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Abstract

We examine the incidence, form, and research consequences of measurement error in measures of fatal injury risk in U.S. workplaces using both BLS and NIOSH data. These data are commonly used in hedonic wage studies. Despite the fact that each of our measures of job risk purport to measure the same thing – the risk of a fatality while on the job – the various measures of job risk are not highly correlated, with the maximum correlation being 0.53. Indeed, many of the estimated value of statistical life estimates are negative. We find that the National Institute of Safety and Health's industry risk measure produces implicit value of life estimates most in line with both economic theory and the mode result for the existing literature than other risk measures examined. Because we find non-classical measurement error that differs across risk measures and is not independent of other regressors, innovative statistical procedures need be applied to obtain statistically improved estimates of wage-fatality risk tradeoffs.

Key Words:
hedonic wage equation, price of risk, implicit value of life, measurement error

JEL Categories:
J28, C13, J81

Subject Matter Classifications:
Health, Valuation Methods, Valuation

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1. Introduction

At least since Adam Smith's *Wealth of Nations*, economists have acknowledged that workers require compensation to accept the risk of fatal or non-fatal injuries at work. A compensating wage premium provides employers with incentives to reduce the risk on the job, and the calculus of the marketplace allows workers and employers to trade the costs of reducing workplace risk against the benefits associated with the risk reduction. When large numbers of workers reveal wage-risk tradeoffs a researcher can calculate the implied value of a statistical life, or the wage reduction associated with reducing by one worker the expected number of deaths. Because the value represents the amount of total wages that workers are willing to forgo to reduce risk the value of a statistical life appears to be a useful tool for evaluating individuals' willingness to pay for reductions in risk in other situations and provides policymakers with valuable information for the benefit side of programs to improve health and safety (Office of Management and Budget 2003). We examine here the amount of heterogeneity in estimated compensating wage differentials for fatal injury risk in the United States across alternative risk measures, whether wage differential differences across risk measures can be reconciled statistically, and discuss the policy and future research consequences of differences in compensating wage differentials across risk measures.

When basing policy on estimates of the price of risk the precision and accuracy of the estimates can be important. Yet, Viscusi (1993) and Viscusi and Aldy (2002), in reviewing labor market studies of the value of life, report that the majority of the estimates are in the $4 to $9.5 million range (excluding the studies that authors considered flawed). Although there is over a 133 percent variation in the point estimates from the most well done studies, Viscusi correctly notes that much variation should be expected, because the studies used different methods and data. Of course, the precision of implicit value of life estimates depends on how accurately job fatality risk is measured. It is well known that random measurement error generally results in estimates of coefficients that are biased toward zero or attenuated (See Griliches (1986) for an excellent review of the early literature and Bound, Brown, and Mathiowetz (2001) for a review of the more recent literature). Here we document that most measures of job risk commonly used in the estimation of hedonic labor market equilibrium models seem to measure poorly the job risk that workers face and examine the statistical issues involved when dealing with the non-random measurement errors in fatality risks for U.S. jobs.

In particular, we match the Outgoing Rotation Groups of the Current Population Survey (ORG-CPS) to multiple measures of job risk: the Bureau of Labor Statistics estimates from their Survey of Working Conditions and the National Institute of Occupational Safety and Health estimates from their National Traumatic Occupational Fatality survey. Because we have multiple measures of job risk, as well as aggregate measures of job risk by demographic groups, we may compare the various measures of job risk to infer the reliability of our job risk measures. The results are not heartening. We find strong evidence that the job risk measures contain noteworthy measurement error. Despite the fact that each of our measures of job risk purport to measure the same thing – the risk of a fatality while on the job – the various measures of job risk are not highly correlated, with the maximum correlation being 0.53.[i] Regression coefficient estimates that do not account for substantial measurement error may be highly attenuated, which Hausman, Newey, and Powell (1991) term the iron law of econometrics. Attenuation bias suggests that existing estimates of the value of a statistical life are severely underestimated.

However, the situation concerning estimated compensated wage differentials is more complex. We find evidence that the measurement error in job risk measures is non-classical. That is, we find that the measurement error is correlated with the covariates that are usually in earnings or wage equations. When the measurement error in fatality risk is correlated with other variables in the wage equation, there may be other biases offsetting the attenuation that usually occurs with purely random measurement error. We conclude by noting that the NIOSH industry based risk measure produces price of risk estimates that are most in line with economic theory and past evidence.

2. Measuring the Price of Risk

The starting point for our analysis is a wage equation of the form:

$$ln(w_i) = X_i\beta + r_i^*\gamma + \varepsilon_i \qquad \textbf{(1)}$$

where $ln(w_i)$ is the natural logarithm of the i^{th} worker's wage, r_i^* is the measure of risk (possibly a vector), X_i is a vector of covariates, (β,γ) are coefficients to be estimated, and ε_i is the error term of the regression. As a point of departure we consider the convenient case that occupies the bulk of the interest in the measurement error literature where $Cov(X_i,\varepsilon_i) = 0$ and $Cov(r_i^*,\varepsilon_i) = 0$, so that the risk measures and other covariates are exogenous. The wage equation (1) with exogenous regressors is what Viscusi (1993) calls the basic approach in the literature and yields a natural interpretation for γ as the implicit price of risk. Accurate estimates of the implicit price of risk and other non-wage job characteristics have taken on increased importance because they are a focal part of attempts to uncover the underlying utility and cost functions (Kniesner and Leeth 1995), which is a subject of renewed interest by econometricians (Ekeland, Heckman, and Nesheim 2002).

2.1 Data On Fatality Rates

Our data on wages and worker characteristics are from Outgoing Rotation Groups of the Current Population Survey (ORG-CPS). We match the ORG-CPS to measures of job risk. There are two major sources of government-reported job risk: (1) the Bureau of Labor Statistics (BLS) estimates from their Survey of Working Conditions and (2) the National Institute of Occupational Safety and Health (NIOSH) estimates from their National Traumatic Occupational Fatality Survey. The NIOSH data provide one-digit occupation or industry mortality rates by state, while the BLS data contain counts of deaths by three-digit occupation or industry codes but do not provide any regional variation. The risk measures have their own distinct costs and benefits for researchers.

The BLS data, available annually from 1995 to 2000, contain very detailed measures of the annual number of deaths, but the data suppression procedure requires at least 5 deaths in a cell before the number of deaths is reported. Thus, there are a substantial number of missing values in the BLS data. The use of annual data may be subject to a great deal of sampling error associated with the annual fluctuation in the number of deaths. Moreover, the BLS data only provide the counts of the number of deaths in each industry or occupation. To create a fatality rate, it is necessary for researchers to estimate the number of workers in an industry or occupation. To estimate the numbers of workers in industries and occupations, we use the ORG-CPS data, which in turn generates additional measurement errors in our risk (fatality rate) measures. Finally, by their construction the BLS data mask geographic variation in job risk.

The NIOSH data provide fatality rates by one-digit industry or occupation codes by state. It reports 5-year averages: 1981–1985, 1986–1990, and 1991–1995. NIOSH data, then, do not require the researcher to estimate the number of workers in an industry or occupation cell, allow job risks measure to vary by state, and smooth much of the sampling variation by using a 5-year average. The use of the 5-year average and the coarser one-digit industry or occupation codes by state reduces, but does not eliminate, the problem of missing values because of data suppression. On the other hand, the NIOSH data treat police officers and dental assistants as having the same job risk as both are in the same one-digit (service worker) occupation. The use of 5-year averages, while smoothing the sampling variation, may miss important time-series variation although having less so-called assignment error as one-digit industry and occupation more accurately reported than the corresponding three-digit industry and occupation (Bound, Brown, and Mathiowetz 2001).

Although we ultimately use both industry-based and occupation-based risk measures, we would be remiss if we did not comment on the relative merits of the two risk measures. At first glance, the use of the industry measure seems inappropriate. Specifically, the industry risk measure assigns the same job risk to a secretary in the coal mining industry as to the coal miner, clearly overstating the secretary's level of job risk and understating the coal miner's job risk. In contrast, the use of occupational risk would combine the job risk of a secretary in the coal mining industry with a secretary in the insurance industry, presumably a pair with a much more homogeneous job risk. However, a worker's industry is measured more accurately than a worker's occupation (Bound, Brown, and Mathiowetz 2001). The employer and employee agree on industry classification 84–92 percent of the time but agree on occupation classification only 58–81 percent of the time with greater agreement the broader the classification (Mellow and Sider 1983). As an indication of the importance of assignment error to the problem at hand, for data in which both the firm and worker agree on the three-digit industry code the estimated price of injury risk is 50 percent higher than in the typical data set with assignment error (Mellow and Sider 1983).

The quality of estimates is necessarily limited by the quality of measurement. No matter how sophisticated the theoretical and econometric models, data of poor quality may still provide estimates of poor quality. In the next section, we suggest why the data from the BLS and NIOSH, while providing extremely accurate measures of the aggregate job risk in the United States, may not provide accurate estimates of the job risk of workers in a representative sample.

2.2 Summary of Measurement Error Problems

There are essentially three problems in measuring of job fatality risk. First, because we divide workers into industries or occupations – some of which are quite small – we may have considerable sampling variation within industry and occupation cells. Although both the BLS and NIOSH data recognize the problem of industry or occupation cells with few fatalities and suppress data when the number of fatalities is too low, the inherent sampling variation still creates measurement error. Second, within occupations, there may be a great deal of heterogeneity in the actual job risk, and the assignment of job risk may be extremely non-random. For instance, employers may assign male and older clerks at convenience stores evening and late night hours when the risk of holdup – and injury during the robbery – are particularly high and assign female and younger clerks daytime hours. Because we only measure the aggregate job risk of convenience stores clerks, we in turn overestimate the job risk of young and female clerks and underestimate the job risk of older and male clerks. Finally, because we need to assign workers to an industry or occupation the quality of our measurement is limited to the quality of the data on industry and occupation assignment, and we have noted that industry and occupation (especially at the three-digit level) are not measured accurately.

3. Econometric Background

If the researcher could measure (X_i, r_i^*) perfectly, Ordinary Least Squares (OLS) estimation of equation (1) would provide consistent and efficient estimates of the parameters (β, γ) if the functional form of the conditional mean function were properly specified and the covariates (X_i, r_i^*) orthogonal to the error term. There are numerous reasons to suggest that the measure of job risk (r_i^*) is mismeasured and perhaps mismeasured badly.

First, government fatality reports are inherently an estimate of job risk: they are realizations of a random variable. For instance, suppose there are N_k workers in the *kth* industry (or occupation) category, and each worker is subjected to a risk, r_k^*. Unfortunately for the researcher, the government's tally of deaths in the *kth* category is not exactly equal to the expected number of deaths, $r_k^* N_k$. The government's tally is equal to the random variable D_k. Using the random variable D_k, the researcher constructs an estimate of r_k^* as $r_k = D_k / N_k$. Although $E(r_k) = r_k^*$, it is almost certain that $r_k \neq r_k^*$ so that $r_k = r_k^* + \eta_k$, where η_k is the measurement error associated with the variable r_k.

Even when workers correctly identify their industry and occupation (and as we will emphasize, there is much measurement error in the industry and occupation measures in the CPS), it is likely that the measurement of job risk is in error. Past studies have indicated that job risk differs by firm size, region, and worker characteristics. Thus, when we make the further substitution for the *ith* worker's risk (who is in the *kth* industry/occupation class) that $r_i^* = r_k$, we are undoubtedly introducing measurement error, or

$$r_k = r_i^* + v_{ik} \qquad \textbf{(2)}$$

where v_{ik} represents the measurement error associated with using r_k as a proxy for r_i^*. The basic form of measurement error in (2) undoubtedly attenuates the estimates of the coefficient of interest in the hedonic wage equation (1), γ. From an empirical standpoint the relevant issue is the severity of attenuation bias that results from the measurement error v_{ik}.

3.1 Determining the Extent of Measurement Error

We have up to four reports on the level of job risk that we may use to determine the extent of the measurement error. To see how multiple measures can be helpful consider two measures of job risk:

$$r_{1i} = r_i^* + v_{1i} \text{ and} \qquad \textbf{(3)}$$

$$r_{2i} = r_i^* + v_{2i}, \qquad \textbf{(4)}$$

where r_i^* is the true measure job risk, v_{ji} is the measurement error associated with the jth measure of job risk, and r_{ji} is the jth observed measure of job risk. The covariance of the two measures is simply

$$Cov(r_{1i}, r_{2i}) = Var(r_i^*) + Cov(v_{1i}, r_i^*) + Cov(v_{2i}, r_i^*) + Cov(v_{1i}, v_{2i}), \qquad \textbf{(5)}$$

and the variances of the two measure are

$$Var(r_{1i}) = Var(r_i^*) + 2\,Cov(v_{1i}, r_i^*) + Var(v_{1i}) \text{ and} \qquad \textbf{(6)}$$

$$Var(r_{2i}) = Var(r_i^*) + 2\,Cov(v_{2i}, r_i^*) + Var(v_{2i}), \qquad \textbf{(7)}$$

which provides us with six unknown parameters and three equations and demonstrates why it is impossible to make much progress on the problem in the form described in (3)–(7): the system is underidentified.

Suppose we follow Griliches (1986) and assume for the time being that our measurement error is classical. If $Cov(\nu_{1i}, r_i^*) = Cov(\nu_{2i}, r_i^*) = Cov(\nu_{1i}, \nu_{2i}) = 0$ our three-equation system reduces to

$$Cov(r_{1i}, r_{2i}) = Var(r_i^*), \qquad \textbf{(8)}$$

$$Var(r_{1i}) = Var(r_i^*) + Var(\nu_{1i}), \text{ and} \qquad \textbf{(9)}$$

$$Var(r_{2i}) = Var(r_i^*) + Var(\nu_{2i}). \qquad \textbf{(10)}$$

With additional covariates one needs to make the additional assumptions that $Cov(\nu_{1i}, X_i) = 0$ and $Cov(\nu_{2i}, X_i) = 0$ so that the measurement errors are uncorrelated with covariates in our basic example case. Because we have up to four measures of job risk, the classic errors-in-variables model has empirical content: the covariance of any two measures of risk should have precisely the same covariance as any other two measures. When the measurement error is classical and there are multiple measures of a variable, one may use Instrumental Variables (IV) to obtain a consistent estimate of the price of risk (Griliches 1986).[ii]

It is useful now to present a convenient decomposition for OLS regressions. Yule (1907) has shown that the estimation of the hedonic wage equation (1) with OLS is equivalent to the results using three simpler regressions. First, one estimates

$$\ln(w_i) = X_i\, b + \varepsilon_i' \qquad \textbf{(11)}$$

and recovers the residuals, which we denote $\ln(w_i)'$. Second, one estimates

$$r_i = X_i\, \delta + u_i' \qquad \textbf{(12)}$$

and recovers the residuals, which we denote r_i'. Finally, one then estimate the equation

$$\ln(w_i)' = r_i'\gamma + \varepsilon_i'' . \quad \textbf{(13)}$$

Because both the dependent variable and independent variables have been purged of their covariation with *X*, estimation of equation (13) will yield precisely the same estimate of γ as the OLS of γ from the multiple regression (1) (Goldberger 1991).

Exploiting Yule's decomposition and continuing with the convenient case where the measurement error is classical, our three equations system of covariances would simply become

$$Cov(r_{1i}, r_{2i} \mid X_i) = Var(r_i^* \mid X_i), \quad \textbf{(14)}$$

$$Var(r_{1i} \mid X_i) = Var(r_i^* \mid X) + Var(v_{1i} \mid X) = Var(r_i^* \mid X) + Var(v_{1i}), \text{ and} \quad \textbf{(15)}$$

$$Var(r_{2i} \mid X_i) = Var(r_i^* \mid X_i) + Var(v_{2i} \mid X_i) = Var(r_i^* \mid X_i) + Var(v_{2i}), \quad \textbf{(16)}$$

where $Var(v_{1i} \mid X) = Var(v_{1i})$ and $Var(v_{2i} \mid X_i) = Var(v_{2i})$ by the assumptions that $Cov(v_{1i}, X_i) = 0$ and $Cov(v_{2i}, X_i) = 0$. As $Var(r_i^*) \geq Var(r_i^* \mid X)$, the addition of covariates must always reduce the signal-to-noise ratio [$Var(r_i^* \mid X) / \left(Var(r_i^* \mid X) + Var(v_{ji})\right)$]. In general, the addition of covariates should increase the attenuation bias associated with the measurement error.

4. Empirical Results

In Table 1 we present the correlation and Yulized residual correlations for the various job risk measures. We use data from the 1995 ORG-CPS. The raw correlation before conditioning on any covariates ranges from 0.53 to 0.30. Because the correlation differs by a magnitude of over 75 percent we have at least some evidence that the measurement error is non-classical. When we condition on the full set of covariates the correlations range from 0.41 to 0.02. Including both state controls and industry and occupation controls reduces the correlation among the various measures. In absence of measurement error the correlations should be 1.0. A quick review of equations (8)–(10) and (14)–(16) reveals another testable implication of the classic errors-in-variable model: the correlation among all four risk measures should be identical. We clearly reject the hypothesis of no measurement error and reject the hypothesis that the measurement error is classical.

4.1 Attenuation

In Table 2, we produce the full range of Yulized residual covariances, which in turn may be used to construct any coefficient estimate desired. The OLS estimates of the price of risk are simply the ratio of the risk measure covariance with the wage measure, divided by the variance of the risk measure; one may form any IV estimate desired by dividing the covariance of risk and wage measures by the covariance of two risk measures. The ratio of the variance of the risk measure to its covariance with another risk measure in turn meters the magnitude of the attenuation bias resulting from measurement error in job risk. The ratios of the variance-to-covariance are large, particularly for the BLS occupation measure, which suggests that OLS estimates of the hedonic wage equation (1) would be substantially attenuated.

For instance, if we focus on last column of Panel C for men, we could construct the OLS estimate using the NIOSH industry measure as

$$\mathrm{cov}(\ln wage, NIOSH\ Ind) / \mathrm{var}(NIOSH\ Ind) = 0.01/13.75 \approx 0.000073 .$$

We may also then use the BLS occupation measure as an instrument, which results in the IV estimates of job risk as

$$\mathrm{cov}(\ln wage, BLS\ Occ) / \mathrm{cov}(NIOSH\ Ind, BLS\ Occ) = 0.06/2.02 \approx 0.0297 .$$

so that the IV estimate is over 40 times the magnitude of the OLS estimate. Relying on the last column makes the degree of attenuation bias particularly severe because the industry and occupation controls remove much of the variation that is common across both measures. The raw correlation between the BLS occupation and NIOSH industry measures is 0.30, but once we condition on industry and occupation, the correlation is reduced to just 0.05.

Negative measures of job risk compensation are substantially attenuated as well. Notice that the covariances of the logarithm of wages and the various job risk measures are quite different and often of the opposite sign, which reinforces the emerging implication that measurement error is non-classical. The negative covariances between wages and job risks suggest that our measures of job risk may be correlated with the regression error.[iii] Indeed, because job safety is a normal good (Viscusi and Aldy 2002), economic theory suggests that factors increasing the wages and hence the wealth of the workers should reduce job risk. There would appear to be a clear theoretical reason suggesting that unobservables that increase wages should be negatively correlated with job risk.

4.2 A Deeper Look at Risk Measures

It may be informative to invert our research focus and consider not whether there is a wide variation of estimates for the price of risk but instead whether there is a discernable pattern to the price of risk coefficients such that certain ones are similar to the estimates highlighted in Viscusi and Aldy (2002). In particular, does one of the risk measures or covariate lists stand out in terms of producing estimated price of risk and implicit value of life estimates that are similar to results that lie in or around the range of \$4 million to \$9.5 million?

In Tables 3 and 4 we present regression results for OLS estimates of the price of risk and value of life for the four basic risk measures: BLS industry and occupation and NIOSH industry and occupation. To avoid the problems of aggregation mentioned earlier we estimate separate regressions for white men and white women. For positive values of the risk coefficient, we also produce values of the statistical life in millions of dollars. Two main results emerge from Tables 3 and 4. First, the regressions estimated with NIOSH industry risk measures, particularly for white men, are most like the results highlighted in Viscusi and Aldy (2002) as being the preferred estimates for applications of economic policy. Our result that the NIOSH industry based risk measure produces price of risk estimates that are most in line with economic theory and past evidence is consistent with Moore and Viscusi (1988) who first identified the relative merits of the NIOSH risk measure in hedonic wage equation research. Second, numerous estimates of the risk coefficient are negative, contrary to theory. For men, 7 of the 16 estimated coefficients are negative, and for women, 9 of the 16 estimated coefficients are negative. For the NIOSH industry measure, however, 7 of the 8 coefficients are positive and the negative coefficient is not statistically different from zero at the five-percent level.

5. Discussion

Existing estimates of the price of risk have generally ignored any measurement problem in the measures of job risk. We assemble compelling evidence of non-ignorable measurement error in the various measures of job-related fatal injury risk. Because we have multiple measures of job risk, we may look at the correlation among the various measures of job risk. The correlation is seldom above 0.5 and the inclusion of richer sets of covariates lowers the correlations among pairs of risk measures.

The form of the measurement error is also econometrically troublesome. We find measurement error in the fatality rate (ν) that is correlated with the covariates (X) typically included in wage or earnings equations. The correlation between ν and X means that typical errors-in-variables models will not reveal unbiased parameter estimates of the price of risk. Given that we find convincing evidence that the measurement error is correlated with observable factors that affect wages (the covariates), we expect that the measurement error will also be correlated with unobservable factors affecting wages (the regression error). Complex correlations among the fatality risk regressor, other regression covariates, and the overall regression error in the hedonic wage equation (1) make obtaining consistent estimates of the price of risk in a hedonic wage equation econometrically challenging.

Our IV estimates illustrate the potential attenuation that may plague the OLS estimates. Coefficient estimates may also be biased away from zero if there is a negative covariance between the measurement error and the true value of job risk (Black, Berger, and Scott 2000; Kane, Rouse, and Staiger 1999). Not accounting for heterogeneity in workers' skills in avoiding work-related accidents may cause us to overestimate the price of risk (Shogren and Stamland 2002). Although the presence of measurement error that we have documented suggests that current estimates of the price of risk are severely attenuated, other biases such as aggregation may cause us to overestimate the price of risk (Kniesner and Leeth 1991; Lalive forthcoming).

The problems are formidable in obtaining statistically consistent point estimates for γ in (1). The existing measurement error literature provides little guidance in how to correct for non-classical measurement error problems of the type we have found. Because job accidents are random variables with a very low incidence the coefficient of variation is quite volatile and some inter-temporal smoothing techniques might be applied fruitfully to the time-series of risks for the large majority of workers with jobs in the low range of *r* (McClellan and Staiger 1999). Job risk undoubtedly varies by the characteristics of the workers and firms in ways in which economists do not yet fully understand but may be handled with specialized IV techniques that explicitly consider the stochastic characteristics of multiple samples (Dickens and Ross 1984).

We conclude by reiterating that existing estimates of the price of fatal injury risk may suffer from substantial attenuation bias to the extent that they have not controlled for measurement error in job risk. However, because of the evidence of non-classical measurement errors in risk that seems widespread we believe that the conventional IV point estimates in Section 4 are most likely not statistically consistent estimates of $\hat{\gamma}$ in (1). If crucial for policy, point estimates should ideally use the NIOSH based industry risk measure with estimators that take account of the particular type of measurement errors labor economists confront in micro data sets on workers. In many policy applications, though, bounding the estimate of the price of risk will be sufficient for informed decision making (Kniesner and Viscusi 2003) so that researchers can make increased use of recent developments in the econometrics of error bounds on parameters (Black, Berger, and Scott 2000; Bound, Brown, and Mathiowetz 2001).

6. Notes

[i] The correlation we find across risk measures is at the middle of the correlations for multiple measures of labor market variables such as transfer payments and education reported in Bound, Brown, and Mathiowetz (2001).

[ii] When the measurement error is non-classical IV estimates may produce inconsistent estimates (Black, Berger, and Scott 2001; Frazis and Loewenstein 2002; Kane, Rouse, and Staiger 1999).

[iii] Black, Sanders, and Taylor (2002) argue that the measurement error in schooling in the 1990 Census is negatively correlated with the regression error, suggesting that less able people are more likely to make reporting mistakes and more likely to receive lower wages.

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Table 1. Correlations and OLS Residual Correlations for Male Workers 1995 CPS Outgoing Rotations Data, BLS Risk Data, and NIOSH Risk Data

Basic Controls	no	yes	yes	yes	Yes
Marital Status	no	no	yes	yes	Yes
State	no	no	no	yes	Yes
Industry/Occupation	no	no	no	no	Yes
Correlations					
NIOSH Ind / NIOSH Occ	0.53	0.43	0.43	0.32	0.28
NIOSH Ind / BLS Ind	0.48	0.46	0.45	0.45	0.06
NIOSH Ind / BLS Occ	0.30	0.27	0.27	0.26	0.05
NIOSH Occ / BLS Ind	0.37	0.33	0.33	0.31	0.07
NIOSH Occ / BLS Occ	0.40	0.38	0.38	0.38	0.09
BLS Ind / BLS Occ	0.43	0.40	0.40	0.39	0.22

Note: The residual correlations are based on the OLS regression of the risk variable on a set of independent variables. The basic controls are dummy variables for age, age quartic, education, race, ethnicity, and union coverage. After estimating the residuals for each regression, we estimated the residual correlations for each set of regressions. The number of observations for the 1995 CPS Outgoing Rotations data is 51,140.
Source: Authors' calculations.

Table 2. Covariances and Variances of Residual Estimates for Male Workers 1995 CPS Outgoing Rotation Data, NIOSH Risk Data, and BLS Risk Data

Panel 1. NIOSH Industry / NIOSH Occupation

	Basic Controls	Marital Status	State	Ind/Occ
VAR (Lnwage)	0.25	0.24	0.23	0.21
VAR (NIOSH Ind)	40.02	39.92	33.91	14.80
VAR (NIOSH Occ)	45.75	45.71	38.57	17.09
COV (NIOSH Ind, NIOSH Occ)	18.50	18.43	11.56	4.51
COV (Lnwage, NIOSH Ind)	0.07	0.06	0.15	0.01
COV (Lnwage, NIOSH Occ)	–0.22	–0.23	–0.13	0.02
R^2 Lnwage on X	0.26	0.27	0.29	0.37
R^2 NIOSH Ind on X	0.03	0.03	0.21	0.64
R^2 NIOSH Occ on X	0.09	0.09	0.24	0.66

Panel 2. NIOSH Industry / BLS Industry

	Basic Controls	Marital Status	State	Ind/Occ
VAR (Lnwage)	0.25	0.25	0.24	0.21
VAR (NIOSH Ind)	47.80	47.67	39.58	18.20
VAR (BLS Ind)	59.46	59.36	58.09	35.65
COV (NIOSH Ind, BLS Ind)	24.56	24.45	21.72	1.53
COV (Lnwage, NIOSH Ind)	0.11	0.09	0.18	0.02
COV (Lnwage, BLS Ind)	–0.06	–0.08	–0.04	–0.06
R^2 Lnwage on X	0.25	0.26	0.28	0.36
R^2 NIOSH Ind on X	0.04	0.04	0.21	0.63
R^2 BLS Ind on X	0.06	0.06	0.08	0.43

Table 2 cont. Covariances and Variances of Residual Estimates for Male Workers 1995 CPS Outgoing Rotation Data, NIOSH Risk Data, and BLS Risk Data

Panel 3. NIOSH Industry / BLS Occupation

	Basic Controls	Marital Status	State	Ind/Occ
VAR (Lnwage)	0.25	0.24	0.24	0.21
VAR (NIOSH Ind)	39.69	39.59	32.60	13.75
VAR (BLS Occ)	154.54	154.35	152.86	115.10
COV (NIOSH Ind, BLS Occ)	21.66	21.58	18.90	2.02
COV (Lnwage, NIOSH Ind)	0.06	0.04	0.13	0.01
COV (Lnwage, BLS Occ)	–0.22	–0.23	–0.18	0.06
R^2 Lnwage on X	0.29	0.30	0.32	0.41
R^2 NIOSH Ind on X	0.03	0.03	0.20	0.66
R^2 BLS Occ on X	0.04	0.04	0.05	0.29

Panel 4. NIOSH Occupation / BLS Industry

	Basic Controls	Marital Status	State	Ind/Occ
VAR (Lnwage)	0.25	0.25	0.24	0.21
VAR (NIOSH Occ)	50.20	50.14	42.08	19.02
VAR (BLS Ind)	59.16	59.06	57.83	35.39
COV (NIOSH Occ, BLS Ind)	18.39	18.31	15.62	1.99
COV (Lnwage, NIOSH Occ)	–0.22	–0.23	–0.14	0.02
COV (Lnwage, BLS Ind)	–0.06	–0.07	–0.03	–0.06
R^2 Lnwage on X	0.25	0.26	0.28	0.36
R^2 NIOSH Occ on X	0.09	0.09	0.24	0.65
R^2 BLS Ind on X	0.06	0.06	0.08	0.43

Table 2 cont. Covariances and Variances of Residual Estimates for Male Workers 1995 CPS Outgoing Rotation Data, NIOSH Risk Data, and BLS Risk Data

Panel 5. NIOSH Occupation / BLS Occupation

	Basic Controls	Marital Status	State	Ind/Occ
VAR (Lnwage)	0.25	0.25	0.24	0.21
VAR (NIOSH Occ)	59.99	59.95	51.18	23.63
VAR (BLS Occ)	175.79	175.53	173.78	131.13
COV (NIOSH Occ, BLS Occ)	39.80	39.72	36.50	5.11
COV (Lnwage, NIOSH Occ)	–0.22	–0.23	–0.14	0.02
COV (Lnwage, BLS Occ)	–0.20	–0.21	0.16	0.05
R^2 Lnwage on X	0.25	0.26	0.28	0.36
R^2 NIOSH Occ on X	0.09	0.09	0.24	0.65
R^2 BLS Occ on X	0.04	0.05	0.06	0.29

Panel 6. BLS Industry / BLS Occupation

	Basic Controls	Marital Status	State	Ind/Occ
VAR (Lnwage)	0.25	0.25	0.24	0.2
VAR (BLS Ind)	67.88	67.78	66.45	42.19
VAR (BLS Occ)	175.28	175.02	173.33	130.92
COV (BLS Ind, BLS Occ)	44.03	43.91	42.56	16.43
COV (Lnwage, BLS Ind)	–0.06	–0.08	–0.04	–0.06
COV (Lnwage, BLS Occ)	–0.20	–0.21	–0.16	0.0
R^2 Lnwage on X	0.25	0.26	0.28	0.3
R^2 BLS Ind on X	0.06	0.06	0.08	0.4
R^2 BLS Occ on X	0.04	0.05	0.05	0.2

Source: Authors' calculations.

Table 3. Estimated Price of Risk for White Male Workers

Panel 1. ORG and NIOSH Industry Risk: 1995*				
Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Non-fatal risk rate (3-digit)	no	no	yes	yes
Industry/Occupation	no	no	no	yes
Risk/100,000	118	379	573	123
	(3.28)	(7.68)	(11.68)	(1.90)
VSL in $1,000,000	4.1	13.3	20.1	4.3
*There are 24,567 observations in the regressions.				
Panel 2. ORG and NIOSH Occupation Risk:1995*				
Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Non-fatal risk rate (3-digit)	no	no	yes	yes
Industry/Occupation	no	no	no	yes
Risk/100,000	-515	-438	205	181
	(-14.62)	(-8.13)	(3.57)	(2.21)
VSL in $1,000,000	----	----	7.2	6.3
*There are 24,586 observations in the regressions.				
Panel 3. ORG and BLS Industry Risk: 1995*				
Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Non-fatal risk rate (3-digit	no	no	yes	yes
Industry/Occupation	no	no	no	yes
Risk/100,000	-135	-53.3	189	-178
	(-4.32)	(-1.34)	(4.03)	(-3.20)
VSL in $1,000,000	----	----	6.6	----
*There are 20.920 observations in the regressions.				
Panel 4. ORG and BLS Occupation Risk: 1995*				
Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Non-fatal risk rate (3-digit	no	no	yes	yes
Industry/Occupation	no	no	no	yes
Risk/100,000	-147	-122	167	76.7
	(-7.13)	(-4.84)	(5.07)	(2.16)
VSL in $1,000,000	----	----	5.8	2.7
*There are 17,836 observations in the regressions.				

Note: The dependent variable is the natural log of the worker's real wage. For the basic regression, the independent variables include a quartic in the worker's age, a vector of dummy variables that control for the worker's education, a vector of dummy variables for marital status, a vector of dummy variables indicating whether the worker is Hispanic, Asian, African American, or other race, and a dummy variable indicating whether the worker is under a union contract or not, and dummy variables for the worker's marital status. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses. The data set is for 1995. In this table, we merged the non-fatel risk rates by 3-digit occupation codes.

Table 4. Estimated Price of Risk for White Female Workers

Panel 1. ORG and NIOSH Industry Risk: 1995*				
Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Non-fatal risk rate (3-digit)	no	no	yes	yes
Industry/Occupation	no	no	no	yes
Risk/100,000	110	268	220	-77
	(2.32)	(4.54)	(3.74)	(-1.09)
VSL in $1,000,000	3.9	9.4	7.7	----
*There are 25,343 observations in the regressions.				
Panel 2. ORG and NIOSH Occupation Risk:1995*				
Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Non-fatal risk rate (3-digit)	no	no	yes	yes
Industry/Occupation	no	no	no	yes
Risk/100,000	-437	-92	294	115
	(-9.79)	(-1.12)	(3.52)	(1.13)
VSL in $1,000,000	----	----	10.3	4.0
*There are 24,960 observations in the regressions.				
Panel 3. ORG and BLS Industry Risk: 1995*				
Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Non-fatal risk rate (3-digit)	no	no	yes	yes
Industry/Occupation	no	no	no	yes
Risk/100,000	-121	-31.5	-50.4	-393
	(-2.16)	(-0.44)	(-0.63)	(-3.80)
VSL in $1,000,000	----	----	----	----
*There are 21,853 observations in the regressions.				
Panel 4. ORG and BLS Occupation Risk: 1995*				
Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Non-fatal risk rate (3-digit)	no	no	yes	yes
Industry/Occupation	no	no	no	yes
Risk/100,000	-202	-192	151	137
	(-2.91)	(-2.18)	(1.51)	(1.25)
VSL in $1,000,000	----	----	5.3	4.8
*There are 15,764 observations in the regressions.				

Note: The dependent variable is the natural log of the worker's real wage. For the basic regression, the independent variables include a quartic in the worker's age, a vector of dummy variables that control for the worker's education, a vector of dummy variables for marital status, a vector of dummy variables indicating whether the worker is Hispanic, Asian, African American, or other race, and a dummy variable indicating whether the worker is under a union contract or not, and dummy variables for the worker's marital status. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses. The data set is for 1995. In this table, we merged the non-fatel risk rates by 3-digit occupation codes.

Appendix Table 1: Selective Means

Variable	Men	Women
logarithm of real wage	2.19 (0.573)	1.90 (0.558)
age	39.7 (9.39)	40.1 (9.33)
Education		
less than junior high	0.004	0.002
junior high	0.010	0.007
some high school	0.056	0.044
some college	0.181	0.207
associate degree	0.085	0.111
bachelor's degree	0.234	0.199
master's degree	0.082	0.063
professional degree	0.023	0.012
Ph.D.	0.019	0.008
union coverage	0.021	0.022
Marital status		
widowed	0.005	0.022
divorced	0.106	0.174
never married	0.177	0.135
non-fatal injury risk	1.68 (2.099)	1.17 (1.538)
NIOSH industry fatal injury rate	5.00 (6.997)	2.91 (5.431)
NIOSH occupation fatal injury rate (men n = 24,586) (women n = 24,960)	5.23 (6.737)	2.33 (4.035)
BLS industry fatal injury rate (men n = 20,920) (women n =21,853)	5.91 (7.640)	2.56 (4.159)
BLS occupation fatal injury rate (men n = 17,836) (women n = 15,764)	6.86 (12.036)	2.08 (4.025)

Notes: Authors' calculations. Except where noted, there are 24,567 observations for men and 25,343 observations for women, with the samples corresponding to Panel 1 of Tables 3 and 4. Standard deviations are given in parentheses.