ESTIMATING THE VALUE OF SAFETY WITH LABOR MARKET DATA: ARE THE RESULTS TRUSTWORTHY?

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Estimating the Value of Safety with Labor Market Data: Are the Results Trustworthy?

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Abstract

We use a panel dataset of UK workers to look for evidence of compensating wage differentials for workplace risk, combined with risk data at the four-digit industry level. We discuss various econometric problems associated with the hedonic wage approach, namely the instability of the estimates to specification changes, unobserved heterogeneity, and endogeneity. We find evidence of significant compensating wage differentials and VSL figures only under the most restrictive assumptions, i.e. when we assume that there is no unobserved heterogeneity and that all regressors are exogenous. However, the VSL values are large and vary dramatically with the inclusion or exclusion of industry and/or occupation dummies, as well as with the addition of nonfatal risk. When we specify models that allow for heterogeneity and endogeneity of risk and of other regressors, we find no evidence of compensating wage differentials. We conclude that if compensating differentials for risk exist, econometric problems and the changing nature of labor markets prevent us from observing them. We also conclude that models and techniques for panel data that account for unobserved heterogeneity and endogeneity present a completely different picture about compensating wage differentials than that inferred by most wage-risk studies, which have generally used single cross sections of data.

Keywords: Value of life, labor market, wage hedonics

JEL classification: J17, J28, J31

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I. Introduction

Over the last two decades, a large body of research has documented the existence of substantial inter-industry wage differentials in the US (since Krueger and Summers, 1988) and several other countries (e.g., Palme and Wright, 1992; Lucifora, 1993; Vainiomäki and Laaksonen, 1995; Erdil and Yetkiner, 2001; Sakellariou, 2004; Hsu, 2005)). Alternative explanations for this phenomenon include the existence of efficiency wages, unobserved worker ability (Keane, 1993; Goux and Maurin, 1999; Bhalotra, 2006), and industry-specific skills (Neal, 1995; Weinberg, 2001; Tang and Tseng, 2004). Over roughly the same period, considerable attention has also been dedicated to estimating worker risk premiums. Viscusi (1993) surveys 24 hedonic wage studies conducted in the US between 1974 and 1991 that found a positive association between workplace fatal injury risks and wages, once other worker and job characteristics were controlled for, and Viscusi and Aldy (2003) report on over 60 studies conducted in the US and all over the world over about thirty years.

The rationale of compensating wage studies is that workers must be offered higher wages for them to accept jobs with a greater risk of dying on the job, *ceteris paribus*, and that employers are willing to do so to the extent that it is cheaper than installing safety equipment in the workplace. The demands of workers and the offerings of firms will meet at the tangency points between the workers' indifference curves and the firms' isoprofit curves in the risk-wage space. The hedonic wage relationship is the locus of these tangency points, and the slope of this locus is the willingness to pay for a marginal decrease in risk (or the willingness to accept for a marginal increase in risk).

The tradeoff between pay and risk implicit in a worker's occupational choice is summarized into the so-called Value of a Statistical Life. This is defined as the rate at which individuals are prepared to trade off income for reductions in the risk of death, and can be equivalently described as the total WTP by a group of N individuals experiencing a uniform reduction of 1/N in their risk of dying. Based on their survey of the US literature, Viscusi and Aldy (2003) recommend VSL figures of \$5-9 million (2000 US dollars). Estimates of the VSL based on compensating wage studies are available for several European countries, including the UK, where they usually range between \$4 and \$11 million,[1] and Switzerland (6.5-9.5 million 2000 dollars; Baranzini and Ferro Luzzi, 2001).[2]

The concept of VSL is deemed as the appropriate construct for ex ante analyses of safety regulations and policies, when the identities of the people whose lives are saved by the policy are not known. The notion of VSL is reasonably well accepted in academic and policy circles, and the estimates of the VSL implicit in workers' occupational choices are considered credible enough for the US Environmental Protection Agency to rely on them to estimate the (avoided) mortality benefits delivered by environmental policies that save lives. In its 2000 Guidelines for Economic Analyses, the EPA relied on 21 compensating

[1] In some cases, studies based on UK data have found the VSL to be much larger than the upper bound of this range. For example, Siebert and Wei (1994), Sandy and Elliott (1996), Arabsheibani and Marin (2000) and Sandy (2001) peg the VSL in the range between €4.3 million and €74.4 million (equal to \$4.0 million to \$68.5 million at the 2000 exchange rate). A meta-analysis by CSERGE (1999) generates a range of VSL figures between €2.9 million and €100 million, resulting in weighted average equal to €6.5 million (all 2000 €, the corresponding dollar amounts being \$2.7, \$92.1 and \$6.0 million).

[2] Using the 1995 Swiss Labor Force Survey (SLFS) and the 1994 Swiss Wage Structure Survey (SWSS), Baranzini and Ferro Luzzi peg the VSL implicit in the choices of Swiss workers in the range of CHF 10 to CHF 15 million (Swiss Francs, equivalent to \$6.5 to \$9.5 million 2000 US dollars). They find that the VSL depends on risk level, union coverage, and age.

wage studies, out of a total of 26 studies, to produce a VSL figure ($6.1 million 2000 dollars). This figure was thus based on evidence from labor markets, but was used in subsequent environmental policy analyses.[3]

Recent research, however, has questioned the credibility of many estimates of the VSL from labor markets. Leigh (1995), Arabsheibani and Marin (2000, 2001), Black and Kniesner (2003), and Black et al. (2003) suggest that the estimates of the VSL from compensating wage studies are econometrically very fragile, for reasons that include poorly measured workplace risk, collinearity of risk estimates with industry dummies used to account for inter-industry wage differentials, endogeneity (an individual's level of risk may be determined simultaneously with his job, and hence with the wage), omitted regressors, and heterogeneous preferences for risk and income.

We study the severity of these problems and their effects on the VSL using a panel dataset of UK workers. To our knowledge, this is the first time panel data are used to study the robustness (or lack thereof) of VSL estimates in the UK labor market context.[4] The panel nature of our data allows us to control for unobserved heterogeneity, even when the unobserved individual-specific effects are correlated with included regressors (as in the "within" estimator, which is well suited to fixed effects model), and to apply alternate instrumental-variable techniques (e.g., Hausman and Taylor, 1981) to address endogeneity of risk with wages.

[3] Clearly, doing so assumes that individuals would apply the same marginal rate of substitution between income and risk in both the original and the new policy context. This reliance on labor market estimates of the VSL—which occurs when original estimates of the willingness to pay to reduce the risk of dying in a specified environment context are not available—is not uncontroversial. The use of VSL figures from compensating wage studies when computing the mortality benefits of environmental policies has been criticized on the grounds of the fact that it mirrors the preferences of healthy males whose average age is 40, rather than those of the primary beneficiaries of environmental policies—the elderly and those in compromised health. Adjustments to the VSL for remaining life years were subsequently proposed, and eventually repealed.

[4] See Kniesner et al. (2006) for use of longitudinal data for US workers.

We find that only under the most restrictive assumptions (i.e., when unobserved heterogeneity is ruled out and workplace risks is treated as exogenous, as in a classical regression model estimated by OLS) is there evidence of compensating wages and is the VSL statistically significant. These OLS-based VSL values are, however, sensitive to the inclusion in the model of non-fatal injury risks and variables such as industry and occupation dummies, which may be correlated with risks; they are also suspiciously large and well above the range of values usually considered acceptable (Viscusi and Aldy, 2003). Our preferred estimation approaches—which allow for unobserved heterogeneity, endogenous risks and other endogenous regressors, and exploit our longitudinal data—find no evidence of compensating wage differentials. These results raise serious doubts about the credibility of VSL figures based on labor market studies—which are generally based on cross-sections of data—and underscore the importance of interpreting and using existing VSL estimates with caution in policy analyses.

Despite these results, we note here that the notion of VSL is the theoretically correct metric to use when estimating the mortality benefits of environmental and safety policies, and that there are many other approaches for obtaining the VSL. For example, the value that people place on risk reductions can be inferred from purchases of safety devices (e.g., smoke detectors), from the extra price people pay for products that are safer than others (e.g., automobiles with side airbags; homes in less polluted areas, Portney, 1981), or from the time spent on risk-reducing behaviors (such as fastening seat belts when driving, etc; Blomquist, 2004, Ashenfelter and Greenstone 2004). Contingent valuation and other stated-preference surveys are another widely used option to elicit people's willingness to trade off income for risk reduction (Jones-Lee et al. 1985, Johannesson et al. 1997, Alberini et al. 2004, Tsuge et al. 2005).

The remainder of the paper is organized as follows. In section II, we present the concept of VSL and illustrate how it is usually estimated using labor market data. In section III, we discuss the main econometric limitations of the conventional approach. Section IV illustrates these limitations and explores possible remedies using UK worker data. Section V concludes.

II. VSL Estimates from Compensating Wage Studies

The VSL is the marginal rate at which individuals are prepared to trade off income for risk reductions:

$$VSL = \frac{\partial WTP}{\partial R} \qquad (1)$$

where R is the risk of dying and WTP is the Willingness to Pay for a reduction in risk, i.e., the maximum amount that can be subtracted from an individual's income to keep his or her expected utility unchanged for specified levels of risk. It has been empirically estimated by looking at the time individuals spend engaging in risk-reducing activities (such as fastening seatbelts, Blomquist, 1979), at how much they pay for additional safety features in their vehicles (Atkinson and Halvorsen, 1990; Andersson, 2005) or for safer bicycle helmets (Jenkins et al., 2001), and by directly asking people to report their WTP for a specified hypothetical risk reduction. The bulk of the literature on the VSL, however, comes from compensating wage studies (Viscusi, 1993; Miller, 2000; Viscusi and Aldy, 2003).

Figure 1 shows the hedonic wage curve, which is the locus of tangency between workers' indifference curves (EU) and the firms' isoprofit curves (OC) in the risk wage space. In a typical compensating wage study, data are gathered on the wage rate, education, experience, occupation, and other individual characteristics of workers and workplace characteristics. These data are then used to run a regression relating the wage rate to the risk of fatal and non-fatal injuries, while controlling for the education and experience of the worker, and other job and worker characteristics thought to influence wages. A frequently used specification of the wage regression is:

(2) $$w_i = \beta_0 + \mathbf{x}_i\beta_1 + p_i\beta_2 + q_i\beta_3 + \varepsilon_i$$

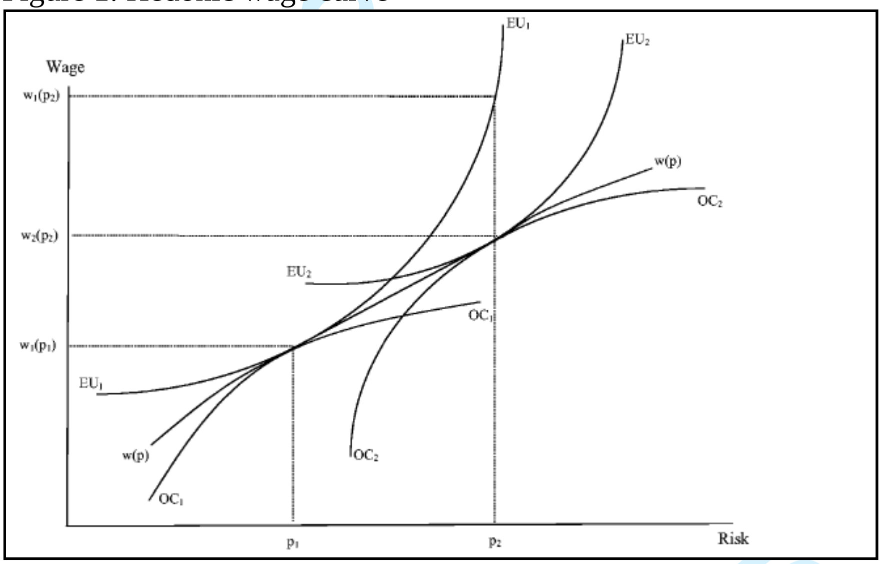
where w_i is the wage rate for worker *i* and, x is a vector of individual, workplace and occupational characteristics, such as experience, education, age, gender, marital status, union status of the worker, industry dummies, occupation dummies, and location dummies.[5] The variable p measures the risk of dying on the job, while q is the risk of non-fatal injuries. The βs are the coefficients to be estimated, and the VSL can be inferred from β_2. For example, if w measures annual earnings and p annual fatal workplace risk in X per 100,000, then the VSL is calculated as $(100{,}000 \times \beta_2)$.

Viscusi (1993) argues that q must be included in the compensating wage equation. Since p and q are generally highly correlated, failure to do so would result in biased estimates of the β_2 coefficient, and hence of the VSL. Viscusi and Aldy (2003) examine some 60 wage-risk studies conducted all over the world, finding that many of them,

[5] In empirical work, the logarithmic transformation of the wage rate often replaces *w* as the dependent variable in the regression. The wage rate, *w*, and fatality risk, *p*, are usually measured on an annual basis.

however, do not control for non-fatal risk.[6] Miller (2000) notes that nonfatal injury risks are highly correlated with fatality risks and that the common practice of including only the former in the hedonic regression to reduce collinearity means that the compensating wage differentials estimated in this fashion are absorbing both VSL and injury risk compensation. Omission of either risk variable is likely to inflate the coefficient on the other risk term (Meng and Smith, 1999).

Figure 1: Hedonic wage curve



Source: Viscusi and Aldy (2003).

Estimates of the compensating wage differentials (the ex ante compensation for risk) should also be influenced by workers' wage replacement rates in the event of an accident (i.e., workers' compensation, which is an ex post compensation) (Moore and Viscusi, 1990a). Meng and Smith (1999) show that the workers' compensation system

[6] In addition, Viscusi (1993) recommends that equation (3) should include expected worker compensation in the event of a non-fatal workplace accident, i.e., (WC×q), where WC is the level of workers compensation paid out to the worker if he experience an accident at work.

reduces risk premium estimates for Ontario workers and conclude that the effects of insurance schemes on pay are substantial.

People's job choices should be influenced by *perceived* risks, not by actual risk levels, but the majority of wage-risk studies has constructed "objective" measures of workplace risks and assigned those measures to individual workers. Until recently workplace fatality and hazard rates were usually available only at the worker's industry or occupation level, but not for industry-occupation pairs (Viscusi, 2003), thus resulting in mismeasured risks. Attention has been usually restricted to male workers and to blue collar professions in an effort to limit measurement errors in the risk variable. It has also been argued—and empirical evidence has been found in support of these notions—that compensating wage differentials should be different for older workers because of their shorter life expectancies (Viscusi and Aldy, 2007), that smokers select into riskier jobs and receive less compensation for such risks (Viscusi and Hersch, 2001) and that compensating wage differentials for risk may vary with race, reflecting different tastes for risk and income that are not correlated with wealth, different life expectancies, and different market opportunities (Viscusi, 2003).

Finally, union status may influence compensating wage differentials for risks, because unions may be better informed about workplace fatality and injury risks and have more bargaining power than individual workers. Mrozek and Taylor (2002) conduct a meta-analysis of the wage-risk studies, and report that those studies where the sample consisted exclusively of union workers have indeed found systematically higher VSLs than the others. In contrast to the US, where most hedonic wage studies report higher VSL for union workers, empirical studies of the UK labor market published in the 1980s and early 1990s detect lower compensating wage differentials for union workers than for non-union workers.[7] Siebert and Wei (1994) attribute this result to the failure to control for endogeneity of risks and union status, both of which are likely to be correlated with unobserved ability. They argue that workers with greater ability demand greater compensation for risk. Since these workers receive higher earnings and choose lower risk jobs, OLS estimates of compensating wage differentials will be downward-biased. At the same time, firms are under pressure to recognize and reward ability so as to offset higher union pay, with the result that union workers have more unmeasured ability than non-union workers. To account for these factors, they estimate switching wage regressions for union and non-union workers, where union membership is endogenous. Risk—which enters in the right-hand side of the two status equations—is also treated as endogenous and instrumented for. Siebert and Wei do find support for their hypothesis that union workers have lower workplace risks and higher VSLs.

III. Exploring and Addressing Limitations of Compensating Wage Studies

Based on Leigh (1995), Black et al. (2003), and Black and Kniesner (2003), one suspects that compensating wage studies are rife with econometric problems. In this section, we discuss three main problems: (1) lack of robustness with respect to specification changes, (2) endogeneity of risk, (3) unobserved heterogeneity among workers, and/or (4) unobserved heterogeneity specifically in the workers' preferences for risk and income.

[7] Dorman and Hagstrom (1998) question the existence of wage-risk compensation for non-union workers in the US.

A. Instability of Risk Coefficients

Evidence from recent studies points to the fact that the estimates of the VSL are not robust to even minor changes in the specification of equation (2), i.e., in the choice of the right-hand side variables. For example, in much empirical work industry dummies are entered in the right-hand side of (2) to capture inter-industry wage differentials, which have been widely documented to exist since Krueger and Summers (1988). Leigh (1995) finds that when industry dummies are included in the regression, the coefficient on risk is no longer significant. He interprets this to imply that inter-industry wage differentials, not compensating wages for risks of dying on the job, explain the positive correlations usually found between wages and mortality risks. He argues that workplace risks tend to be highly correlated with, and end up capturing, unpleasant aspects of jobs in certain industries. Another interpretation is, of course, that even broad industry dummies are strongly correlated with workplace mortality risks, and that such collinearity makes it impossible to disentangle the effects of risks from those of one's industry.

In a recent study commissioned by the US Environmental Protection Agency, Black et al. (2003) explore the issue of stability of the estimates of the price of workplace risk, using three sources of data about individual workers and two sources of risk data. They show that (i) the estimates of the coefficient on risk vary dramatically with small changes in the inclusion of covariates in the right-hand side of the regression model; (ii) many of these coefficient estimates are negative, instead of positive; and (iii) using flexible functional forms confirms these OLS results.

B. Endogeneity and Unobserved Heterogeneity

There is reason to believe that job risk is endogenous with the wage rate, the dependent variable in the regression equation. This is because wage rates and one's job choice (and hence workplace risks) are both likely to be affected by the same individual characteristics (e.g., skills), and these characteristics are usually not well captured using the variables available in most datasets. This results in correlation between risk and the error term in equation (2): $E(p_i\varepsilon_i) \neq 0$ and $E(q_i\varepsilon_i) \neq 0$.

Using a very rich dataset (the National Longitudinal Study, which reports Armed Forces Qualification Test scores), Black et al. find evidence that job risk is indeed correlated with individual characteristics and behaviors not usually available in most commonly used datasets, confirming the above argument and implying that the OLS estimates of the coefficient on risk is biased. Measurement error and self-selection are likely reasons why an effort to estimate a compensating wage model for Italian workers by Barone and Nese (2002) failed to detect a significant relationship between wage rates and objectively measured job risk.

Yet, few empirical studies attempt to correct for endogeneity of wage and risk. Arabsheibani and Marin (2000) begin with treating risk as endogenous, which means that they must instrument for it, and at the same time allow for heterogeneous preferences for risk and income by treating the coefficient on risk as a random variable. However, because of severe collinearity problems, they are not able to compute reliable VSL estimates when correcting for risk endogeneity and end up reporting only results based on treating risk as exogenous.[8] In a later study, Arabsheibani and Marin (2001) suggest that poor instruments are the main cause of these collinearity problems.[9]

This implies that it should be possible to circumvent this problem by finding good instruments and/or by applying techniques based on panel data that construct instruments through transformations of whatever exogenous variables are available (such as the Hausman and Taylor approach, 1981), as we do below.

Using a panel dataset it is also possible to control for unobserved heterogeneity among workers, which occurs when two individuals with identical observed characteristics have systematically different wage rates. Standard approaches include fixed-effects models, which are estimated by the "within" estimator, and random-effects models, which are estimated by GLS. The latter rely on the assumption that the unobserved effects are uncorrelated with the regressors included in the model, while the former are robust to this possibility, but are less efficient (Hsiao, 2003).

C. Possible Estimation Approaches

As described in more detail in the next section, we use a panel dataset that follows workers in the UK to study the issues described in III.A and III.B. Our plan is comprised of five main tasks. The first is to instrument for workplace risks. The second is to estimate fixed effects and random effects models without instrumenting for risk. Large differences in the estimated coefficients across the coefficients from the fixed and

[8] When treating workplace mortality as exogenous, they find evidence of the existence of compensating wage differentials consistent with an average VSL to be around £9.7 million, with lower values for manual workers and larger values for managerial/professional workers.

[9] Wei (1999) instruments for risk and assumes that the north and south of England are separate labor markets to identify the VSL using a structural approach similar to that previously used in Biddle and Zarkin (1988).

random effects models—to be formally assessed using the Hausman test—would be interpreted as evidence of correlation between the individual-specific effects and the regressors included in the model.

Next, we allow for risk to be endogenous in a fixed-effects model, which we estimate by first-differencing the data and using past risk as the identifying instrument for the first-difference in risk. Formally, if the original model is

$$y_{it} = \mathbf{x}_{it}\beta + p_{it}\alpha + c_i + \varepsilon_{it}, \quad (3)$$

with p_{it} potentially correlated with the error term, first-differencing swipes out the fixed effects c_i, resulting in

$$\Delta y_{it} = \Delta \mathbf{x}_{it}\beta + \Delta p_{it}\alpha + \Delta\varepsilon_{it}. \quad (4)$$

If the new error term $\Delta\varepsilon_{it}$ is stationary, this model can be estimated by pooled 2SLS (see Wooldridge, 2002, page 308-309), where $\mathbf{x}_{it}$ (assumed to be strictly exogenous) and p_{it-2} serve as instruments for Δp_{it}. Another advantage of this approach is that if workplace risk is measured with error, and if the p_{it} in (3) is an m-year moving average of the raw annual risk rates (in our application below and in Viscusi, 2004, m=3), taking the first difference should reduce the measurement error that remains in Δp_{it} (Kniesner et al., 2006).[10] As always with instrumental-variable estimation, it is important to test for overidentifying restrictions to make sure that the selected instruments are

[10] The labor economics literature has devoted considerable attention to the issue of mismeasured wages (see, for example, Griliches and Hausman, 1984; Bound and Krueger, 1991; Bound et al., 2001, and Kim and Solon, 2005) and the possible correlation between the measurement error and wages, deriving under which conditions the biases of the coefficients are exacerbated or mitigated by the use of panel data and first-differences. We do not have social security earnings against which to compare the reported wages, so in this paper we are forced to ignore this issue and to take wages at face value. We are also forced to ignore the issue that risks might be mismeasured, with similar consequences, but first-differencing should reduce the seriousness of the risk mismeasurement.

appropriate, i.e., that they are correlated with the endogenous variable Δp_{it} and uncorrelated with the error term in the main equation, $\Delta\varepsilon_{it}$.

Our fourth task is to replace the fixed effect c_i in model (3) with a random effect, and estimate the random-effects model thus obtained using the Hausman-Taylor approach, which allows risk as well as other variables to be endogenous. The Hausman-Taylor approach focuses on the regression equation:

$$\mathbf{y}_i = \mathbf{X}_i\beta + \mathbf{Z}_i\alpha + [v_i\iota + \eta_i], \tag{5}$$

where $\mathbf{y}_i$ is a T×1 vector of observations on the dependent variable for individual i, $\mathbf{X}_i$ is a T×k matrix of time-varying regressors and $\mathbf{Z}_i$ is a T×G matrix of time-invariant regressors. In the term in brackets, v_i is the individual-specific component of the error term, ι is a T×1 vector of ones, and η_i is a T×1 vector of independent error terms. After stacking individual vectors and matrices of variables, equation (5) becomes

$$\mathbf{y} = \mathbf{X}\beta + \mathbf{Z}\alpha + [\mathbf{VW} + \varsigma], \tag{6}$$

where $\mathbf{y}$, $\mathbf{X}$, $\mathbf{Z}$ and ς have nT rows, $\mathbf{V}$ is an nT×1 matrix of individual dummies, and $\mathbf{W}$ is an n×1 vector of idiosyncratic error terms. Further let $\mathbf{\Omega}$ denote the variance covariance matrix of the error terms in brackets.

The Hausman-Taylor approach first distinguishes between exogenous and endogenous (i.e., correlated with $\mathbf{W}$) time-varying and time-invariant regressors, namely $\mathbf{X} = [\mathbf{X}_1 \ \vdots \ \mathbf{X}_2]$ and $\mathbf{Z} = [\mathbf{Z}_1 \ \vdots \ \mathbf{Z}_2]$, where the subscripts 1 and 2 denote the exogenous and endogenous subsets, respectively. It then implements an instrumental-estimation approach corrected for the non-spherical nature of the variance-covariance matrix of the error terms in the brackets in equation (6):

(7) $$\begin{bmatrix}\hat{\beta}\\ \hat{\alpha}\end{bmatrix} = \left[\mathbf{W}'\Omega^{-1/2}\mathbf{P_A}\Omega^{-1/2}\mathbf{W}\right]^{-1}\mathbf{W}'\Omega^{-1/2}\mathbf{P_A}\Omega^{-1/2}\mathbf{y},$$

where $\mathbf{P_A} = \mathbf{A}(\mathbf{A}'\mathbf{A})^{-1}\mathbf{A}'$, and $\mathbf{A}$ is a matrix of instruments. Hausman and Taylor propose using (i) the deviations from the individual's means of the $\mathbf{X}_1$, (ii) the deviations from the means of $\mathbf{Z}_{1,}$ and (iii) $\mathbf{X}_1$ and $\mathbf{Z}_1$ as instruments.

Finally, an alternative type of heterogeneity is the one studied in Arabsheibani and Marin (2000, 2001), who treat risk as endogenous and preferences for risk and income as heterogeneous by allowing the coefficient on risk to be random.[11] Formally, fatal and non-fatal risks are expressed as

(8) $$p_i = \gamma_0 + \mathbf{z}_i\gamma_1 + \varepsilon_{2i}$$

(9) $$q_i = \delta_0 + \mathbf{z}_i\delta_1 + \varepsilon_{3i}.$$

In equations (8) and (9), vector $\mathbf{z}_i$ includes all covariates contained in $\mathbf{x}_i$ plus, as per the definition of instrument, additional variables that determine risk choice but are not correlated with the error term in the wage equation. In practice, we choose the same instruments as in our regular 2SLS procedure.

Coefficients β_2 and β_3 in equation (2) are replaced with $\beta_{2i} = \overline{\beta}_2 + u_{1i}$ and $\beta_{3i} = \overline{\beta}_3 + u_{2i}$, respectively. After substituting these into the wage equation, one obtains:

(10) $$\ln w_i = \beta_0 + \mathbf{x}_i\beta_1 + p_i\overline{\beta}_2 + p_i u_{1i} + q_i\overline{\beta}_3 + q_i u_{2i} + \varepsilon_{1i}.$$

Garen (1984, 1988) proposed a consistent two-step estimation procedure for this model. In the first step, one runs OLS on equations (8) and (9) and forms the residuals $\hat{\varepsilon}_{2i}$ and $\hat{\varepsilon}_{3i}$. In the second step, one runs OLS on the equation:

[11] This type of heterogeneity is thus conceptually distinct from the heterogeneity usually represented by individual-specific fixed or random effects.

$$\text{(11)} \qquad \ln w_i = \beta_0 + \mathbf{x}_i \beta_1 + p_i \overline{\beta}_2 + c_1 \hat{\varepsilon}_{2i} + c_2 \hat{\varepsilon}_{3i} + c_3 \hat{\varepsilon}_{2i} p_i$$
$$+ c_4 \hat{\varepsilon}_{2i} q_i + c_5 \hat{\varepsilon}_{3i} p_i + c_6 \hat{\varepsilon}_{3i} q_i + e_i .$$

For all models and estimation techniques, we fit four specifications to make sure that we examine the effect of adding industry and occupation controls, and non-fatal workplace risks, on the compensating wage differentials.

IV. Application

A. Data

We illustrate the problems discussed in the preceding sections and study their effects on the estimates of the VSL using data on worker characteristics from the British Household Panel Survey (BPHS). These data are collected through annual surveys, called waves, from 1991 through 2003. Each wave roughly corresponds to a different calendar year. Among other things, the dataset contains information about the workers' occupation, hours worked, earnings, experience, tenure with the present employer, education, and family status.

For waves 11, 12, and 13 of the BHPS, which correspond to 2001, 2002, and 2003, we were able to match the BHPS data with three-year moving averages of the workplace fatality rates for the worker's industry at the four digit-SIC code from the UK's Health and Safety Executive (HSE).[12] Using three-year moving averages in lieu of the raw mortality rates is common in compensating wage studies (Viscusi, 2004) to smooth out any unusual fluctuations in fatalities and to reduce the prevalence of industry

[12] Specifically, we average the risk of the current year with that those of the two previous years.

or occupation cells where the mortality rate is zero in a given year. We express fatality rates as X per 100,000 a year.

As in earlier studies, attention is restricted to full-time male workers aged 20-65 living in England and Wales. Because of the very distinct risk-related circumstances of their work environment, we excluded farmers, agricultural workers, firefighters, police officers, persons in the Armed Forces and security personnel from our sample.[13] We also created a subset of workers in blue-collar professions.[14] The total number of workers in our full sample is 2458, for a total of 4940 observations.[15] The sample of blue-collar workers is comprised of 1267 persons and 2352 observations. The panel is unbalanced, with roughly equal numbers of workers being followed for one, two, or all three years.

Descriptive statistics of the samples are displayed in table 1. Summary information about occupational risks is reported in table 2. As shown in table 2, the average risks are 1.24 in 100,000 a year for all workers and 1.79 in 100,000 a year for blue-collar workers. There is also more variability in the risks of blue collar works, which suggests that this subsample might lend itself better to a compensating wage differential study (Moore and Viscusi, 1990b).

[13] SIC 92 categories A, B, P and Q and SOC 90 codes 600-619, 900-903.
[14] We define as blue-collar jobs SOC 80 codes 500-599 (craft and related occupations), 620-699 (personal occupations), 719-722 (sales assistants & check-out operators plus other sales representatives, 733 (scrap dealers and scrap metal merchants), 800-899 (plant & machine operatives), and 910-990 (other occupations except for agriculture, forestry & fishing).
[15] For the 2001-03 period, the BHPS contains 26,703 observations. We lose about half of this sample once we exclude women, 979 observations when we restrict attention to persons aged 18-65, and 734 when we rule out persons in high-risk professions. Of the 11,921 remaining observations, for 285 we were unable to match risk data. For 1,139 we did not have income information; 600 are excluded because these workers report being employed full time, but wages are less than £4,000 a year, and 539 are excluded because the workers do not work full time. This leaves us with 9,358 observations, from which we drop persons that live in Wales or Northern Ireland (losing 2,878 observations) and observations for which three-year average risk rates could not be computed (49 observations). This leaves us with a sample of 6,431 observations, but the usable sample is 4,940 because of missing values for a number of covariates.

Table 1: Descriptive statistics

Waves 11-13		Full sample (n=4940)				Blue collar workers (n=2352)			
Variable	Description	Mean	St.Dev	Min	Max	Mean	St.Dev	Min	Max
incdef	Annual income in 1996 pounds	20,628	12,016	3,781	221,873	16,616	6,947	3,781	142,612
educ	Education in years	10.257	1.089	6	15	9.850	0.810	6	14
experience	Work experience in years	24.3	11.0	3	52	24.1	11.4	3	52
expsq	Experience squared	712.5	574.6	9	2704	711.9	593.7	9	2704
jbot	Overtime hours in normal week	5.296	7.015	0	80	5.557	7.216	0	80
tenure	Years at current job	5.360	6.532	0	46	6.108	7.062	0	46
healthy	(1/0) 1=healthy (self-reported)	0.923	0.266	0	1	0.921	0.270	0	1
white	(1/0) 1=white	0.266	0.442	0	1	0.259	0.438	0	1
union	(1/0) 1=member of union	0.312	0.463	0	1	0.358	0.480	0	1
inlondon	(1/0) 1=lives in inner London	0.022	0.148	0	1	0.014	0.116	0	1
outlondon	(1/0) 1=lives in outer London	0.051	0.220	0	1	0.034	0.181	0	1
southeast	(1/0) 1=lives in SE region	0.173	0.379	0	1	0.137	0.344	0	1
swea	(1/0) 1=lives in SW region or east anglia	0.128	0.334	0	1	0.134	0.341	0	1
midlands	(1/0) 1=lives in Midlands region	0.157	0.364	0	1	0.164	0.370	0	1
manmersey	(1/0) 1=lives in Manchester/Merseyside	0.052	0.223	0	1	0.049	0.216	0	1
restengland	(1/0) 1=lives in other region in England	0.182	0.386	0	1	0.200	0.400	0	1
wales	(1/0) 1=lives in Wales	0.233	0.423	0	1	0.268	0.443	0	1
managers	(1/0) 1: SOC=100-199	0.197	0.398	0	1	0.000	0.000	0	0
professionals	(1/0) 1: SOC=200-299	0.106	0.308	0	1	0.000	0.000	0	0
associates	(1/0) 1: SOC=300-399	0.097	0.295	0	1	0.000	0.000	0	0
clerical	(1/0) 1: SOC=400-499	0.096	0.295	0	1	0.000	0.000	0	0
craft	(1/0) 1: SOC=500-599	0.204	0.403	0	1	0.429	0.495	0	1
service	(1/0) 1: SOC=600-699	0.023	0.151	0	1	0.049	0.217	0	1
sales	(1/0) 1: SOC=700-799	0.043	0.204	0	1	0.042	0.200	0	1
operatives	(1/0) 1: SOC=800-899	0.168	0.374	0	1	0.354	0.478	0	1
y2001	(1/0) 1: year=2001	0.399	0.490	0	1	0.396	0.489	0	1
y2002	(1/0) 1: year=2002	0.284	0.451	0	1	0.284	0.451	0	1
mining	(1/0) 1: SIC 92 section C	0.004	0.067	0	1	0.008	0.090	0	1
manufacturing	(1/0) 1: SIC 92 section D	0.308	0.462	0	1	0.400	0.490	0	1
gaselectric	(1/0) 1: SIC 92 section E	0.016	0.124	0	1	0.015	0.123	0	1
construction	(1/0) 1: SIC 92 section F	0.091	0.288	0	1	0.139	0.346	0	1
wholesale	(1/0) 1: SIC 92 section G	0.129	0.335	0	1	0.125	0.331	0	1
hotelrest	(1/0) 1: SIC 92 section H	0.021	0.142	0	1	0.028	0.164	0	1
transport	(1/0) 1: SIC 92 section I	0.105	0.307	0	1	0.144	0.351	0	1
finance	(1/0) 1: SIC 92 section J	0.020	0.139	0	1	0.001	0.036	0	1
realestate	(1/0) 1: SIC 92 section K	0.117	0.321	0	1	0.051	0.219	0	1
public	(1/0) 1: SIC 92 section L	0.071	0.257	0	1	0.020	0.139	0	1
education	(1/0) 1: SIC 92 section M	0.047	0.211	0	1	0.008	0.087	0	1
health	(1/0) 1: SIC 92 section N	0.034	0.181	0	1	0.023	0.148	0	1
social	(1/0) 1: SIC 92 section O	0.038	0.192	0	1	0.039	0.194	0	1
maedhi_1	(1/0) 1:mother never went to school	0.010	0.097	0	1	0.006	0.080	0	1
maedhi_2	(1/0) 1:mo. left school with no quals	0.390	0.488	0	1	0.427	0.495	0	1
maedhi_3	(1/0) 1:mo. left school with some quals	0.273	0.446	0	1	0.230	0.421	0	1
maedhi_4	(1/0) 1:mo. got further education quals	0.116	0.320	0	1	0.099	0.299	0	1
maedhi_5	(1/0) 1:mo. got univ./ higher degree	0.031	0.173	0	1	0.013	0.114	0	1
motheducmiss	(1/0) 1:mother's education missing	0.180	0.384	0	1	0.223	0.416	0	1
paedhi_1	(1/0) 1:father never went to school	0.008	0.089	0	1	0.006	0.074	0	1
paedhi_2	(1/0) 1:fa. left school without quals	0.352	0.478	0	1	0.371	0.483	0	1
paedhi_3	(1/0) 1:fa. left school with some quals	0.169	0.375	0	1	0.162	0.369	0	1
paedhi_4	(1/0) 1:fa. got further education quals	0.221	0.415	0	1	0.194	0.395	0	1
paedhi_5	(1/0) 1:fa. got univ./ higher degree	0.051	0.220	0	1	0.020	0.139	0	1
fatheducmiss	(1/0) 1:father's education missing	0.200	0.400	0	1	0.247	0.432	0	1

Table 2: Job-related risks by four-digit SIC.

	Full sample (N=4,940)		Blue collar workers (N=2,352)	
	fatal injuries	nonfatal inj.	fatal injuries	nonfatal inj.
Mean (per 100,000)	1.24	161.20	1.79	203.27
Standard deviation	2.60	157.69	3.07	164.07
Minimum	0	0	0	0
Maximum	55.42	1,952.15	55.42	1,952.15

Since we wish to estimate, among other things, fixed-effects models, which rely crucially on the variation in risk over time for the same workers, it is useful to assess the relative importance of the "within" and "between" variation in risk. When attention is restricted to those workers for whom we have repeated observations (1,590 workers in the full sample, and 711 workers in the blue-collar sample), we find that the "within" variation accounts for about 25% of the total variation in risk for the full sample, and for 29% for the blue collar sample. Out of the 1,590 full-sample workers for whom we have repeated observations, 717 (about 45%) change jobs (industry) during the three study years. Out of the 711 blue-collar workers for whom we have repeated observations, 320 (about 45%) change industry during the study period. For both all workers and blue collar workers, there is a change in the industry of employment in 17% of the total observations.[16]

Despite this variability over time, the risks in our sample are small when compared with those in Viscusi (2004), who uses worker data from the 1996 Current Population Survey in the US and where the average fatality risk over 1992-1997 is 4.02 in 100,000. Perusal of workplace fatality rates over four decades show that they have declined over time in the UK, and that they are at their lowest exactly for the period

[16] We are, however, concerned that in some cases this change might be only apparent, perhaps due to a better identification of the industry the worker belongs to. For example, when we look at blue-collar workers whose SIC code changes over the period covered by our study and at the same time report a tenure of zero years (indicating that they are beginning a new job), about 665 appear to be in this situation.

covered by this study: As shown in Figure 2, in the UK fatal workplace accident rates per 100,000 employees fell from about 5.8 in 1959 to 0.7 in 2004. The decline has been quite steady, approximately following an exponential decrease rate of 4.75% per annum.[17]

Figure 2: UK overall fatality rates 1959-2004 (all industries and occupations)

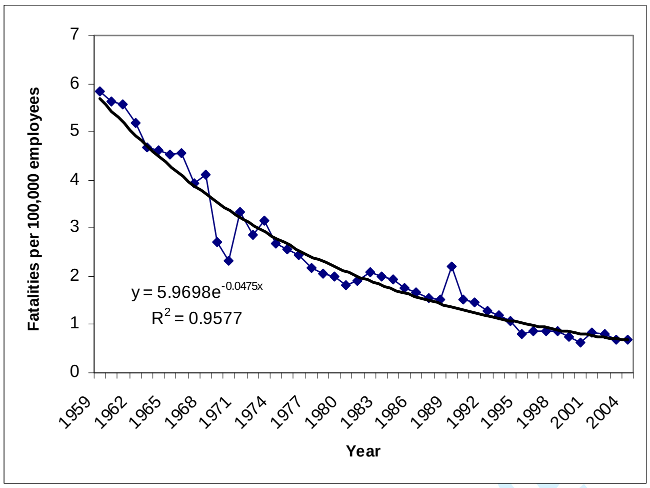


Table 2 also reports descriptive statistics about nonfatal injury risk. Of the two rates provided by HSE, we restrict attention to major injuries, defined as those that result in an absence of work of three or more days. We do so in hopes of limiting reporting incentives and other effects previously noted, among others, by Siebert and Wei (1994). Note that the risk of a fatal injury on the job is not significantly different for union vs. non-union workers.[18]

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[17] Fatality rates declined by 3.1 per 100,000 in the ten years between 1959 and 1969, but the corresponding decrease between 1994 and 2004 was only 0.36. If this process continues at the same rate, only 0.28 lives per 100,000 employees will be saved by safety measures imposed between 2004 and 2014. Further, workplace accidents account for a smaller and smaller share of all-cause mortality risks. For males aged 25-35, for example, the ratio of job mortality risk to the risk of dying for all causes fell from 2.7 % in 1976 to 0.7 % in 2003, the year of wave 13 of the BHPS. Similar trends are seen for other age groups as well.

[18] In the full sample, there are 1,540 and 3,400 union and non-union workers, with mean fatality risks of 1.29 in 100,000 and 1.23 in 100,000, respectively. The standard deviations are 2.98 and 2.41 in 100,000, and the t statistic for the difference in means is 0.879, which means that there are no significant differences across the two groups. Among blue collar workers, 842 are unionized and 1,510 non-unionized. The

B. Robustness of VSL Estimates to the Inclusion/Exclusion of Variables

As discussed above, earlier literature has suggested that the compensating wage differentials, and hence the estimates of the VSL, are not robust to adding explanatory variables—such as industry dummies or non-fatal injury risk—to the right-hand side of the wage equation. Are these results common when fitting hedonic wage equations, or are they specific to the US data for which they were originally claimed?

To answer this question, we specify a linear model similar to equation (3), where the dependent variable is log annual wages and the independent variables include education, experience, experience squared, overtime worked, tenure with the present employer, a good health dummy, a race dummy, union status, regional dummies, plus the risk variable(s) and, depending on the specification, industry and occupation dummies. The model is estimated using OLS.

The results of this exercise are shown in table 3. They suggest that for the full sample the VSL remains relatively stable when industry and occupation dummies are added to the regression equations (columns A and B, respectively), but that further controlling for non-fatal risk almost doubles the VSL (column C), which then falls by about 30% when industry and occupation dummies are further added.[19] Although there appears to be evidence of significant compensating wage differentials, there are reasons to question these results. First, the VSL values are high and well above the values considered acceptable by Viscusi and Aldy (2003) ($5-10 million). The second is that

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corresponding means (standard deviations) are 1.86 (3.6) and 1.75 (1.7) per 100,000, with a resulting t-statistic of 0.766.

[19] The VSL is calculated as the product of β_2 by average wage, times 100,000 (because risk is expressed as XE-05).

they change dramatically when non-fatal risks are included in the equation, and the coefficient on non-fatal risks is *negative*, which does not bode well for establishing the existence of and measuring compensating wage differentials.

Table 3: OLS coefficient on risk variables for different model specifications

Risk based on SIC, OLS	Fatal risk only		Fatal and non-fatal risk	
	A	B	C	D
	No industry or occupation dummies	With industry & occupation dummies	No industry or occupation dummies	With industry & occupation dummies
All workers (n=4940)				
Coeff.	0.0072417	0.0054978	0.0137614	0.009623
t-stat	3.12	2.23	4.60	3.27
Coeff. On nonfatal			-0.0001723	-0.0001272
t-stat			-3.44	-2.56
VSL (million 1996 £)	14.94	11.34	28.39	19.85
Blue collar workers (n=2352)				
Coeff.	0.0134518	0.0069669	0.0139539	0.0117124
t-stat	5.69	2.71	4.43	3.57
Coeff. On nonfatal			-0.0000143	-0.0001421
t-stat			-0.24	-2.33
VSL (million 1996 £)	22.35	11.58	23.19	19.46

All regressions control for experience, experience squared, overtime hours worked, tenure, and dummies for health status, union membership, race and location. Mean labor income: £20,628 (full sample), £16,616 (blue collar). The VSL is expressed in million British pounds in 1996 prices; to convert to 2000 dollars, multiply by 1.66.

The second panel displays results and implicit VSLs for blue collar workers. When we control for fatal and non-fatal risk, the VSL (column C) is similar to that of column A, and adding further industry and occupation controls (column D) has a relatively modest effect (a 15% reduction) on the VSL.[20] However, there is a 50% reduction in the VSL when industry and occupation controls are added in the equation that uses only fatal risks. Moreover, the VSL figures are, again, high. Caution should be

[20] As Leigh (1995) has shown, apparent wage/risk differentials can be caused by inter-industry wage differentials. In our data, coefficients remained significant even after including industry dummies, which is probably due to the fact that risk is measured on the 4-digit SIC level, whereas the industry dummies were based on major categories.

used when interpreting these results. Viscusi (2003) notes that if most accidents happen to blue-collar workers, but fatality rates are computed using all workers in a particular industry as the denominator, blue-collar risks will be understated and their VSL overstated. Since we are unable to calculate a risk rate specific for blue collar workers only, our estimates of the VSL for blue collar workers are potentially affected by this problem.

C. Exploiting the Panel Structure of the Data

Results for the fixed effects (FE) and the random-effects (RE) models are reported in the top panel of table 4 for all workers and of table 5 for blue-collar workers. For the full sample, the "within" and GLS coefficients on fatal risk are generally positive (with one exception), but always small and statistically insignificant. Controlling for non-fatal risks strengthens results somewhat, but, as with OLS estimation, non-fatal risks continue to be negatively associated with wages.

Taken together, these results question the very existence of compensating wage differentials, or at least our ability to identify them. The estimates of the VSL are statistically insignificant in five cases out of eight and significant at the 10% level in two cases. Hausman tests reject the null of no correlation between individual-specific effects and the included regressors, suggesting that we should not trust the results from GLS estimation of the RE model. If we rely on the FE model, however, there is no evidence at all of an association between risk and wages.

For blue collar workers, the "within" coefficients on risks are positive but insignificant, whereas their GLS counterparts are positive and significant, and their magnitudes double when non-fatal workplace risks are added in the regression equation. As before, Hausman tests reject the null of uncorrelation between individual-specific effects and included regressors, implying that the GLS estimates and VSLs are biased.

D. Endogeneity of Risk

The Hausman tests confirm the notion that individual characteristics that influence wages but cannot be observed and get soaked up in the error term of (3) may be correlated with workplace risk, making the OLS estimates biased and inconsistent. To circumvent the problem of endogeneity, we instrument for workplace risk and estimate equation (3) using 2SLS. Our instruments are all of the right-hand side variables of equation (3) plus dummies for the educational attainment of the worker's mother and father, respectively. (We had earlier considered as candidate identifying instruments, and subsequently rejected, non-labor income, marriage status and wife income, and homeownership status, which were found to have virtually no predictive power for the choice of workplace risks and/or failed tests of overidentifying restrictions.) Note that union membership was never a significant predictor of risk.

The results from 2SLS and the Garen procedure are displayed in the second panel of table 4 (for the full sample) and table 5 (for blue-collar workers). For simplicity, we restrict attention to the models that include only fatal risk. The coefficients on this variable are very similar across the two estimation procedures and robust to adding industry and occupation dummies, but *negative* and significant, which is against the notion of compensating wage differentials. In general, we are dissatisfied with 2SLS and the Garen procedure: The coefficients on risk for all workers are an order of magnitude larger (in absolute value) than those for blue collar workers, and the (absolute) magnitude of the former is implausible.[21]

Our next approach allows for fixed effects and endogeneity between risks and wages. We first-difference the data to swipe out the fixed effects, and then we instrument for Δp_{it} using the lagged exogenous variables (job overtime, health status of the worker, location dummy) and risk in the levels lagged twice as our identifying instrument (Wooldridge, 2002). The results of the pooled 2SLS on the first differences are reported in the bottom panel of table 4 for the full sample and in the bottom panel of table 5 for the blue collar workers. In both cases, the coefficient on risk is negative and insignificant at the conventional levels. There is no evidence of compensating wage differentials, but the econometric procedure is more satisfactory than the earlier ones, in that (i) the tests for overidentifying restrictions never reject the null, and (ii) the coefficients on risk are similar across the two samples. Similar results—no significant association between risk and wages—are obtained if we use P_{t-1} or P_{t-3} in lieu of P_{t-2}, and if we use P_{t-2} and P_{t-3} as identifying instruments. The sign of the estimated α in equation (4) sometimes changes as we replace one lagged risk with another, but the estimated coefficients are always completely insignificant (results available from the authors upon request).

[21] One possible explanation for this is that only roughly 3-5% of the variation in job risk for all workers can be predicted using the instruments, with the remainder being absorbed into the residuals $\hat{\varepsilon}_{2i}$ As a consequence, the residuals are highly correlated with job risk and with $\hat{\varepsilon}_{2i}p_i$ in equation (9). The correlation coefficient between $\hat{\varepsilon}_{2i}$ and p_i is 0.98 for all workers and also for blue-collar workers, which explains why the regression coefficient β_2 is so sensitive to the inclusion of the residuals. Arabsheibani and Marin (2001) report that they encountered the very same problem, despite using a broader set of instruments and obtaining much better first-stage R squares, and conclude that compensating risk differentials extracted in this way should be viewed with great caution due to multicollinearity.

Table 4: Compensating wage regression results for the full sample

Risk based on SIC, all workers	Fatal risk only		Fatal and nonfatal risk	
	No industry or occupation dummies	With industry & occupation dummies	No industry or occupation dummies	With industry & occupation dummies
FE (n=4072)				
Coeff. on mortality rates	-0.0007503	0.0002574	0.0008379	0.0018583
t-stat	-0.37	0.12	0.34	0.74
Coeff. on nonfatal injury rates			-0.0000479	-0.0000544
t-stat			-1.18	-1.27
VSL	n/a	0.53	1.73	3.83
RE (n=4072)				
Coeff. on mortality rates	0.0012881	0.0024189	0.0039372	0.0047166
z-stat	0.68	1.15	1.71	1.93
Coeff. on nonfatal injury rates			-0.0000772	-0.0000757
z-stat			-2.02	-1.82
Hausman test statistic	265	1267	273	1298
Chisq degrees of freedom	12	31	14	33
p-value	<0.0001	<0.0001	<0.0001	<0.0001
VSL	2.66	4.99	8.12	9.73
2SLS (n=4940)				
Coeff. on mortality rates	-0.1949241	-0.1581687		
t-stat	-4.27	-2.48		
VSL	n/a	n/a		
Test for overidentifying instr.	20.25	22.23		
Chisq degrees of freedom	26	46		
p-value	0.7796	0.9988		
Garen proc. (n=4940)				
Coeff. on mortality rates	-0.193525	-0.15676		
t-stat	-6.79	-3.39		
VSL	n/a	n/a		
First differences 2SLS (n=2197)				
Coeff. on mortality rates	-0.0038741	-0.0046378	-0.0102986	-0.0082599
t-stat	-0.81	-0.85	-1.03	-0.85
Coeff. on nonfatal injury rates			0.0001229	0.0000919
t-stat			0.79	0.59
VSL	n/a	n/a	n/a	n/a
Test for overidentifying instr.	15.38	15.38	14.94	40.21
Chisq degrees of freedom	24	43	24	43
p-value	0.9091	>0.9999	0.9225	0.5930
Hausman-Taylor (n=4072)				
Coeff. on mortality rates	-0.0006194	0.0004424	0.0010411	0.0020218
z-stat	-0.34	0.22	0.47	0.88
Coeff. on nonfatal injury rates			-0.0000501	-0.0000537
t-stat			-1.36	-1.36
VSL	n/a	0.91	2.15	4.17

All regressions control for experience, experience squared, overtime hours worked, tenure, and dummies for health status, union membership, race and location. Mean labor income: £20,628 (full sample), £16,616 (blue collar). The VSL is expressed in million British pounds in 1996 prices; to convert to 2000 dollars, multiply by 1.66.

Table 5: Compensating wage regression results for blue-collar workers

Risk based on SIC, blue collar workers only	Fatal risk only		Fatal and nonfatal risk	
	No industry or occupation dummies	With industry & occupation dummies	No industry or occupation dummies	With industry & occupation dummies
FE (n=1796)				
Coeff. on mortality rates	0.0008267	0.0020324	0.004904	0.0049259
t-stat	0.33	0.79	1.55	1.52
Coeff. on nonfatal injury rates			-0.0001227	-0.0000934
t-stat			-2.05	-1.49
VSL	1.37	3.38	8.15	8.18
RE (n=1796)				
Coeff. on mortality rates	0.0047789	0.0048204	0.0081148	0.0093041
z-stat	2.14	2.03	2.82	3.13
Coeff. on nonfatal injury rates			-0.0000992	-0.0001422
z-stat			-1.84	-2.51
Hausman test statistic	114	223	118	247
Chisq degrees of freedom	11	25	12	26
p-value	<0.0001	<0.0001	<0.0001	<0.0001
VSL	7.94	8.01	13.48	15.46
2SLS (n=2352)				
Coeff. on mortality rates	-0.0331052	-0.0920726		
t-stat	-1.12	-1.68		
VSL	n/a	n/a		
Test for overidentifying instr.	38.57	23.99		
Chisq degrees of freedom	26	42		
p-value	0.0536	0.9885		
Garen proc. (n=2353)				
Coeff. on mortality rates	-0.0307729	-0.0913419		
t-stat	-1.12	-2.13		
VSL	n/a	n/a		
First differences 2SLS (n=970)				
Coeff. on mortality rates	-0.0043936	-0.0053483	-0.0158598	-0.0226084
t-stat	-0.53	-0.55	-0.50	-0.73
Coeff. on nonfatal injury rates			0.0001824	0.0002946
t-stat			0.42	0.69
VSL	n/a	n/a	n/a	n/a
Test for overidentifying instr.	14.16	11.74	13.58	10.96
Chisq degrees of freedom	22	35	22	35
p-value	0.8957	0.9999	0.9157	>0.9999
Hausman-Taylor (n=1796)				
Coeff. on mortality rates	0.0007297	0.0020311	0.0047708	0.0049165
z-stat	0.24	0.82	1.21	1.12
Coeff. on nonfatal injury rates			-0.0001216	-0.0000917
t-stat			-1.63	-1.07
VSL	1.21	3.37	7.93	8.17

All regressions control for experience, experience squared, overtime hours worked, tenure, and dummies for health status, union membership, race and location. Mean labor income: £20,628 (full sample), £16,616 (blue collar). The VSL is expressed in million British pounds in 1996 prices; to convert to 2000 dollars, multiply by 1.66.

One limitation of the above approach is that we would like to allow other regressors to be endogenous with wage, and are concerned about good instruments and the efficiency of the estimates. To circumvent these problems, we implement the Hausman-Taylor procedure, treating education as an endogenous time-invariant regressor, and experience, experience squared, tenure, union status, and occupation and industry dummies as endogenous time-varying regressors. Race, good health, geographical dummies, and job overtime are regarded as exogenous variables.

The results show that whether or not one controls for non-fatal risk, the coefficients on fatal risk are positive, but statistically insignificant for the both the full sample and the subset of blue-collar workers. (Only in one specification, that of column (A) for the full sample, do we get a negative coefficient on risk.) The estimates of the VSL are insignificant, but within reasonable ranges—£3-8 million for all workers and £1.21-8.17 for blue collar workers.

V. Conclusions

Much of the earlier literature on compensating wages and VSL is based on cross-sections of workers and on the assumption that workplace risks are exogenous, raising the question whether the VSL figures they obtain are biased. Unlike previous studies, we use panel data documenting wages and other individual characteristics of UK workers to examine whether the evidence of compensating wage differentials for workplace risk and to recover estimates of the VSL implicit in workers' job choices. We have risk data at the four-digit industry level.

With this particular dataset, any evidence of compensating wage differential is at best inconclusive. We get large VSL figures only under the strongest possible assumptions, i.e., when unobserved heterogeneity and endogenous regressors are ruled out. Even so, the VSLs are suspiciously large and often very sensitive to the inclusion/exclusion of certain key regressors. Another problematic result is that when we include non-fatal workplace risks in the regression, its coefficient is negative (and often significant). We attribute the latter result to the correlation between fatality rates and the risk of non-fatal injuries (Miller, 2000, Dillingham et al., 1996), pervasive measurement errors in non-fatal injury rates (Kniesner and Black, 2003), and reporting problems with non-fatal injuries (Siebert and Wei, 1994).

As soon as we allow for unobserved heterogeneity and use an estimation technique that does not rely on the assumption of uncorrelation between unobserved effects and included regressors, the evidence of risk premiums disappears. We reach the same conclusion when we further allow for endogenous risks and right-hand side variables, in addition to unobserved effects. In many cases, the coefficient on fatal risks is negative and insignificant, providing no empirical support for the existence of compensating wage differentials.

In general, one would expect the OLS estimates of the coefficient on risks to be downward biased if risks are endogenous or mismeasured, assuming that the measurement error is classical (i.e., there is no correlation between it and the included regressors or the dependent variable). Remedying these problems through appropriate estimation techniques would thus be expected to produce larger coefficients on risks, and hence larger VSLs. Siebert and Wei (1994) show that if workers with greater unmeasured ability obtain better pay and choose safer jobs than less skilful workers, pay may appear to be negatively correlated with risks, even though more skilful workers actually receive large compensating wage differentials for a given level of risk. On the assumption that union workers are likely to have greater unobserved ability, Siebert and Wei estimate switching regression equations where union membership is endogenous and workplace risks are also treated as endogenous, and find that union workers have *higher* compensating wage differentials. Pooled-sample OLS estimation would incorrectly lead one to the opposite conclusion.

In our case, when we instrument for risks and try to purge out the measurement error, we see any evidence of compensating wage differentials vanish. Our results are thus in contrast with the standard attenuation bias story and with the hypothesis that more skilful workers will select better pay and lower risks. They are, however, consistent with the possibility that risk measurement errors might be correlated with other included regressors, in which case it is difficult to sign the bias of the OLS estimates (Kniesner and Black, 2003; Black et al., 2003). This would happen if, for example, within a certain industry assignment to riskier shifts is correlated with worker characteristics such as age, gender, and ethnicity.[22] They are also consistent with the possibility formally analyzed by Shogren and Stamland (2002) that workers might be heterogeneous in preferences (for risks and income) *and* skills, in which case higher-risk jobs are chosen by individuals

[22] Kniesner and Black illustrate this case for the fast-food industry. If younger and female clerks are assigned to daytime shifts, while older males do nighttime shifts, when the likelihood of robberies is higher, then the risks of younger and female clerks are overstated and those of older males are understated. Clearly, the measurement error is correlated with age and gender.

who are more risk-tolerant or (feel that they) have skills to avoid fatal accidents on the job. Shogren and Stamland show that this would overstate the VSL.[23]

It is also possible that compensating wage differentials these days are detectable only when attention is restricted to specific occupations, industries, or firm sizes, and that our approach has simply been unable to pick them up. It is natural to think about the contrast between union and non-union workers, but we do not believe our data warrant the model deployed by Siebert and Wei. In our sample, pay is higher among union members when we look at the full sample, but when we restrict attention to blue collar workers the association between pay and union status (controlling for other worker characteristics) is weaker. Moreover, we find that risks tend to be roughly the same among union and non-union individuals.

In sum, our results raise doubts about the existence of compensating wage differentials for job risk and/or the analyst's ability to detect them using recent labor market data, despite the availability of longitudinal data and the opportunity to control for unobserved worker characteristics. Caution should, of course, be used when interpreting these results. For starters, workplace risks in our dataset are low during our study period—indeed, at a historical low for the UK—which may have impaired our ability to detect compensating wage differentials (or to rule out with confidence their existence). Mrozek and Taylor's meta-analysis (2002) finds that the estimated VSLs tend to be quadratic in risks, with the lowest VSLs being found in the studies with the lowest and highest risk levels. The average risks in our sample are well *below* those in the studies

[23] This would happen because we infer the compensating wage differential from a marginal worker—a low-skill, high-risk worker—who is the one that demands the most compensation for workplace risks. But this maximum compensating differential is divided by the average risks to get the VSL, resulting in an overestimate of the latter.

covered by Mrozek and Taylor, even when attention is restricted to blue collar workers, placing us in the range where small or no compensating wage differentials are observed.

Second, workplace fatality risk has been declining sharply over the period 1959-2004. This reflects changes that may be legally and technologically driven, making the notion of stable compensating wage differentials that result from negotiation between the worker and the employer very suspect. This does not, of course, mean that people do not value safety, and indeed Costa and Kahn (2004) have documented that in the US over the last few decades, workplace risks have declined and the VSL has risen, which means that individuals have placed an increasingly large value on safety.

Third, we took great pains to look for good instruments for workplace risks, but future studies might be designed to exploit exogenous changes in workplace regulations, which might serve as instruments for workplace risks. Since we have not been able to track the recent dynamics of such shifts, we conclude that *if* compensating differentials for risk exist, a host of econometric problems as well as a changing workplace risk environment prevent us from observing them.

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