a specific application to the estimation of the VSL parameter. When taxes change, the pre-tax wages of risky jobs should shift relative to the pre-tax wages of safe jobs in a manner proportional to the value of a statistical life. The benefits of this insight are that I can control for fixed effects without using industry-specific changes in risk as a source of identification. This strategy yields VSL estimates between \$50 million and \$75 million, an order of magnitude higher than the previous literature.

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1 Introduction

The theory of compensating differentials has been well-established since the writings of Adam Smith in 1776. Non-wage amenities should impact the wages of workers, and much empirical research has been dedicated to studying the relationship between wages and amenities. However, there is little research studying how compensating differentials interact with income and wage taxes. While non-wage amenities are untaxed, the compensating differential is subject to taxation. Consequently, observed compensating differentials are a function of tax rates. This insight suggests an innovative method for estimating compensating differentials. When tax rates change, the pre-tax wages of jobs with different non-wage amenities must shift differentially. This paper details the power of this empirical strategy by applying it to the estimation of the relationship between wages and occupational hazards.

A vast literature has studied the existence and magnitude of compensating differentials with respect to occupational hazards. Individuals working in risky jobs should be compensated for the additional risk with higher wages. This literature has focused on the effect of work-related fatality rates as a means of estimating the value of a statistical life (VSL).

The value of a statistical life is an especially important parameter for government agencies deciding on policies which tradeoff risk and money. The U.S. Environmental Protection Agency (EPA), the U.S. Department of Agriculture (USDA), the Department of Transportation, and the U.S. Food and Drug Administration (FDA) have all used VSL estimates to decide on the efficiency of policy implementations. Because individuals do not directly purchase public goods such as "cleanliness of air" which reduce mortality risks, it is difficult to determine how much they would be willing to pay for a given reduction in mortality risk. Instead, it is necessary to infer their willingness-to-pay from other decisions. U.S. Environmental Protection Agency [1997] studies the benefits and costs of the Clean Air Act, reporting VSL estimates from 26 different studies. Twenty-one of these studies used the wage-risk tradeoff. These studies relate variation in job risk to differences in wages. The EPA still refers to these 26 studies for the central basis of their VSL estimates.

The existing literature primarily studies the cross-sectional relationship between wages and risk measures by industry or occupation. This variation is extremely problematic as workers and firms simultaneously select both their wage rate and risk level. Hwang et al. [1992] consider a theoretical model of job choice with cross-sectional variation in risk and wages and conclude that the bias due to skill heterogeneity could be substantial. High-skilled workers choose jobs which offer lower risk and higher wages. They suggest, "Unfortunately, techniques for correcting the effects of unobserved productivity heterogeneity are not generally applicable." Furthermore, injury and fatality rate measures are extremely noisy and measurement error concerns are likely first-order. This paper introduces a methodology which addresses these problems by using variation that is plausibly orthogonal to them.

While the VSL literature has typically ignored or misspecified the role of income taxes in the wage-risk tradeoff, this paper focuses on the interaction of job riskiness and income tax rates. This approach offers an innovative means of identifying the true wage-risk tradeoff. When taxes increase, untaxed amenities such as occupational safety become relatively more valuable. To the extent that occupational safety is fixed by industry, the pre-tax wages of dangerous jobs must increase relative to the pre-tax wages of safe jobs. This relative increase is directly related to the underlying VSL. The main contribution of this paper is a new route for identification of the value of a statistical life. Tax changes alone can identify the magnitude of the compensating differential without relying on industry-specific changes in risk. The main benefit of this insight is a more plausibly exogenous source of identification to estimate compensating differentials. Specifically, I am able to (1) control for fixed effects (2) without identifying off changes in risk over time.

The results of this paper are consistent with the idea that the literature's VSL estimates are biased downward. I find VSL estimates between $50 million and $75 million. These numbers are well-beyond any estimate suggested by the existing literature. Given the long history of more modest estimates in this literature, it is appropriate to treat these larger findings with skepticism concerning their wider implications for wages. Therefore, at the end of this paper, I benchmark the estimates by calculating the magnitude of the compensating differentials that should be observed in the economy and find that the estimates are not implausible.

The next section of this paper briefly summarizes the existing literature on estimating the empirical tradeoff between wages and risk. Section 3 presents a basic model illustrating the interaction between wages, risk, and taxes. Section 4 details the data and sample. Section 5 describes the empirical strategy. Section 6 presents the results and Section 7 concludes.

2 Literature Review

2.1 Value of a Statistical Life

2.1.1 VSL Estimates

A vast literature has studied the estimation of the value of a statistical life. Viscusi and Aldy [2003] provide a thorough review and summarize that the majority of the studies estimate the value of a statistical life between \$5 million and \$12 million, with a median value of \$7 million. The value of a statistical injury has been estimated as between \$20,000 and \$70,000.

Adopting similar notation as Viscusi and Aldy [2003], the typical VSL specification is as follows:

$$w_{ij} = \alpha + X_{ij}'\gamma + \beta_1 p_j + \beta_2 q_j + \beta_3 q_j WC_i + \epsilon_{ij} \quad (1)$$

where w_{ij} is the wage of worker i in industry j. X_{ij} is a set of control variables, p_j is the fatality rate, q_j is the injury rate, and WC_i is the worker's compensation replacement rate. While this exact specification is not frequently employed, the general idea is to estimate the observed relationship between wages and risk rates.

Since Viscusi and Aldy [2003], there have been a few important studies that I should highlight. First, Black and Kniesner [2003] review the differences between the fatality rates provided by the National Traumatic Occupational Fatality Surveillance System (NTOF) and Census of Fatal Occupational Injuries (CFOI). The primary purpose of this study is to look at how measurement error can impact the magnitude of VSL estimates, but its findings illustrate some of the difficulties associated with the traditional VSL specification. Using different samples of workers, different fatality measures, and different sets of controls, they end up with 32 different permutations of the typical VSL regression. They find a positive coefficient only 16 times. Proponents of the typical VSL specification could convincingly argue that it is unfair to weight each of those regressions equally since some are "better" than others. However, it is important to note the instability of these VSL estimates.

Viscusi [2004] uses confidential data from the Census of Fatal Occupational Injuries (CFOI), which provides fatality numbers by industry and occupation. Viscusi argues that this severely diminishes concerns of attenuation bias due to measurement error. He estimates the value of a statistical life at \$4.7 million and the value of a statistical injury at $9,570.

In a recent working paper, Kniesner et al. [2007] control for individual fixed effects because of concerns that skill heterogeneity severely biases VSL estimates. They note that there are two reasons that an individual might experience different fatality probabilities over time. First, the individual might change jobs. Second, the fatality rate of the industry-occupation might change. They argue that the first source of variation is endogenous and limit the exogenous variation of their fatality variable to the second source. In the end, they report VSL estimates between $5.5 million and $7.5 million.

2.1.2 Theory

The theory behind compensating differential estimation is also well-explored and difficult to summarize concisely. Many papers, such as Hwang et al. [1992], detail the problems of estimating compensating differentials in the presence of skill heterogeneity and are, in fact, the inspiration for the type of empirical strategy introduced in this paper.

Other work focuses on the interpretation of compensating differential estimates. Rosen [1986] shows that the estimation of a compensating differential by measuring the effect of an amenity on a wage provides the valuation of the *marginal workers*. Workers with low VSL numbers will tend to work in dangerous jobs while workers with high VSL numbers will tend to work in safe jobs. The wage is set by the marginal worker, who may not be representative of the general population. This estimate is useful, but it cannot necessarily be extended to every worker in the economy. The Rosen critique is relevant depending on the interpretation of compensating differential estimates. The marginal worker's valuation may be the exact estimate that we are interested in. A problem occurs only when we extrapolate the results to the entire population.

The Rosen critique is applicable to my empirical strategy as well. I am still estimating the marginal worker's valuation of safety. In a panel data context, however, the Rosen model offers slightly different implications, but it will be helpful to delay this discussion until later (section 5.7).[1]

[1] Shogren and Stamland [2002] make a similar point as Rosen by building a model where the marginal worker in the dangerous industry is the one that most values safer working conditions. The implications for my strategy should be similar to those that I describe for the Rosen model.

2.2 Taxes

At its core, this topic is about taxation and intersects with the literature examining the effects of income and wage taxes. It is well-known that wage taxes distort the demand for non-wage amenities. Many papers, such as Gruber and Lettau [2004], study the provision of these amenities as a response to this tax subsidy. When tax rates change, the relative price between taxable income and non-taxable amenities shifts.

This paper adds a key contribution to this literature. In my context, is is plausible that firms respond to higher taxes by increasing safety standards to reduce fatality and injury risks, and I will discuss how my empirical strategy is robust to this possibility later. However, on a basic level, some jobs are simply riskier than others by their inherent nature. Thus, firms must respond on a different margin than the provision of the non-wage amenity. This paper examines how pre-tax wages respond when a non-wage amenity is prohibitively costly to provide.

3 Model

A very simple model can illustrate how the value of a statistical life is identified from tax changes. In this model, workers maximize utility which is a function of consumption. Occupational safety is also valued, and I include it as an equivalent variation term to create an obvious VSL parameter. Thus, the marginal worker faces the following maximization problem:

Let $c =$ consumption

$y =$ non-labor income (assumed exogenous)

$r =$ occupational risk

$w(r)$ is the market wage function

$T[z]$ is tax burden for total income z

$\phi(r)$ is the implicit cost of risk with $\phi' > 0$

The marginal worker faces the following maximization problem:

$$\max_r U(c - \phi(r)) \quad \text{s.t.} \quad c = w(r) + y - T[w(r) + y] \tag{2}$$

Table 1: Comparison of Approaches

	Existing Literature	This Model
$\frac{\partial w}{\partial r} =$	$\frac{\partial \phi}{\partial r}$	$\frac{\frac{\partial \phi}{\partial r}}{1-T'}$
$\frac{\partial^2 w}{\partial r \partial(\frac{1}{1-T'})} =$	0	$\frac{\partial \phi}{\partial r}$

$$\implies \max_r U\left\{w(r) + y - T[w(r) + y] - \phi(r)\right\} \tag{3}$$

First order condition:

$$\left\{\frac{\partial w}{\partial r}[1 - T'] - \frac{\partial \phi}{\partial r}\right\} \times U' = 0 \tag{4}$$

$$\implies \left\{\frac{\partial w}{\partial r}[1 - T'] - \frac{\partial \phi}{\partial r}\right\} = 0 \tag{5}$$

The first order condition defines the wage function which keeps the marginal worker indifferent between jobs with different risk levels.

Rearranging, we find

$$\frac{\partial w}{\partial r} = \frac{\frac{\partial \phi}{\partial r}}{1 - T'} \tag{6}$$

Now, take the derivative of equation (6) with respect to $\frac{1}{1-T'}$:

$$\frac{\partial^2 w}{\partial r \partial(\frac{1}{1-T'})} = \frac{\partial \phi}{\partial r} \tag{7}$$

Table 1 summarizes how this model differs from the traditional literature which typically ignores the role of taxes. The first row shows that the first derivative, which is typically estimated by the literature, is not correctly scaled because it is missing the marginal net-of-tax rate. The second row shows that the existing literature assumes that the observed compensating differential does not change when the tax rate changes.

$\frac{\partial \phi}{\partial r}$ is the parameter of interest. I will make the traditional assumption that $\phi(r) = \beta r$. Then, I am left with my identifying equation:

$$\boxed{\frac{\partial^2 w}{\partial r \partial(\frac{1}{1-T'})} = \beta} \tag{8}$$

The important variable, then, is the interaction of the tax rate and risk. My empirical strategy will look at how the wages of risky jobs change relative to the wages of safe jobs when tax rates change. As the model suggests, the variable of interest is the observed risk rate interacted with the tax rate.

3.1 Discussion of Model

3.1.1 Endogeneity of Risk to Taxes

It is certainly possible that risk levels are themselves responsive to taxes, implying that risk is an endogenous variable in the model above. A more comprehensive model could factor in the cost to the firm of improving occupational safety and weigh these costs against the higher wages. However, I will not be using changes - endogenous or exogenous - in risk for identification so this is not a central concern. A rearrangement of equation (8) gives $\beta = \frac{\frac{\partial w}{\partial r}}{\partial(\frac{1}{1-T'})}$. Notice that this equation is entirely silent on endogenous changes in risk. If a dangerous industry responded to taxes by becoming relatively safer, this action would *not* affect $\frac{\partial w}{\partial r}$. This endogenous response is only problematic if risky industries could respond to higher tax rates by becoming as safe as the safest industries. Then, $\frac{\partial w}{\partial r}$ would not be identified in multiple tax regimes and my strategy would not work. The first stage in my empirical strategy implicitly tests for this scenario. Identification of the VSL parameter simply requires (1) cross-sectional variation in risk and (2) time variation in the marginal tax rate.

3.1.2 The Role of Taxes

Most previous work in this literature has failed to correctly use the marginal tax rate. Very few compensating differential studies even consider taxes. Viscusi and Aldy [2003] note this, "While many studies have included pre-tax wages as the dependent variable, this would not likely bias the results significantly so long as workers' income levels and tax rates do not differ substantially." This statement is misleading. Even if all workers had the same tax rate, it would affect the magnitude of the estimates, requiring the VSL results to be scaled downward. To my knowledge, only the following studies have used after-tax wages: Moore and Viscusi [1988]; Dillingham et al.

[1996]; and Meng and Smith [1999]. However, even these studies use the *average* pre-tax wage. This is equivalent to adjusting each pre-tax wage by the person's average tax rate. But the "additional" wages paid for the riskiness of the job are taxed at the marginal rate.

It is important to clarify, however, that the innovation of this paper is not to simply adjust existing VSL estimates by accounting for tax rates. Adjusting for marginal tax rates is relatively unimportant if the compensating differential itself is difficult to identify. Instead, I use tax rate changes as new source of variation.

3.1.3 Identification Implied by Model

The model illustrates how the interaction of taxes and risk identifies the VSL parameter. While we may be skeptical of learning about compensating differentials by comparing the wages of high risk industries to the wages of low risk industries, we may be more willing to consider tax schedule changes exogenous since individuals take these changes as given. My strategy relies on tax variation while holding industry-specific risk constant over time.

The main insight of this paper is that compensating differentials can be identified without using changes in risk. Cross-sectional variation in risk is a very suspect route for identification. Through job choice, workers simultaneously select both their wage and risk. More recent studies employ panel data to control for fixed effects. The two possible sources of variation in these studies are (1) changes in job riskiness over time and (2) individual decisions to change industries. Neither of these sources of variation circumvents the problems caused by individuals simultaneously choosing their wage and risk. Individuals who increase their marginal productivity through experience may change jobs. The additional compensation they receive in the new job is potentially a combination of higher wages and lower risk. Furthermore, industry-level changes in risk are unlikely to be exogenous and potentially signal other changes in the industry which correlate with its wage levels.

Finally, risk measures are plagued with multiple sources of measurement error. First, the risk measures are themselves mismeasured. Second, risk measures represent risk probabilities at an aggregate level and are not accurate for each person in an industry.

Using exogenous changes in the tax schedule overcomes these problems. Individuals and industries take the tax schedule as given. Pre-tax wages should respond to tax schedule changes based on job risk and the implicit cost of this risk to workers. The important point here is that this is true *without any industry-specific changes in risk*. This source of identification allows me to control for fixed effects without relying on changes in risk over time. Put slightly differently, I only use cross-sectional variation in risk, but I am able to control for any cross-sectional correlation between omitted variables and risk completely flexibly through the use of fixed effects.

There are two necessary levels of variation in this model - (1) cross-sectional variation in risk, (2) time series variation in tax rates. This coincides perfectly with the premise of this paper. When tax rates increase, we want to compare how wages of risky jobs change relative to the wages of safe jobs.

For a better understanding of this identification strategy, let us temporarily assume that there is no measurement error in the risk measures (the risk rates are the exact risk faced by the individual) and there is a flat tax which changes over time.

We can think of wages as defined by the following equation:

$$w = \gamma + \beta \left(\frac{1}{1 - T'} \right) Risk + \underbrace{\alpha}_{unobserved} + \epsilon \tag{9}$$

Wages are a function of risk where the relationship (β) between wages and risk is itself a function of $\frac{1}{1-T'}$. There is an unobserved component, α which is correlated with risk. This unobserved component ensures that identification based on variation in risk leads to biased estimates as shown in the following equation:

$$\implies \frac{\partial w}{\partial Risk} = \beta \left(\frac{1}{1 - T'} \right) + \frac{\partial \alpha}{\partial Risk} \tag{10}$$

In a cross-sectional regression, the risk variable is correlated with the error term since α is unobserved. Using panel data allows one to include a fixed effect, but any change in risk is related to a change in α, again biasing the results. The central problem here is not the presence of α, but that *identification relies on variation which correlates with* α.

The identification of my empirical strategy suggests that under these assumptions (no measurement error, flat tax rates), I could compare $\frac{\partial w}{\partial Risk}$ under different tax rates to estimate the VSL. This suggestion is surprising given that I have illustrated how $\frac{\partial w}{\partial Risk}$ itself is biased

Using equation (9) again, we see the following:

$$\frac{\partial^2 w}{\partial Risk \partial(\frac{1}{1-T'})} = \frac{\partial \beta}{\partial(\frac{1}{1-T'})} + \frac{\frac{\partial \alpha}{\partial Risk}}{\partial(\frac{1}{1-T'})} \tag{11}$$

The first term ($\frac{\partial \beta}{\partial(\frac{1}{1-T'})}$) is the term of interest, how the compensating differential changes based on the tax rate. The second term is how the omitted variable bias changes with respect to the tax rate. The benefit of this approach is evident. By including fixed effects and not identifying off changes in risk, the strategy is unaffected by the correlation between risk and the omitted variables. Instead, the only term left over is the relationship between the omitted variable *bias* and the tax rate.

There are reasonable stories that would imply that the omitted variable bias is potentially correlated with the tax rate, depending on the type of specification employed. For example, the main specifications in this paper use repeated cross-sections and include industry fixed effects. We can think of α as representing the skill of the industry. When tax rates increase, risky industries could be willing to sacrifice some of the skill in their labor force to pay lower wages. These types of stories would argue that workers may re-sort themselves across industries based on their unobserved skill levels whenever taxes change.

There are a few reasons that I think the empirical approach of this paper is valuable despite this possibility. First, the existing literature actually identifies off risk variation, while any bias in my empirical strategy is second-order. Second, in general, we would think that $\frac{\frac{\partial \alpha}{\partial Risk}}{\partial(\frac{1}{1-T'})}$ is negative. When taxes increase (i.e. $(\frac{1}{1-T'})$ increases) the relatively high-skilled workers leave the risky jobs, implying that $\frac{\partial \alpha}{\partial Risk}$ decreases. This biases the results downward. Given that I find such large results, it seems less likely that this is causing a problem.

Third, I show in my robustness checks that specifications including individual-level fixed effects produce similar results. With individual fixed effects, the "bias" term would be non-zero if and only if workers in dangerous industries experience a different shock to their unobserved skill levels relative to workers in safe industries and this differential skill shock is correlated with changes in the tax schedule. This story seems less plausible, though I will discuss the possibility that exogenous trends are driving the results in my robustness checks. I find no evidence that these trends are causing problems.

4 Data

4.1 Wages and Taxes

I use the 1983-2002 March CPS which provides individual-level data on income, hours worked, industry, and other characteristics. These years were chosen because the Census industrial coding system used by the CPS stays relatively stable over the time period. I calculate tax rates by using NBER's Taxsim program. This program takes information on different forms of income, number of dependents, and filing status. It provides state and federal marginal taxes and the marginal FICA tax rates for each household. The wage income variable in the CPS is pre-tax wage income[2] for the previous year. I divide this quantity by the hours worked[3] in the previous year to get my wage variable. The resulting sample covers 1982-2001.

4.2 Workers' Compensation

The U.S. Chamber of Commerce publishes a series *Workers' Compensation Laws* which provides detailed parameters regarding each state's workers' compensation coverage. I coded the income benefits for temporary total disability - the percent of wages, the minimum benefit, and the maximum benefit. I calculate each observation's average weekly wage and subsequently find the potential benefit level upon injury given that wage. I divide this benefit level by the weekly after-tax wage to get the replacement rate. It is unclear how to define the after-tax wage in this circumstance. I chose to adjust the pre-tax wage by the marginal tax rate (as opposed to the average tax rate). Since my identification strategy focuses on changes in marginal tax rates, the central concern is that these tax changes are also impacting the replacement rate. To remain on the safe side, I chose to use the same variable in the replacement rate.

I also calculate each observation's replacement rate in cases of fatal injury. Both of these rates are important since I look at both injury and fatality rates in this paper. The "death benefit" replacement rate, however, must be treated differently

[2] I add "wage and salary income" and "non-farm business income" to get wage income. Non-farm business income is primarily for self-employed workers. While I am excluding the self-employed in my analysis, some workers may earn extra money through this variable. Since the reported hours variable should include this work, I must include both types of income to keep the numerator and denominator consistent.

[3] As customary with CPS and Census data, I define "hours worked" as "weeks worked last year" $\times$ "usual hours worked per week (last year)."

since it is not relevant for workers that are single with no children. In these cases, I simply force the effect of this replacement rate to be 0.

4.3 Fatality Rates

The National Institute for Occupational Safety and Health collected fatality data between 1980 and 2001[4] through the National Traumatic Occupational Fatality Surveillance System (NTOF). The NTOF records fatalities listed as work-related on death certificates which are coded as externally-caused for those that were 16 or older. These fatalities are then categorized by industry. The NTOF typically provides these data at the 1-digit SIC level. There are only ten such divisions (including agriculture/forestry/fishing and public administration, both of which are rarely used in this type of analysis), severely limiting the amount of useful variation and reducing any confidence that such a fatality rate accurately describes the true risk experienced by the workers.

By request[5], I received more detailed fatality data from the NTOF system. It was provided for 49 separate industry categories. To give an example of the importance of this breakdown, the aggregate data set reports one fatality rate for manufacturing. The more detailed data lists fatality rates for 16 different categories within the manufacturing industry.

I divide the fatality numbers by the total number of hours worked in that industry-year according to the March CPS to arrive at my fatality rate variable.

Figure 1 shows the trend in fatality rates over the time period studied in this paper. There is a noticeable downward trend throughout my sample.

Figure 2 shows the trends in fatality rates by initial risk. While each set of industries is experiencing a decrease in risk, the trend in Figure 1 is driven primarily by the most dangerous industries becoming safer.

To illustrate the magnitudes and variation in these data, I list the fatality rates for the top 10 and bottom 10 industries during 1982-2001 in Table 2.

The NTOF is not without its faults. Using death certificates as the only raw data source leads to an undercount of the number of fatalities. This undercount can be estimated by comparisons to the Census of Fatal Occupational Injuries (CFOI). The Bureau of Labor Statistics currently maintains the CFOI which is a highly-regarded

[4]The 2001 data exclude fatalities resulting from the September 11 terrorist attacks.
[5]Special thanks to Suzanne Marsh.

source for the number of fatalities by industry. However, the CFOI did not begin until 1992. Due to the relatively small federal tax schedule changes between 1992 and 2001, this paper requires fatality data for the pre-1992 period.

The CFOI utilizes multiple sources including death certificates, obituaries, and OSHA reports. Leigh [2000] reports that 72% of CFOI fatalities in 1992 would be "found" using only death certificates. This issue is extremely important in the scaling of the VSL estimates later in this paper. I compared the CFOI and NTOF rates for 1992-2001 as shown in Figure 3. The NTOF recorded 80.6% as many fatalities as the CFOI. This number was extremely consistent over time. The values ranged from 78.5% to 83.3%, suggesting that the overall average can be assumed for the pre-1992 period. It is also worth noting that the correlation by NTOF industry-year between NTOF and CFOI fatality rates over this time period is 0.95. This correlation suggests that there is no systematic bias by industry and that scaling the resulting estimates by 0.8 should be a valid approach to adjust for the NTOF undercount.

While it is customary in the literature to use fatalities per 100,000 equivalent full-time workers, I - unless otherwise noted - use fatalities per 100 equivalent full-time workers to keep the units the same as the injury rate variable.

4.4 Injury Rates

The Bureau of Labor Statistics has recorded injury rates by detailed industry since 1976 as part of their series *Occupational Injuries and Illnesses in the United States by Industry*. They survey about 250,000 firms every year. Over 1982-2001, two variables are consistently recorded - the total injury (and illness) rate and the rate of injuries (and illnesses) resulting in 1+ days away from work. I focus on this latter variable because it is more commonly used in the literature. Before 1992, these numbers included fatalities. Since fatalities make up an extremely small percentage of all injuries, it should be acceptable to merge the pre-1992 and 1992-2001 data together. Injury rates are reported to one decimal point. Even the injury rates of the highest fatality rate industries would be unaffected by excluding fatality rates at this level. I also show results for an early sub-sample which does not cross this 1992 data change and the estimates appear to be unaffected. Note that the BLS simultaneously collects hours data from the surveyed firms and constructs injuries per 200,000 hours (or 100 full-time equivalent workers).

Injury rates are provided at a combination of 2-, 3-, and 4-digit industries, depending on the industry. Overall, over 800 industries are listed, classified by the Standard Industrial Classification (SIC) system. However, since the CPS uses the Census coding system, I had to merge the two data sets with a crosswalk which required aggregating many of these industries. In the end, I am left with about 180 separate industries. The aggregate injury rate is charted by year in Figure 4.

Table 3 shows the 10 most dangerous industries and the 10 least dangerous industries ranked by the overall injury rate. The correlation between the injury and fatality rates in my data is 0.39.

4.5 Sample

My sample includes all workers in the private labor force ages 25-55 that are not self-employed. I exclude all agricultural industries. This leaves me with 757,647 observations. I drop 45,365 observations with allocated wage income, hours worked, or weeks worked. I drop 6,622 observations because they have wages below $2 or above $200 in 2001 dollars. I exclude 19,813 observations because I attribute a workers' compensation replacement rate (injury or fatality) over 200%[6] to them. Finally, I only use workers who are listed as the head of the household or the spouse of the head of the household, which excludes 81,490 observations. I am more confident about the tax rates of household heads and their spouses because it is otherwise difficult to determine the tax filing situation. I am left with 604,352 observations.

Table 4 presents summary statistics for the entire sample and sub-samples based on overall risk for the entire 1982-2001 period. Comparing across sub-samples, we can see that the safest jobs have the highest wages. They also tend to have the highest percentage of college educated workers.

I report full-sample estimates, but I also split the sample into two smaller time periods. Any breaking point is somewhat arbitrary, but there is a natural point. The Census industrial coding system changes in 1992, corresponding to 1991 wages. The classification changes were minor, but "crossing" this 1991 threshold requires aggregating a few industries together. Therefore, I decided to concentrate on the time periods 1982-1990 and 1991-2001.

[6]These workers tend to report very low hours worked per week.

5 Specification

5.1 Derivation of Specification

Initially, I will focus solely on the tax variable before detailing the entire specification. Let $\tilde{w}$ be the log of the pre-tax wage net of covariates and fixed effects. The existing literature estimates $\frac{\partial \tilde{w}}{\partial r} = \overline{\theta}$. My model suggest that the true specification is

$$\frac{\partial \tilde{w}}{\partial r} = \overline{\theta} + (\theta - \overline{\theta}) \tag{12}$$

where the variation in the pre-tax (observed) compensating differential is a function of the tax rate. Substituting in, we have

$$\frac{\partial \tilde{w}}{\partial r} = \phi + \beta \left(\ln(1-\tau) - \overline{\ln(1-\tau)} \right) + \tilde{\mu} \tag{13}$$

where we believe that β is negative. As the net-of-tax rate increases, the observed compensating differential should decrease.

If $\frac{\partial \tilde{w}}{\partial r}$ were known for each industry-year, this equation would be an ideal specification. I would use exogenous tax variation and estimate the parameter of interest, β. Because $\frac{\partial \tilde{w}}{\partial r}$ is unknown, I must estimate

$$\tilde{w} = Risk \times \phi + \beta \left[Risk \times \left(\ln(1-\tau) - \overline{\ln(1-\tau)} \right) \right] + \mu \tag{14}$$

The above equations include a term for the average compensating differential and models deviations from this mean as a function of the tax rate. The mean is orthogonal to deviations from the mean and, therefore, it is unnecessary to identify the mean compensating differential to consistently estimate β. This point is important because the motivation of this paper is to not identify off changes in risk. Stated differently, this paper does not believe that variation in risk identifies the mean compensating differential, ϕ. Using tax changes relative to the mean, however, circumvents this problem and focuses on heterogeneity in the compensating differential. I can transform the above equations to eliminate the term for the level of the compensating differential. Econometrically, this transformation can be viewed as a straightforward application of the Frisch-Waugh Theorem[7] where I have annihilated the risk term. Alternatively,

[7] The instruments must also be appropriately de-meaned for this statement to be true. By using within-industry tax changes, the instruments are already naturally de-meaned.

one can think of ϕ as part of the error term. Since only tax *changes* are used as a source of identification, the instruments will be orthogonal to this term.

Thus, the equation of interest is

$$\frac{\partial \tilde{w}}{\partial r} = \beta \left[\ln(1-\tau) - \overline{\ln(1-\tau)} \right] + \breve{\mu} \tag{15}$$

Equivalently, partialling $Risk$ out in (14) leaves

$$\tilde{w} = \beta \left[Risk \times \left(\ln(1-\tau) - \overline{\ln(1-\tau)} \right) \right] + \grave{\mu} \tag{16}$$

This analysis illustrates the importance of de-meaning the tax term and its equivalence to annihilating the $Risk$ term. The value of this transformation is that I have eliminated a term that I cannot separately identify. Intuitively, we can see why this works. De-meaning the tax variable is equivalent to looking at the relationship of the observed (pre-tax) compensating differential to tax variation. In this paper, I have discussed the difficulties in estimating compensating differentials such as in equation (14). This strategy focuses the analysis on estimating how tax changes impact the observed compensating differential.

I should briefly discuss the ϕ term in (13). This term measures the average observed compensating differential. The average pre-tax compensating differential estimate is, according to my model, a function of the average tax rate. My analysis uses industry-specific tax changes, but industries have different tax levels as well. I am not identifying off this variation so the variation is orthogonal to the fixed compensating differential. However, it may be more appropriate to model the pre-tax compensating differential as

$$\frac{\partial \tilde{w}}{\partial r} = \phi_j + \beta \left(\ln(1-\tau) - \overline{\ln(1-\tau)_j} \right) + \tilde{\mu} \tag{17}$$

where j represents industry. There is little reason to believe that de-meaning by the entire sample should produce different results than de-meaning by industry and is similar to simply controlling for an exogenous variable. In fact, while I present results de-meaned by industry in this paper, the results are extremely consistent when de-meaned at the entire sample and the final conclusions would be unchanged.

For the full specification, it is important to note that we also think that the observed VSL is a function of the workers' compensation replacement rate. Changes in the replacement rate are not orthogonal to changes in tax rates. When tax rates change, the after-tax replacement rate also shifts. The replacement rate should affect the compensating differential because high risk industries benefit disproportionately from workers' compensation. Thus, my specification is:

$$\frac{\partial \tilde{w}}{\partial r} = \phi_j + \beta_1 \left(\ln(1-\tau) - \overline{\ln(1-\tau)_j} \right) + \beta_2 \left(\ln(WC) - \overline{\ln(WC)_j} \right) + \nu \quad (18)$$

Translating this into an estimable equation, we have

$$\tilde{w} = \beta_1 \left(\left(Risk - \overline{Risk_t} \right) \times \left[\ln(1-\tau) - \overline{\ln(1-\tau)_j} \right] \right) + \beta_2 \left(\left(Risk - \overline{Risk_t} \right) \times \left[\ln(WC) - \overline{\ln(WC)_j} \right] \right) + \epsilon \quad (19)$$

I de-mean the risk variables by year to focus the estimation on the cross-sectional compensating differential. The results are not meaningfully changed if the risk variables are not de-meaned.

Then, substituting in for $\tilde{w}$, the entire specification is

$$\ln w = \gamma_t + \alpha_s + \lambda_j + X'\delta_t + \beta_0 \ln(1-\tau) + \beta_1 \left(\left(Risk - \overline{Risk_t} \right) \times \left[\ln(1-\tau) - \overline{\ln(1-\tau)_j} \right] \right) + \beta_2 \left(\left(Risk - \overline{Risk_t} \right) \times \left[\ln(WC) - \overline{\ln(WC)_j} \right] \right) + \epsilon \quad (20)$$

5.2 Specification

This final specification fits perfectly with the model and basic premise of this paper. My model suggests that the interaction of marginal net-of-tax rates and risk identify the value of a statistical life and injury. Therefore, the variables of interest for this paper are $Risk' \times \ln(1-\tau)$. The advantage of my approach is that I can non-parametrically account for differences between industries through the inclusion of industry-level and year fixed effects.

Because of nonlinearities in the tax schedule, different industries experience different tax changes. Consequently, I must separately account for $\ln(1-\tau)$ to ensure that all comparisons of industries with different risk levels occurs *for a given tax change*. Converting the estimated coefficients into VSL numbers will be shown in section 5.3.

In practice, I use either federal tax variation or federal *and* state tax variation. The specifications are slightly different depending on the level of variation. When I use federal tax variation only, the specification is as follows:

$$\begin{aligned} \ln w_{ijkst} = \gamma_t + \alpha_s + \lambda_j + X'_{ijkst}\delta_t + \beta_0 \ln(1-\tau_{ijkst}) \\ + \Big(\big(Risk'_{kt} - \overline{Risk'_t}\big) \times \Big[\ln(1-\tau_{ijkst}) - \overline{\ln(1-\tau)_j}\Big]\Big)' \beta_1 \\ + \Big(\big(Risk'_{kt} - \overline{Risk'_t}\big) \times \Big[\ln(WC_{ijkst}) - \overline{\ln(WC)_j}\Big]\Big)' \beta_2 + \epsilon_{ijkst} \end{aligned} \quad (21)$$

where i indexes individual, j lowest industry, k industry aggregate, s state, t year. w_{ijkst} represents the wage, τ_{ijkst} is the marginal tax rate (FICA + federal + state),[8] WC_{ijkst} refers to the worker's compensation after-tax replacement rate (for injuries, deaths, or both) , and X_{ijkst} is a vector of individual-level covariates. $Risk_{kt}$ refers to the injury rate, the fatality rate, or both. The state fixed effects in the above specification are included to account for fixed cost-of-living differences.

When I use federal and state tax variation, I must include state-industry interactions:

$$\begin{aligned} \ln w_{ijkst} = \gamma_t + \alpha_{sj} + X'_{ijkst}\delta_t + \beta_0 \ln(1-\tau_{ijkst}) \\ + \Big(\big(Risk'_{kt} - \overline{Risk'_t}\big) \times \Big[\ln(1-\tau_{ijkst}) - \overline{\ln(1-\tau)_j}\Big]\Big)' \beta_1 \\ + \Big(\big(Risk'_{kt} - \overline{Risk'_t}\big) \times \Big[\ln(WC_{ijkst}) - \overline{\ln(WC)_j}\Big]\Big)' \beta_2 + \epsilon_{ijkst} \end{aligned} \quad (22)$$

"Industry aggregate" refers to the level of variation of the risk variable. This is different depending on whether the injury rate is included or the fatality rate is included. The "lowest industry" refers to the level that the industry fixed effects control for and the level of variation for the tax instrument. There are 204 of these industries. To clarify, the value of the risk variables can be the same for several of these industries. The exogenous tax variation, however, will vary for each lowest industry.

As discussed earlier, it is important to control for the workers' compensation

[8] I only use the portion of the FICA tax rate paid by the worker and exclude the employer portion. I do this because the pre-tax wage variable implicitly includes the portion of the FICA taxes that s/he must pay, but it does not include the part paid by the employer

replacement rate and the form it takes above is the same as the specifications in the literature. As suggested in Viscusi and Aldy [2003], the replacement rate should be interacted with the risk variables because workers in high risk industries are more likely to benefit from higher replacement rates. I "attach" the injury rate to the temporary total disability replacement rate and the fatality rate to the death benefit replacement rate. I control for the replacement rate(s) related to the risk measures included. As mentioned above, I force the coefficient on the death benefit replacement rate to be equal to 0 for single workers with no children. Practically, I accomplish this by setting $\ln(WC_{ijkst}^{death}) = 0$.

The covariates are allowed to have different coefficients for each year. The returns to individual characteristics, especially education, are changing over this time period and it is important to account for these changes. I include the following covariates: 5 year age group dummies, gender dummies, education dummies, and race dummies.

β_1 is the coefficient of interest and we expect it to be negative. We should expect β_2 to be negative as well.[9]

This specification measures how the observed (pre-tax) compensating differential changes with respect to the marginal tax rate. Note the importance of demeaning the tax variable (and replacement rate). Instead of attempting to estimate both the mean observed compensating differential and how it changes with tax rates, this specification simply measures how tax rates - relative to the mean - impact the observed compensating differential. The mean compensating differential is orthogonal to this variable and does not need to be separately identified and estimated.

The existing literature tends to use cross-sectional variation for identification which potentially leads to biased estimates because risk is possibly correlated with unobserved factors. In my instruments, only cross-section risk variation will be used.

[9] Variation in the workers' compensation replacement rate could also potentially be thought of as a change in price which would identify the parameters of interest. There are two reasons that I do not attempt to interpret these coefficients in such a way. First, the death benefit is not actually received by the individual and, therefore, it is difficult to convert this into a value of a statistical life parameter since it also carries some notion of a bequest motive. Second, I specified the replacement rate in such way to make sure that the coefficient on the risk-tax interaction was not being driven by workers' compensation. I did not want identification of the replacement rate to result from choice in functional form so I made sure that the replacement rate variable implicitly had the same term as the risk-tax interaction variable. When $Risk \times \ln(WC)$ is expanded, we get $Risk \times [\ln(\text{potential benefit}) - \ln(\text{weekly wage}) - \ln(1-\tau)]$. This choice ensures that the replacement rate and tax variables are not identified separately by functional form. However, the form of the replacement rate variable is not ideal for interpretation of the coefficient as a value of a statistical life or injury parameter.

Industry fixed effects can account for the cross-sectional correlation between risk and unobserved factors in a completely flexible manner.

All standard errors are adjusted for clustering at the industry aggregate level.

5.3 VSL Calculation

The specifications are directly related to the model presented earlier. The model shows that the VSL is calculated by estimating $\frac{\partial^2 w}{\partial Risk \partial(\frac{1}{1-T'})}$. Since the dependent variable and the net-of-tax rate are in logs, I evaluate the VSL at the mean wage ($\bar{w}$) and mean net-of-tax rate ($\overline{1-\tau}$). I multiply the relevant coefficient, β_1, by 200,000 since the risk rates are expressed as injuries/fatalities per 200,000 hours. Starting from specifications (21) and (22) while holding the replacement rate constant and excluding the mean of the $\ln(1-\tau)$ term for notational simplicity,[10] we find

$$\begin{aligned}\frac{\partial w}{\partial Risk} &= \beta_1 \times 200{,}000 \times \bar{w} \times \ln(1-\tau) \\ &= -\beta_1 \times 200{,}000 \times \bar{w} \times \ln\left(\frac{1}{1-\tau}\right)\end{aligned}$$

$$\frac{\partial^2 w}{\partial Risk \partial(\frac{1}{1-\tau})} = -\beta_1 \times 200{,}000 \times \bar{w} \times (\overline{1-\tau})$$

The final formula is as follows:

$$VSL = -\hat{\beta}_1 \times 200{,}000 \times \bar{w} \times (\overline{1-\tau}) \tag{23}$$

5.4 Heterogeneity in Tax Incidence

While I have primarily focused on heterogeneity in the $\frac{\partial w}{\partial Risk}$ term due to taxes, the results of this paper can also be used to calculate heterogeneity in the incidence of the tax rate, $\frac{\partial \ln w}{\partial \ln(1-\tau)}$. For a given tax rate change, wages should be more elastic for high risk industries. I present statistics for this measure as well by reporting the implied $\frac{\partial \ln w}{\partial \ln(1-\tau)}|_{Risky} - \frac{\partial \ln w}{\partial \ln(1-\tau)}|_{Safe}$. I define the "risky" industry as the 75th percentile most dangerous industry and the "safe" industry as the 25th percentile. When both

[10] This term eventually drops out.

risk rates are used, I define the percentiles by the weighted average of the injury and fatality rate using the regressions coefficients as the weights.

5.5 Description of Instruments

The main innovation of this paper is to identify the above equation without using industry-specific changes in risk. Instead, I use tax changes. Since the marginal tax rate is a function of an individual's wage, I must instrument the tax variables. In the spirit of Currie and Gruber [1996a,b], I use a "simulated instrument" by holding a baseline sample constant and allowing the instrument to vary due to tax schedule changes only. I include two permutations of this idea - one which uses only federal tax schedule changes and a second which also uses state tax schedule changes.

To implement this strategy, I create a baseline sample for each industry and then let tax rates change based on tax schedule changes only. To illustrate this approach, I will detail how I calculated predicted tax rates for each industry in 1985 using federal tax variation only.

1. Create baseline sample (1982 CPS).
2. Inflate incomes to 1985 values.
3. Find tax rate for each person using 1985's federal tax schedule (and FICA).
4. Average marginal tax rates by industry to get predicted tax rate ($\hat{\tau}_{jkt}$).

When I focus on my later (1991-2001) sample, my baseline sample is the 1991 CPS. The resulting instrument is $\ln(1 - \hat{\tau}_{jkt})$. The variation comes solely from federal tax schedule changes. Industry-level fixed effects account for industry-specific wage level differences.

To identify off federal and state taxes, I create a baseline sample for each state-industry. Since not every state-industry is represented in each year of the CPS, the baseline sample actually uses everyone in that region-industry. I perform the same process as above, but I now include state taxes as well. The instrument is $\ln(1 - \hat{\tau}_{jkst})$. The variation comes from both federal and state tax schedule changes. In this case, I control for state-industry interactions.

The workers' compensation replacement rate must also be instrumented since the replacement rate is a function of the wage. The predicted replacement rate is formed in the exact same way that the predicted tax rates are created. The baseline sample is adjusted for inflation for each year and that state-year's workers' compensation parameters are applied to each observation. The replacement rates are calculated and averaged by industry and state to get $\ln(\widehat{WC}_{jkst})$.

The innovation of this paper is to estimate the value of compensating differentials without requiring movements in risk. Therefore, the exogenous variation in $Risk'_{kt} \times \ln(1-\tau_{ijkst})$ must come *only* from movements in taxes, holding risk constant. The corresponding instrument interacts the log of the predicted net-of-tax rate (as described above) with a risk variable that does not allow for industry-specific changes over time. An obvious candidates for this risk proxy is the average risk for an industry over 1982-2001.

The resulting set of instruments (using federal tax variation only) are the following:

1. $\ln(1-\hat{\tau}_{jkt})$
2. $\overline{Risk}'_{k.} \times \ln(1-\hat{\tau}_{jkt})$
3. $\overline{Risk}'_{k.} \times \ln(\widehat{WC}_{jkst})$

Because of the inclusion of industry fixed effects, the tax variables are all implicitly de-meaned in each variable. Focusing on the risk-tax interaction,

$$\overline{Risk}'_{k.} \times \ln(1-\hat{\tau}_{jkt}) - \overline{\overline{Risk}'_{k.} \times \ln(1-\hat{\tau}_{jkt})} = \overline{Risk}'_{k.} \times \left(\ln(1-\hat{\tau}_{jkt}) - \overline{\ln(1-\hat{\tau})}_j\right) \quad (24)$$

5.6 Sources of Variation

My empirical strategy relies on time variation in taxes and cross-sectional variation in risk. The tax schedule is non-linear and we typically think of identification of tax changes as stemming from differential changes in the non-linearities. To clarify the source of identification in this paper, it is useful to point out that my empirical strategy does not require such differential changes. The specifications above would still be identified under a flat tax regime as long as the tax rate changed over time. The nonlinearities do mean that tax schedule changes affect different industries differentially, but I control independently for the tax rate. Thus, the coefficient of interest can be interpreted as how the wages of riskier jobs change relative to the wages of safer jobs *for a given tax rate*.

The Tax Reform Act of 1986 is the most significant tax change for my sample. There are also small movements in the FICA tax rate. Figure 5 shows the progression of the average marginal tax rate during the time period 1982-2001.

5.7 Industry-Level Responses

As discussed earlier, the identification strategy in this paper is theoretically robust to endogenous changes in risk as firms react to changes in tax rates. This paper estimates the effect of taxes on the compensating differential, $\frac{\partial w}{\partial r}$. The risk levels themselves are unimportant.

To clarify this point further, consider the estimation of a compensating differential with a discrete variable. Instead of a continuous risk variable, say that there are "safe" industries and "dangerous" industries. In this case, a similar estimation strategy might be to compare how safe industries react to tax rate changes relative to dangerous industries:

$$\ln w_{ijt} = \gamma_t + \theta_j + \beta_1 \big[\ln(1-\tau_{ijt}) \times \mathbf{1}(\text{safe}_{jt}) \big] + \beta_2 \big[\ln(1-\tau_{ijt}) \times \mathbf{1}(\text{dangerous}_{jt}) \big] + \epsilon \quad (25)$$

With instruments:

1. $\ln(1 - \hat{\tau}_{jt}) \times \mathbf{1}(\text{safe}_{j0})$
2. $\ln(1 - \hat{\tau}_{jt}) \times \mathbf{1}(\text{dangerous}_{j0})$

This strategy uses predicted tax rates as exogenous variation and interacts them with whether the industry was safe or dangerous in the initial period. In later periods, industries could exogenously or endogenously switch categories. This switching would affect the first stage, but the results are still valid. The only potential problem occurs when every industry becomes safe (or dangerous) when tax rates change. In this situation, there is no first stage.

This discussion has suggested that industry-level responses do not theoretically impact the interpretation of the results of this paper. In practice, it is important to note in specifications (21) and (22) and the list of instruments that the tax rates vary at a lower level than the risk rates. When state taxes are used, this point becomes even more of a concern. I use this level of tax variation simply to better account for the independent effects of taxes on industry wages. Furthermore, it provides slightly more tax variation. However, any endogenous reactions by industries *within an industry aggregate* are now potentially problematic.

In general, the industries within each "industry aggregate" experience such similar tax changes that this is not an issue. In my robustness checks, I only use tax variation at the "industry aggregate" level to show that this decision is not driving the results.

5.8 Interpretation of VSL Estimates

As discussed earlier, Rosen [1986] explains that empirical compensating differentials must be interpreted as the valuation of the marginal worker. This holds true with the above empirical strategy as well and, in general, it is the "same" marginal worker as the traditional VSL literature since I am comparing workers in dangerous industries to workers in safe industries (my risk variation is cross-sectional). There is one caveat to this description, however.

Since I am using tax changes as variation, it is possible the marginal worker changes over time. When tax rates increase, workers in risky jobs are disproportionately harmed. Pre-tax wages must increase (relative to safe jobs), but there is also the potential that workers might leave the risky jobs. The workers that leave are the ones that - relative to the other workers in the industry - have high valuations for safety (i.e. high implicit VSL). Thus, the new marginal worker has a lower VSL than the previous one.

While it difficult to directly account for this point in my empirical strategy, there are two reasons that I do not think it is too important in my context. First, this argument implies that if the marginal worker did not change that the VSL estimate would be even higher. Given the large VSL estimates I find in this paper, this seems less likely to be causing problems. Second, the argument implies that when tax rates increase, workers should leave risky jobs. I find no evidence of this. In fact, when I replicate specification (21) with employment as the dependent variable, I find no significant results and the relevant estimated coefficients suggest that, if anything, risky jobs *gain* workers when tax rates increase. These findings suggest that wages are fully-adjusting, keeping the marginal worker relatively constant.

6 Results

6.1 Traditional VSL Specification

For the sake of comparison with the main results of this paper, I estimated the "traditional" VSL specification. It is very likely that vast improvements could be made on this specification and identification strategy, but I simply want to give a general idea of what my data generate using more traditional sources of identification.

I did cross-sectional analysis for each year, estimating:

$$\ln w_{ik} = \gamma + X_i'\delta + Risk_k'\beta + \nu_{ik} \tag{26}$$

A summary of those results is presented in Table 5. Identification depends solely on cross-sectional variation in risk. The implied value of a statistical life is very unstable and is actually estimated to be negative more often than positive. This set of results is generally consistent with the findings of Black and Kniesner [2003]. The value of a statistical injury is consistently negative, which is surprising given previous findings in the literature.

6.2 Main Results

Turning to the estimation of this paper's specification, the OLS results shown in Table 6 are extremely hard to interpret given that tax rates are endogenous, replacement rates are endogenous, and some of the identification originates from industry-specific changes in risk.

Table 7 presents the first stage for the entire sample when both risk rates are included, using federal tax variation only. With up to 5 endogenous variables and instruments, it can be difficult to determine if a specification is identified. For example, if one instrument were highly-correlated with all the endogenous variables, simple F-statistics might suggest that the equation is identified when it is clearly not. In all of my tables, I include Shea's Partial R^2 statistics. This statistic measures the explanatory power of the instruments for each endogenous variable independent of the other variables. The first stage is relatively strong for all the variables.

It is important to realize that the strength of the first stage varies based on the sample used. Since most of the tax changes occur in the early part of the sample, the first stage should be much stronger for the 1982-1990 sample than the 1991-2001 one. Table 8 illustrates this fact. The Shea's R^2 statistics decrease substantially for the later sample. Consequently, we should expect the estimates from the early sample to be much more precise than those from the later one.

The main results of this paper are shown in Tables 9-14. These tables contain a lot of information, including the estimated coefficients on several variables, the corresponding VSL estimates, and the implied heterogeneity in tax incidence. The tax incidence heterogeneity numbers compare the elasticity of the wage with respect to the marginal net-of-tax-rate for the 75th percentile most dangerous industry to the 25th percentile for the last year of the relevant sample (1990 or 2001).

Tables 9 and 10 reports the VSL estimates for the full (1982-2001) sample. The IV results generally imply very large VSL estimates. When both risk measures are included, the effect of each one decreases, as we would expect. The results suggest a VSL of around \$60 million and a value of a statistical injury of around \$500,000. The implied heterogeneity in tax incidence is estimated as between 0.1 and 0.4.

The 95% confidence intervals are fairly large. Importantly, however, they rule out very low VSL estimates. For example, Viscusi [2004] estimates a VSL of \$4.7 million, but the 95% confidence interval extends below 0. The estimates in Tables 9 and 10 can rule out such values.

The results are very similar when I focus on the 1982-1990 time period as shown in Tables 11 and 12. Notably, the 95% confidence interval is now tighter and rules out VSL estimates higher than the literature has estimated. As mentioned previously, there are large tax schedule changes during this time period. The first stage strength is much weaker after 1990.

Tables 13 and 14 uses the later time period, 1991-2001. The first stages now are *very* weak. However, the federal tax variation point estimates are consistent with those of the earlier time period. When state tax variation is also employed, the estimates increase dramatically, though the confidence intervals include a wide range of values. In general, I consider the results of this later period as not independently persuasive, but consistent with the other sets of results. Nothing in these tables contradicts the findings of Tables 9 to 12.

6.3 Trends

A large literature details the growth of wage inequality during the time period of my sample. A major concern of the analysis in this paper is that wage trends are driving the results. When taxes decrease - such as after the enactment of the TRA86 - my variable of interest may simply pick up on existing wage trends.

The specific story that I am concerned about assumes that risky jobs are relatively unskilled jobs. Then, due to wage trends, the wages of the risky jobs dropped relative to the wages of the safe jobs. Taxes decrease in the middle of the sample and my specification attributes the relative drop of the wages of the risky jobs to this tax decrease.

My results should be robust to this critique for three reasons. First, I let the return to individual covariates change over time. Goldin and Katz [2007] state, "The majority of the increase in wage inequality since 1980 can be accounted for by rising educational wage differentials..."

Second, I use all tax changes as sources of exogenous variation. While TRA86 did decrease taxes, there are also periods where taxes increased during my sample. The 1991-2001 estimates are more imprecisely measured than the earlier period estimates, but they still suggest very large effects during a period where tax rates increased on average.

Third, to drive the results of this paper, these wage trends must occur *within a given tax rate*. My specification separately controls for the after-tax rate and, therefore, implicitly compares industries with similar initial wages. I am not suggesting that the after-tax rate is an adequate proxy for wage trends if one wanted to study wage trends specifically. Instead, because identification originates from tax changes, any spurious correlation due to wage trends must occur within a given tax change.

The fact that I am implicitly comparing industries with similar initial wages is consistent with the robustness of the VSL estimates when initial wages are more explicitly accounted for.

In Table 15, I summarize a series of regressions which controls for initial wages. Each block represents the same regressions seen in the previous tables, but I only report the resulting VSL estimates for the sake of simplicity. In the first two blocks, I control for the 1982 wage interacted with year dummies.[11] These variables let wages

[11]I drop 1982 in the subsequent analysis to avoid a simultaneity issue.

vary in each year based on initial values. The results suggest that wage trends are not driving the results for the full sample and the early sub-sample.

A linear term in wages might be too parametric. I also compare industries within wage deciles by interacting the year fixed effects with fixed effects based on 1982 wage deciles. These results are shown in the third and fourth blocks. Again, the VSL estimates are consistent with the earlier results.

Importantly, the different controls utilized in Table 15 soak up all the predictive power of the instruments for the net-of-tax rates. Shea's Partial R^2 for $\ln(1-\tau)$ in these regressions is always between 0.0001 and 0.0002. The fact that the VSL estimates stay the same under these circumstances is fairly remarkable. The wage controls are accounting for basically all of the predictive power of the predicted tax rate, suggesting that all variation is originating from comparisons of industries with very similar initial wages and tax rates.

6.4 Robustness Checks

6.4.1 Endogenous Response of Risk

If risk rates are responding endogenously to changes in taxes, it may not be appropriate to allow the tax instruments to vary at a level different than the risk rates. In Table 16, I only use variation at the "industry aggregate" level. For injury rates, this is essentially the same as before and, in fact, the results are basically identical. This issue is more pertinent to fatality rates, but the results stay consistent.

6.4.2 Choice of Risk in Instrument

My empirical strategy interacts predicted tax rates based on the initial year with mean risk rates. Some might worry that it is more appropriate to use initial risk instead of mean risk. This choice does not appear to make much of a difference as shown in Table 17. Comparing column 1 to column 2 and then column 3 to column 4, it is apparent that the choice of risk measure in the instrument is unimportant.

6.4.3 Individual-Level Heterogeneity

Finally, it could be argued that simply accounting for industry or industry-state heterogeneity is inadequate. Instead, we might be concerned that when taxes change, the skill composition of industries change based on risk. This story suggests that when tax rates change, workers re-sort themselves across industries. To consider this possibility, I use individual-level panel data to account for individual heterogeneity.

The Panel Survey of Income Dynamics (PSID) records wage and income information for families for multiple years. I estimate the following specification for the years 1981-1994[12]:

$$\begin{aligned}\ln w_{ijkst} = \gamma_t + \alpha_s + \lambda_i + X'_{ijkst}\delta_t + \beta_0 \ln(1-\tau_{ijkst}) \\ + \left(\left(Risk'_{kt} - \overline{Risk'_t}\right) \times \left[\ln(1-\tau_{ijkst}) - \overline{\ln(1-\tau)_j}\right]\right)' \beta_1 \\ + \left(\left(Risk'_{kt} - \overline{Risk'_t}\right) \times \left[\ln(WC_{ijkst}) - \overline{\ln(WC)_j}\right]\right)' \beta_2 + \epsilon_{ijkst} \quad (27)\end{aligned}$$

The strategy is similar. I use tax rates predicted at the industry-level in the instruments. Risk rates are held constant in the instrument and *assume that the individual does not change industries*. Thus, as before, all variation originates from tax schedule changes. Furthermore, most people are included in the sample for a significant length of time. It could be argued that an individual fixed effect spanning 14 years is inadequate. Instead, I treat each 5-year span for an individual as a separate "person"/fixed effect. In other words, a person in my sample for 1981-1990 is treated as two separate people - one for 1981-1985 and one for 1986-1990.[13] The standard errors must be appropriately adjusted and I use the multi-dimensional clustering algorithm suggested by Cameron et al. [2006] to account for clustering at the individual level and the levels of the risk measures.[14]

The PSID sample is much smaller than the CPS so we would expect the estimate to be less precise. There is some evidence of this, but the results in Table 18 are consistent with the CPS results, suggesting that skill and taste heterogeneity are not biasing the results presented in this paper.

[12] More recent PSID data have not been finalized yet.

[13] The results are very robust to other permutations of this breakdown.

[14] Since the injury and fatality rates are provided at different levels, this method implies that I adjust for 2-way clustering when one risk rate is included and 3-way clustering when both are included.

6.5 Plausibility of Results

One of the possible objections to these results is that they appear to be implausibly large. The worry is that these estimates might imply much larger cross-sectional differences in income than we actually observe. This does not appear to be the case, however. For example, Viscusi [2004] calculates this his VSL estimate of $4.7 million implies that the mean yearly income is increased by a modest $190 due to occupational risk (an implicit comparison of the income at the mean risk level to a risk of 0). This number seems extremely small, suggesting that larger estimates can also produce plausible cross-sectional differences.

Any cross-sectional regression is, by construction, going to provide a better fit of cross-sectional wages than the analysis in this paper. Without considering the methodologies behind them, suggesting that some estimates are more reasonable than others relies on personal judgments about the magnitude and importance of unobserved variables such as skill.

We can get a basic idea of the importance of these unobserved factors by examining the importance of the some observed factors which correlate with skill. The traditional VSL specification essentially assumes that control variables, such as education variables, can adequately account for any correlation between skill and fatality rates. In my 2001 data, there is a correlation of -0.15 between the fatality rate and an indicator variable for whether the individual attended any college. When I do not control for my education dummy variables, the coefficient on the fatality rate changes drastically. For example, in my 2001 data, I find a VSL estimate of $2.6 million (with a standard error of $10.8 million) when I look at the cross-sectional relationship between wages and fatality rates, controlling for education, age, gender, and race. When I leave out the education dummy variables, the VSL estimate drops to -$44.9 million (standard error is $17.0 million), a decrease of $47.5 million. While this is one of the larger drops of all the cross-sections in my data for this exercise, the smallest decrease resulting from not controlling for education is still over $11 million. Given that education indicators are not perfect correlates with skill, this comparison illustrates that the magnitude of omitted variable bias is substantial. The traditional VSL specification is heavily-reliant on control variables soaking up the correlation between skill and risk. The empirical strategy of this paper purports to eliminate this omitted variable bias and, in fact, finds a jump in the estimates equivalent to the jump witnessed by a simple inclusion of two education indicators in the cross-sectional framework.

But are these numbers reasonable? Table 9 presents the implied income (wage

$\times 2{,}000$ hours) compensating differentials for different industries relative to the median risky industry (Business Services). I do this exercise with the traditional $7 million VSL estimate and this paper's estimate of $60 million. The table shows the results of this paper do not imply implausibly large cross-sectional incomes. Comparing the 90th percentile to the 10th percentile, there is only a $6000 premium for working in one of the most dangerous industries relative to one of the safest. This is despite the fact that the risk of dying is 22 times greater.

7 Conclusion

I estimate a much larger compensating differential than the existing literature using a unique source of variation. Rescaling the above results to account for the NTOF undercount and focusing on the most precise estimates, this paper finds a VSL estimate of around $70 million. I cannot rule out much smaller VSL estimates, though my most precisely measured estimates suggest the VSL is not below $30 million. These findings illustrate that omitted variables bias is potentially extremely problematic in cross-sectional analysis of this kind.

This paper also illustrates that tax changes cause differential wage changes based on non-wage amenities. I find significant heterogeneity in the incidence of tax changes, ranging between 0.1 and around 0.5.

I believe that the methodology presented in this paper provides a useful avenue for future research on compensating differentials. It is difficult to find exogenous shocks to non-wage amenities that do not impact wages. This paper suggests a strategy to estimate those compensating differentials. The existing literature on the value of a statistical life has struggled with these concerns and recognized that existing estimates are likely biased downward. I find a significant downward bias and very large VSL estimates.

A Data Appendix

A.1 Injuries

Injury data were found in the *Survey of Occupational Injuries and Illnesses* series and online at the Bureau of Labor Statistics website (www.bls.gov). I used the variable titled "Cases involving days away from work." The 1982-1988 data are categorized by the 1977 Standard Industrial Classification system while the 1989-2001 data use the 1987 Standard Industrial Classification system. The data are published at the 2-, 3-, or 4-digit level based on industry. The Census Industrial Classification system used by the CPS, however, is most related to the 3-digit SIC level. The 4-digit level (reported for manufacturing industries) is too detailed and never used while a few 2-digit industries correspond directly to the CIC system.

If no injury rate is reported for a given 3-digit industry,[15] I impute the value using the injury rate given for its 2-digit industry and the other 3-digit industries in that 2-digit category. The SOII also reports employment data, so I can calculate the injury rate of the "missing industries" within a 2-digit category.

I use a crosswalk to assign each industry to a CIC category. When one CIC industry corresponds to multiple SIC industries, I average the injury rates, weighted by employment, to the CIC level.

A.2 NTOF Fatality Rates

5.7% of fatalities are listed as "Not Classified" in the NTOF data. I calculate the percentage of classified fatalities that occur in each industry and make the assumption that the unclassified fatalities occurred randomly. Thus, an industry with 2% of all classified fatalities in 1985 will be assigned 2% of the unclassified fatalities in that year as well.

Fatality rates were merged to CIC coding system using the crosswalk provided in Appendix II of *Fatal Injuries to Civilian Workers in the United States, 1980-1995*.

[15] There are several reasons that an injury rate might be missing but, in general, these tend to be very small industries.

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B Figures

Figure 1: Fatality Rates, 1982-2001

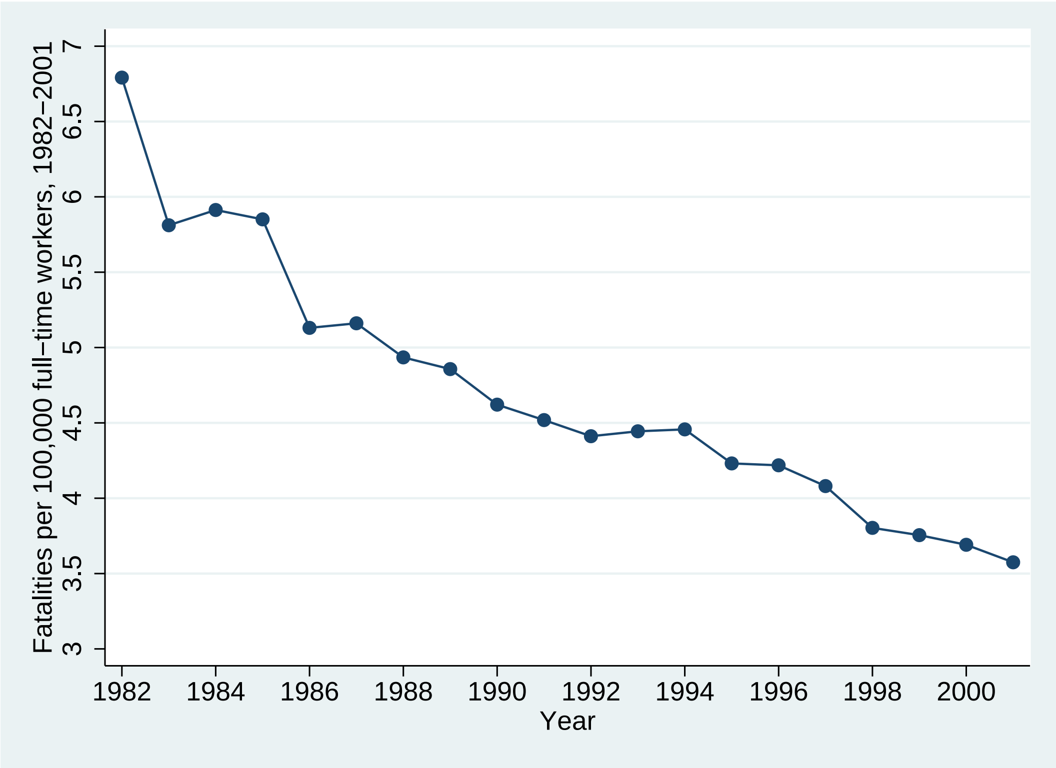


Figure 2: Fatality Rates by Initial (1982) Risk

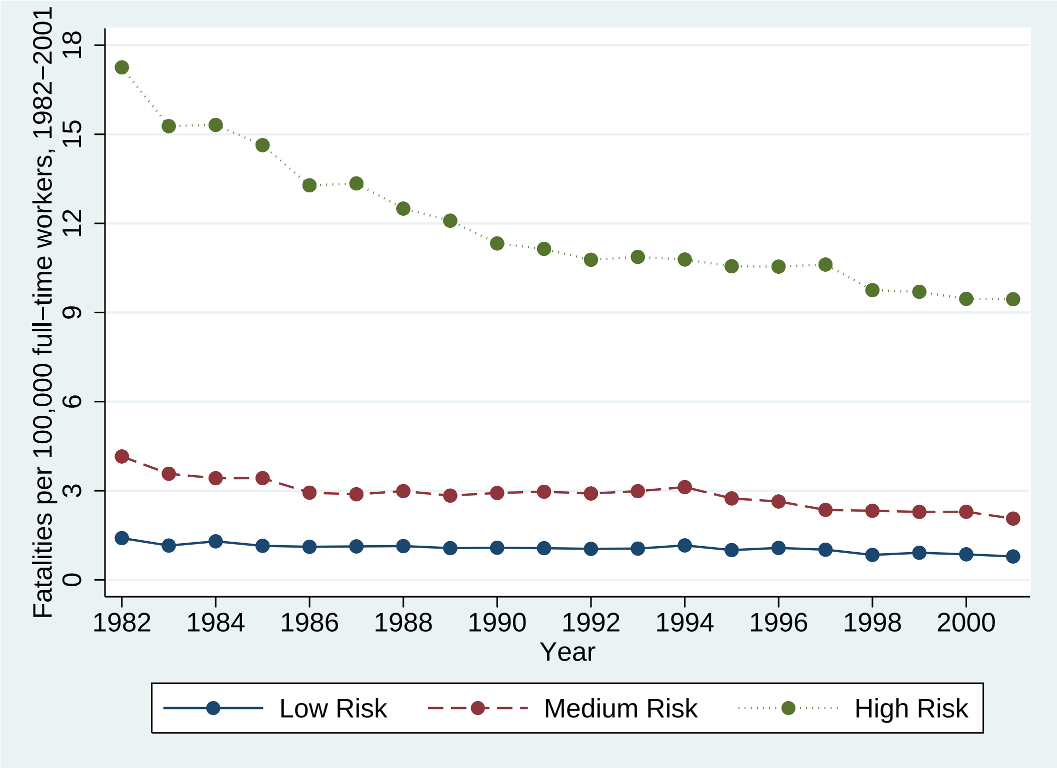


Figure 3: NTOF vs. CFOI Fatality Rates, 1992-2001

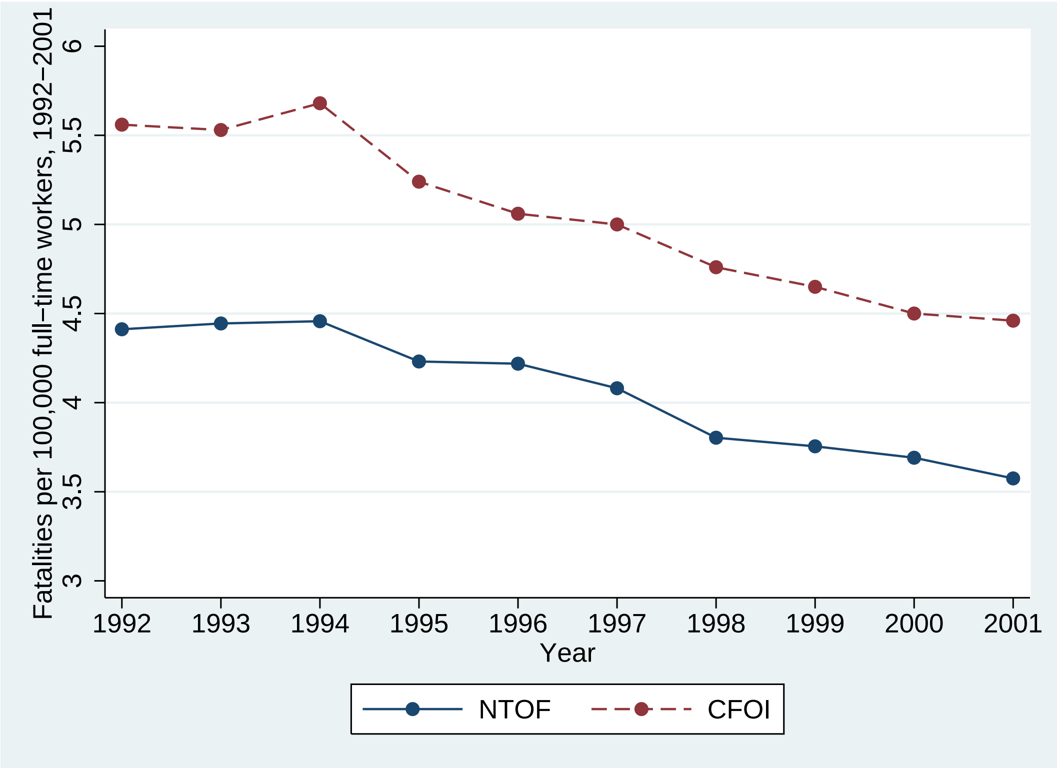


Figure 4: Injury Rates, 1982-2001

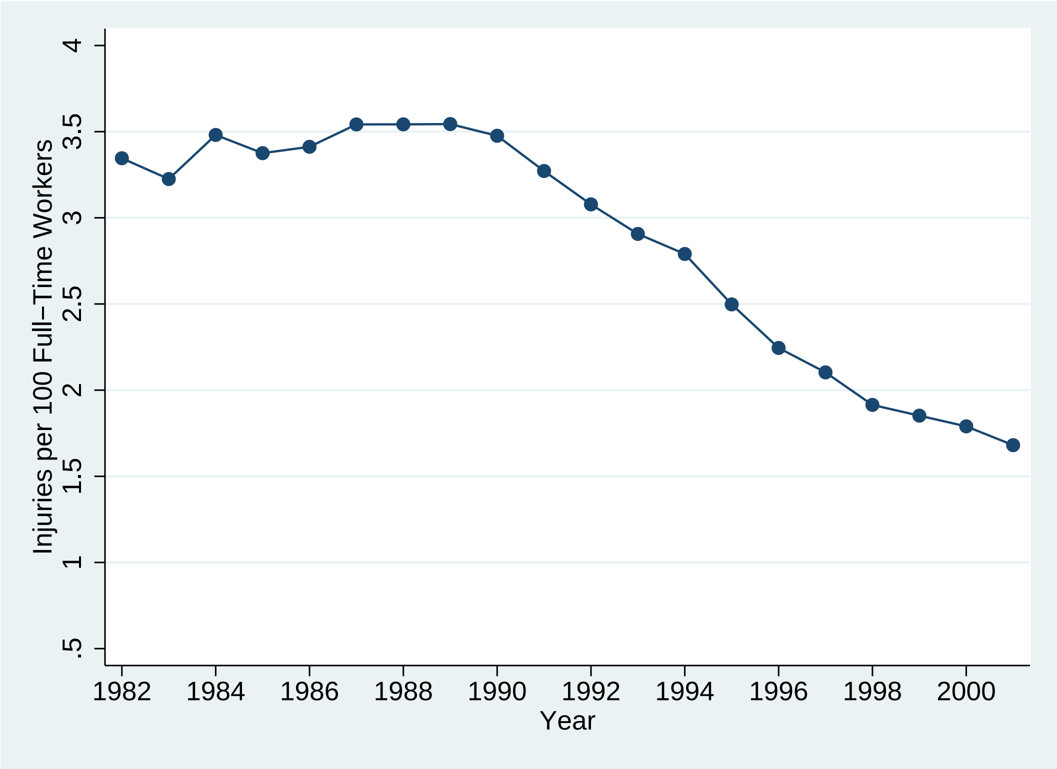
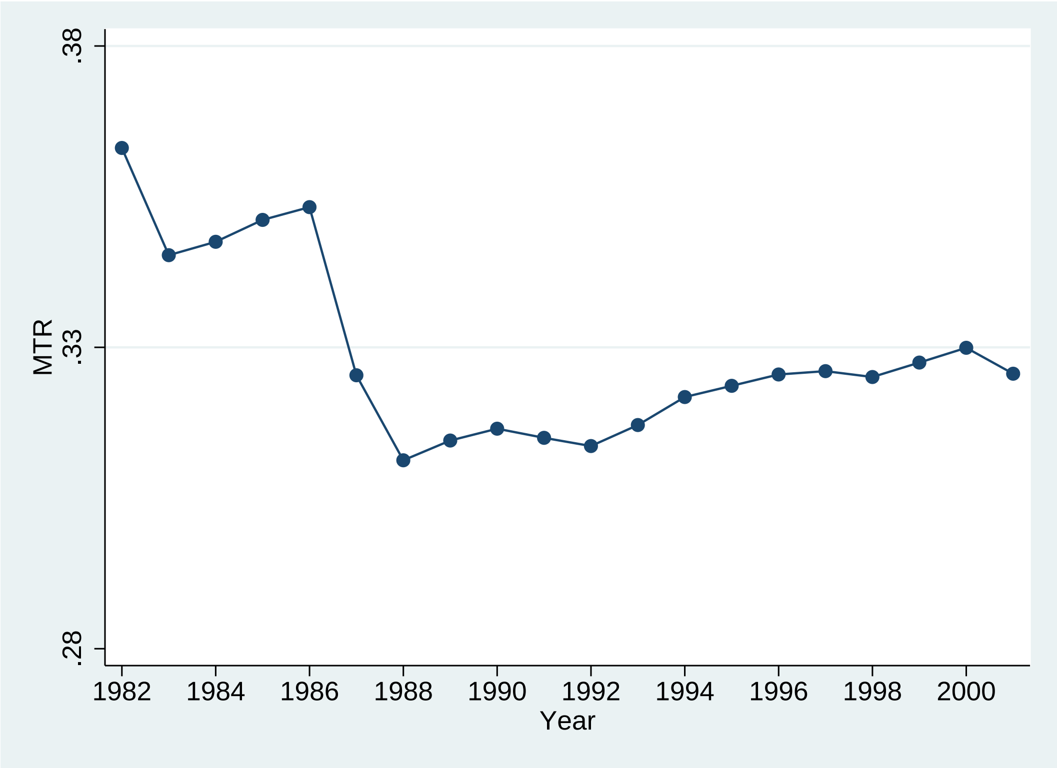


Figure 5: Average Marginal Tax Rate, 1982-2001



C Tables

Table 2: Top and Bottom 10 Fatality Rates by Industry, 1982-2001 NTOF Data

Industry	Fatalities per 100,000 FTE Workers	Injuries per 100 FTE Workers
Forestry & Fisheries	44.15	3.59
Metal/Coal/Nonmetal Mining	28.14	4.54
Lumber & Wood	26.54	6.32
Oil & Gas Extraction	21.98	3.33
Trucking/Warehousing/Storage	20.55	6.13
Agricultural Production	18.88	3.54
Construction	14.08	4.87
Agricultural Services	11.56	3.72
Other Transportation	9.81	4.97
Electric Light & Power	9.57	1.38
Mean	4.43	2.73
Printing/Publishing/Allied	1.41	2.27
Insurance & Real Estate	1.27	1.08
Apparel & Accessory Stores	1.24	1.16
Electrical Machinery	1.20	2.08
Other Professional Services	1.10	1.03
Educational Services	0.76	1.22
Health Services, Except Hospitals	0.74	2.69
Hospitals	0.69	3.29
Apparel & Other Textile	0.65	2.35
Banking and Other Finance	0.64	0.48

Table 3: Top and Bottom 10 Injury Rates by Industry, 1982-2001 BLS

Industry	Injuries per 100 FTE Workers	Fatalities per 100,000 FTE Workers
Logging	8.66	27.03
Ship and boat building and repairing	8.02	2.99
Leather: tanned, curried, and finished	7.27	5.87
Wood building and mobile homes	7.27	25.07
Air transportation	7.15	9.69
Coal mining	6.58	28.20
Beverage industries	6.44	4.13
Other primary iron and steel industries	6.32	8.97
Trucking service	6.22	20.38
Nursing and personal care facilities	6.18	0.74
Mean	2.73	4.52
Beauty shops	0.62	2.27
Offices and clinics of dentists	0.57	0.74
Insurance	0.55	1.26
Banking	0.54	0.64
Offices and clinics of physicians	0.52	0.74
Credit Agencies	0.34	0.62
Brokerage and investments	0.34	0.63
Computer and data programming service	0.34	2.33
Legal services	0.31	1.10
Accounting, auditing, bookkeeping services	0.29	1.11

Table 4: Summary Statistics

Summary Statistics by Fatality Rate Group

Entire Sample			Lowest Fatality Rate Industries		
	Mean	Std Dev		Mean	Std Dev
wage	17.76	13.24	wage	18.73	14.54
τ	0.33	0.11	τ	0.34	0.10
Injury Rate	2.81	1.96	Injury Rate	1.93	1.55
Fatality Rate	4.36	5.77	Fatality Rate	1.05	0.48
Age	39.23	8.34	Age	39.43	8.31
%College	50.02	50.00	%College	62.18	48.50
%Female	45.65	49.81	%Female	61.57	48.64
%White	87.70	32.90	%White	87.20	33.41

Middle Fatality Rate Industries			Highest Fatality Rate Industries		
	Mean	Std Dev		Mean	Std Dev
wage	16.59	12.54	wage	17.92	11.83
τ	0.32	0.12	τ	0.32	0.11
Injury Rate	2.62	1.53	Injury Rate	4.45	2.07
Fatality Rate	2.85	0.87	Fatality Rate	11.80	7.51
Age	39.03	8.40	Age	39.21	8.30
%College	45.87	49.83	%College	36.58	48.17
%Female	43.13	49.53	%Female	23.83	42.61
%White	87.00	33.63	%White	89.32	30.89

Summary Statistics by Injury Rate Group

Entire Sample			Lowest Injury Rate Industries		
	Mean	Std Dev		Mean	Std Dev
wage	17.76	13.24	wage	19.83	15.36
τ	0.33	0.11	τ	0.35	0.10
Injury Rate	2.81	1.96	Injury Rate	1.07	0.61
Fatality Rate	4.36	5.77	Fatality Rate	2.07	2.03
Age	39.23	8.34	Age	39.24	8.31
%College	50.02	50.00	%College	65.08	47.67
%Female	45.65	49.81	%Female	55.67	49.68
%White	87.70	32.90	%White	88.18	32.28

Middle Injury Rate Industries			Highest Injury Rate Industries		
	Mean	Std Dev		Mean	Std Dev
wage	16.30	12.20	wage	16.94	11.17
τ	0.32	0.12	τ	0.32	0.11
Injury Rate	2.67	0.87	Injury Rate	4.97	1.75
Fatality Rate	3.19	4.56	Fatality Rate	8.36	7.60
Age	39.05	8.36	Age	39.42	8.36
%College	47.01	49.91	%College	35.59	47.88
%Female	48.91	49.99	%Female	30.24	45.93
%White	86.61	34.05	%White	88.18	32.29

Table 5: Summary of VSL regressions year-by-year

	Value of Statistical Injury ($)	Value of Statistical Life ($ millions)
Mean Estimate	-182,566	0.1
Std Dev of Estimates	64,436	7.1
Minimum	-374,704	-11.3
Maximum	-96,712	12.4
# Pos&Sig	0	0
# Pos&NS	0	8
# Neg&NS	0	12
# Neg&Sig	20	0

A separate cross-sectional regression was estimated for each year. Standard errors clustered by industry aggregate. Covariates include state dummy variables and the following individual characteristics interacted with year dummies: age dummies, gender dummy, race dummies, education dummies.

Table 6: OLS Results, 1982-2001

Dependent Variable: log(wage)			
	(1)	(2)	(3)
$\ln(1-\tau)$	-1.406***	-1.411***	-1.406***
	(0.033)	(0.032)	(0.033)
Injury Rate x $\ln(1-\tau)$	-0.010		0.016
	(0.029)		(0.029)
Injury Rate x $\ln(WC)$	0.047		0.065
	(0.056)		(0.058)
Fatality Rate x $\ln(1-\tau)$		-11.485**	-13.411**
		(5.458)	(5.906)
Fatality Rate x $\ln(WC)$		-2.871	-11.475
		(17.185)	(14.391)
N	597833	604342	597833
Fixed Effects	Industry	Industry	Industry
Implied Value of a Statistical:			
Injury ($)	23,450		-37,695
	(-112,358 - 159,259)		(-176,868 - 101,478)
Life ($ millions)		27.4	32.0
		(1.1 - 53.8)	(3.5 - 60.5)

Significance levels: * 10%, ** 5%, *** 1%. Standard errors in parentheses are clustered by industry aggregate. Using equation 23, $VSL = -\hat{\beta}_1 \times 200,000 \times \bar{w} \times (\overline{1-\tau})$. Covariates include state dummy variables and the following individual characteristics interacted with year dummies: age dummies, gender dummy, race dummies, education dummies.

Table 7: First Stage using Federal Tax Variation, 1982-2001

	$\ln(1-\tau)$	Injury $\times \ln(1-\tau)$	Fatality $\times \ln(1-\tau)$	Injury $\times \ln(WC)$	Fatality $\times \ln(WC)$
$\ln(1-\hat{\tau})$	0.526***	-0.154	-0.000	0.875**	0.001
	(0.056)	(0.192)	(0.001)	(0.340)	(0.001)
$\overline{Injury} \times \ln(1-\hat{\tau})$	-0.004	0.998***	0.000*	-1.068***	-0.000
	(0.015)	(0.054)	(0.000)	(0.095)	(0.000)
$\overline{Fatality} \times \ln(1-\hat{\tau})$	3.515	9.927	0.927***	34.294	-0.255
	(3.770)	(15.592)	(0.093)	(26.394)	(0.279)
$\overline{Injury} \times \ln(1-\widehat{WC})$	-0.005***	-0.121***	-0.000**	0.867***	0.001**
	(0.002)	(0.010)	(0.000)	(0.057)	(0.000)
$\overline{Fatality} \times \ln(1-\widehat{WC})$	0.296	2.594	-0.016	6.916	0.447***
	(0.180)	(2.166)	(0.014)	(8.464)	(0.087)
N	592105	592105	592105	592105	592105
Fixed Effects	Industry	Industry	Industry	Industry	Industry
Shea's R^2	0.0013	0.0231	0.0287	0.0607	0.0765

Significance levels: * 10%, ** 5%, *** 1%. Standard errors in parentheses are clustered by industry aggregate. Covariates include state dummy variables and the following individual characteristics interacted with year dummies: age dummies, gender dummy, race dummies, education dummies.

Table 8: Shea's R^2 Statistics by Sample and Tax Variation

	1982-2001	1982-2001	1982-1990	1982-1990	1991-2001	1991-2001
$\ln(1-\tau)$	0.0013	0.0030	0.0021	0.0030	0.0000	0.0003
Injury x $\ln(1-\tau)$	0.0231	0.0264	0.0326	0.0279	0.0008	0.0006
Fatal x $\ln(1-\tau)$	0.0287	0.0299	0.0400	0.0417	0.0006	0.0005
Injury x $\ln(WC)$	0.0607	0.0261	0.0961	0.0121	0.0067	0.0008
Fatal x $\ln(WC)$	0.0765	0.0426	0.1071	0.0252	0.0464	0.0108
N	592105	591786	249847	249096	311194	310587
Fixed Effects	Industry	Ind x State	Industry	Ind x State	Industry	Ind x State
Tax Variation	Fed Only	Fed+State	Fed Only	Fed+State	Fed Only	Fed+State

Each column reports the Shea R^2 statistics for each variable for that sample and level of tax variation. Covariates include state dummy variables and the following individual characteristics interacted with year dummies: age dummies, gender dummy, race dummies, education dummies.

Table 9: IV Results with Federal Tax Variation Only, 1982-2001

Dependent Variable: log(wage)			
	(1)	(2)	(3)
$\ln(1-\tau)$	-1.126***	-0.747***	-1.057***
	(0.269)	(0.260)	(0.287)
Injury Rate x $\ln(1-\tau)$	-0.246***		-0.202***
	(0.039)		(0.047)
Injury Rate x $\ln(WC)$	-0.036***		-0.030***
	(0.008)		(0.008)
Fatality Rate x $\ln(1-\tau)$		-56.078***	-23.202**
		(9.973)	(11.404)
Fatality Rate x $\ln(WC)$		-5.335***	-1.971
		(1.340)	(1.353)
N	594326	598614	592105
Fixed Effects	Industry	Industry	Industry
Tax Variation	Fed Only	Fed Only	Fed Only
Implied Value of a Statistical:			
Injury ($)	588,607		482,812
	(405,958 - 771,256)		(263,008 - 702,616)
Life ($ millions)		133.9	55.4
		(87.2 - 180.6)	(2.0 - 108.8)
Implied Tax Incidence	0.344	0.105	0.295
Heterogeneity:	(0.055)	(0.019)	(0.055)

Significance levels: * 10%, ** 5%, *** 1%. Standard errors in parentheses are clustered by industry aggregate. Using equation 23, $VSL = -\hat{\beta}_1 \times 200,000 \times \bar{w} \times (\overline{1-\tau})$. Tax Incidence compares 75th percentile most dangerous industry in final sample year to 25th. Covariates include state dummy variables and the following individual characteristics interacted with year dummies: age dummies, gender dummy, race dummies, education dummies.

Table 10: IV Results with Federal and State Tax Variation, 1982-2001

Dependent Variable: log(wage)			
	(1)	(2)	(3)
$\ln(1-\tau)$	-0.457***	-0.300*	-0.434***
	(0.149)	(0.166)	(0.159)
Injury Rate x $\ln(1-\tau)$	-0.248***		-0.198***
	(0.041)		(0.049)
Injury Rate x $\ln(WC)$	-0.053***		-0.040***
	(0.013)		(0.015)
Fatality Rate x $\ln(1-\tau)$		-57.700***	-25.391**
		(9.809)	(11.903)
Fatality Rate x $\ln(WC)$		-8.614***	-4.334
		(3.087)	(3.392)
N	593984	598301	591786
Fixed Effects	Industry x State	Industry x State	Industry x State
Tax Variation	Fed+State	Fed+State	Fed+State
Implied Value of a Statistical:			
Injury ($)	591,360		471,810
	(398,448 - 784,272)		(243,273 - 700,347)
Life ($ millions)		137.8	60.6
		(91.9 - 183.7)	(4.9 - 116.4)
Implied Tax Incidence	0.346	0.108	0.292
Heterogeneity:	(0.058)	(0.018)	(0.058)

Significance levels: * 10%, ** 5%, *** 1%. Standard errors in parentheses are clustered by industry aggregate. Using equation 23, $VSL = -\hat{\beta}_1 \times 200,000 \times \bar{w} \times (\overline{1-\tau})$. Tax Incidence compares 75th percentile most dangerous industry in final sample year to 25th. Covariates include state dummy variables and the following individual characteristics interacted with year dummies: age dummies, gender dummy, race dummies, education dummies.

Table 11: IV Results with Federal Tax Variation Only, 1982-1990

Dependent Variable: log(wage)			
	(1)	(2)	(3)
$\ln(1-\tau)$	-0.931***	-0.471*	-0.845***
	(0.248)	(0.249)	(0.265)
Injury Rate x $\ln(1-\tau)$	-0.157***		-0.114***
	(0.025)		(0.034)
Injury Rate x $\ln(WC)$	-0.012**		-0.001
	(0.005)		(0.006)
Fatality Rate x $\ln(1-\tau)$		-42.115***	-21.383***
		(6.712)	(7.949)
Fatality Rate x $\ln(WC)$		-5.591***	-5.229***
		(1.868)	(1.837)
N	250433	254947	249847
Fixed Effects	Industry	Industry	Industry
Tax Variation	Fed Only	Fed Only	Fed Only
Implied Value of a Statistical:			
Injury ($)	360,862		261,367
	(247,717 - 474,007)		(110,009 - 412,724)
Life ($ millions)		96.6	49.0
		(66.4 - 126.7)	(13.3 - 84.8)
Implied Tax Incidence	0.519	0.113	0.411
Heterogeneity:	(0.083)	(0.018)	(0.086)

Significance levels: * 10%, ** 5%, *** 1%. Standard errors in parentheses are clustered by industry aggregate. Using equation 23, $VSL = -\hat{\beta}_1 \times 200,000 \times \bar{w} \times (\overline{1-\tau})$. Tax Incidence compares 75th percentile most dangerous industry in final sample year to 25th. Covariates include state dummy variables and the following individual characteristics interacted with year dummies: age dummies, gender dummy, race dummies, education dummies.

Table 12: IV Results with Federal and State Tax Variation, 1982-1990

Dependent Variable: log(wage)			
	(1)	(2)	(3)
$\ln(1-\tau)$	-0.248	0.005	-0.187
	(0.202)	(0.218)	(0.197)
Injury Rate x $\ln(1-\tau)$	-0.143***		-0.092***
	(0.024)		(0.034)
Injury Rate x $\ln(WC)$	-0.017		-0.005
	(0.013)		(0.015)
Fatality Rate x $\ln(1-\tau)$		-42.712***	-25.657***
		(6.319)	(7.722)
Fatality Rate x $\ln(WC)$		-4.353	-4.368
		(3.864)	(4.398)
N	249659	254222	249096
Fixed Effects	Industry x State	Industry x State	Industry x State
Tax Variation	Fed+State	Fed+State	Fed+State
Implied Value of a Statistical:			
Injury ($)	328,562		210,443
	(220,093 - 437,031)		(56,113 - 364,773)
Life ($ millions)		97.9	58.8
		(69.5 - 126.3)	(24.1 - 93.5)
Implied Tax Incidence	0.473	0.114	0.360
Heterogeneity:	(0.080)	(0.017)	(0.089)

Significance levels: * 10%, ** 5%, *** 1%. Standard errors in parentheses are clustered by industry aggregate. Using equation 23, $VSL = -\hat{\beta}_1 \times 200,000 \times \bar{w} \times (\overline{1-\tau})$. Tax Incidence compares 75th percentile most dangerous industry in final sample year to 25th. Covariates include state dummy variables and the following individual characteristics interacted with year dummies: age dummies, gender dummy, race dummies, education dummies.

Table 13: IV Results with Federal Tax Variation Only, 1991-2001

Dependent Variable: log(wage)			
	(1)	(2)	(3)
$\ln(1-\tau)$	0.648	1.051	0.757
	(1.811)	(2.143)	(1.886)
Injury Rate x $\ln(1-\tau)$	-0.297		-0.242
	(0.194)		(0.212)
Injury Rate x $\ln(WC)$	-0.023		-0.019
	(0.036)		(0.039)
Fatality Rate x $\ln(1-\tau)$		-67.055	-28.325
		(60.880)	(70.207)
Fatality Rate x $\ln(WC)$		-3.145	-1.758
		(5.394)	(6.278)
N	312176	312242	311194
Fixed Effects	Industry	Industry	Industry
Tax Variation	Fed Only	Fed Only	Fed Only
Implied Value of a Statistical:			
Injury ($)	729,515		594,250
	(-205,963 - 1664993)		(-426,241 - 1,614,742)
Life ($ millions)		164.9	69.7
		(-128.5 - 458.3)	(-268.7 - 408.0)
Implied Tax Incidence	0.415	0.126	0.354
Heterogeneity:	(0.271)	(0.114)	(0.245)

Significance levels: * 10%, ** 5%, *** 1%. Standard errors in parentheses are clustered by industry aggregate. Using equation 23, $VSL = -\hat{\beta}_1 \times 200,000 \times \bar{w} \times (\overline{1-\tau})$. Tax Incidence compares 75th percentile most dangerous industry in final sample year to 25th. Covariates include state dummy variables and the following individual characteristics interacted with year dummies: age dummies, gender dummy, race dummies, education dummies.

Table 14: IV Results with Federal and State Tax Variation, 1991-2001

Dependent Variable: log(wage)			
	(1)	(2)	(3)
$\ln(1-\tau)$	-1.177*	-0.617	-1.140*
	(0.610)	(0.529)	(0.609)
Injury Rate x $\ln(1-\tau)$	-0.938***		-0.754**
	(0.354)		(0.337)
Injury Rate x $\ln(WC)$	-0.440***		-0.403**
	(0.156)		(0.163)
Fatality Rate x $\ln(1-\tau)$		-130.022*	-88.749
		(73.090)	(80.524)
Fatality Rate x $\ln(WC)$		-20.137*	-13.34
		(10.301)	(10.039)
N	311510	311642	310587
Fixed Effects	Industry x State	Industry x State	Industry x State
Tax Variation	Fed+State	Fed+State	Fed+State
Implied Value of a Statistical:			
Injury ($)	2,306,579		1,853,181
	(601,798 - 4,011,359)		(228,549 - 3,477,813)
Life ($ millions)		319.7	218.2
		(-32.5 - 672.0)	(-169.9 - 606.3)
Implied Tax Incidence	1.311	0.244	1.104
Heterogeneity:	(0.494)	(0.137)	(0.454)

Significance levels: * 10%, ** 5%, *** 1%. Standard errors in parentheses are clustered by industry aggregate. Using equation 23, $VSL = -\hat{\beta}_1 \times 200,000 \times \bar{w} \times (\overline{1-\tau})$. Tax Incidence compares 75th percentile most dangerous industry in final sample year to 25th. Covariates include state dummy variables and the following individual characteristics interacted with year dummies: age dummies, gender dummy, race dummies, education dummies.

Table 15: VSL Estimates with Initial Wage x Year Controls

Controls (Sample)	Value of Statistical	(1)	(2)	(3)
Mean 1982 Wage	Injury ($)	583,835		494,875
x Year Interactions		(368,457 - 799,213)		(253,953 - 735,797)
(1983-2001)	Life ($ millions)		130.6	51.3
			(80.3 - 180.9)	(-2.2 - 104.7)
Mean 1982 Wage	Injury ($)	332,612		238,165
x Year Interactions		(155,554 - 509,670)		(20,361 - 455,968)
(1983-1990)	Life ($ millions)		91.7	54.3
			(50.6 - 132.7)	(12.1 - 96.5)
Wage Decile	Injury ($)	606,997		528,040
x Year Interactions		(376,733 - 837,261)		(269,811 - 786,270)
(1983-2001)	Life ($ millions)		130.8	43.6
			(80.3 - 181.2)	(-5.4 - 92.6)
Wage Decile	Injury ($)	312,881		243,118
x Year Interactions		(119,252 - 506,510)		(37,561 - 448,675)
(1983-1990)	Life ($ millions)		79.7	39.1
			(35.8 - 123.6)	(-4.9 - 83.1)

Each block is a set of 3 regressions. The blocks differ from one another based on the types of wage x year controls included and the sample. The corresponding VSL estimates with 95% confidence intervals are reported.

Table 16: Tax Variation at Level of Risk Rates, 1982-2001

Dependent Variable: log(wage)			
	(1)	(2)	(3)
$\ln(1-\tau)$	-1.127***	-0.782***	-1.095***
	(0.270)	(0.257)	(0.288)
Injury Rate x $\ln(1-\tau)$	-0.247***		-0.205***
	(0.039)		(0.047)
Injury Rate x $\ln(WC)$	-0.036***		-0.031***
	(0.008)		(0.008)
Fatality Rate x $\ln(1-\tau)$		-53.357***	-20.701*
		(9.932)	(11.276)
Fatality Rate x $\ln(WC)$		-5.291***	-1.899
		(1.333)	(1.365)
N	594326	595818	589309
Fixed Effects	Industry	Industry	Industry
Tax Variation	Fed Only	Fed Only	Fed Only
Implied Value of a Statistical:			
Injury ($)	588,894		489,694
	(405,999 - 771,788)		(267,794 - 711,595)
Life ($ millions)		127.4	49.4
		(80.9 - 173.9)	(-3.3 - 102.2)
Implied Tax Incidence	0.345	0.100	0.296
Heterogeneity:	(0.055)	(0.019)	(0.055)

Significance levels: * 10%, ** 5%, *** 1%. Standard errors in parentheses are clustered by industry aggregate. Using equation 23, $VSL = -\hat{\beta}_1 \times 200,000 \times \bar{w} \times (\overline{1-\tau})$. Tax Incidence compares 75th percentile most dangerous industry in final sample year to 25th. Covariates include state dummy variables and the following individual characteristics interacted with year dummies: age dummies, gender dummy, race dummies, education dummies.

Table 17: Mean vs. Initial Risk

	Dependent Variable: log(wage)			
	(1)	(2)	(3)	
$\ln(1-\tau)$	-1.057***	-1.031***	-0.845***	-0.822***
	(0.287)	(0.281)	(0.265)	(0.262)
Injury x $\ln(1-\tau)$	-0.202***	-0.187***	-0.114***	-0.112***
	(0.047)	(0.045)	(0.034)	(0.032)
Injury x $\ln(WC)$	-0.030***	-0.032***	-0.001	-0.005
	(0.008)	(0.008)	(0.006)	(0.006)
Fatal x $\ln(1-\tau)$	-23.202**	-22.665**	-21.383***	-21.161***
	(11.404)	(11.429)	(7.949)	(7.875)
Fatal x $\ln(WC)$	-1.971	-1.291	-5.229***	-4.200**
	(1.353)	(1.454)	(1.837)	(1.804)
N	592105	588296	249847	249516
Risk	Mean	Initial	Mean	Initial
Fixed Effects	Industry	Industry	Industry	Industry
Tax Variation	Fed Only	Fed Only	Fed Only	Fed Only
Sample	1982-2001	1982-2001	1982-1990	1982-1990
Implied Value:				
Injury ($)	482,812	447,410	261,367	255,864
	(263,008 - 702,616)	(237,757 - 657,064)	(110,009 - 412,724)	(111,424 - 400,305)
Life ($ millions)	55.4	54.1	49.0	48.5
	(2.0 - 108.8)	(0.6 - 107.6)	(13.3 - 84.8)	(13.1 - 83.9)
Implied Heterogeneity:	0.295	0.265	0.411	0.403
	(0.055)	(0.063)	(0.086)	(0.083)

Significance levels: * 10%, ** 5%, *** 1%. Standard errors in parentheses are clustered by industry aggregate. Using equation 23, $VSL = -\hat{\beta}_1 \times 200,000 \times \bar{w} \times (\overline{1-\tau})$. Tax Incidence compares 75th percentile most dangerous industry in final sample year to 25th. Covariates include state dummy variables and the following individual characteristics interacted with year dummies: age dummies, gender dummy, race dummies, education dummies.

Table 18: Individual-Level Data, 1981-1994

Dependent Variable: log(wage)			
	(1)	(2)	(3)
$\ln(1-\tau)$	0.382	0.065	0.408
	(0.401)	(0.328)	(0.416)
Injury Rate x $\ln(1-\tau)$	-0.256***		-0.228***
	(0.083)		(0.082)
Injury Rate x $\ln(WC)$	-0.108**		-0.110**
	(0.044)		(0.043)
Fatality Rate x $\ln(1-\tau)$		-38.007***	-18.158
		(13.313)	(15.200)
Fatality Rate x $\ln(WC)$		-10.510	-0.948
		(7.395)	(6.953)
N	27917	30674	27917
Fixed Effects	Individual	Individual	Individual
Tax Variation	Federal	Federal	Federal
Implied Value of a Statistical:			
Injury ($)	589,000		525,000
	(216,000 - 962,000)		(153,000 - 896,000)
Life ($ millions)		87.6	41.8
		(27.4 - 147.7)	(-26.8 - 110.5)

Significance levels: * 10%, ** 5%, *** 1%. Standard errors in parentheses are clustered by industry aggregate. Using equation 23, $VSL = -\hat{\beta}_1 \times 200,000 \times \bar{w} \times (\overline{1-\tau})$. Covariates include state dummy variables and the following individual characteristics interacted with year dummies: age dummies, gender dummy, race dummies, education dummies, tenure at current job, tenure-squared.

Table 19: Implied 2001 Income Compensating Differential Relative to Median Risk Industry

Fatality (Percentile)	Industry Name	Actual Income	Implied Differential Relative to Median for VSL=$7 million	Implied Differential Relative to Median for VSL=$60 million
90th	Trucking/Warehousing	$57,000	$589	$5,048
75th	Automobile/Repair	$32,000	$274	$2,347
60th	Wholesale Trade	$43,000	$46	$392
50th	Business Services	$48,000	$0	$0
40th	Fabricated Metals	$35,000	-$28	-$238
25th	Furniture Manufacturing	$31,000	-$49	-$420
10th	Educational Services	$47,000	-$103	-$883