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Abstract

Our research addresses fundamental long-standing concerns in the compensating wage differentials literature and its public policy implications: the econometric properties of estimates of the value of statistical life (*VSL*) and the wide range of such estimates from about $0 to almost $30 million. Here we address most of the prominent econometric issues by applying panel data, a new and more accurate fatality risk measure, and systematic application of panel data estimators. Controlling for measurement error, endogeneity, latent individual heterogeneity that may be correlated with the regressors, state dependence, and sample composition yields an estimated value of a statistical life of about $7 million–$12 million, which we show can clarify greatly the cost-effectiveness of regulatory decisions. We show that probably the most important econometric issue is controlling for latent heterogeneity; less important is how one does it.

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Key Words: VSL, panel data, fixed effects, random effects, long-differences, PSID

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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 econometric practices affects the estimated *VSL* so as to narrow the range of 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 industry in some specifications), state and region of residence, plus

[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.

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 and 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 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 those pertinent to one's industry or general occupation, may

[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).

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 very 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).

We devote particular attention to measurement errors, which have been noted in Black and Kniesner (2003), Ashenfelter and Greenstone (2004b), and Ashenfelter (2006). Although we do not have information on subjective risk beliefs, we use very detailed data on objective risk measures and consider the possibility that workers are driven by risk expectations. Published industry risk beliefs are strongly correlated with subjective risk values,[3] and we follow the standard practice of matching to workers in the sample an objective risk measure. 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.

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

[3] See Viscusi and Aldy (2003) for a review.

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 only previous 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 as well as longer 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 and endogeneity 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 mentioned 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, 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

[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.

jobs across occupations or industries possibly endogenously rather than from variation in risk levels over time in a given job setting.

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 $7 million–$12 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 deviation from time-mean (within) estimator and the time-difference (first- and long-differences) 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. With a finite number of periods ($T > 2$), estimates from the two different fixed-effects estimators can diverge due to possible non-stationarity in wages, measurement error, 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

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

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). 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. For completeness we also note how the first-difference and longer-differences estimates compare to the within estimates.

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 2008, Chapter 15) 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 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 explicit inclusion of 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 and that the typical work-year is about 2000 hours, the estimated value of a statistical life at the mean level of wages is

$$\overline{VSL} = \left[\left(\frac{\partial \hat{w}}{\partial \pi} = \hat{\alpha}_1 \times \overline{w}\right) \times 2000 \times 100,000 \right]. \qquad (5)$$

Although the *VSL* function in equation (5) can be evaluated at various points in the wage distribution, most studies report only the mean effect. 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*.

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 thus 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 response 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.[5] 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 focal 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 state of residence, dummy indicators for region of country (northeast, north central, and west with south the omitted region), race (white =

[5] 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.

1), union status (coverage = 1), marital status (married = 1), and one-digit occupation. Table 1 presents summary statistics.

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.[6]

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

[6] 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. In our subsequent robustness checks in Table 8 we examine what happens to *VSL* if we apply the same screen to the three-year change or eliminate the screen completely.

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 and by occupation, it will provide 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, which differ according to the numerator. The first measure simply uses the number of fatalities in each industry-occupation cell. 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). The denominator for each measure used to construct the fatality risk is the number of employees for that industry-occupation group in survey year *t*. Both of our two measures of the fatality risk are time-varying because of changes in both the numerator and the denominator.[7]

We expect there to 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. 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). But to further reduce the influence of large swings in fatality risk, we drop person-years where the percentage change in fatality risk exceeds a positive 300 percent or 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 within or 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-

[7] We used the bi-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.

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 for ease of presentation, each regression model we use controls for a quadratic in age, years of schooling, indicators for region, marital status, union status, race, and one-digit occupation. Because of the substantial heterogeneity of jobs in different occupations, the regressions include a set of one-digit occupation dummies. In addition, because there might be unmeasured differences in labor markets across states that do not vary with time, we include a full set of state fixed effects. Likewise, workers in a given year may face common macroeconomic shocks to wages, and so we include a vector of year dummies in all models. However, our baseline estimating equations do not include industry dummy variables as well because doing so could introduce substantial multicollinearity with respect to the fatality risk variable, which involves matching workers to fatality risk based on their industry and occupation. (In our subsequent robustness checks we add industry dummies.) Reported standard errors are clustered by industry and occupation and are also robust to the relevant heteroskedasticity and serial correlation. 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 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 model's coefficient for the annual fatality rate in Table 3 using the sample mean wage of $21 in (5) is $6.9 million, with a confidence interval of $6.8 million–$7.1 million.[8] We emphasize that a novel aspect of our research is that it helps clarify the size of possible measurement error effects. If measurement error in fatality risk is random it will attenuate coefficient estimates and should be reduced by letting the fatality rate encompass a wider time interval. Compared to *VSL* from the more typical annual risk measure, the estimated *VSL* in Table 3 is about 13 percent larger when fatality risk is a three-year average. The last two columns of Table 3 report the results for widest possible differences ($\ln w_{2001} - \ln w_{1993}$) as well as difference-in-differences from equation (3), which should remove possible spurious estimated effects from variables that are not difference stationary. The main message from Table 3 is that correcting for measurement error in most cases enlarges estimated *VSL*, and that even for the relatively basic panel models using differencing, the range for *VSL* is not uncomfortably large: about $7 million–$9 million when using a 2000 hour work year (CI = $6.8 million–$9.7 million) and about $8 million–$10 million when using sample average hours to compute *VSL* (CI = $7.5 million–$10.9 million).

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 2006). Some changes in fatality risk will occur because of within industry-occupation cell changes and others will

[8] The confidence interval uses a first-order Taylor series expansion to estimate the variance of the mean *VSL*, which from equation (5) is $Var(\overline{VSL}) = 2000^2 * 100,000^2 * \left(\overline{w}^2 * Var(\hat{\alpha}_1) + \hat{\alpha}_1^2 Var(\overline{w})\right)$.

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 *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. Specifically, based on the results of Griliches and Hausman (1986) we interchangeably use the (t−1) and (t–3) levels of the fatality risk, or the (t−1) less (t−3) difference. We limit the focus to the annual fatality rate so as to have enough lagged fatality and fatality differences as instruments.[9] The main result is a fairly narrow range for the estimated *VSL*, approximately $7 million–$8 million when we instrument the annual change in fatality risk (CI = $6.6–$8 million).

Table 6 presents our final focal panel results from dynamic first-difference regressions. The short-run effects from the dynamic model appear in column 1 and the long-run (steady state) estimates appear in column 2. 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 $2 million, ranging from $7 million–$8 million versus $9 million–$10 million using a standard work

[9] Greene (2008, Chapter 15) 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.

year and about $8 million–$9 million versus about $10 million–$11 million using sample average annual hours worked. Again, the range of *VSL* estimates is not great when panel data are used with estimators that accommodate endogeneity, weak instruments, measurement error, latent heterogeneity and possible state dependence.

4.2 Comparison Results From Cross-Section Estimators

Table 7 presents the comparison models that flesh out the most salient econometric issues when compared to the focal panel results from Tables 3–6 just presented.

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 1 and 2 of Table 7 produce large implied *VSLs*, about $16 million–$28 million.

In contrast, column 3 of Table 7 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; and Hausman test results indicate a similar rejection here. The simple fixed effects within estimator in the last column is preferred over the simple random effects estimator, with an estimated *VSL* of about $6–$8 million. 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 up to 60 percent relative to a model that ignores latent heterogeneity.

The difference in estimated *VSL* with 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.

4.3 Panel Data Estimator Specification Checks

As a final dimension of our research we present Tables 8–10, which contain 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 the many options the researcher has in estimating a linear panel model. In particular, we further explore the importance of econometric modeling choices for covariates, endogeneity, dynamics, expectations, and sample composition in panel data based estimates of *VSL*.

The results of Table 8 show little effect on *VSL* from whether or not one trims the set of observations by the size of change in fatality rates between observation years or adds an additional control for industry. What matters more to the size of *VSL* is how the researcher addresses injury risk expectations and wage dynamics, which we now discuss.

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 first 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 $4 million–$6 million (CI = $3.4 million–$5.9 million). To be fair, 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 9. When we estimate more sophisticated rational expectations type models with multiple lagged values as instruments, as in Table 9, the comparison results are similar to our earlier findings: the model passes the standard weak instrument check and *VSL* is about $7 million–$9 million using a standard (2000 hour) work year and about $8 million–$10 million using the higher sample average work year.

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.[10] The Arellano-Bond results in Table 10 produce *VSL*s that are about the same or a little higher than the dynamic models that use much smaller temporally fixed instrument sets, as in Table 6.

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

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

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 those 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 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 $7 million–$12 million depending on the time-frame (short-run versus long-run) and work year (standard or sample average) in the calculation.

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 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 = –$5.4 million–$28.1 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 distributions becomes 27 policies that clearly pass a benefit-cost test, 23 that fail a benefit-cost test, with only 3 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 the 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)	5.716	8.390
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	3.290
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	46.240	24.351
3-Year Fatality Rate (per 100,000)	64.927	43.908	21.019

Table 3: First-Difference Estimates of Wage-Fatal Risk Tradeoff

	Static First Difference Estimates	First-Difference Estimator for 2001minus1993	Difference in Differences Estimator
Annual Fatality Rate x 1,000	1.6007	1.9438	1.4851
	(0.4793)	(1.7223)	(0.5196)
Implied VSL ($Millions)	6.9	9.1	6.7
95% CI	[6.8, 7.1]	[8.5, 9.7]	[6.5, 6.9]
VSL - using average hours	7.9	10.2	7.7
95% CI	[7.7, 8.1]	[9.5, 10.9]	[7.5, 7.9]
Number of Person-Years	4338	1017	2788
3-Year Fatality Rate x 1,000	1.7785	1.8627	1.8567
	(0.5435)	(1.5412)	(0.6339)
Implied VSL ($Millions)	7.8	8.8	8.5
95% CI	[7.7, 8.0]	[8.3, 9.3]	[8.3, 8.7]
VSL - using average hours	9.0	9.9	9.8
95% CI	[8.8, 9.1]	[9.3, 10.5]	[9.5, 10.0]
Number of Person-Years	4916	1171	2992

Notes: Standard errors are recorded in parentheses. 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, state, and year effects. To construct the *VSL* using equation (5) the coefficients in the table are divided by 1,000. 95% Confidence Intervals are constructed based on a 1st order Taylor series expansion.

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

	Static First-Difference	First-Difference Estimator for 2001 minus 1993
Never Change Industry-Occupation		
Annual Fatality Rate x 1,000	0.1234	0.3097
	(1.4164)	(3.0008)
Implied VSL ($Millions)	0.6	1.6
	[0.2, 0.9]	[-0.2, 3.4]
VSL - using average hours	0.7	1.8
	[0.2, 1.1]	[-0.3, 3.9]
3-Year Fatality Rate x 1,000	-0.8074	0.5758
	(3.4029)	(5.0319)
Implied VSL ($Millions)	-3.8	3.0
	[-4.7, -3.0]	[0.0, 6.0]
VSL - using average hours	-4.4	3.4
	[-5.4, -3.4]	[0.0, 6.9]
Number of Person-Years	1303 / 1390	282 / 296
Ever Change Industry-Occupation		
Annual Fatality Rate x 1,000	1.6405	1.9125
	(0.5088)	(1.7859)
Implied VSL ($Millions)	6.8	8.6
	[6.7, 7.0]	[7.9, 9.3]
VSL - using average hours	7.8	9.6
	[7.6, 8.0]	[8.8, 10.4]
3-Year Fatality Rate x 1,000	1.9845	1.8399
	(0.5776)	(1.5713)
Implied VSL ($Millions)	8.3	8.4
	[8.1, 8.5]	[7.8, 9.0]
VSL - using average hours	9.4	9.4
	[9.2, 9.7]	[8.7, 10.1]
Number of Person-Years	3035	735 / 868

Notes: Standard errors are recorded in parentheses. 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, state, and year effects. To construct the *VSL* using equation (5) the coefficients in the table are divided by 1,000. 95% Confidence Intervals are constructed based on a 1st order Taylor series expansion.

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

	Static First-Difference	First-Difference Estimator for 2001 minus 1993
Only When Change Industry-Occupation		
Annual Fatality Rate x 1,000	1.6607	1.7111
	(0.5471)	(1.8036)
Implied VSL ($Millions)	6.9	7.4
	[6.7, 7.1]	[6.7, 8.1]
VSL - using average hours	7.8	8.2
	[7.6, 8.1]	[7.4, 9.0]
3-Year Fatality Rate x 1,000	1.9156	1.6764
	(0.5660)	(1.5877)
Implied VSL ($Millions)	8.2	7.4
	[7.9, 8.4]	[6.7, 8.0]
VSL - using average hours	9.3	8.2
	[9.0, 9.5]	[7.5, 8.9]
Number of Person-Years	1920 / 2261	597 / 699

Notes: Standard errors are recorded in parentheses. 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, state, and year effects. To construct the *VSL* using equation (5) the coefficients in the table are divided by 1,000. 95% Confidence Intervals are constructed based on a 1st order Taylor series expansion.

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
Annual Fatality Rate x 1,000	1.5574	1.5926
	(0.6412)	(0.6429)
Implied VSL ($Millions)	6.7	6.9
95% CI	[6.6, 6.9]	[6.7, 7.0]
VSL - using average hours	7.7	7.9
95% CI	[7.5, 7.9]	[7.7, 8.0]
First Stage Results		
t–1 fatality rate	0.7752	
	(0.0118)	
t–3 fatality rate	-0.7553	
	(0.0118)	
(t–1 rate) – (t–3 rate)		0.7653
		(0.0108)
R^2	0.63	0.63
Partial R^2	0.54	0.54
Robust Wald {p-value}	106 {0.00}	163 {0.00}
Number of Person-Years	4338	4338

Notes: Standard errors are recorded in parentheses. 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, 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. 95% Confidence Intervals are constructed based on a 1st order Taylor series expansion.

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

	Dynamic First-Difference Estimates with lag differenced wage instrumented	
	Short-Run Effect	Long-Run Effect
Annual Fatality Rate x 1,000	1.6023	1.9546
	(0.5346)	[0.039]
Implied VSL ($Millions)	7.2	8.8
95% CI	[7.1, 7.4]	[8.6, 9.1]
VSL - using average hours	8.3	10.2
95% CI	[8.1, 8.6]	[9.9, 10.4]
First Stage Partial R^2	0.15	
Robust Wald {p-value}	230,100 {0.00}	
Number of Person-Years	2788	
3-Year Fatality Rate x 1,000	1.7427	2.2164
	(0.6175)	[0.062]
Implied VSL ($Millions)	8.0	10.2
95% CI	[7.8, 8.2]	[10.0, 10.5]
VSL - using average hours	9.2	11.7
95% CI	[9.0, 9.5]	[11.4, 12.0]
First Stage Partial R^2	0.15	
Robust Wald {p-value}	75,527 {0.00}	
Number of Person-Years	3162	

Notes: Standard errors are recorded in parentheses and *p*-values of the null hypothesis that the long-run effect is zero are recorded 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, state, and year effects. One and two period 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. 95% Confidence Intervals are constructed based on a 1st order Taylor series expansion.

Table 7: Cross Section and Panel Data Estimates of Wage-Fatal Risk Tradeoff

	Pooled Cross Section Time Series Estimator	Between-Group Estimator	Random-Effects Estimator	Fixed-Effects Estimator
Annual Fatality Rate x 1,000	4.625	5.9552	2.6043	1.7979
	(1.2082)	(1.5108)	(0.5950)	(0.6339)
Implied VSL ($Millions)	19.1	24.5	10.7	7.4
95% CI	[18.8, 19.4]	[24.1, 25.0]	[10.6, 10.9]	[7.3, 7.5]
VSL - using average hours	21.5	27.6	12.1	8.4
95% CI	[21.1, 21.8]	[27.2, 28.1]	[11.9, 12.3]	[8.2, 8.5]
Number of Person-Years	6625	2036	6625	6468
3-Year Fatality Rate x 1,000	3.7666	4.4039	2.087	1.4516
	(1.2696)	(1.6207)	(0.7003)	(0.7566)
Implied VSL ($Millions)	16.2	18.9	9.0	6.2
95% CI	[15.9, 16.5]	[18.6, 19.3]	[8.8, 9.1]	[6.1, 6.4]
VSL - using average hours	18.4	21.5	10.2	7.1
95% CI	[18.0, 18.8]	[21.1, 21.9]	[10.0, 10.4]	[6.9, 7.3]
Breusch-Pagan Test for Random Effects {p-value}			2807 {0.00}	
Hausman Test for Fixed –vs.- Random Effects {p-value}				454 {0.00}
Number of Person-Years	5866	2012	5866	5728

Notes: Standard errors are recorded in parentheses. 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, state, and year effects. To construct the *VSL* using equation (5) the coefficients in the table are divided by 1,000. 95% Confidence Intervals are constructed based on a 1st order Taylor series expansion.

Table 8: Specification Checks for First-Difference Estimates of Wage-Fatal Risk Tradeoff

	First Difference Estimates using Lagged Fatality Rates	First-Difference Estimates with Industry Dummies	First-Difference Estimates with Untrimmed Fatality Rates	First-Difference Estimates Using Alternative Trim Horizon
Annual Fatality Rate x 1,000	1.1611	1.3455	1.4281	1.4988
	(0.5356)	(0.5136)	(0.4253)	(0.4332)
Implied VSL ($Millions)	5.1	5.8	6.3	6.6
95% CI	[4.9, 5.2]	[5.7, 5.9]	[6.2, 6.4]	[6.5, 6.7]
VSL - using average hours	5.8	6.6	7.2	7.5
95% CI	[5.7, 5.9]	[6.5, 6.8]	[7.1, 7.4]	[7.4, 7.7]
Number of Person-Years	4406	4338	5242	4916
3-Year Fatality Rate x 1,000	0.7777	1.4107	1.7186	1.9304
	(0.5553)	(0.6050)	(0.5366)	(0.5729)
Implied VSL ($Millions)	3.5	6.2	7.6	8.3
95% CI	[3.4, 3.6]	[6.1, 6.3]	[7.5, 7.7]	[8.2, 8.5]
VSL - using average hours	4.0	7.1	8.7	9.5
95% CI	[3.9, 4.2]	[6.9, 7.3]	[8.5, 8.9]	[9.3, 9.7]
Number of Person-Years	3695	4916	5242	4338

Notes: Standard errors are recorded in parentheses. 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, state, and year effects. To construct the *VSL* using equation (5) the coefficients in the table are divided by 1,000. 95% Confidence Intervals are constructed based on a 1^{st} order Taylor series expansion. For the alternative trim horizon person-years are dropped from the annual fatality rate equation if the three-year average fatality rate exceeds positive 300 percent or negative 75 percent; likewise person-years are dropped from the three-year average fatality rate equation if the annual fatality rate exceeds positive 300 percent or negative 75 percent.

Table 9: Specification Checks for Instrumental Variables Estimates of Wage-Fatal Risk Tradeoff

	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
Annual Fatality Rate x 1,000	2.0237	2.019	1.6134	1.589
	(0.7849)	(0.7845)	(0.7498)	(0.7496)
Implied VSL ($Millions)	8.7	8.7	7.1	7.0
95% CI	[8.6, 8.9]	[8.5, 8.9]	[6.9, 7.3]	[6.8, 7.2]
VSL - using average hours	10.0	10.0	8.2	8.1
95% CI	[9.8, 10.2]	[9.7, 10.2]	[8.0, 8.4]	[7.8, 8.3]
First Stage Results				
t–2 fatality rate	0.6994			
	(0.0134)			
t–3 fatality rate	-0.7019			
	(0.0132)			
(t–2 rate) – (t–3 rate)		0.7008		
		(0.0122)		
t–2 fatality rate			0.6476	
			(0.0155)	
t–4 fatality rate			-0.6570	
			(0.0141)	
(t–2 rate) – (t–4 rate)				0.6537
				(0.0135)
R^2	0.54	0.54	0.55	0.55
Number of Person-Years	4338	4338	3235	3235

Notes: Standard errors are recorded in parentheses. 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, 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. 95% Confidence Intervals are constructed based on a 1st order Taylor series expansion.

Table 10: Arellano-Bond Dynamic First Difference Estimates of Wage-Fatal Risk Tradeoff

	Dynamic First-Difference Estimates with lag differenced wage instrumented	
	Short-Run Effect	Long-Run Effect
Annual Fatality Rate x 1,000	1.9094	2.2893
	(0.9150)	[0.039]
Implied VSL ($Millions)	8.6	10.4
95% CI	[8.4, 8.9]	[10.1, 10.7]
VSL - using average hours	9.9	11.9
95% CI	[9.7, 10.3]	[11.5, 12.3]
Number of Person-Years	2788	
3-Year Fatality Rate x 1,000	1.7056	2.1563
	(0.9050)	[0.062]
Implied VSL ($Millions)	7.9	9.9
95% CI	[7.6, 8.1]	[9.7, 10.2]
VSL – using average hours	9	11.4
95% CI	[8.8, 9.3]	[11.1, 11.7]
Sargan Overidentifying Restrictions Test-Annual Fatality {p-value}	79.78 {0.16}	
Sargan Overidentifying Restrictions Test-3-Year Fatality {p-value}	88.96 {0.05}	
Number of Observations	3162	

Notes: Standard errors are recorded in parentheses and *p*-values of the null hypothesis that the long-run effect is zero are recorded 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, 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. 95% Confidence Intervals are constructed based on a 1st order Taylor series expansion.

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