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ABSTRACT

Pinning Down the Value of Statistical Life*

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 $0.5 million to about $21 million. We address most of the prominent econometric issues by applying panel data, a new and more accurate fatality risk measure, and systematic selection of panel estimator in our research. Controlling for measurement error, endogeneity, individual heterogeneity, and state dependence yields both a reasonable average level and narrow range for the estimated value of a statistical life of about $5.5–$7.5 million.

JEL Classification: I10, J17, J28, K00

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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 the mortality costs of the Iraq war (Wallsten and Kosec 2005, Bilmes and Stiglitz 2006). Notwithstanding the wide use of the *VSL* approach, there is still concern over excessively large/small estimates and wide range of the estimates for *VSL*. One approach to the dispersion of *VSL* estimates that 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 pins down the estimated *VSL* to a greater degree of refinement than in previous studies.

Our paper works within the econometrically familiar framework of the hedonic wage equation used in the value of statistical life literature. 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 tradeoff between the wage and risk of fatality is

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

where $\ln w_{ijkt}$ is the natural log of the hourly wage rate, and 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), region of residence, plus the usual demographic variables: worker education, age, race, marital status, and

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

union status. Finally, δ_t is a vector of time effects, and u_{ijkt} is an error term allowing an individual-specific effect plus conditional heteroskedasticity and within industry by occupation autocorrelation.[2] Our research will subsequently expand the structure summarized by equation (1) in a variety of ways, many of which will exploit the capabilities of panel data by using the Panel Study of Income Dynamics in conjunction with fatality risk measures that vary by year.

The apparent instability of the labor market *VSL* estimates has generated a series of prominent econometric controversies reviewed by Viscusi and Aldy (2003). The underlying hedonic model for equation (1) is that it traces out the locus of labor market equilibria involving the offer curves of firms and the supply curves of workers. Many of the most salient concerns involve 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 involve substantial measurement error. There have been concerns regarding the potential endogeneity of the job risk measure as well as state dependence. Equation (1) may also omit important characteristics of the job or the worker, leading to omitted variables bias. 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,

[2] The econometric structure in (1) is different than Brown's (1980) panel data model where the job risk variable was the same in all years and was given by the 1967 Society of Actuaries data, which provided information on overall mortality risks for people in 37 relatively high risk occupational groups and produced a *VSL* of only about $1.9 million. Moreover, the time variation in risk in his model arose from changes in occupation over time. In contrast, our research uses a highly refined fatality risk measure for 720 industry-occupation cells for which there is variation across time as well as variation that arises as workers change either their occupation or industry. Finally, 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).

to resolve many of the most prominent issues in the hedonic labor market literature. Our focus is on the average *VSL* across a broad sample of workers and will consequently not explore emerging concerns regarding the heterogeneity of *VSL* by age and other personal characteristics.

We devote particular attention to the measurement error issue emphasized by Black and Kniesner (2003) and Ashenfelter (2006). While we do not have information on subjective risk beliefs, we will use very detailed data on objective risk measures. Published industry risk beliefs are strongly correlated with subjective risk values[3] and we will 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 fluctuations.[4] Because the fatality rate data are available by year, workers in our panel who do not change jobs can 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 for 37 narrowly defined high risk occupations for all years, so that all possible variation in risk was restricted to job changers (Brown 1980). Our research also explores

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

using adjacent year first differences as well as long differences, for which the influence of measurement error should be less pronounced. We also examine how instrumental variable estimates for each approach attenuates measurement error and endogeneity bias. Finally, our dynamic first-difference 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.

Many studies have noted that potential biases in *VSL* estimates arise due to possible omitted variables, such unmodeled worker productivity and safety-related productivity.[5] We infer the role of omitted variables through a variety of estimation approaches, most of which exploit the capabilities of our large panel data set. Fixed effect models sweep out the individual effects for both the adjacent year differences and the long differences. In each instance, we use the pertinent instrumental variables estimator, following Griliches and Hausman (1986). Our work also distinguishes job movers from job stayers. We find that most of the variation in risk and most of the evidence of positive *VSL*s stems from people changing jobs across occupations or industries possibly endogenously rather than from variation in risk levels over time in a given job setting.

Our econometric refinements using panel data have a substantial effect on the estimated *VSL* levels. They reduce the estimated *VSL* by more than 75 percent from the implausibly large cross-section PSID-based *VSL*s of $18–$21 million. We demonstrate how careful econometric practice narrows the estimated value of a statistical life from

[5] Hwang, Reed, and Hubbard (1992) hypothesize that unobserved worker productivity biases *VSL* estimates downwards. Viscusi and Hersch (2001) examine safety-related productivity, but do not offer any directional hypothesis regarding the induced bias. Shogren and Stamland (2002) theorize that unobservable worker skill in promoting safety leads *VSL* estimates to be too high, but their result stems from analysis of infra-marginal workers who will not be captured in market evidence.

about \$0.5–\$21 million (Viscusi and Aldy 2003) to only about \$5.5–\$7.5 million, which greatly clarifies the choice of the proper *VSL* to be used in policy evaluations.

2. Panel Data Econometric Framework

Standard panel-data estimators permitting latent worker-specific heterogeneity through person-specific intercepts in (1) are the deviation from time-mean (within) estimator and the time-difference (first-differences) estimator. 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 (MaCurdy 1982; Abowd and Card 1989), we adopt the preferred first-difference model as a baseline.

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

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

where Δ refers to the first-difference operator and $\tilde{\delta}_t$ is a re-normalized vector of time dummies (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 (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 (2) relative to within-differences regression (Griliches and Hausman 1986). 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 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 (2) that is

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

where $\Delta^2 = \Delta_t - \Delta_{t-1}$, commonly known as the difference-in-difference operator, and $\tilde{\tilde{\delta}}_t$ is a re-normalized vector of time dummies. We also estimate a dynamic version of (2) by adding $\gamma \Delta \ln w_{ijkt-1}$ to the right-hand side and using the first-difference instrumental variables estimator recommended in Arellano (1989). As is standard in the dynamic panel literature our dynamic estimator uses the two-period lagged level of the dependent variable as an identifying instrument for the one-period lagged difference in the dependent variable. 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 $E[u_{ijk} \mid \pi_{jk}, X_{ijk}] = 0$, which is the standard zero conditional mean assumption of least squares regression, then OLS estimation of the hedonic equilibrium in (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 is likely to provide improved estimates of the wage-fatal risk tradeoff over OLS estimates of equation (1).

The random-effects model differs from the OLS model in (1) by specifying components of the overall error as $u_{ijkt} = \mu_i + \upsilon_{ijkt}$, where μ_i is person-specific and time-invariant unobserved heterogeneity, and υ_{ijkt} is an independently and identically distributed random error component. The random-effects estimator is a weighted average of the between-groups variation and the within-groups variation.

Consistency of the random-effects estimator requires $E[\mu_i \mid \pi_{jkt}, X_{ijkt}] = 0$ and $E[\upsilon_{ijkt} \mid \pi_{jkt}, X_{ijkt}] = 0$. The first condition implies that the time-invariant unobserved heterogeneity is randomly distributed in the population. 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.

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 IV first-differences and double-differences estimators in equations (2) and (3) and the IV 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 (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 and pins down *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. 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 2,106 men and 7,931 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 indicators for region of country (northeast, north central, and west with south the omitted region), race (white = 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

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[6] Ziliak and Kniesner (1998) show that if nonrandom attrition is present our differenced data models should sweep it out along with the other time-invariant factors.

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.[7]

The CFOI provides the most comprehensive inventory to date of all work-related fatalities. The CFOI data come from reports by the Occupational Safety and Health Administration, workers' compensation reports, death certificates, and medical examiner reports. In each case there is an examination of the records to determine that the fatality was in fact a job-related incident.

The underlying assumption in our analysis 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 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

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[7] 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.

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.[8]

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

[8] 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.

matching (Mellow and Sider 1983, Black and Kniesner 2003). Table 1 lists the means and standard deviations for both fatality risk measures. The sample mean fatality risk for the annual measure is 5.7/100,000. 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 fatal 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 cross-group variation exceeds within-group variation (Table 2), the within variation in our more disaggregate measures is sufficiently large (about 50 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 cross-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, one-digit occupation, and year effects. Because of the substantial heterogeneity of jobs in different occupations, the regressions include a set of one-digit occupation dummies. The equations do not include industry dummy variables as well because doing so would introduce substantial multicollinearity with respect to the fatality risk variable, which involves matching workers to fatality risk based on their industry and occupation. 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 variables that exhibit little variation across the time periods. Because few workers move out of state, we do not include a workers' compensation variable. Studies that have included workers' compensation generally use a variable based on the state's maximum benefit level, which exhibits little variation for our panel sample.

4.1 Focal Estimates from Panel Data

The baseline first-difference estimates from equation (2) appear in Table 3. The results are our basic 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 coefficient for the annual fatality rate in Table 3 using the sample mean wage of $21 is $6.1 million. We emphasize that a novel aspect of our research is that it helps clarify the size of possible measurement error effects. If measurement error in fatality risk is random it will attenuate coefficient estimates and should be reduced by letting the fatality rate encompass a wider interval. Compared to *VSL* from the more typical annual risk measure, the estimated *VSL* in Table 3 is about 20 percent larger when fatality risk is a three-year average. The last two columns of Table 3 report the results for widest possible differences ($\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 enlarges estimated *VSL*, and that even for the relatively basic panel models using differencing, the range for *VSL* is not large, \$5.8–\$7.6 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 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, this 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, 55 percent switch to lower fatality risk jobs and 45 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 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 cell. 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 6 times larger for switchers (see Table 2). There is too little within-cells variation to reveal much of a compensating differential. 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 important 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. 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 very narrow range of estimated *VSL*, \$5.6–\$5.7 million when we instrument the annual change in fatality risk.

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

[9] The instrument set we use is standard and well-established in the econometric literature on dynamic panel models and will not be discussed further here. The interested reader should consult Arellano (1989) for elaboration.

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 $7–8 million versus $10–11 million. Again, the range of *VSL* estimates is not wide when panel data are used with state-of-the-art estimators appropriate for the issues of endogeneity, measurement error, latent heterogeneity and possible state dependence.

4.2 Comparison Results From Cross-Section Estimators

Table 7 presents the comparison models, which flesh out the most salient econometric issues when compared to the focal 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 produce large implied *VSLs*, which also have a much numerically larger range than the panel estimates, $16–22 million.

In contrast, column 3 of Table 7 reports estimates from the panel random-effects estimator. 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; we find a similar rejection here. This implies that the simple fixed effects within estimator in the last column is preferred over the simple random effects estimator, with an estimated *VSL* of about $5.5 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 75 percent relative to a model that ignores latent heterogeneity (the pooled least squares estimates). The difference in estimated *VSL* with versus without latent individual heterogeneity in the model is consistent with the theoretical prediction 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.

5. Conclusion and Policy Implications

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 first-difference estimation results reported here use more refined fatality risk measures than employed in earlier studies, making it possible to control for measurement errors and workplace safety endogeneity 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 up to 75 percent and narrows greatly the *VSL* range to about \$5.5–\$7.5 million.

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 \$0.5 million to \$21 million. Previous studies using the Panel Study of Income Dynamics have often yielded extremely high *VSL* estimates around \$20 million. Earlier research did not control for the host of econometric problems we address here. The econometrically most general first-difference estimates we report range from \$5.5 million to \$7.5 million.

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 for \$0.5 to \$21 million. Moreover, there is always the issue of what studies should be included in the meta analysis given the differences in data sets, specifications, and study quality. As a consequence of the associated indeterminacies, 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 using Figure 1. Using *VSL* estimates from the previous literature, policies with a cost per life saved of \$500,000 or less are desirable, those with a cost per life saved over \$21 million fail a benefit-cost test, and the desirability of policies in the intermediate range is unclear. Based on our results, denoted by KVWZ, policies with a cost per life saved at or below \$5.5 million are in the acceptable range, those with a cost per life saved above \$7.5 million fail a benefit-cost test, and policies in the intermediate range have unclear economic desirability. For a hypothetical distribution of policies indicated by the bell shaped curve in Figure 1 with a mean *VSL* of \$10 million, it is clear that the range of indeterminacy is greatly reduced by application of our *VSL* range.

The implications of this hypothetical example are also borne out for the distribution of U.S. health and safety regulations. Using the widely cited estimates from the U.S. Office of Management and Budget cited by Breyer (1993), among others, and updating the values to \$2001, illustrates the tremendous 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, 25 that fail a benefit-cost test, with only 1 policy 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 methodology 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	21.058	13.352
Log Real Hourly Wage	2.881	0.570
Age	40.895	8.450
Marital Status (1=Married)	0.820	0.384
Race (1=White)	0.764	0.425
Union (1=member)	0.230	0.421
Years of Schooling	13.585	2.216
Live in Northeast	0.177	0.382
Live in Northcentral	0.288	0.453
Live in South	0.372	0.483
Live in West	0.163	0.370
One-Digit Industry Groups:		
Mining	0.008	0.087
Construction	0.106	0.308
Manufacturing	0.259	0.438
Transportation and Public Utilities	0.109	0.311
Wholesale and Retail Trade	0.130	0.337
Fire, Insurance, and Real Estate	0.045	0.208
Business and Repair Services	0.066	0.248
Personal Services	0.009	0.097
Entertainment and Professional Services	0.169	0.375
Public Administration	0.098	0.297
One-Digit Occupation Groups:		
Executive and Managerial	0.187	0.390
Professional	0.162	0.368
Technicians	0.058	0.234
Sales	0.032	0.177
Administrative Support	0.066	0.248
Services	0.086	0.280
Precision Production Crafts	0.207	0.405
Machine Operators	0.078	0.268
Transportation	0.080	0.272
Handlers and Labors	0.045	0.208
Annual Fatality Rate (per 100,000)	5.704	8.973
3-Year Fatality Rate (per 100,000)	5.565	8.414
Number of Men = 2,106		
Number of Person Years = 7,931		

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)	80.519	52.484	28.035
3-Year Fatality Rate (per 100,000)	70.801	50.298	20.503
Never Change Industry-Occupation			
Annual Fatality Rate (per 100,000)	75.696	70.032	5.664
3-Year Fatality Rate (per 100,000)	71.667	69.452	2.215
Ever Change Industry-Occupation			
Annual Fatality Rate (per 100,000)	82.574	45.031	37.543
3-Year Fatality Rate (per 100,000)	70.439	42.164	28.275
Only When Change Industry-Occupation			
Annual Fatality Rate (per 100,000)	88.309	53.274	35.035
3-Year Fatality Rate (per 100,000)	71.669	49.001	22.668

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

	Original Static First Difference Estimates	First-Difference Estimator for 2001minus1993	Difference in Differences Estimator
Annual Fatality Rate x 1,000	1.4425	1.6646	1.5553
	(0.4175)	(1.3584)	(0.5091)
Implied *VSL* ($Millions)	6.1	7.0	6.6
3-Year Fatality Rate x 1,000	1.7531	1.3834	1.7979
	(0.5276)	(1.4344)	(0.6142)
Implied *VSL* ($Millions)	7.4	5.8	7.6
Number of Observations	5242	1255	3373

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

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	0.3306	-0.1188
	(1.2132)	(2.8783)
Implied *VSL* ($Millions)	1.4	–0.5
3-Year Fatality Rate x 1,000	-0.5653	2.1041
	(2.2522)	(3.9626)
Implied *VSL* ($Millions)	–2.4	8.9
Number of Person-Years	1493	330
Ever Change Industry-Occupation		
Annual Fatality Rate x 1,000	1.5483	1.9423
	(0.4473)	(1.4353)
Implied *VSL* ($Millions)	6.5	8.2
3-Year Fatality Rate x 1,000	1.8660	1.4322
	(0.5352)	(1.5141)
Implied *VSL* ($Millions)	7.9	6.0
Number of Person-Years	3749	925
Only When Change Industry-Occupation		
Annual Fatality Rate x 1,000	1.7252	1.7662
	(0.4996)	(1.4580)
Implied *VSL* ($Millions)	7.3	7.4
3-Year Fatality Rate x 1,000	2.0045	1.3121
	(0.5604)	(1.5303)
Implied *VSL* ($Millions)	8.4	5.5
Number of Person-Years	1033	745

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

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

	First-Difference IV Estimator, t–1 and t–3 Fatality as Instruments	First-Difference IV Estimator, Lag Differenced Fatality as Instrument
Annual Fatality Rate x 1,000	1.3377 (0.6676)	1.3417 (0.6677)
Implied *VSL* ($Millions)	5.6	5.7
First Stage Results		
t–1 fatality rate	0.6528 (0.0114)	
t–3 fatality rate	–0.6512 (0.0113)	
(t–1 rate) – (t–3 rate)		0.6520 (0.0103)
R^2	0.54	0.54
Number of Observations	5242	5242

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, and year effects. First stage regressions include all exogenous explanatory variables in addition to the noted instruments. To construct the *VSL* using equation (5) the coefficients in the table are divided by 1,000.

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

	Dynamic First-Difference Estimates with lag wage instrumented	
	Short-Run Effect	Long-Run Effect
Annual Fatality Rate x 1,000	1.7583	2.4825
	(0.5390)	[0.0024]
Implied *VSL* ($Millions)	7.4	10.5
3-Year Fatality Rate x 1,000	1.8154	2.5623
	(0.6629)	[0.0088]
Implied *VSL* ($Millions)	7.6	10.8
Number of Observations	3373	

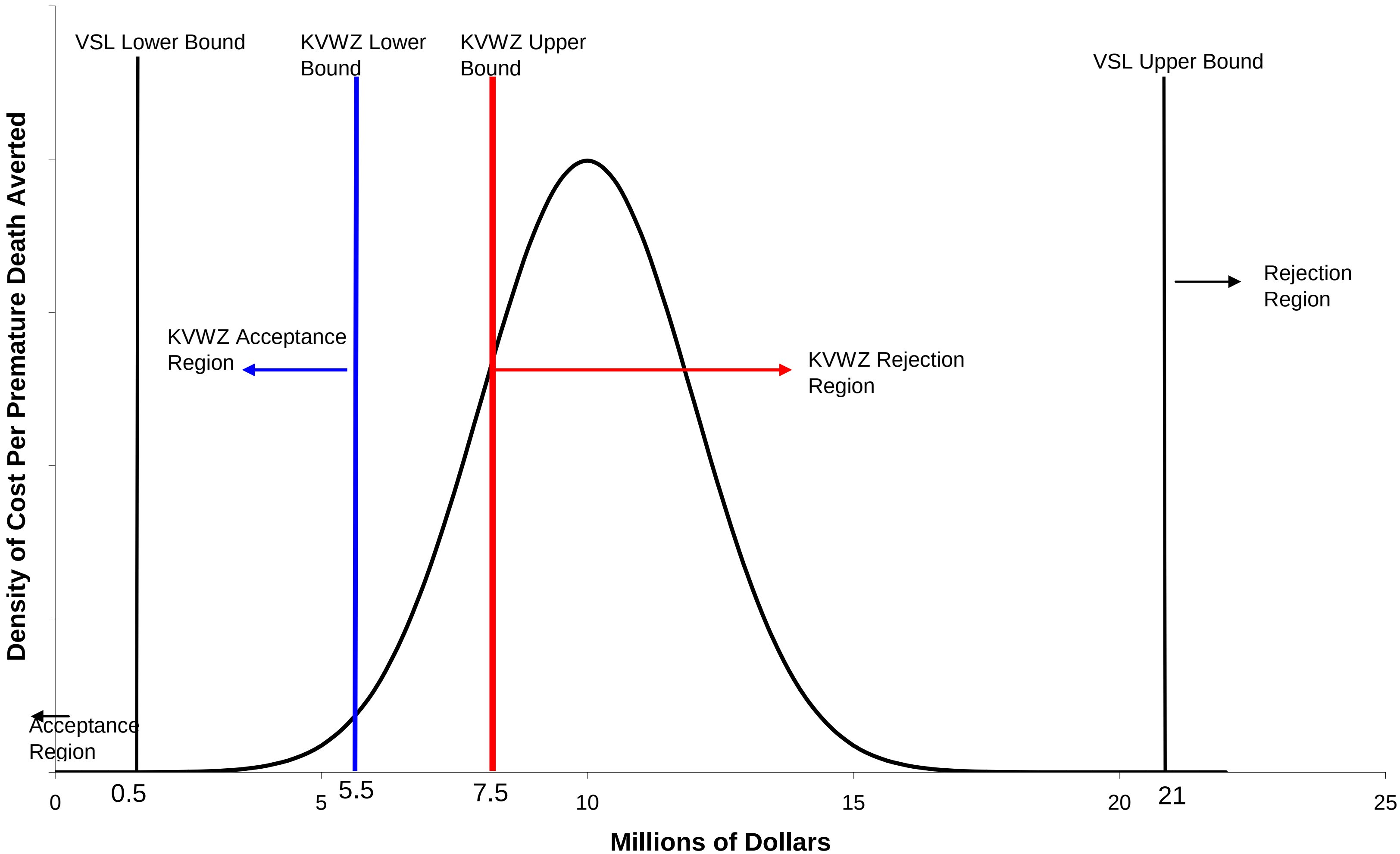
Notes: Standard errors are recorded in parentheses and *p*-values of the null hypothesis that the long-run effect is zero are recorded in square brackets. Standard errors are robust to heteroskedasticity and within industry-by-occupation autocorrelation. Models control for a quadratic in age, years of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

Table 7: 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	3.8702	5.2443	1.7401	1.2498
	(0.9972)	(1.5944)	(0.5185)	(0.5382)
Implied *VSL* ($Millions)	16.3	22.1	7.3	5.3
3-Year Fatality Rate x 1,000	4.3338	5.0506	2.0445	1.3352
	(1.0316)	(1.5811)	(0.6074)	(0.6452)
Implied *VSL* ($Millions)	18.3	21.3	8.6	5.6
Number of Observations	7928	2106	7928	7737

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

Figure 1: VSL Range and Program Evaluation



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