THE VALUE OF A STATISTICAL LIFE: EVIDENCE FROM PANEL DATA

Thomas J. Kniesner, W. Kip Viscusi, Christopher Woock, and James P. Ziliak*

Abstract—We address long-standing concerns in the literature on compensating wage differentials: 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.

I. Introduction

THE concept at the value of statistical life (VSL) based on econometric estimates of wage-fatality risk trade-offs 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 & Kosec, 2005; Bilmes & Stiglitz, 2006) to a refined measurement of economic growth (Jena et al. 2008). Notwithstanding the wide use of the VSL approach, there is still concern over excessively large or small estimates and the wide range of VSL estimates. One approach to dealing with the dispersion of VSL estimates, which the U.S. Environmental Protection Agency has used, 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 literature on the VSL. For worker i ($i = 1,\ldots, N$) in industry j ($j = 1,\ldots, J$) and occupation k ($k = 1,\ldots, K$) at time t ($t = 1,\ldots, T$), the hedonic trade-off 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, and the usual demographic variables: worker education, age and age squared, race, marital status, and union status; and 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 individual differences in personal safety productivity that leads higher-wage workers to take what appear to be more dangerous jobs because the true danger level for such a worker is lower than the measured fatality rate; the second individual effect reflects unmeasured job productivity that leads more productive and higher-wage workers to take safer jobs. Our research, using equation (1) in conjunction with a variety of econometric techniques, 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 & Greenstone, 2004a) and possible state dependence in wages (MaCurdy, 2007). Here we 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 not explore emerging interest in the heterogeneity of VSL

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* Kniesner: Center for Policy Research, Syracuse University, and IZA; Viscusi: Vanderbilt University; Woock: Abbott; Ziliak: Center for Poverty Research, University of Kentucky.

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

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

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by age and other personal characteristics (Kniesner, Viscusi, & Ziliak, 2006, 2010; Aldy & Viscusi, 2008).

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; 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 & Hausman, 1986). In addition, we examine how instrumental variable estimates for each approach attenuate 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 unmodeled worker productivity and safety-related productivity as reflected in (α_{0i}^{+}) and (α_{0i}^{-}) in equation (1) (Hwang, Reed, & Hubbard, 1992; Viscusi & Hersch, 2001; Shogren & Stamland, 2002). Panel data allow the researcher to sweep out all such time-invariant individual effects and 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 VSLs stem 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 plus industry and 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% from the implausibly large cross-section PSID-based VSLs of \$20 million to \$30 million. We demonstrate how systematic econometric modeling narrows the estimated value of a statistical life from about 0 to \$30 million, and then to about \$4 million to \$10 million, which we then show clarifies the choice of the proper labor market–based VSL for policy evaluations.

II. Panel Data Econometric Framework

Standard panel-data estimators permitting latent worker-specific heterogeneity through person-specific intercepts in equation (1) are the deviations from the time-mean (within) 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 toward 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 nonstationarity in wages, measurement errors, or model misspecification (Wooldridge, 2010). Because wages from longitudinal data on individuals have been shown to be nonstationary in other contexts (Abowd & 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,

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

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

The first-difference model could exacerbate errors-in-variables problems relative to the within model (Griliches & 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 (two years or more) differences. If $\Delta \geq 2$, then measurement error effects are mitigated in equation (2) relative to within-differences regression (Griliches & Hausman, 1986; Hahn,

[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, et al. (2006) also used the 720 cell measure but not the multiyear averages. Neither study employed panel data econometric techniques.

Hausman, & 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 multiyear averages of fatalities.

Lillard and Weiss (1979) demonstrated that earnings functions may have not only idiosyncratic differences in levels but also 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,

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

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: (a) 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) and (b) an instrument set that grows as the time-series dimension of the panel evolves (Arellano & 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 VSLs.

A. Comparison Estimators

If $E[u_{ijk}|\pi_{jk}, X_{ijk}] = 0$ and $E[\alpha_{0i}^{+,-}|\pi_{jk}, X_{ijk}] = 0$, 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' times means of the variables

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

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 & Kniesner, 2003; Ashenfelter, 2006), the between-groups estimator should provide improved estimates of the wage–fatal risk trade-off 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 and within-groups variations.

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 nonrandom 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 nonrandom job switching.

B. 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

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

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 $\overline{\text{VSL}}$ 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% confidence intervals around the mean VSL.[5]

[5] The 95% 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 $\overline{\text{VSL}} \pm 1.96 \times \text{Var}(\overline{\text{VSL}})$, where $\text{Var}(\overline{\text{VSL}}) = 100{,}000^2 \times h^2 \times \bar{w}^2 \times \text{Var}(\hat{\alpha}_1)$. 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 $\text{Var}(\overline{\text{VSL}}) = 100{,}000^2 \times h^2 \times \left(\bar{w}^2 \times \text{Var}(\hat{\alpha}_1) \times \hat{\alpha}_1^2 \times \text{Var}(\bar{w})\right)$, with little change in the estimated intervals.

III. 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.

A. *PSID Sample*

The sample we use consists of male heads of household ages 18 to 65 who are in the random Survey Research Center (SRC) portion of the PSID, and so it excludes the oversample of the poor in the Survey of Economic Opportunity (SEO) and the Latino subsample. The male heads in our regressions (a) worked for hourly or salary pay at some point in the previous calendar year, (b) are not permanently disabled or institutionalized, (c) are not in agriculture or the armed forces, (4) have a real hourly wage greater than $2 per hour and less than $100 per hour, and (5) have no missing data on wages, education, region, industry, and occupation.

Beginning in 1997, the PSID moved to interviewing every other year. 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% of the men are present for all five waves (nine years); another 25% 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, biweekly, 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 the 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, and 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]

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 North-central	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
Three-year fatality rate (per 100,000)	6.260	8.769
Number of Men: 2,036		
Number of person-years: 6,625		

B. *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

[6] Ziliak and Kniesner (1998) show that when there is nonrandom attrition, 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.

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. For an injury be classified as work related, the decedent must have been employed at the time of the fatal event and engaged in legal work activity that required him or her to 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 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 measures of the fatality risk are time varying because of changes in the numerator and the denominator.[9]

We expect there may be less measurement error in the three-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 & Sider, 1983; Black & 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% or (in absolute value) a negative 75%. 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 three-year average.

Our research also avoids a problem plaguing past attempts to estimate the wage–fatal risk trade-off 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% to 40% 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.

IV. 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; and 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 occupa-

[8] The fatality data can be obtained on CD-ROM by 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% and no more than +300%.

[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.

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
Three-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
Three-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
Three-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
Three-year fatality rate (per 100,000)	64.927	43.908	21.019

TABLE 3.—CROSS-SECTION AND PANEL DATA ESTIMATES OF WAGE–FATAL RISK TRADE-OFF

	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 Two-Digit SIC Fatality Rate
	(1)	(2)	(3)	(4)	(5)	(6)
Annual fatality rate × 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 ($millions)	5.8	6.8	15.4	25.9	6.2	5.7
	[0.8, 10.8]	[1.1, 12.5]	[5.3, 25.6]	[8.9, 42.9]	[0.3, 12.2]	[−2.1, 13.5]
VSL using average hours	6.6	7.8	17.4	29.2	7	6.5
	[0.9, 12.4]	[1.3, 14.4]	[5.9, 28.8]	[10.1, 48.3]	[0.4, 13.7]	[−2.4, 15.4]
Number of observations	4,338	2,788	6,625	6,625	6,625	5,085
Three-year fatality rate × 1,000	1.7556	2.4679	3.0373	4.5069	1.09	1.5387
	(0.6812)	(0.8107)	(1.4460)	(2.2777)	(0.8962)	(0.9312)
Implied VSL ($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	4,916	2,992	5,866	5,866	5,866	5,240

Standard errors are recorded in parentheses, and 95% confidence intervals are in 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, model (6), does not include controls for two-digit SIC.

tions 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%. 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, from 1992 to 2001, 34 states had benefit growth rates that were confined to a 1.7% 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.

A. *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 misrepresented if certain basic econometric issues are mishandled.

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 equation (5) is \$6.6 million, with a 95% confidence interval of \$0.9 million to \$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 one-third larger when the 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 & Aldy, 2003). Notice that the cross-section estimators in columns 3 and 4 of table 3 produce large implied VSLs—about \$17 million to \$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% 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 et al. (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% higher, so 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 to \$8 million when using a 2000 hour work-year (CI = \$0.8 million to \$13.6 million) and about \$7 million to \$9 million when using sample average hours to compute VSL (CI = \$0.9 million to \$15.6 million).

B. *First-Difference Estimator Specification Checks*

An issue seldom addressed in panel wage equations producing VSL is the endogeneity of the fatality change regressor, which may result from dynamic decisions that workers make to change jobs (Solon, 1986, 1989; Spengler & 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 multiperiod 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% switched to lower-fatality-risk jobs and 46% switched to higher-fatality-risk jobs, so that on balance, there was 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 eight 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

TABLE 4.—ESTIMATES OF WAGE–FATAL RISK TRADE-OFF BY JOB CHANGE STATUS

	Annual Fatality Rate × 1,000	Three-Year Fatality Rate × 1,000
	(1)	(2)
Never-change industry-occupation		
Annual fatality rate × 1,000	0.1234	−0.8074
	(1.4164)	(3.4029)
Implied VSL ($millions)	0.6	−3.8
	[−12.4, 13.6]	[−35.5, 27.9]
VSL using average hours	0.7	−4.4
	[−14.3, 15.6]	[−40.8, −32.0]
Number of person-years	1,303	1,390
Ever-change industry-occupation		
Annual fatality rate × 1,000	1.4645	2.1051
	(0.6555)	(0.7667)
Implied VSL ($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	3,035	30,35
Only when change industry-occupation		
Annual fatality rate × 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	1,920	2,261

Standard errors are recorded in parentheses and 95% confidence intervals in 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.

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 to $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 to $7 million versus $7 million to $8 million using a standard work year and about $7 million to $8 million versus about $7 million to $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 column 1; in column 6, we instead add two-digit industry-by-year dummies to the base case in column 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 column 1; and in column 9, we add one digit occupation-by-two-digit industry controls to column 1. The regressions in table 7 make clear that controlling for linear two-digit industry and one-digit occupation in the base case helps

[10] Greene (2012) 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 and occupation is beyond the scope of this paper, but as Ashenfelter (2006) noted, it is a research area in need of more comprehensive measurement the labor market VSL.

TABLE 5.—INSTRUMENTAL VARIABLES ESTIMATES OF WAGE–FATAL RISK TRADE-OFF

	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 × 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)					
$(t-1 \text{ rate}) - (t-3 \text{ rate})$		0.6398				
		(0.0121)				
$t-2$ fatality rate			0.5313			
			(0.0144)			
$t-3$ fatality rate			−0.5428			
			(0.0141)			
$(t-2 \text{ rate}) - (t-3 \text{ rate})$				0.5377		
				(0.0134)		
$t-2$ fatality rate					0.4861	
					(0.0164)	
$t-4$ fatality rate					−0.5113	
					(0.0149)	
$(t-2 \text{ rate}) - (t-4 \text{ rate})$						0.5038
						(0.0145)
R^2	0.67	0.67	0.60	0.60	0.61	0.61
Number of observations	4,338	4,338	4,338	4,338	3,235	3,235

Standard errors are recorded in parentheses and 95% confidence intervals in 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 TRADE-OFF

	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 × 1,000	1.2907	1.3784	1.4591	1.6701
	(0.7694)	{0.098}	(0.8915)	{0.103}
Implied VSL ($millions)	5.8	6.2	6.6	7.6
	[−1.0, 12.7]	[−1.0, 13.6]	[−1.3, 14.5]	[−1.5, 16.6]
VSL using average hours	6.7	7.2	7.6	8.7
	[−1.1, 14.5]	[−1.2, 15.6]	[−1.5, 16.7]	[−1.7, 19.1]
Number of observations	2,788		2,788	
Three-year fatality rate × 1,000	1.7876	1.9876	1.8584	2.2609
	(0.8395)	{0.035}	(1.1042)	{0.095}
Implied VSL ($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	3,162		3,162	

Standard errors are recorded in parentheses 95% confidence intervals in brackets, and *p*-values of the null hypothesis that the long-run effect is 0 are recorded in braces. 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.

precisely identify the wage–fatal risk trade-off, 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]

C. *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 & 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

[13] Our data do not contain information on injury rates, and the publicly available injury data vary only across two-digit industry and not industry-by-occupation. In results not tabulated, when we include the change in the two-digit SIC injury rate, the coefficient on the fatality rate falls by about 40%. With the injury rate variable included, we lose about 500 person-years, or over 10% 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 rerun the base model using the subsample of those with nonmissing 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% 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.

TABLE 7.—SPECIFICATION CHECKS FOR FIRST-DIFFERENCE ESTIMATES OF WAGE–FATAL RISK TRADE-OFF

	Base Case from Table 3, Column 1	Drop Industry and Occupation Controls from Column 1	Add One-Digit Occupation Controls to Column 2	Add Two-Digit Industry Controls to Column 2	Add One-Digit Occupation-by-Year and Two-Digit Industry Controls to Column 1	Add One-Digit Occupation and Two-Digit Industry-by-Year Controls to Column 1	Add Census Division-by-Year Controls to Column 2	Add One-Digit Occupation, Two-Digit Industry, and State-by-Year Controls to Column 1	Add One-Digit Occupation by Two-Digit Industry Controls to Column 1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Annual fatality rate × 1,000	1.3438	0.9255	1.6007	0.414	1.3768	0.8151	0.8506	1.0158	0.4011
	(0.5943)	(0.4375)	(0.4793)	(0.5082)	(0.6120)	(0.6043)	(0.4321)	(0.5744)	(0.3820)
Implied VSL ($millions)	5.8	4	6.9	1.8	5.9	3.5	3.7	4.4	1.7
	[0.8, 10.8]	[0.3, 7.7]	[2.9, 11.0]	[−2.5, 6.1]	[0.8, 11.1]	[−1.6, 8.6]	[0.02, 7.3]	[−0.5, 9.2]	[−1.5, 5.0]
VSL using average hours	6.6	4.6	7.9	2	6.8	4	4.2	5	2
	[0.9, 12.4]	[0.3, 8.8]	[3.3, 12.5]	[−2.9, 6.9]	[0.9, 12.7]	[−1.8, 9.9]	[0.02, 8.4]	[−0.5, 10.6]	[−1.7, 5.7]
Number of observations	4,338	4,338	4,338	4,338	4,338	4,338	4,338	4,338	4,338
Three-year fatality rate × 1,000	1.7556	0.8147	1.7785	0.3824	1.7624	1.3331	0.7517	0.6395	0.1558
	(0.6812)	(0.5394)	(0.5435)	(0.6070)	(0.6936)	(0.7119)	(0.5374)	(0.5367)	(0.5342)
Implied VSL ($millions)	7.7	3.6	7.8	1.7	7.8	5.9	3.3	2.8	0.7
	[1.9, 13.6]	[−1.1, 8.2]	[3.1, 12.5]	[−3.6, 6.9]	[1.8, 13.7]	[−0.3, 12.0]	[−1.3, 7.9]	[−1.8, 7.4]	[−3.9, 5.3]
VSL using average hours	8.8	4.1	9	1.9	8.9	6.7	3.8	3.2	0.8
	[2.1, 15.6]	[−1.2, 9.4]	[3.6, 14.3]	[−4.1, 7.9]	[2.0, 15.7]	[−0.3, 13.7]	[−1.5, 9.1]	[−2.1, 8.5]	[−4.5, 6.1]
Number of observations	4,916	4,916	4,916	4,916	4,916	4,916	4,916	4,916	4,916

Standard errors are recorded in parentheses and 95% confidence intervals in 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 TRADE-OFF 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	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
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Annual fatality rate × 1,000	1.3438	1.1486	3.9705	1.3641	1.4678	1.9496	1.0639	1.2038	0.6577
	(0.5943)	(0.5627)	(1.1810)	(0.7449)	(0.5946)	(0.6828)	(1.0566)	(1.0466)	(0.8925)
Implied VSL ($millions)	5.8	5	16.4	5.6	6.1	8.1	4.7	5.3	3
	[0.8, 10.8]	[0.2, 9.7]	[6.8, 25.9]	[−0.4, 11.6]	[1.2, 11.0]	[2.5, 13.6]	[−4.5, 13.9]	[−3.7, 14.4]	[−4.9, 10.9]
VSL using average hours	6.6	5.7	18.4	6.3	7	9.2	5.4	6.1	3.4
	[0.9, 12.4]	[0.2, 11.1]	[7.7, 29.2]	[−0.4, 13.1]	[1.4, 12.5]	[2.9, 15.5]	[−5.1, 15.9]	[−4.3, 16.5]	[−5.7, 12.5]
Number of observations	4,338	4,338	6,625	6,468	3,035	1,920	3,235	3,235	2,788

Standard errors are recorded in parentheses and 95% confidence intervals in 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.

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 to \$6 million (CI = \$0.2 million to \$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 of 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 (2,000 hour) work year and about \$6 million using the higher sample average work year—but neither is 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 VSLs 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 dominates 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.

V. 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 PSID have often yielded extremely high VSL estimates of \$20 million or more, 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 trade-off. 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 to \$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 first-stage R^2s) 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 of September 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 or more. 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 VSLs 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. When we apply 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 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 or 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.

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