The Value of a Statistical Life: Evidence from Panel Data

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Abstract

We address long-standing concerns in the compensating wage differentials literature: the econometric properties of the estimated value of statistical life (*VSL*) and the wide range of such estimates. We confront prominent econometric issues using panel data, a more accurate fatality risk measure, and systematic application of panel data estimators. Controlling for measurement error, endogeneity, latent individual heterogeneity possibly correlated with regressors, state dependence, and sample composition yields *VSL* estimates of $4 million to $10 million. The comparatively narrow range clarifies the cost-effectiveness of regulatory decisions. Most important econometrically is controlling for latent heterogeneity; less important is how one does it.

1. Introduction

The value of statistical life (*VSL*) concept based on econometric estimates of wage-fatality risk tradeoffs in the labor market is well established in the economics literature. The method provides the yardstick that the U.S. Office of Management and Budget (OMB) requires agencies to use in valuing fatality risks reduced by regulatory programs.[1] More recently, *VSL* estimates have also provided the basis for assessing a broad range of issues from the mortality costs of the Iraq war (Wallsten and Kosec, 2005, Bilmes and Stiglitz, 2006) to a refined measurement of economic growth (Jena, Mulligan, Philipson, and Sun, 2008). Notwithstanding the wide use of the *VSL* approach, there is still concern over excessively large/small estimates and the wide range of *VSL* estimates. One approach to dealing with the dispersion of *VSL* estimates, which has been used by the U.S. Environmental Protection Agency, has been to rely on meta analyses of the labor market *VSL* literature. Our research demonstrates how using the best available data and improved econometric practices yields a fairly narrow range of VSL estimates.

We begin with an econometric framework that is a slight extension of the usual hedonic wage equation used in the value of statistical life literature. For worker i (i = 1,…,N) in industry j (j = 1,…,J) and occupation k (k = 1,…,K) at time t (t = 1,…,T) the hedonic tradeoff between the wage and risk of fatality is described by

$$\ln w_{ijkt} = \alpha_{0i}^{+} + \alpha_{0i}^{-} + \alpha_1 \pi_{jkt} + X_{ijkt}\beta + u_{ijkt}\,, \qquad (1)$$

where ln w_{ijkt} is the natural log of the hourly wage rate; π_{jkt} is the industry and occupation specific fatality rate; X_{ijkt} is a vector containing dummy variables for the worker's one-digit occupation and two-digit industry, state and region of residence, plus the usual demographic variables: worker education, age and age squared, race, marital status, and union status; u_{ijkt} is an

error term allowing conditional heteroskedasticity and within industry by occupation autocorrelation.[2] Equation (1) is slightly unfamiliar as it contains two latent individual effects: one that is positively correlated with wages and the fatality rate (α_{0i}^{+}) and one that is positively correlated with wages and negatively correlated with the fatality rate (α_{0i}^{-}). The first individual effect reflects unmeasured job productivity that leads more productive/higher wage workers to take safer jobs; the second individual effect reflects unmeasured individual differences in personal safety productivity that leads higher wage workers to take what appears to be more dangerous jobs because the true danger level for such a worker is lower than the measured fatality rate. Our research uses equation (1) in conjunction with a variety of econometric techniques, which demonstrates the capabilities of individual panel data that incorporate fatality risk measures that vary by year to account for these two latent effects.

To set the stage, an extremely wide range of labor market *VSL* estimates from micro cross-section data has generated a series of prominent econometric controversies reviewed by Viscusi and Aldy (2003). Hedonic equilibrium in the labor market means that equation (1) traces out the locus of labor market equilibria involving the offer curves of firms and the supply curves of workers. A salient concern in estimating and interpreting equation (1) involves the fatality risk variable, which ideally should serve as a measure of the risk beliefs of workers and firms for the particular job. Broadly defined risk measures, such as risk pertinent to one's industry or general occupation, may involve substantial measurement error. Other concerns are over the potential endogeneity of the job risk measure (Ashenfelter and Greenstone, 2004a) and possible state dependence in wages (MaCurdy, 2007). Here we will exploit the capabilities of a highly refined risk measure defined over time and by occupation and industry, coupled with panel data on workers' labor market decisions, to resolve many prominent issues in the hedonic labor market

literature. Because our focus is on the average *VSL* across a broad sample of workers, we will consequently not explore emerging interest in the heterogeneity of *VSL* by age and other personal characteristics (Kniesner, Viscusi, and Ziliak, 2006; Aldy and Viscusi, 2008; Kniesner, Viscusi, and Ziliak, 2010).

We devote particular attention to measurement errors, which have been noted in Black and Kniesner (2003), Ashenfelter and Greenstone (2004b), and Ashenfelter (2006). We use more detailed data on objective risk measures than in the VSL studies that are discussed in these articles on measurement error of the fatality risk variable. Published industry risk beliefs are strongly correlated with subjective risk values, and we follow the standard practice of matching to workers in the sample an objective risk measure.[3] Where we differ from most previous studies is the pertinence of the risk data to the worker's particular job, and ours is the first study to account for the variation of the more pertinent risk level within the context of a panel data study. Our econometric specifications also account for the possibility that workers are driven by risk expectations.

We address the pivotal issue of measurement error in several ways. The fatality risk variable is not by industry or occupation alone, as is the norm in almost all previous studies, but is a refined measure based on 720 industry-occupation cells. We use not only one-year but also three-year averages to reduce the influence of random year-to-year fluctuations.[4] Because the fatality rate data are available by year, workers in our panel who do not change jobs can also have a different fatality risk in different years. In contrast, the most prominent panel-based labor market *VSL* study used the same occupational risk measure based on the 1967 Society of Actuaries data for 37 narrowly defined high risk occupations for all years, so that all possible variation in risk was restricted to workers who changed occupations (Brown, 1980). Our research

also explores using adjacent observation differences, for which the influence of measurement error should be less pronounced (Griliches and Hausman, 1986). In addition, we examine how instrumental variable estimates for each approach attenuates measurement error bias. Finally, our rational expectations and dynamic first-difference models' estimates make it possible to include longer-run worker adaptations to changes in their job risk level that may occur if they are not perfectly informed about the risk initially.

As noted earlier, potential biases in *VSL* estimates can arise from un-modeled worker productivity and safety-related productivity as reflected in (α_{0i}^{+}) and (α_{0i}^{-}) in equation (1) (Hwang, Reed, and Hubbard, 1992; Viscusi and Hersch, 2001; Shogren and Stamland, 2002). Panel data allow the researcher to sweep out all such time invariant individual effects and to infer their relative importance in terms of biasing *VSL* if ignored econometrically. In each instance, we use the pertinent instrumental variables estimator. Our work also distinguishes job movers from job stayers. We find that most of the variation in risk and most of the evidence of positive *VSL*s stems from people changing jobs across occupations or industries possibly endogenously rather than from variation in risk levels over time in a given job setting. Although our study addresses many forms of endogeneity (latent heterogeneity, measurement error, state dependence), we do not formally model the joint choice of wages and industry-occupation, and the attendant fatality risk, as discussed in Ashenfelter (2006).

Our econometric refinements using panel data have a substantial effect on the estimated *VSL* levels. They reduce the estimated *VSL* by more than 50 percent from the implausibly large cross-section PSID-based *VSL*s of $20–30 million. We demonstrate how systematic econometric modeling narrows the estimated value of a statistical life from about $0–$30 million to about $4

million–$10 million, which we then show clarifies the choice of the proper labor market based *VSL* for policy evaluations.

2. Panel Data Econometric Framework

Standard panel-data estimators permitting latent worker-specific heterogeneity through person-specific intercepts in equation (1) are the deviations from time-mean (within) estimator and the time-difference (first-difference) estimators. The fixed effects include all person-specific time-invariant differences in tastes and all aspects of productivity, which may be correlated with the regressors in X. The two estimators yield identical results when there are two time periods and when the number of periods converges towards infinity. When there is a finite number of periods with $T > 2$, estimates from the two different fixed-effects estimators can diverge due to possible non-stationarity in wages, measurement errors, or model misspecification (Wooldridge 2002). Because wages from longitudinal data on individuals have been shown to be non-stationary in other contexts (Abowd and Card, 1989; MaCurdy, 2007), we adopt the first-difference model as a baseline.

The first-difference model eliminates time-invariant effects by estimating the changes over time in hedonic equilibrium

$$\Delta \ln w_{ijkt} = \alpha_1 \Delta \pi_{jkt} + \Delta X_{ijkt} \beta + \Delta u_{ijkt} , \qquad (2)$$

where Δ refers to the first-difference operator (Weiss and Lillard, 1978).

The first-difference model could exacerbate errors-in-variables problems relative to the within model (Griliches and Hausman, 1986). If the fatality rate is measured with a classical error, then the first-difference estimate of $\hat{\alpha}_1$ may be attenuated relative to the within estimate. An advantage of the regression specification in equation (2), which considers intertemporal changes in hedonic equilibrium outcomes, arises because we can use so-called wider (2+ year)

differences. If $\Delta \geq 2$ then measurement error effects are mitigated in equation (2) relative to within-differences regression (Griliches and Hausman, 1986; Hahn, Hausman, and Kuersteiner, 2007). Our baseline model sets $\Delta = 2$, and as discussed in the data section below, we additionally address the measurement error issue in the fatality rate by employing multi-year averages of fatalities.

Lillard and Weiss (1979) demonstrated that earnings functions may not only have idiosyncratic differences in levels but also have idiosyncratic differences in growth. To correct for wages that may not be difference stationary as implied by equation (2) we estimate a double differenced version of equation (2) that is

$$\Delta^2 \ln w_{ijkt} = \alpha_1 \Delta^2 \pi_{jkt} + \Delta^2 X_{ijkt}\beta + \Delta^2 u_{ijkt}, \tag{3}$$

where $\Delta^2 = \Delta_t - \Delta_{t-1}$, commonly known as the difference-in-difference operator.

Finally, we also estimate a dynamic version of equation (2) by adding $\gamma\Delta \ln w_{ijkt-1}$ to the right-hand side and using two first-difference instrumental variables estimators: (i) using the two-period lagged level of the dependent variable as an identifying instrument for the one-period lagged difference in the dependent variable (Greene, 2012, Chapter 11) and (ii) using an instrument set that grows as the time-series dimension of the panel evolves (Arellano and Bond 1991). The lagged dependent variable controls for additional heterogeneity and serial correlation plus sluggish adjustment to equilibrium (state dependence). We therefore compare the estimated short-run effect, $\hat{\alpha}_1$, to the estimated long-run effect, $\hat{\alpha}_1/(1-\hat{\gamma})$, and their associated *VSL*s.

2.1 Comparison Estimators

If $E[u_{ijk} \mid \pi_{jk}, X_{ijk}] = 0$ and $E[\alpha_{0i}^{+,-} \mid \pi_{jk}, X_{ijk}] = 0$, which are the zero conditional mean assumptions of least squares regression, then OLS estimation of the hedonic equilibrium in

equation (1) using pooled cross-section time-series data is consistent. If the zero conditional mean assumption holds, which is unlikely to be the case, then the two basic estimators frequently employed with panel data, the between-groups estimator and the random-effects estimator, will also yield consistent coefficient estimates.

The between-groups estimator is a cross-sectional estimator using individuals' time-means of the variables

$$\overline{\ln w_{ijk}} = \alpha_1 \overline{\pi_{jk}} + \overline{X_{ijk}}\beta + \overline{\delta} + \overline{u_{ijk}}\,, \tag{4}$$

with $\overline{\ln w_{ijk}} = \frac{1}{T}\sum_{t=1}^{T} \ln w_{ijkt}$ and other variables similarly defined. A potential advantage of the between-groups estimator is that measurement-error induced attenuation bias in estimated coefficients may be reduced because averaging smoothes the data generating process. Because measurement error affects estimates of the *VSL* (Black and Kniesner, 2003; Ashenfelter, 2006), the between-groups estimator should provide improved estimates of the wage-fatal risk tradeoff over pooled time-series cross-section OLS estimates of equation (1).

The random-effects model differs from the OLS model in equation (1) by explicitly including the latent heterogeneity terms, $\alpha_{0i}^{+}, \alpha_{0i}^{-}$, in the model's error structure, but is similar to OLS in that this additional source of error is also treated as exogenous to the fatality risk and other demographic variables. The implication is that selection into possibly risky occupations and industries on the basis of unobserved productivity and tastes is purely random across the population of workers. Although both the pooled least squares and between-groups estimators remain consistent in the presence of random heterogeneity, the random-effects estimator will be more efficient because it accounts for person-specific autocorrelation in the wage process. The

random-effects estimator is thus a weighted average of the between-groups variation and the within-groups variation.

Finally, suppose that selection into a particular industry and occupation is not random with respect to time-invariant unobserved productivity and risk preferences. In the non-random selection case, estimates of *VSL* based on the pooled cross-section, between-groups, or random-effects estimators will be biased and inconsistent; the first-differences and double-differences estimators in equations (2) and (3), as well as the dynamic first-difference estimator, can be consistent despite non-random job switching.

2.2 Research Objective

The focal parameter of interest in each of the regression models we estimate is $\hat{\alpha}_1$, which is used in constructing estimates of the value of a statistical life. Accounting for the fact that fatality risk is per 100,000 workers, the estimated value of a statistical life at the level of wages, *w*, and annual hours of work, *h*, is

$$\widehat{VSL} = \left[\left(\frac{\partial \hat{w}}{\partial \pi} = \hat{\alpha}_1 \times w\right) \times h \times 100,000 \right]. \tag{5}$$

Although the *VSL* function in equation (5) can be evaluated at various points in the wage and hours distributions, most studies report only the effect at mean wages and a fixed hours point of 2000. To highlight the differences in estimates of the *VSL* with and without controls for unobserved individual differences, we follow the standard convention of focusing on $\overline{VSL}$ in our estimates presented below. Our primary objective is to examine how following systematic econometric practices for panel data models reduces the estimated range of *VSL*. However, we also present estimates of the mean *VSL* using the sample average of hours worked, $\bar{h}$, in lieu of 2,000 hours. In addition, we provide 95 percent confidence intervals around the mean *VSL*. [5]

3. Data and Sample Descriptions

The main body of our data come from the 1993–2001 waves of the Panel Study of Income Dynamics (PSID), which provides individual-level data on wages, industry and occupation, and demographics. The PSID survey has followed a core set of households since 1968 plus newly formed households as members of the original core have split off into new families.

3.1 PSID Sample

The sample we use consists of male heads of household ages 18–65 who are in the random Survey Research Center (SRC) portion of the PSID, and so excludes the oversample of the poor in the Survey of Economic Opportunity (SEO) and the Latino sub-sample. The male heads in our regressions (i) worked for hourly or salary pay at some point in the previous calendar year, (ii) are not permanently disabled or institutionalized, (iii) are not in agriculture or the armed forces, (iv) have a real hourly wage greater than $2 per hour and less than $100 per hour, and (v) have no missing data on wages, education, region, industry, and occupation.

Beginning in 1997 the PSID moved to every other year interviewing. For consistent spacing of survey responses we use data from the 1993, 1995, 1997, 1999, and 2001 waves. The use of every other year responses will be one of many mechanisms to reduce the influence of measurement error in our estimated *VSL*. We do not require individuals to be present for the entire sample period; we have an unbalanced panel where we take missing values as random events.[6] Our sample filters yield 2,036 men and 6,625 person-years. About 40 percent of the men are present for all five waves (nine years); another 25 percent are present for at least four waves.

The dependent variable from the PSID in our models of hedonic labor market equilibrium is the hourly wage rate. For workers paid by the hour the survey records the gross hourly wage

rate. The interviewer asks salaried workers how frequently they are paid, such as weekly, bi-weekly, or monthly. The interviewer then norms a salaried worker's pay by a fixed number of hours worked depending on the pay period. For example, salary divided by 40 is the hourly wage rate constructed for a salaried worker paid weekly. We deflate the nominal wage by the personal consumption expenditure deflator for 2001 base year. We then take the natural log of the real wage rate to minimize the influence of outliers and for ease of comparison with others' estimates.

The demographic controls in the model include years of formal education, a quadratic in age, dummy variables for race (white = 1), union status (coverage = 1), marital status (married = 1), one-digit occupation, two-digit industry, state of residence, and residence in one of nine Census regions. We also control for year effects. Table 1 presents summary statistics of selected variables.[7]

3.2 Fatality Risk Measures

We use the fatality rate for the worker's two-digit industry by one-digit occupation group. We distinguished 720 industry-occupation groups using a breakdown of 72 two-digit SIC code industries and the 10 one-digit occupational groups. After constructing codes for two-digit industry by one-digit occupation in the PSID we then matched each worker to the relevant industry-occupation fatality risk. We constructed a worker fatality risk variable using proprietary U.S. Bureau of Labor Statistics data from the Census of Fatal Occupational Injuries (CFOI) for 1992–2002.[8]

The CFOI provides the most comprehensive inventory to date of all work-related fatalities in a given year. The CFOI data come from reports by the Occupational Safety and Health Administration, workers' compensation reports, death certificates, and medical examiner

reports. To be classified as a work-related injury the decedent must have been employed at the time of the fatal event and engaged in legal work activity that required the worker be present at the site of the fatal incident. In each case the BLS verifies the work status of the decedent with two or more of the above source documents or with a follow-up questionnaire in conjunction with a source document.

The underlying assumption in our research and almost the entire hedonic literature more generally is that the subjective risk assessments by workers and firms can be captured by objective measures of the risk. Workers and firms use available information about the nature of the job and possibly the accident record itself in forming risk beliefs. The models do not assume that workers and firms are aware of the published risk measures at any point in time. Rather, the objective measures serve as a proxy for the subjective beliefs. Previous research reviewed in Viscusi and Aldy (2003) has indicated a strong correlation between workers' subjective risk beliefs and published injury rates. Because our fatality risk variable is by industry-occupation group, it provides a much more pertinent measure of the risk associated with a particular job than a more broadly based index, such as the industry risk alone, which is the most widely used job risk variable. For example, miners and secretaries in the coal mining industry face quite different risks, so that taking into account the occupation as well as the industry as we do here substantially reduces the measurement error in the fatality risk variable.

The importance of the industry-occupation structure of our risk variable is especially great within the context of a panel data analysis. The previous panel study by Brown (1980) used a time-invariant fatality risk measure for 37 relatively high risk occupations. By using a fatality risk variable that varies over time and is defined for 720 industry-occupation groups, we greatly expand the observed variance in workers' job risks across different periods.

We construct two measures of fatal risk. The first measure simply uses the number of fatalities in each industry-occupation cell in survey year t, divided by the number of employees for that industry-occupation cell in survey year t. The second measure uses a three-year average of fatalities surrounding each PSID survey year (1992–1994 for the 1993 wave, 1994–1996 for the 1995 wave, and so on), divided by a similar three-year average of employment. Both of our two measures of the fatality risk are time-varying because of changes in both the numerator and the denominator.[9]

We expect there may be less measurement error in the 3-year average fatality rates relative to the annual rate because the averaging process will reduce the influence of random fluctuations in fatalities as well as mitigate the small sample problems that arise from many narrowly defined job categories. However, the annual measure should be a more pertinent measure of the risk in that particular survey year. We also expect less reporting error in the industry information than in the occupation information, so even our annual measure should have less measurement error than if the worker's occupation were the basis for matching (Mellow and Sider, 1983, Black and Kniesner, 2003, Viscusi, 2004). To reduce the influence of large swings in fatality risk further, we also drop person-years where the percentage change in fatality risk exceeds a positive 300 percent or (in absolute value) a negative 75 percent. Table 1 lists the means and standard deviations for both fatality risk measures. The sample mean fatality risk for the annual measure is 6.4/100,000. As expected, the variation in the annual measure exceeds that of the 3-year average.

Our research also avoids a problem plaguing past attempts to estimate the wage-fatal risk tradeoff with panel data. If the fatality rate is an aggregate by industry or occupation the first-difference transformation leaves little variation in the fatality risk measure to identify credibly

the fatality parameter. Most of the variation in aggregate fatality risk is of the so-called between-groups variety (across occupations or industries at a point in time) and not of the within-groups variety (within either occupations or industries over time). Although between-group variation exceeds within-group variation (Table 2), the within variation in our more disaggregate measures is sufficiently large (about 33–40 percent of the between variation) so that it may be feasible to identify the fatal risk parameter and *VSL* in our panel data models. Finally, we also address the issue that between-group variation in fatality risk may be generated by endogenous job switching.

4. Wage Equation Estimates

Although we suppress the coefficients other than for fatal risk for ease of presentation, unless stated otherwise every regression model controls for a quadratic in age, years of schooling, indicators for marital status, union status, race, one-digit occupation, two-digit industry, region, state, and year. Despite their high correlation with our fatality risk measure, the regressions include a set of one-digit occupation dummies and two-digit industry dummies to account for the substantial heterogeneity of jobs in different occupations and industries. In addition, because there might be unmeasured differences in labor markets across states and regions that do not vary with time, we include a full set of state and region (nine Census divisions) fixed effects. Likewise, workers in a given year may face common macroeconomic shocks to wages, so we include a vector of year dummies in all models. Reported standard errors are clustered by industry and occupation and are also robust to the relevant heteroskedasticity. Note that our first-difference regressions automatically net out the influence of industry and other job characteristics that do not change over time, and the double-difference regressions net out additional trending factors.

Because our primary focus is on the panel estimates, we do not include regressors that exhibit little variation across the time periods. Within the panel data context workers' compensation benefit levels are fixed in real terms for most workers. The main benefit measures that have been used in the hedonic literature pertain to the weekly benefit level for temporary partial disability. The associated wage replacement rate changed for only five states during the nine years of our data, and the changes were minor. There is also not much variation across states in replacement rates. For half the states the replacement rate is at two-thirds of the worker's wage, and many other states have similar time-invariant replacement rates such as 70 percent. States exhibit greater variation with respect to the maximum weekly benefits that will be paid for temporary partial disability. However, the benefit maximums tend to increase steadily over time, reflecting adjustments for price inflation. Indeed, during 1992–2001, 34 states had benefit growth rates that were confined to a 1.7 percent growth rate band surrounding the rate of price inflation. Thus, with the panel data context workers' compensation benefit levels will tend to be fixed for most workers in the sample, and we do not include a workers' compensation variable. However, to the extent that there is cross-state variation in benefit levels these differences will be absorbed in our controls for state fixed effects.

4.1 Focal Estimates from Panel Data

The baseline first-difference estimates from equation (2) appear in column (1) of Table 3. The results begin our attempt to address systematically not only latent heterogeneity and possibly trended regressors, but also measurement error. Comparing estimates both down a column and across a row reveals the effect of measurement error. The results are reasonable from both an econometric and economic perspective and provide the comparison point for our core research

issue, which is how badly *VSL* can be mis-represented if certain basic econometric issues are mis-handled.

The *VSL* implied by the baseline first-difference model's coefficient for the annual fatality rate in Table 3 using the sample mean wage of $21 and sample means hours of work of about 2,287 in (5) is $6.6 million, with a 95 percent confidence interval of $0.9 million–$12.4 million. We emphasize that a novel aspect of our research is that it helps clarify the size of possible measurement error effects. If measurement error in fatality risk is random it will attenuate coefficient estimates and the error should be reduced by letting the fatality rate encompass a wider time interval, raising the coefficient. Compared to *VSL* from the more typical annual risk measure, the estimated *VSL* in Table 3 is about a third larger when fatality risk is a three-year average. The second column of Table 3 reports the results for difference-in-differences from equation (3), which should remove possible spurious estimated effects from variables that are not difference stationary. The estimated VSL is about $1 million higher than the base case in the annual measure, and about $4 million higher with the three-year average fatality rate.

One problematic result in the literature is the regularly occurring large value for *VSL* when the PSID is used as a cross-section (Viscusi and Aldy, 2003). Notice that the cross-section estimators in columns 3 and 4 of Table 3 produce large implied *VSLs*, about $17 million–$29 million. In contrast, column 5 of Table 3 reports estimates from the panel random-effects estimator, where a Breusch-Pagan test supports heterogeneous intercepts. Recall that the random-effects estimator accounts for unobserved heterogeneity, which is assumed to be uncorrelated with observed covariates. It is fairly common in labor-market research to reject the assumption of no correlation between unobserved heterogeneity and observed covariates;

Hausman test results indicate a similar rejection here. However, allowing for the possibility of unobserved productivity and preferences for risk, even if it is improperly assumed to be randomly distributed in the population, reduces the estimated *VSL* by about 60 percent relative to a model that ignores latent heterogeneity.

The difference in estimated *VSL* with latent individual heterogeneity versus without latent individual heterogeneity in the model is consistent with the theoretical emphasis in Shogren and Stamland (2002) that failure to control for unobserved skill results in a potentially substantial upward bias in the estimated *VSL*. Taking into account the influence of individual heterogeneity implies that, on balance, unobservable person-specific differences in safety-related productivity and risk preferences are a more powerful influence than unobservable productivity generally, which Hwang, Reed, and Hubbard (1992) hypothesize to have the opposite effect.

The final column of Table 3 presents estimates of the *VSL* using the more familiar fatality rate that varies only by two-digit industry rather than two-digit industry by occupation. The estimated size of the *VSL* lies within the confidence interval of the baseline estimate in column (1), but the standard error on the fatality risk coefficient is about 50 percent higher so that it is no longer statistically significant. Thus, the key advantage of our industry-by-occupation fatality risk is improved efficiency. The main message from Table 3 is that correcting for latent heterogeneity is more important than correcting for measurement error, and that even for the relatively basic panel models using differencing in column (1), the range for *VSL* is not uncomfortably large: about $6 million–$8 million when using a 2000 hour work year (CI = $0.8 million–$13.6 million) and about $7 million–$9 million when using sample average hours to compute *VSL* (CI = $0.9 million–$15.6 million).

4.2 First Difference Estimator Specification Checks

An issue seldom addressed in panel wage equations producing *VSL* is endogeneity of the fatality change regressor, which may result from dynamic decisions workers make to change jobs (Solon, 1986, 1989; Spengler and Schaffner, 2010). Some changes in fatality risk will occur because of within industry-occupation cell changes and others will occur because workers switch industry-occupation cells. Within the context of potentially hazardous employment, much of the mobility stems from workers learning about the risks on the job and then quitting if the compensating differential is insufficient given that information (Viscusi, 1979). Within the context of multi-period Bayesian decisions, a desire to switch does not require that workers initially underestimated the risk, as imprecise risk beliefs can also generate a greater willingness to incur job risks than is warranted by the mean risk level. Interestingly, for the job changers in our sample, 51 percent switch to lower fatality risk jobs and 46 percent switch to higher fatality risk jobs so that on balance there is some effort to sort into safer employment.

We examine the practical importance of job changing status for panel-based estimation in Table 4, where we stratify the data by whether $\Delta\pi_t$ is due to within or between cell changes, including immediately before and after a worker changes cells. The main econometric contribution to compensating differentials for fatality risk comes from workers who generate differences in risk over time by switching industry-occupation cells. The difference in estimated *VSL* in Table 4 comes from the fact that $\sigma^2_{\pi_t}$ is at least 8 times larger for switchers (see Table 2). There is too little within-cells variation to reveal much of a compensating differential for job stayers. More important, because so much of the variation producing the wage differential in Table 3 comes from job changers, and the variation for switchers may be related to wages, it is imperative to treat $\Delta\pi$ as endogenous.

The estimated range for mean estimates of *VSL* narrows even further when we allow for endogeneity and instrument the change in fatality risk. The instrumental variables regressions in Table 5 control for both classical measurement errors and endogeneity more generally. Specifically, based on the results of Griliches and Hausman (1986) we interchangeably use the (t–1) and (t–3) levels of the fatality risk, the (t–1) – (t–3) difference, the (t–2) and (t–3) levels and difference, and the (t–2) and (t–4) levels and difference. We limit the focus to the annual fatality rate so as to have enough lagged fatality and fatality differences as instruments.[10] The main result is a fairly narrow range for the estimated *VSL*, approximately $6 million–$10 million when we instrument the annual change in fatality risk, though the confidence intervals widen as is typical in IV models compared to OLS (whether in the cross section or panel context).

Table 6 presents our final focal panel results from dynamic first-difference regressions, based on both the simple Anderson-Hsiao just-identified IV estimator and the heavily over-identified Arellano-Bond dynamic GMM estimator.[11] The short-run effects from the dynamic model appear in the first column and the long-run (steady state) estimates appear in the second column for each of the two estimators. Note that our first-differences estimator focuses on changes in wages in response to changes in risk. The mechanism by which the changes will become reflected in the labor market hinges on how shifts in the risk level will affect the tangencies of the constant expected utility loci with the market offer curve. To the extent that the updating of risk beliefs occurs gradually over time, which is not unreasonable because even release of the government risk data is not contemporaneous, one would expect the long-run effects on wages of changes in job risk to exceed the short-run effects. Limitations on mobility will reinforce a lagged influence (state dependence).

As one would then expect, the steady state estimates of *VSL* after the estimated three-year adjustment period in the results in Table 6 are larger than the short-run estimates. The difference between the short-run and long-run *VSL* is about $1 million, ranging from $6 million–$7 million versus $7 million–$8 million using a standard work year and about $7 million–$8 million versus about $7 million–$9 million using sample average annual hours worked. Again, the central tendency of *VSL* estimates is not great when panel data are used with estimators that accommodate generic endogeneity, weak instruments, measurement error, latent heterogeneity and possible state dependence.[12]

Table 7 contains results from an extensive set of additional specification checks designed to examine whether the level and range of *VSL* from the baseline first difference panel data results of Table 3 are sensitive to the many options the researcher has in selecting control variables. For convenience in column (1) we reproduce the base-case estimates. In column (2) we drop controls for two-digit industry and one-digit occupation from the base case, in column (3) we add back one-digit occupation, in column (4) we instead add controls for two-digit industry (but no occupation controls), in column (5) we add dummies for one-digit occupation-by-year to the base case in (1), in column (6) we instead add two-digit industry-by-year dummies to the base case in (1), in column (7) we add census division-by-year (but not industry or occupation controls) to column (2), in column (8) we add state-by-year controls to the base case in (1), and in column (9) we add one digit occupation-by-two-digit industry controls to (1). The regressions in Table 7 make clear that controlling for linear two-digit industry and one-digit occupation in the base case helps precisely identify the wage-fatal risk tradeoff, but overfitting the model by say inclusion of industry-by occupation controls in column (9) wipes out identification as it is collinear with the requisite variation of our fatality variable.[13]

4.3 Panel Data Estimator Specification Checks with Lagged Fatality Rate

As a final dimension of our research we present Table 8, which contains results from an extensive set of specification checks designed to examine whether the level and range of *VSL* from panel data discussed thus far are sensitive to using the lagged fatality rate rather than the contemporaneous fatality rate in the model.

Our use of the contemporaneous fatality rate follows the norm in the *VSL* literature, which assumes that the current year fatality rate reflects the risk beliefs of workers and employers at that time. So, another possible sensitivity check with respect to the fatality rate is to hypothesize that risk beliefs are governed by an expectations model in which previous fatality rates influence current risk beliefs. Although the three year moving average fatality rate incorporates previous fatality risks, it also includes the current rate and places an equal weight on rates in all three years. Ideally, one might want to formulate a distributed lag model with multiple lagged values.[14] But estimation of such lags in a panel data model requires the imposition of additional assumptions, such as the assumed absence of correlation of the *x* values and the lag coefficients (Pakes and Griliches, 1984). Matters are further complicated by the influence of job changers. For those who change jobs, the risk expectations for the two different jobs will involve distributed lags on past fatality rates for the two different positions, where the time periods for the lags will overlap and may include periods in which the worker was not even in the particular industry-occupation group, but nevertheless is assumed to be using experiences of that group to form risk beliefs. As an illustrative sensitivity test, we present results using a single lagged fatality rate variable rather than a fully articulated distributed lags model.

Table 8 reports the counterparts of a diverse selection of our previous regressions using the lagged fatality rate variable as the death risk measure. For comparison the first column of

Table 8 reproduces the base case estimate from column 1 of Table 3. We simplify our discussion by focusing on the standard hours *VSL* estimates because comparisons with *VSL* using average hours are available in the table for the reader. The counterpart for lagged fatality rates is the static first differences estimates appearing in column 2 of Table 8, which implies a *VSL* about $1 million less than in the base case. The opposite pattern is observed for the pooled cross section-time series estimate in column 3 of Table 8, which is $1 million more than the counterpart in column 3 of Table 3. The random-effects estimator *VSL* in column 4 of Table 8 is $0.6 million less than the value in column 5 of Table 3. The strongest parallel is for the job changers-ever change job results, which are identical for the lagged values in column 5 of Table 8 and in the middle panel of Table 4. For the job changer result restricted to workers only when they change jobs, the *VSL* estimate is somewhat higher in the lagged fatality rate case in column 6 of Table 8 than in the bottom panel of Table 4. The final three sets of estimates in Table 8 do not have statistically significant lagged fatality rate coefficients, but the statistical significance of the equations in Table 5 (columns 1 and 4) and Table 6 (column 3) is also not as strong as in the other results. The overall pattern is that use of the single year lagged fatality rate sometimes leads to higher or lower estimates, but in most instances the *VSL* results are quite similar.

It is possible that workers base their willingness to work in a given setting on an expected rather than actual observed fatality risk. A simple econometric implementation of the expectations possibility would be to use the lagged fatality measure rather than a concurrent fatality measure as the focal regressor, which is the set of results in the second column of Table 8. Direct substitution of a lagged regressor is also a simple IV estimator for an endogenous fatality regressor. The simple substitution of lagged fatality lowers the estimated *VSL* to $5 million–$6 million (CI = $0.2 million–$11.1 million). In the interest of completeness, one should

also check more sophisticated representations of expectations such as rational expectations that are IV estimates using multiple fatality lags, which are the specifications in Tables 5 and 8. When we estimate the less complete rational expectations type dynamic models with multiple lagged values as instruments, seen in columns 7 and 8 of Table 8, the comparison point estimates are at the low end of our panel estimates of *VSL*, about $5 million using a standard (2000 hour) work year and about $6 million using the higher sample average work year, but neither are statistically significant.

Our final comparison model is the most complex econometric approach, which is the Arellano-Bond dynamic first differences model. In the previously discussed IV models that include dynamics presented in Table 6 the instrument set for the lagged wage regressor always contains two (further) lagged values. In the Arellano-Bond model lagged values of wages are instruments but the instrument set grows as the sample evolves temporally so that the last time period observation has the most instruments and the earliest time period observation has the fewest instruments. The Arellano-Bond results in column (9) are for a less complete rational expectations representation than the parallel results in Table 6 and produce estimated *VSL*s that are the lowest with the lowest *p*-values of all the alternative lagged fatality rate regressions presented in Table 8. One implication is that a more complete instrument set that goes with the more complete rational expectations formulation residing inside the dynamic Arellano-Bond regressions in Table 6 dominate the less complete expectations specification that does not use the lagged fatality rate in the instrument set, instead starting with fatality rate at t–2, as in Table 8.

5. Implications for Regulatory Cost-Effectiveness

Obtaining reliable estimates of compensating differential equations has long been challenging because of the central roles of individual heterogeneity and state dependence in

affecting both the market offer curve and individual preferences. The often conflicting influence of different unobservable factors has led to competing theories with predictions of different direction.

The wide variation of *VSL* estimates in the literature also has generated concern that underlying econometric problems may jeopardize the validity of the estimates. The range for *VSL* in the existing literature is extremely wide, from about $0 million to $20 million. Previous studies using the Panel Study of Income Dynamics have often yielded extremely high *VSL* estimates of $20+ million, which is also the case in our own cross-section based estimates with the PSID. Earlier research did not control for the host of econometric problems we address here. A most important finding here is that controlling for latent time-invariant heterogeneity is crucial – much more so than how one does it econometrically.

Our first-difference estimation results use more refined fatality risk measures than employed in earlier studies to control for measurement errors and workplace safety endogeneity in econometric specifications considering state dependence, expectations and heterogeneity when examining the wage-fatality risk tradeoff. Comparison of the various first-difference results with various cross-section estimates implies that controlling for latent worker-specific heterogeneity reduces the estimated *VSL* by as much as two-thirds and narrows greatly the *VSL* range to about $4 million–$10 million depending on the time-frame (short-run versus long-run) and work year (standard or sample average) in the calculation.

We offer several justifications for focusing on the $4 million to $10 million range for *VSL*. First, the estimates using the single year fatality rate variable rather than the moving three-year average better capture any temporal shifts in the fatality rate, which is the focus of our panel estimates. The single year results are more in line with the $4 million to $10 million zone.

Second, as Table 4 indicates, the main implication of looking at different labor market groups is that the compensating differentials are concentrated among job changers, not workers who did not change jobs. Both of the job changers' results are in our *VSL* range. Third, all the IV results and the dynamic first-differences results in Tables 5 and 6, as well as many specifications in Table 3 (static first-differences, and so on), are in the $4 million to $10 million range if we continue to focus on the single year fatality rate variable.

In short, the models that yield the $4 million to $10 million range are preferred because they control comprehensively for selection on unobservables (via fixed effects, state effects, and industry occupation effects) and the selection on observables. The regressions associated with the preferred range are also robust to well-specified IV models (ones with high 1st stage R^2's) and pin down the key variable needed for identification – job change. The models that yield estimates well above $10 million do so because they do not control for selection on unobservables via person fixed effects, and those that yield estimates lower than $6 million tend to be based on inadequate variation (job stayers) or proxies with lower power (lagged fatality).

Narrowing *VSL* as we do here has substantial benefits for policy evaluation. In its Budget Circular A4 (Sept. 17, 2003), the U.S. Office of Management and Budget requires that agencies indicate the range of uncertainty around key parameter values used in benefit-cost assessments. Attempting to bound the *VSL* based on a meta-analysis produces a wide range of estimates from nearly $0 to $20+ million. In addition to the issue of what studies should be included in the meta analysis given the differences in data sets, specifications, and study quality, we can also produce *VSL*s that mimic the literature with ones as low as $0 (or negative) if we limit the sample to workers who never change jobs and ones as high as $28 million if we use the between estimator with the PSID as a cross-section (CI = –$40 million to $48.3 million). As a consequence of the

perceived indeterminacies in *VSL*, agencies often have failed to provide any boundaries at all to the key *VSL* parameter in their benefit assessments.

The advantage of using our *VSL* range in policy assessments can be illustrated by an example of the cost-effectiveness of U.S. health and safety regulations. Using the widely cited cost estimates from the U.S. Office of Management and Budget cited by Breyer (1993), among others, and updating the values to $2001 to be consistent with our *VSL* estimates, we illustrate the reduction of policy uncertainty achievable by application of our estimates. Applying the meta analysis *VSL* range, 10 policies pass a benefit-cost test, 20 fail a benefit-cost test, and 23 are in the indeterminate zone. Using our estimated *VSL* range, the distribution becomes 27 policies that clearly pass a benefit-cost test, 24 that fail a benefit-cost test, and with only 2 policies in the indeterminate range. Our narrowing of the acceptable cost-per-life-saved range greatly reduces the range of indeterminacy and is of substantial practical consequence given the actual distribution of regulatory policy performance.

From a more conceptual standpoint, our research has resolved several econometric issues giving rise to the very high/low levels and wide ranges of published *VSL* estimates. The disparate results in previous studies may reflect the influence of omitted unobservable effects, among other repairable econometric specification errors. Failure to address the underlying econometric issues may have produced continuing controversy in the economics literature over the hedonic method and unduly muddled the policy debate over the use of *VSL* estimates in benefit calculations for government policies.

Table 1: Selected Summary Statistics

	Mean	Standard Deviation
Real Hourly Wage	20.610	13.041
Log Real Hourly Wage	2.862	0.566
Age	40.832	8.452
Marital Status (1=Married)	0.817	0.386
Race (1=White)	0.758	0.428
Union (1=member)	0.230	0.421
Years of Schooling	13.506	2.221
Live in Northeast	0.172	0.378
Live in Northcentral	0.283	0.451
Live in South	0.376	0.484
Live in West	0.168	0.374
One-Digit Industry Groups:		
Mining	0.008	0.089
Construction	0.127	0.333
Manufacturing	0.231	0.421
Transportation and Public Utilities	0.115	0.319
Wholesale and Retail Trade	0.139	0.346
Fire, Insurance, and Real Estate	0.045	0.206

Business and Repair Services	0.070	0.256
Personal Services	0.010	0.098
Entertainment and Professional Services	0.188	0.391
Public Administration	0.067	0.250
One-Digit Occupation Groups:		
Executive and Managerial	0.191	0.393
Professional	0.158	0.365
Technicians	0.042	0.202
Sales	0.031	0.174
Administrative Support	0.050	0.219
Services	0.082	0.274
Precision Production Crafts	0.231	0.421
Machine Operators	0.079	0.270
Transportation	0.090	0.286
Handlers and Labors	0.046	0.209
Annual Fatality Rate (per 100,000)	6.415	9.144
3-Year Fatality Rate (per 100,000)	6.260	8.769
Number of Men = 2,036		
Number of Person Years = 6,625		

Table 2: Between and Within Group Variation for Industry by Occupation Fatality Rates

	Overall Variance	Between Group Variance	Within Group Variance
Annual Fatality Rate (per 100,000)	69.866	50.447	19.419
3-Year Fatality Rate (per 100,000)	52.077	39.401	12.676
Never Change Industry-Occupation			
Annual Fatality Rate (per 100,000)	71.646	68.356	3.29
3-Year Fatality Rate (per 100,000)	52.458	51.629	0.828
Ever Change Industry-Occupation			
Annual Fatality Rate (per 100,000)	69.094	42.799	26.295
3-Year Fatality Rate (per 100,000)	51.914	34.189	17.726

Only When Change Industry-Occupation			
Annual Fatality Rate (per 100,000)	70.591	46.24	24.351
3-Year Fatality Rate (per 100,000)	64.927	43.908	21.019

Table 3: Cross Section and Panel Data Estimates of Wage-Fatal Risk Tradeoff

	Static First Difference Estimates	Difference in Differences Estimator	Pooled Cross Section Time Series Estimator	Between-Group Estimator	Random-Effects Estimator	Static First Difference Based on 2-Digit SIC Fatality Rate
	(1)	(2)	(3)	(4)	(5)	(6)
Annual Fatality Rate x 1,000	1.3438	1.5101	3.7386	6.2827	1.5157	1.2992
	(0.5943)	(0.6445)	(1.2565)	(2.1005)	(0.7312)	(0.8992)
Implied VSL ($Millions)	5.8	6.8	15.4	25.9	6.2	5.7
	[0.8, 10.8]	[1.1, 12.5]	[5.3, 25.6]	[8.9, 42.9]	[0.3, 12.2]	[-2.1, 13.5]
VSL - using average hours	6.6	7.8	17.4	29.2	7	6.5
	[0.9, 12.4]	[1.3, 14.4]	[5.9, 28.8]	[10.1, 48.3]	[0.4, 13.7]	[-2.4, 15.4]

Number of Observations	4338	2788	6625	6625	6625	5085
3-Year Fatality Rate x 1,000	1.7556	2.4679	3.0373	4.5069	1.09	1.5387
	(0.6812)	(0.8107)	(1.4460)	(2.2777)	(0.8962)	(0.9312)
Implied VSL ($Millions)	7.7	11.3	13.0	19.3	4.7	6.8
	[1.9, 13.6]	[4.0, 18.6]	[0.9, 25.2]	[0.2, 38.5]	[-2.8, 12.2]	[-1.3, 14.9]
VSL - using average hours	8.8	13	14.8	22	5.3	7.8
	[2.1, 15.6]	[4.6, 21.3]	[1.0, 28.7]	[0.2, 43.8]	[-3.3, 13.9]	[-1.4, 17.0]
Number of Observations	4916	2992	5866	5866	5866	5240

Notes: Standard errors are recorded in parentheses, and 95% confidence intervals in square brackets. Standard errors for the pooled times series cross-section estimator and the first difference estimator are robust to heteroskedasticity and within industry-by-occupation autocorrelation. Each model controls for a quadratic in age, years of schooling, indicators for region, marital status, union status, race, one-digit occupation, two-digit industry, state and year effects. To construct the *VSL* using equation (5) the coefficients in the table are divided

by 1,000. By construction the model (6) does not include controls for 2-digit SIC.

Table 4: Estimates of Wage-Fatal Risk Tradeoff by Job Change Status

	Annual Fatality Rate x 1,000	3-Year Fatality Rate x 1,000
	(1)	(2)
Never Change Industry-Occupation		
Annual Fatality Rate x 1,000	0.1234	-0.8074
	(1.4164)	(3.4029)
Implied VSL ($Millions)	0.6	-3.8
	[-12.4, 13.6]	[-35.5, 27.9]
VSL - using average hours	0.7	-4.4
	[-14.3, 15.6]	[-40.8, -32.0]
Number of Person-Years	1303	1390
Ever Change Industry-Occupation		
Annual Fatality Rate x 1,000	1.4645	2.1051
	(0.6555)	(0.7667)
Implied VSL ($Millions)	6.1	8.8
	[0.8, 11.4]	[2.5, 15.0]
VSL - using average hours	7	10

	[0.9, 13.0]	[2.9, 17.1]
Number of Person-Years	3035	3035
Only When Change Industry-Occupation		
Annual Fatality Rate x 1,000	1.5271	2.0684
	(0.7321)	(0.7308)
Implied VSL ($Millions)	6.3	8.8
	[0.4,12.3]	[2.7, 14.9]
VSL - using average hours	7.2	10
	[0.4,13.9]	[3.1, 16.9]
Number of Person-Years	1920	2261

Notes: Standard errors are recorded in parentheses, and 95% confidence intervals in square brackets. Standard errors for the pooled times series cross-section estimator and the first difference estimator are robust to heteroskedasticity and within industry-by-occupation autocorrelation. Each model controls for a quadratic in age, years of schooling, indicators for region, marital status, union status, race, one-digit occupation, two-digit industry, state and year effects. To construct the VSL using equation (5) the coefficients in the table are divided by 1,000.

Table 5: Instrumental Variables Estimates of Wage-Fatal Risk Tradeoff

	First-Difference IV Estimator, t–1 and t–3 Fatality as Instruments	First-Difference IV Estimator, Lag Differenced Fatality as Instrument	First-Difference IV Estimator, t–2 and t–3 Fatality as Instruments	First-Difference IV Estimator, Lag Differenced Fatality as Instrument	First-Difference IV Estimator, t–2 and t–4 Fatality as Instruments	First-Difference IV Estimator, Lag Differenced Fatality as Instrument
	(1)	(2)	(3)	(4)	(5)	(6)
Annual Fatality Rate x 1,000	1.7514	1.7953	2.0209	1.9611	1.4385	1.287
	(0.9051)	(0.9038)	(1.1391)	(1.1357)	(1.1489)	(1.1566)
Implied VSL ($Millions)	7.6	7.8	8.7	8.5	6.4	5.7
	[-0.1, 15.2]	[0.1, 15.4]	[-0.9, 18.4]	[-1.1,18.1]	[-3.6, 16.3]	[-4.3, 15.7]
VSL - using average hours	8.6	8.9	9.9	9.7	7.3	6.5

	[-0.1, 17.4]	[0.1, 17.6]	[-1.0, 21.0]	[-1.3, 20.7]	[-4.1, 18.7]	[-4.9, 18.0]
First Stage Results						
t–1 fatality rate	0.6460					
	(0.0129)					
t–3 fatality rate	-0.6342					
	(0.0128)					
(t–1 rate) – (t–3 rate)		0.6398				
		(0.0121)				
t–2 fatality rate			0.5313			
			(0.0144)			
t–3 fatality rate			-0.5428			
			(0.0141)			
(t–2 rate) – (t–3 rate)				0.5377		
				(0.0134)		

t–2 fatality rate					0.4861	
					(0.0164)	
t–4 fatality rate					-0.5113	
					(0.0149)	
(t–2 rate) – (t–4 rate)						0.5038
						(0.0145)
R^2	0.67	0.67	0.60	0.60	0.61	0.61
Number of Observations	4338	4338	4338	4338	3235	3235

Notes: Standard errors are recorded in parentheses, and 95% confidence intervals in square brackets. Standard errors are robust to heteroskedasticity and within industry-by-occupation autocorrelation. Each model controls for a quadratic in age, years of schooling, indicators for region, marital status, union status, race, one-digit occupation, two-digit industry, state and year effects. First stage regressions include all exogenous explanatory variables in addition to the noted instruments. To construct the *VSL* using equation (5) the coefficients in the table are divided by 1,000.

Table 6: Dynamic First Difference Estimates of Wage-Fatal Risk Tradeoff

	Anderson-Hsiao Dynamic IV Estimates with lag differenced wage instrumented		Arellano-Bond Dynamic GMM Estimator with lag differenced wage instrumented	
	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect
Annual Fatality Rate x 1,000	1.2907	1.3784	1.4591	1.6701
	(0.7694)	{0.098}	(0.8915)	{0.103}
Implied VSL ($Millions)	5.8	6.2	6.6	7.6
	[-1.0, 12.7]	[-1.0, 13.6]	[-1.3, 14.5]	[-1.5, 16.6]
VSL - using average hours	6.7	7.2	7.6	8.7
	[-1.1, 14.5]	[-1.2, 15.6]	[-1.5, 16.7]	[-1.7, 19.1]
Number of Observations	2788		2788	
3-Year Fatality Rate x 1,000	1.7876	1.9876	1.8584	2.2609

	(0.8395)	{0.035}	(1.1042)	{0.095}
Implied VSL ($Millions)	8.2	9.2	8.6	10.4
	[0.7, 15.8]	[0.6, 17.7]	[-1.4, 18.5]	[-1.8, 22.5]
VSL - using average hours	9.5	10.5	9.8	12
	[0.8, 18.2]	[0.7, 20.3]	[-1.6, 21.3]	[-2.0, 25.9]
Number of Observations	3162		3162	

Notes: Standard errors are recorded in parentheses, 95% confidence intervals in square brackets, and *p*-values of the null hypothesis that the long-run effect is zero are recorded in curly brackets. Standard errors are robust to heteroskedasticity and within industry-by-occupation autocorrelation. Models control for a quadratic in age, years of schooling, indicators for region, marital status, union status, race, one-digit occupation, two-digit industry, state and year effects. One and two year lags of the independent variables, except for the fatality rates, are included as instruments for the lag wage. To construct the *VSL* using equation (5) the coefficients in the table are divided by 1,000.

Table 7: Specification Checks for First-Difference Estimates of Wage-Fatal Risk Tradeoff

	Base Case from Table 3, Column (1)	Drop industry or occupation controls from (1)	Add 1-digit occupation controls to (2)	Add 2-digit industry controls to (2)	Add 1-digit occupation-by-year and 2-digit industry controls to (1)
	(1)	(2)	(3)	(4)	(5)
Annual Fatality Rate x 1,000	1.3438	0.9255	1.6007	0.414	1.3768
	(0.5943)	(0.4375)	(0.4793)	(0.5082)	(0.6120)
Implied VSL ($Millions)	5.8	4	6.9	1.8	5.9

	[0.8, 10.8]	[0.3, 7.7]	[2.9, 11.0]	[-2.5, 6.1]	[0.8, 11.1]
VSL - using average hours	6.6	4.6	7.9	2	6.8
	[0.9, 12.4]	[0.3, 8.8]	[3.3, 12.5]	[-2.9, 6.9]	[0.9, 12.7]
Number of Observations	4338	4338	4338	4338	4338
3-Year Fatality Rate x 1,000	1.7556	0.8147	1.7785	0.3824	1.7624
	(0.6812)	(0.5394)	(0.5435)	(0.6070)	(0.6936)
Implied VSL ($Millions)	7.7	3.6	7.8	1.7	7.8
	[1.9, 13.6]	[-1.1, 8.2]	[3.1, 12.5]	[-3.6, 6.9]	[1.8, 13.7]
VSL - using average hours	8.8	4.1	9	1.9	8.9
	[2.1, 15.6]	[-1.2, 9.4]	[3.6, 14.3]	[-4.1, 7.9]	[2.0, 15.7]
Number of					

Observations	4916	4916	4916	4916	4916

Notes: Standard errors are recorded in parentheses, and 95% confidence intervals in square brackets. Standard errors are robust to heteroskedasticity and within industry-by-occupation autocorrelation. Each model controls for a quadratic in age, years of schooling, marital status, union status, race, and year effects. To construct the *VSL* using equation (5) the coefficients in the table are divided by 1,000.

Table 7 continued: Specification Checks for First-Difference Estimates of Wage-Fatal Risk Tradeoff

	Add 1-digit occupation and 2-digit industry-by-year controls to (1)	Add census division-by-year controls to (2)	Add 1-digit occupation, 2-digit industry, and state-by-year controls to (1)	Add 1-digit occupation by 2-digit industry controls to (1)
	(6)	(7)	(8)	(9)
Annual Fatality Rate x 1,000	0.8151	0.8506	1.0158	0.4011
	(0.6043)	(0.4321)	(0.5744)	(0.3820)
Implied VSL ($Millions)	3.5	3.7	4.4	1.7
	[-1.6, 8.6]	[0.02, 7.3]	[-0.5, 9.2]	[-1.5, 5.0]
VSL - using average	4	4.2	5	2

hours				
	[-1.8, 9.9]	[0.02, 8.4]	[-0.5, 10.6]	[-1.7, 5.7]
Number of Observations	4338	4338	4338	4338
3-Year Fatality Rate x 1,000	1.3331	0.7517	0.6395	0.1558
	(0.7119)	(0.5374)	(0.5367)	(0.5342)
Implied VSL ($Millions)	5.9	3.3	2.8	0.7
	[-0.3, 12.0]	[-1.3, 7.9]	[-1.8, 7.4]	[-3.9, 5.3]
VSL - using average hours	6.7	3.8	3.2	0.8
	[-0.3, 13.7]	[-1.5, 9.1]	[-2.1, 8.5]	[-4.5, 6.1]
Number of Observations	4916	4916	4916	4916

Notes: Standard errors are recorded in parentheses, and 95% confidence intervals in square brackets. Standard errors are robust to heteroskedasticity and within industry-by-occupation autocorrelation. Each model controls for a quadratic in age, years of schooling,

marital status, union status, race, and year effects. To construct the *VSL* using equation (5) the coefficients in the table are divided by 1,000.

Table 8: Specification Checks for First-Difference Estimates of Wage-Fatal Risk Tradeoff using Lagged Fatality Rate

		Lagged Fatality Rate			
	Base Case from Table 3, Column (1)	Static First Differences	Pooled Cross Section Time Series Estimator	Random-Effects Estimator	Job Changers--Ever Change Job
	(1)	(2)	(3)	(4)	(5)
Annual Fatality Rate x 1,000	1.3438	1.1486	3.9705	1.3641	1.4678
	(0.5943)	(0.5627)	(1.1810)	(0.7449)	(0.5946)
Implied VSL ($Millions)	5.8	5	16.4	5.6	6.1
	[0.8, 10.8]	[0.2, 9.7]	[6.8, 25.9]	[-0.4, 11.6]	[1.2, 11.0]
VSL - using average	6.6	5.7	18.4	6.3	7

hours					
	[0.9, 12.4]	[0.2, 11.1]	[7.7, 29.2]	[-0.4, 13.1]	[1.4, 12.5]
Number of Observations	4338	4338	6625	6468	3035

Notes: Standard errors are recorded in parentheses, and 95% confidence intervals in square brackets. Standard errors are robust to heteroskedasticity and within industry-by-occupation autocorrelation. Each model controls for a quadratic in age, years of schooling, indicators for region, marital status, union status, race, and year effects. To construct the *VSL* using equation (5) the coefficients in the table are divided by 1,000.

Table 8 continued: Specification Checks for First-Difference Estimates of Wage-Fatal Risk Tradeoff using Lagged Fatality Rate

	Lagged Fatality Rate			
	Job Changers--Only When Change Job	First-Difference IV Estimator, t–1 and t–3 Fatality as Instruments	First-Difference IV Estimator, Lag Differenced Fatality as Instrument	Arellano-Bond Dynamic GMM Estimator
	(6)	(7)	(8)	(9)
Annual Fatality Rate x 1,000	1.9496	1.0639	1.2038	0.6577
	(0.6828)	(1.0566)	(1.0466)	(0.8925)

Implied VSL ($Millions)	8.1	4.7	5.3	3
	[2.5, 13.6]	[-4.5, 13.9]	[-3.7, 14.4]	[-4.9, 10.9]
VSL - using average hours	9.2	5.4	6.1	3.4
	[2.9, 15.5]	[-5.1, 15.9]	[-4.3, 16.5]	[-5.7, 12.5]
Number of Observations	1920	3235	3235	2788

Notes: Standard errors are recorded in parentheses, and 95% confidence intervals in square brackets. Standard errors are robust to heteroskedasticity and within industry-by-occupation autocorrelation. Each model controls for a quadratic in age, years of schooling, indicators for region, marital status, union status, race, and year effects. To construct the *VSL* using equation (5) the coefficients in the table are divided by 1,000.

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1. See U.S. Office of Management and Budget Circular A-4, Regulatory Analysis (Sept. 17, 2003) which is available at http://www.whitehouse.gov/omb/circulars/a004/a-4.pdf.

2. We adopt a parametric specification of the regression model representing hedonic equilibrium in (1) for comparison purposes with the existing literature. An important emerging line of research is how more econometrically free-form representations of hedonic labor markets facilitates identification of underlying fundamentals, which would further generalize estimates of *VSL* (Ekeland, Heckman, and Nesheim, 2004).

3. See Viscusi and Aldy (2003) for a review and Viscusi (1979) for supporting data.

4. The only previous use of the fatality rate data at our level of disaggregation and for different periods of time is in Viscusi (2004). Kniesner, Viscusi, and Ziliak (2006) also used the 720 cell measure but not the multi-year averages. Neither study employed panel-data econometric techniques.

5. The 95 percent confidence interval assumes that wages and hours are fixed constants and thus the random variation comes from the estimated fatality risk parameter. It is constructed as $\overline{VSL} \pm 1.96 * Var(\overline{VSL})$, where $Var(\overline{VSL}) = 100{,}000^2 * h^2 * \overline{w}^2 * Var(\hat{\alpha}_1)$. We present

estimates for $h = 2000$ and $h = \bar{h}$. We also employed a first-order Taylor series expansion to estimate the variance of the mean *VSL* treating the mean wage as stochastic, which from equation (5) is $Var(\overline{VSL}) = 100,000^2 * h^2 * \left(\overline{w}^2 * Var(\hat{\alpha}_1) + \hat{\alpha}_1^2 * Var(\overline{w})\right)$, with little change in the estimated intervals.

6. Ziliak and Kniesner (1998) show that when there is nonrandom attrition then our differenced data models should remove it along with the other time-invariant factors.

7. The state fixed effect parameters are identified by imposing the constraint that the state fixed effects sum to 0 within region. The interpretation is that they are deviations from the overall region mean as captured by the region fixed effects. So, for example, the coefficient on the state of Indiana dummy variable is interpreted as the deviation from the overall Midwest Region mean effect.

8. The fatality data can be obtained on CD-ROM via a confidential agreement with the U.S. Bureau of Labor Statistics. Our variable construction procedure follows that in Viscusi (2004), which describes the properties of the 720 industry-occupation breakdown in greater detail. In our basic estimation sample we limit observations to those where the annual change in fatality risk is no less than –75 percent and no more than +300 per cent.

9. We used the annual employment averages from the U.S. Bureau of Labor Statistics, Current Population Survey, unpublished table, Table 6, Employed Persons by Detailed Industry and Occupation for 1993–2001.

10. Greene (2012, Chapter 11) notes that the large sample variance of the dynamic difference estimator is smaller when lagged levels rather than lagged differences are part of the instruments,

which here include all exogenous explanatory variables. The first-stage results here and in subsequent tables pass the standard weak instruments check based on a partial R^2 of at least 0.10.

11. The Arellano-Bond model has also proved useful in studying job injury risk is the outcome of interest. See Kniesner and Leeth (2004).

12. We also note that the form of endogeneity we control for is consistent with recursive models. A full model that incorporates the joint choice of wages with industry/occupation is beyond the scope of the current paper, but as noted in Ashenfelter (2006) is an research area of need to more comprehensively measure the labor market *VSL*.

13. Our data do not contain information on injury rates, and the publically available injury data only varies across two-digit industry and not industry-by-occupation. In results not tabulated, when we include the change in the 2-digit SIC injury rate the coefficient on the fatality rate falls by about 40 percent. With the injury rate variable included we lose about 500 person years, or over 10 percent from our first-difference estimation sample. The reason for the loss of person years is that the BLS does not publish injury rates for all industries. When we re-run the base model using the sub-sample of those with non-missing injury rates, but excluding the injury rate from the model, we get the same coefficient on the fatality rate as if we include the injury rate in the model. In other words, it is not inclusion of the injury rate that reduces the fatality coefficient it is instead the loss of the 500 person years, where 75 percent are job changers.

14. Unlike many economics expectations models, it is not the lag time in the release of pertinent data that is likely to account for the lagged adjustment. The firm-specific risk data are never released by BLS, and aggregative statistics are not released until August of the following year.