Panel Econometrics of Labor Market Outcomes

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Abstract

We survey some of the key problems confronting empirical applications in labor economics and how panel data can be utilized to robustly estimate parameters of economic interest. Topics include whether and how to introduce heterogeneity in intercept and slope parameters; measurement errors in regressors; endogeneity bias and associated panel instrumental variables estimators; sample composition dynamics to control for selection on (un)observables; and model specification and selection issues such as a static or dynamic framework. The pedagogical framework for our discussion is the Mincer-type human-capital wage equation used in quantifying the value of statistical life (VSL) in labor market data. We show in our application that whether we control for latent intercept heterogeneity is more important than how, and conditional on latent heterogeneity, the robust range of VSL estimates is in the $6 million - $10 million range when we additionally address measurement error, endogeneity, and dynamics.

Over the past four decades, advances in panel data econometrics have been intertwined with advances in labor economics. The tight link surely owes in part to the early availability of household panel data such as the Panel Study of Income Dynamics (PSID), the National Longitudinal Survey (NLS), and the negative income tax experiments. But it also stems from the fact the panel data offer numerous benefits for labor market research in terms of economically and econometrically richer models. This led to seminal research on life-cycle models of labor supply that developed new panel methods for separating state dependence from unobserved heterogeneity, allowing endogenous wages (Heckman 1978; MaCurdy 1981), considering earnings dynamics with growth rate heterogeneity and other autocorrelation processes (Lillard and Weiss 1979; MaCurdy 1982), modeling human capital investments that yielded new approaches to controlling for latent ability (Hausman and Taylor 1981), and to considering labor market interventions that in turn yielded new techniques for the evaluation of programs (Heckman and Robb 1985). The use of panel data also come with additional complications for labor market research that in turn led to advances in panel econometrics, such as how to control for attrition (Hausman and Wise 1979) and measurement error (Griliches and Hausman 1986). In our chapter we cover some of the most commonly encountered issues in panel-data applications in labor economics.

Specifically, our interests here include whether and how to introduce heterogeneity in intercept and slope parameters; measurement errors in regressors; endogeneity bias and associated panel instrumental variables estimators; sample composition dynamics to control for selection on (un)observables; and model specification and selection issues such as a static or dynamic framework. The formal econometric theory behind many of the topics we cover appears elsewhere (Baltagi (2008) and other chapters of the handbook (Chapters 1, 4, 8)). We also do not cover other important labor-related topics in our brief survey, such as program evaluation and limited-dependent variable models (Chapters 9 and 11). Our aim is instead to demonstrate how the panel-data techniques labor economists use manifest themselves in an applied setting, specifically the canonical hedonic labor-market equilibrium model of the wage-fatal risk tradeoff (Thaler and Rosen 1975).

The pedagogical framework of labor-market wage-fatal risk setting is of interest for several reasons. First, the empirical framework builds on the standard Mincer wage equation used in scores of papers on inequality, returns to schooling and experience, and race and gender discrimination. In the case we consider the Mincer model is augmented with a proxy for the risk of a fatal injury on the job, which is the central parameter that governs estimates of the so-called value of statistical life (VSL) or value of mortality risk reduction (VMRR) (Viscusi 2013 forthcoming). That is, the VSL captures a worker's willingness to trade off wages for a small probability of death on the job and, as such, is used in a variety of settings to assess the cost effectiveness of health and safety programs. So, for example, a group of workers each requiring a $700 annual wage premium to face an extra 1 in 10,000 probability of death on the job would have a VSL of $7 million (700/0.0001). A second reason for focusing on the compensating wage model is that obtaining reliable estimates of the VSL has long been challenging owing to the central roles of latent individual heterogeneity that is correlated with the regressors and state dependence which affects both the market offer curve and individual preferences. Perhaps surprising, with the lone exception of Brown (1980), the VSL literature has not until recently used panel data techniques to control for the variety of econometric problems in estimation (Kniesner, Viscusi, and Ziliak 2010; Kniesner, et al. 2012). A third reason to focus on the wage-fatal risk model is that the wide variation of VSL estimates in the literature, which spanned from

$0 to $20 million (Viscusi and Aldy 2003; Viscusi 2013, forthcoming), has generated concern that underlying econometric problems may jeopardize the validity of the estimates.

We begin by demonstrating the critical importance of controlling for latent intercept heterogeneity to estimates of the VSL by comparing results from pooled OLS and between-groups estimators to estimates obtained from standard random and fixed effects estimators. Here we find a real payoff to panel data in that the estimated VSL falls by 75 percent once allowing for intercept heterogeneity. But we also obtain the surprising result in our focal application that how one controls for intercept heterogeneity matters little. Typically random effects are statistically rejected in favor of fixed effects, and the same is true in our application, but economically there is little distinction in VSL estimates across fixed and random effects models, which is not necessarily a standard result in labor. We then examine how the VSL varies across the wage distribution by using some relatively new quantile estimators for panel data (Koenker 2004; Lamarche 2010), showing how there is nontrivial slope heterogeneity in the VSL.

Once we develop some basics of heterogeneity we next explore possible solutions to measurement error in the panel setting. Our emphasis here is on mismeasured covariates and, in particular, in our measure of fatality risk. Because on-the-job fatalities are relatively rare in many industries and occupations, there are possible concerns that measurement error may bias downward estimates of the VSL (Black and Kniesner 2003). Panel data offer several possible solutions, including averaging across several periods as is common in the intergenerational mobility literature (Solon 1992), taking wider differences to raise the signal to noise (Griliches and Hausman 1986), or instrumental variables (Baltagi 1981, 2008). We find that measurement error (attenuation) bias is pervasive in our application, and tends to bias estimates of the VSL downward by around 25 percent. This is fairly consistent across techniques.

More generally, labor economists confront the challenge of endogenous regressors, and again panel data offer a wider array of possible solutions than is available in cross-sectional or repeated cross-sectional data. With few exceptions, most research in labor relies on exclusion restrictions to identify endogenous variables. For example, early work on cross-sectional labor supply would often use nonlinearities and interaction terms to identify the endogenous wage rate (Pencavel 1986); that is, if age and education affect hours of work, then age squared and age times education would be included in the reduced-form wage equation. In the last two decades labor economists have considered the so-called natural experiment approach to identifying endogenous regressors, exploiting some exogenous variation in policy that is external to the model (Card 1990; Card and Krueger 1994). Panel data admit both approaches and permit additional instruments by imposing structure on the time-series process (MaCurdy 1982) or the latent heterogeneity (Hausman and Taylor 1981; Cornwell, Schmidt, and Wyhowski 1992; Keane and Runkle 1992). Estimates of the VSL are fairly robust to several of the alternative approaches.

Missing data in the form of item nonresponse, wave nonresponse, and panel nonresponse also pervades panel data research in labor. This leads to unbalanced panels where the key issue is whether the unbalanced design imparts any bias on labor market parameters such as the risk of fatality. That is, determining whether the data are missing completely at random, missing conditionally at random, or missing non-randomly are key to consistency of model estimates (Baltagi and Chang 1994, 2000;Verbeek and Nijman 1992; Ziliak and Kniesner 1998; Baltagi and Song 2006; Wooldridge 2007). Few, if any, applications in labor satisfy the missing completely at random assumption, but the missing conditionally at random is more widely applicable with robust controls for selection on observables (Fitzgerald, Gottschalk, and Moffitt

1998). Ziliak and Kniesner (1998) find that modeling the missing non-randomly process as a person-specific fixed effect is adequate for males' life-cycle labor supply estimates, and the addition of a selection correction based on the idiosyncratic time-varying error adds little. This implies that the decision to attrite is time invariant, and a similar result holds in our VSL application here, suggesting that at least for some labor outcomes, controlling for latent heterogeneity is sufficient for nonrandom attrition.

Panel data offer opportunities to expand the economic framework from the static setting to the life cycle. Moreover, within the context of the life-cycle model, additional model richness can be explored such as time nonseparability in preferences owing to state dependence (Hotz, Kydland, and Sedlacek 1988), or nonseparability in budgets owing to endogenous human capital (Shaw 1989) or tax policy (Ziliak and Kniesner 1999). Related, there has been an explosion of research on the econometrics of dynamic panels (Arellano and Bond 1991; Chapters 4 and 14 (this volume)). We conclude our chapter with a summary of what research that panel data have facilitated in the important area of wage outcomes. Among other things, we note how estimates of the VSL differ in the short run versus the long run when we attempt to separate state dependence from latent heterogeneity. We find that the long-run parameter is about 10-15 percent higher than the short-run VSL, suggesting that the adjustment to labor-market equilibrium in wages is relatively quick.

II. Economic Framework

The organizing economic framework for our chapter on panel-data labor econometrics is based on the compensating differences model of the wage-fatal injury risk tradeoff. The most common motivation for this hedonic model is a desire to identify society's willingness to pay for alternative policy effects, such as the expected number of lives saved from a government policy or regulation. The standard measure used to capture the willingness-to-pay value is the tradeoff rate between money and fatal injury risks—higher risk jobs are compensated with higher wages—or what is known as the value of statistical life (VSL). Because there is great heterogeneity in the risk of injury across jobs, the most common method of eliciting the VSL is via labor-market studies that estimate the effect of fatal injuries on hourly wage rates, or

$$lnw_i = \alpha + \beta\pi_i + X_i\gamma + u_i, \tag{1}$$

where $lnw_i$is the natural log of the hourly wage rate of worker i, π_i is the measure of labor-market risk of fatal injury on the job, X_i is a vector of demographic controls found in the typical Mincer wage equation such as education, race, age, and marital status, and u_i is an idiosyncratic error term.

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. This implies that the focal parameter of interest is β as this summarizes the wage-fatal risk tradeoff and is central to the VSL, defined as

$$VSL_i = \frac{\partial w_i}{\partial \pi_i} = 100{,}000 \times \beta \times w_i \times h_i \tag{2}$$

where the expression in equation (2) accounts for the fact that the model in equation (1) is estimated in log wages, but the VSL is expressed in dollars and thus β is multiplied by the hourly wage. In addition the fatality rate π_i is generally measured in terms of the number of fatalities per 100,000 workers and thus we need to scale the VSL up by 100,000. Lastly, because the wage rate is measured as wages per hour of work, to arrive at an annual estimate of the VSL we multiply by annual hours of work, h_i. Even though the rate at which wages are traded for risk is constant across workers in equation (1) via a common β, the VSL can vary across workers as the expression in (2) can be evaluated at different points in the wages and hours distributions. In our examples below we follow most studies and report only the effect at mean wages and a fixed hours point of 2000. Viscusi and Aldy (2003) provide a comprehensive review of estimates of the VSL based on cross-sectional data. Our primary objective in this chapter is to examine how following systematic econometric practices for panel data models refines the estimated range of VSL.

III. Heterogeneous Intercepts and Slopes

The most widely stated advantage of panel data in labor economics is that it permits the introduction of latent heterogeneity in intercepts; that is, with data on the same worker *i* (*i*=1,…,N) for multiple time periods *t* (*t*=1,…,T) we can rewrite the hedonic model as

$$lnw_{it} = \alpha_i^+ + \alpha_i^- + \beta\pi_{it} + X_{it}\gamma + u_{it}, \quad (3)$$

where equation (3) contains two latent individual effects: one that is positively correlated with wages and the fatality rate (α_i^+) and one that is positively correlated with wages and negatively correlated with the fatality rate (α_i^-).

The first 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; the second individual effect reflects unmeasured job productivity that leads more productive/higher wage workers to take safer jobs.[1] The economic interpretation of $\alpha_i$in the Mincer-type wage model for returns-to-schooling is that it captures pre-labor market factors that are fixed over time such as unobserved labor-market skill that reflects nature (genetic factors based down through families), nurture (environmental factors determined by familial and social forces), and the possible interaction of nature and nurture. One of the factors in the schooling model is the usual

[1] We note that as an alternative to individual-level panel data we may instead follow cohorts over time. See, for example, Deaton (1985) for a discussion on the merits and restrictions of pseudo panel (repeated cross-sections) data.

latent person-specific cognitive ability that makes the person have a higher wage rate and go to school more, the other is a trait such as beauty or likability that makes the person have a higher wage rate but go to school less because one does not need as much investment (Heckman, et al. 2008). In a life-cycle model the latent heterogeneity will also embed the marginal utility of initial wealth (MaCurdy 1981). For the remainder of the analysis we suppress the $+/-$ distinction on the latent heterogeneity.

If we impose the following zero conditional mean assumptions A.1 and A.2

A.1 $E[u_{it}|\alpha_i, \pi_{it}, X_{it}] = 0$

A.2 $E[\alpha_i|\pi_{it}, X_{it}] = 0$

then OLS estimation of the hedonic equilibrium in equation (3) using pooled cross-section time-series data is consistent. The standard errors are serially correlated because of the presence of α_i and need to be clustered to account for repeated observations of the same individual. An alternative to pooled OLS under the same assumptions is the between-groups estimator, which as we discuss below in the section on measurement error is potentially useful in mitigating attenuation bias as the averages smooth out idiosyncratic noise in any given time period.

[Table 1 here]

In the first two columns of Table 1 we report the results of pooled OLS and between groups estimates of the fatality rate coefficient, $\hat{\beta}$, along with the implied VSL evaluated at the mean wage of $21 and 2,000 hours of work (Kniesner, et al. 2012). The data are of prime-age working men from the 1993-2002 Panel Study of Income Dynamics (PSID). The panel is unbalanced and in later sections we discuss the role of possible nonrandom nonresponse.[2] Both

[2] In the wage-hedonic literature the individual's risk exposure is proxied with the fatal risk on the job. As risk information is not collected at the person level, the literature has historically used the publically released fatality rates by one or two-digit industry. Kniesner, et al. (2012) obtained proprietary data from the Census of Fatal Occupational Industries to construct a measure of fatality risk that varies by 72 two-digit industries and 10 one-digit

estimates reveal that there is economically and statistically strong evidence of a wage-fatal risk tradeoff. The pooled OLS model shows that the implied VSL is just over $15 million, while the between-groups estimate suggests it is closer to $26 million, perhaps indicating significant attenuation bias. The estimates of the VSL are within the range of cross-sectional results summarized in Viscusi and Aldy (2003).

A. Intercept Heterogeneity Models

The pooled OLS and between groups estimators ignore the latent heterogeneity in equation (3), and thus under assumptions A.1 and A.2 a more efficient estimator is possible with the random-effects estimator. The random-effects model is a GLS estimator by explicitly including the latent heterogeneity term α_i in the model's error structure to account for autocorrelation, but is similar to OLS in that the additional source of error is treated as exogenous to the regressors. The random effects structure is attractive for several reasons, including that it permits inference to the general population and it preserves identification of time-invariant regressors. For example, race is a typical covariate in Mincer wage models, and once there are controls for other observed factors of productivity, the race coefficient reflects the unexplained racial wage gap. Under random effects we can tighten the estimate of the racial gap by inclusion of (random) unobserved productivity. However, the implication of assumption A.2 for our application to the hedonic wage model 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. Such random sorting is unlikely to hold in the data, and in most labor-

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occupations, yielding a potential of 720 different fatal risk outcomes. Each regression model controls for a quadratic in age, years of schooling, indicators for marital status, union status, race, one-digit occupation, two-digit industry, region, state, and year. Standard errors are clustered by industry and occupation and are also robust to the relevant heteroskedasticity.

market applications the data reject the assumption of zero correlation between unobserved heterogeneity.

Instead, most work in labor economics relaxes A.2 to allow correlation between the latent heterogeneity and the regressors,

A.2' $E[\alpha_i|\pi_{it}, X_{it}] \neq 0$,

yielding the fixed-effects estimator, whether in the form of the least-squares dummy variable model, the within estimator, the first-difference estimator, or the orthogonal deviations estimator.

The least-squares dummy variable model is convenient when there are only a limited number of cross-sectional units; for example, in U.S. state panel-data models we need only include 50 dummy variables for each state, but the dummy variable method becomes impractical in most household panel surveys with large N. Most often the decision confronting the labor economist is whether to apply the within or first difference transformation in equation (3). The two estimators yield identical results when there are two time periods and when the number of periods converges towards infinity. When there is a finite number of periods ($T > 2$), estimates from the two different fixed-effects estimators can diverge due to possible non-stationarity, measurement errors, or model misspecification (Wooldridge 2010). Because wages, hours, and consumption from longitudinal data on individuals have been shown to be non-stationary in other contexts (Abowd and Card 1989; MaCurdy 2007), in our application to the hedonic wage model we adopt the first-difference model. However, first-differences comes at a cost of fewer observations than the within estimator because the first time period must be dropped for each cross-sectional unit, and as we discuss below, it may exacerbate measurement errors.

Lillard and Weiss (1979) demonstrated that earnings functions may not only have idiosyncratic differences in levels but also have idiosyncratic differences in growth. A straightforward approach to modeling growth rate heterogeneity is to put an idiosyncratic factor loading on a linear trend

$$lnw_{it} = \alpha_i + \beta\pi_{it} + X_{it}\gamma + \delta_i t + u_{it}, \tag{4}$$

where δ_i reflects person-specific growth in wages. Lillard and Weiss (1979) treated the growth heterogeneity as random in their error-components model, but a more robust approach is to modify assumption A.2' as

A.2" $E[\alpha_i, \delta_i|\pi_{it}, X_{it}] \neq 0$.

Notice that first differencing equation (4) still leaves $\delta_i(= \delta_i t - \delta_i(t-1))$. In models with limited cross sections it is possible to add dummy variables akin to the least squares dummy variable approach to control for latent growth heterogeneity (this is common among applications using 50-state panel data), but again in large panels it is impractical. Instead one can double difference the model as

$$\Delta^2 lnw_{it} = \beta\Delta^2\pi_{it} + \Delta^2 X_{it}\gamma + \Delta^2 u_{it}, \tag{5}$$

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

In columns (3)-(5) of Table 1 we report estimates of the fatal risk parameter and the corresponding estimate of the VSL for the random-effects estimator, the first-difference estimator, and the double-difference estimator. A formal Breusch-Pagan test rejects the null hypothesis of common intercepts as assumed in the pooled OLS and between-groups estimators, and the rejection has significant economic implications in our application—the VSL from random effects is 60 percent lower than the pooled OLS estimate, and 75 percent lower than the between-groups estimate. In many labor-market applications there is not such a divergence between the OLS and random effects estimate as here, but it underscores the importance of testing for heterogeneous effects and focusing attention on the economic consequences of the homogeneity assumption.

Although a formal Hausman test rejects random effects in favor of fixed effects, comparing columns (3) and (4) shows that the estimated VSL is fairly robust to the random effects assumption. Again, formal testing can determine the statistical and economic consequences of seemingly innocuous assumptions. In the final column we see that the estimated VSL is robust to growth heterogeneity as the difference-in-difference estimate of the VSL is comparable to the random effects and first difference models.

The takeaway of our empirical example is that controlling for latent intercept heterogeneity is crucial to produce more accurate estimates of the VSL, but how one controls for the latent heterogeneity is less important. Although we believe the former result of the primacy of controlling for latent heterogeneity pervades most applications in labor economics, the latter result that the form of the heterogeneity does not matter economically is perhaps more unique to the VSL application but underscores the importance of estimating alternative models in practice.

B. Intercept and Slope Heterogeneity Models

Models with marginal effect heterogeneity where regression coefficients are individual specific fixed or random effects are well known (Swamy 1971, Chapter 12 this volume), but until recently have been much less widely adopted in practice.[3] Here our exploration of the heterogeneity of regression parameters considers slope differences using a recently developed model of panel quantile regression (Koenker 2004; Lamarche 2010). We demonstrate the applicability of the estimator to an important policy question, which is how VSL might be

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[3] Slope heterogeneity is distinct from more ubiquitous parametric forms of heterogeneity obtained by interacting a parameter of interest with other variables, often demographic factors. For example, Aldy and Viscusi (2008) interact the fatal risk variable with the worker's age to estimate how the VSL varies over the life cycle, while Kniesner, et al. (2006) show how the VSL varies with the age profile of life-cycle consumption and emphasize the need for panel data.

indexed for income growth (Kniesner, Viscusi and Ziliak 2010). Specifically, because the VSL from a quantile wage regression varies with the potential wage ($\hat{w}$), it admits the possibility that the VSL varies positively with income levels, which captures distributional issues not evident in the mean regression models discussed above. Even a simple comparison of the mean versus median VSL is instructive for safety policy where the VSL is a benefit comparison point for evaluating life-saving programs with different cost levels. In particular, using the median program benefit instead of the mean benefit as a cutoff value ensures that a majority of the affected population will benefit from the program.

In a quantile regression model one no longer has a single parameter vector to estimate but rather a parameter vector for each τ_j^th^ quantile, or

$$Q_{lnw_{it}}(\tau_j|\pi_{it}, X_{it}, \alpha_i) = \alpha_i(\tau_j) + \beta(\tau_j)\pi_{it} + X_{it}\gamma(\tau_j). \qquad (6)$$

With a finite number of time periods it is general practice to assume that the latent heterogeneity is common to all the conditional quantiles of the regression outcomes, $\alpha_i(\tau_j) = \alpha_i \;\forall\; \tau_j$. However, even with such quantile invariant intercept heterogeneity the panel quantile model is computationally intensive if there are a large number of cross-sectional units.

Koenker (2004) and Lamarche (2010) offer an innovative solution to the panel quantile version of the incidental parameters problem by proposing a shrinkage estimator wherein a tuning parameter controls the degree of inter-person intercept differences. The tuning parameter, which if fixed ex ante, allows one to vary the degree of heterogeneity from no individual heterogeneity to individual heterogeneity modeled as fixed effects. As an alternative one can solve for the tuning parameter optimally by minimizing the trace of the covariance matrix when estimating the tuning parameter and thus implicitly permit the data to determine the extent of latent heterogeneity.

Let us write the minimization problem as

$$argmin(\alpha,\beta,\gamma)\sum_{j=1}^{J}\sum_{t=1}^{T}\sum_{i=1}^{N}\omega_{\tau_j}\rho_{\tau_j}(lnw_{it}-\alpha_i-\beta(\tau_j)\pi_{it}-X_{it}\gamma(\tau_j))+\lambda\sum_{i=1}^{N}|\alpha_i| \quad (7)$$

where ω_{τ_j} is the relative weight of the j^{th} quantile, $\rho_{\tau_j}(u)=u(\tau_j-I(u\leq 0))$ is the quantile loss function, and J is the number of quantiles that are estimated simultaneously. The tuning parameter regulates the influence of the latent heterogeneity on the quantile functional. In the case where $\lambda = 0$ the fixed effects estimator emerges and for the case where $\lambda > 0$ a penalized (shrinkage) fixed-effects estimator appears (Lamarche 2010). Note that in solving for the unknown parameters in equation (7) there are two kinds of heterogeneity: intercept heterogeneity, whereby the wage equation intercepts vary with the person indicator (*i*); and slope heterogeneity, whereby the curvature of the hedonic locus varies with τ to reflect both latent worker and firm differences in risk tolerance and cost functions.

[Table 2 here]

Table 2 presents quantile regression estimates of the fatal injury risk for no latent intercept heterogeneity and the regression for $\lambda = 1$, which minimized the trace of the variance-covariance matrix, where at the median quantile of 0.5 the estimated VSL is about $7.6 million, which is less than half that obtained at the same point of the wage distribution but without intercept heterogeneity.[4] Note that there is a sharp increase in the implied VSL at the 75th and 90th quantiles such that the VSL jumps to $14.6 million and $22 million, respectively. Again, though, the statistically preferred panel quantile estimates are less than half those obtained if we were to incorrectly ignore intercept heterogeneity, highlighting the crucial importance of panel data with models permitting greater heterogeneity.

[4] The implied VSL at the median is evaluated at the quantile-specific wage rate in conjunction with the quantile-specific estimate of the fatal risk parameter, $\hat{\beta}(0.5)$=0.0022.

IV. Measurement Error

Labor-market data reported in household surveys are noisy (Bound and Krueger 1991; Bound, et al. 2001). Problems of measurement error generally arise owing to misreporting by the respondent or interviewer, or possibly the respondent refusing to report information and the statistical agency using imputation methods to allocate values. Traditionally measurement error is addressed by using proxy variables or instrumental variables for continuous variables, but in the case of discrete regressors bounding techniques may be necessary (Bollinger 1996).

As detailed in Meijer, Spierdijk, and Wansbeek (Chapter 8), the standard panel data estimator generally exacerbates measurement error compared to the cross-sectional counterpart, and within the class of fixed effect estimators, the first-difference estimator tends to have a lower signal to noise ratio compared to the within estimator (Griliches and Hausman 1986). For example, in Table 1 we reported both cross-sectional and first difference estimates of the wage-fatal risk tradeoff, and likely part of the difference in the estimated VSLs across estimators is due to attenuation effects in the first-difference estimates.

At the same time that panel data create additional problems of measurement error, they also open up new avenues for correcting attenuation bias. For example, in Table 1 we see that the between-groups estimates of the VSL are nearly 70 percent higher than the corresponding pooled OLS estimates. Under assumptions A.1 and A.2 it is clear the advantage of constructing time means to mitigate measurement error. However, as we demonstrated earlier, the assumption of A.2 is rejected, leading us to panel estimators that admit latent intercept heterogeneity. But measurement error in the fatality risk variable also leads to a violation of assumption A.1 (Black and Kniesner 2003; Ashenfelter and Greenstone 2004b; Ashenfelter 2006).

Griliches and Hausman (1986) provide the seminal treatment of measurement error in panel data, and one transparent solution they suggest is to take wider differences. That is, instead of subtracting the (t-1) value of a variable, one instead could subtract the (t-2) or further lag of the variables. Indeed, Hahn, Hausman, and Kuersteiner (2007) suggest that it is optimal to take the widest possible difference available in the data. For example, with five years of data on wages the corresponding regression model would be

$$lnw_{it} - lnw_{it-4} = \beta(\pi_{it} - \pi_{it-4}) + (X_{it} - X_{it-4})\gamma + (u_{it} - u_{it-4}), \qquad (8)$$

which amounts to a simple cross-sectional regression. In results not tabulated, in our application we find that the estimated VSL is about 20 percent higher in the wide difference model compared to the first-difference estimate reported in Table 1.[5]

Another solution to the measurement error problem proposed by Griliches and Hausman (1986) is to exploit the iid assumption of the model's error term u_{it} and to use various combinations of lags of the endogenous regressors as instrumental variables. For example, in the first difference model it is possible to use interchangeably the (t–1) and (t–3) levels of the fatality risk, the (t–1) – (t–3) difference, the (t–2) and (t–3) levels and difference, and the (t–2) and (t–4) levels and difference. In Table 3 we report the results of various differenced instrumental variable models; the mode result is that the first-difference OLS estimates in Table 1 are attenuated by measurement error by about 20 percent compared to the first-difference IV estimates in Table 3.

[Table 3 here]

[5] Another solution to measurement error problems that is somewhat idiosyncratic to the hedonic model in Kniesner, et al. (2012) is to combine the ideas of the between-groups estimator with first (or wider) differences. Specifically, the fatality risk is measured at the industry and occupation level, and because this is exogenous to the individual, Kniesner, et al. compute 3-year moving averages of the fatality risk to include in the regression model. In the first difference model reported in Table 1 the use of 3-year averages results in an estimate of the VSL of $7.7 million, or about one-third higher than reported in Table 1.

V. Endogeneity

One of the most commonly encountered challenges in empirical labor economics is identification of model parameters in the presence of endogenous regressors. Access to panel data at once introduces additional opportunities for and complications to identification. For example, one opportunity with panel data as described in the last section on measurement error is access to lags of the endogenous regressor $(\pi_{t-1}, \pi_{t-2}, \ldots)$ in the case of our wage equation model with potentially endogenous fatality rate. A complication, though, is that to use lags of endogenous regressors one has to impose structure on the time-series process. For example, a common approach is to assume the time-varying error u_{it} is iid or MA(1); the iid case permits use of instruments dated (t-2) and earlier if the regressor is endogenous, and the MA(1) case permits use of instruments dated (t-3) and earlier under endogeneity. If u_{it} is an AR(1) process then lags of the endogenous regressor are ruled out as instruments.

A second complication arises from the fact that the choice of transformation to sweep out the latent heterogeneity is not innocuous. If the within transformation is used to eliminate the fixed effect then lags of endogenous (or predetermined for that matter) regressors are not valid instruments because they are correlated with the time mean of the error term. Instead, one must use the first-difference or orthogonal-deviations transformation, along with assumptions on the structural error term, or must have access to strictly exogenous instrumental variables such as policy reforms used in the natural experiment literature.

To formalize ideas we begin by re-writing the wage equation in matrix form as

$$W_i = D_i\Gamma + \varepsilon_i \tag{9}$$

where W_i is the $T \times 1$ vector of log wages for person i, $D_i = [\iota_T F_i', X_i]$ is the $T \times (G + P)$ matrix of regressors for person i in which ι_T is a $T \times 1$ vector of ones, F_i is a $G \times 1$ vector of time invariant regressors such as race, gender, and education attainment (assuming no students in the sample), X_i is a $T \times P$ vector of time-varying regressors including the fatality rate, Γ is a $(G + P) \times 1$ vector of unknown parameters to estimate, and $\varepsilon_i = \iota_T \alpha_i + u_i$ is the $T \times 1$ error component.

The most general treatment of the problem is by Arellano and Bover (1995), who proposed GMM estimation of (9) within the context of the correlated random effects setup of Hausman and Taylor (1981). The idea is to find a nonsingular transformation, C, and a matrix of instruments, M_i, such that the population moment conditions $E(M_i' C \varepsilon_i) = 0$ are satisfied. Arellano and Bover suggest the transformation

$$C = \binom{H}{\iota_T'/T}, \qquad (10)$$

where H is a $(T - 1) \times T$ matrix containing the first-difference, within, or orthogonal deviations operator, and ι_T'/T converts a variable into its time mean. Notice that H eliminates α_i from the first (T-1) rows, thus allowing the identification of the coefficients on time-varying regressors, while ι_T'/T creates an equation in levels (between-groups), and permits identification of the coefficients on time-invariant regressors. For the instruments, define the block-diagonal matrix $M_i = I_T \otimes [m_i', m_i', \dots, m_i', \widetilde{m}_i']$, where I_T is a $T \times T$ identity matrix, $m_i = (F_i, x_i)$ is a typical row from D_i , and $\widetilde{m}_i$ is a subset of m_i that is assumed to be uncorrelated in levels with α_i.

Stacking the observations across all i, the GMM estimator then is given as

$$\hat{\Gamma} = [D'\bar{C}'M\left(M'\bar{C}\hat{\Omega}\bar{C}'M\right)^{-1}M'\bar{C}D]^{-1}D'\bar{C}'M\left(M'\bar{C}\hat{\Omega}\bar{C}'M\right)^{-1}M'\bar{C}W, \qquad (11)$$

where $\bar{C} = I_N \otimes C$, with I_N an $N \times N$ identity matrix, and $\widehat{\Omega}$ is a conformable matrix $\hat{u}\hat{u}'$ with estimated residuals from a first-stage 2SLS regression.

A. Identification

The key to identification for correlated random effects is the choice of instruments that comprise $\widetilde{m}_i$. It is important to emphasize that, unlike standard instrumental variables, identification of the correlated random effects estimator generally does not come from exclusion restrictions outside of the system, but instead from inside the system via assumptions about correlation with α_i and u_i. Cornwell, Schmidt, and Wyhowski (1992) proposed a classification scheme where the time-varying X_i are decomposed as $[X_{1i}, X_{2i}, X_{3i}]$ and the time-invariant F_i as $[F_{1i}, F_{2i}, F_{3i}]$. X_{1i} and F_{1i} are called endogenous because they are correlated with both α_i and u_i (assumptions A.1 and A.2 do not hold), X_{2i} and F_{2i} are called singly exogenous because they are assumed to be correlated with α_i but not u_i (assumption A.1 holds but not A.2), and X_{3i} and F_{3i} are called doubly exogenous as they are assumed to be uncorrelated with both α_i and u_i (both assumptions A.1 and A.2 hold). It is the doubly exogenous X_3 that are critical for identification of the endogenous F_1; that is, identification requires the number of time-varying doubly exogenous variables (X_3) to be at least as large as the number of time-invariant endogenous variables (F_1).

Hausman and Taylor (1981) suggest one possibility for $m_i = [\bar{x}_{3i}, F_{3i}]$, where $\bar{x}_{3i}$ is the individual time-mean of the doubly exogenous X's. In addition, leads and lags of the singly and doubly exogenous time-varying variables (X_2 and X_3) can be used to identify the endogenous time-varying X_1, as can lags of the endogenous X_1 depending on the time series properties of u_i. Amemiya and MaCurdy (1986) and Breusch, Mizon, and Schmidt (1989) suggest additional instruments for the correlated random effects model.

A number of important cases obtain from the general endogeneity framework we have been discussing. First, the typical panel application in labor economics eschews identification of the coefficients on time-invariant variables and thus F_i is dropped from the analysis. This in turn implies that the transformation in equation (10) is now reduced to H because the time-mean transform ι_T'/T is no longer needed, and demands for identification are relaxed as there is no longer a need for doubly exogenous X_3. Second, it is also typical to assume that all time-varying regressors are singly exogenous, implying that even if there was an interest in including time invariant variables the F_1 and F_2 cannot be identified unless rather stringent assumptions are imposed or there are instruments available outside the system (Breusch, et al. 1989). An example of the outside instruments case is Ziliak (2003), who used state-by-year variation in the generosity of a state's welfare program to identify the effect endogenous (time-invariant) welfare income on asset holdings. Third, it is common that the structural model of interest contains at least one endogenous X_1. Identification in the one or more endogenous components of X_1 case rests on time-series properties of the structural error. If $u_i \sim iid(0, \sigma^2)$ and H is a first-difference transformation then x_{1t-2}, x_{1t-3},… are valid instrumental variables. If u_i is MA(1) then x_{1t-3} and earlier are valid instruments. In the iid case the panel length must be at least 3 periods, in the MA(1) case the panel length must be at least 4 periods, so that the time dimension increases in importance as the researcher introduces more flexibility into the error process.

For example, Kniesner, et al. (2012) assumed that all regressors were singly exogenous, but they were also concerned that the fatality rate in equation (3) might be endogenous if there are idiosyncratic preferences for on-the-job risk correlated with wages beyond the fixed effect. They assumed that the time-varying error u_i was *iid*, and thus after first differencing the fatality rate at (t-2) was a valid instrument for identification. Indeed, as they were concerned about endogeneity emanating from measurement error as well as the more general simultaneity bias, they used the results of Griliches and Hausman (1986) and considered a number of alternative instrument sets such as the (t-1) and (t-3) levels of the fatality risk, the (t-1) - (t-3) difference, the (t-2) and (t-3) levels and difference, and the (t-2) and (t-4) levels and difference. As noted earlier in our discussion of IV as a treatment for measurement error bias, the main result of such regressions is a fairly narrow range for the estimated VSL, approximately $6 million to $8.5 million, though the confidence intervals widen as is typical in IV models compared to OLS.

An obvious question arises now. Which set of IV results are most preferred? That is, it is fairly standard to estimate a variety of specifications and examine how sensitive the key parameters of interest are to alternative identification assumptions. Deciding which models to emphasize are guided first and foremost by the theory underlying the economic model. Theory is then supplemented with model specification tests such as the first-stage R^2 (or partial R^2 with multiple endogenous regressors) of instrument correlation (Shea 1997; Stock et al. 2007), and the Sargan-Hansen test of overidentifying restrictions (Hansen 1982). Moreover, in the GMM framework of Arellano and Bover (1995), one can employ pseudo likelihood ratio tests comparing unrestricted to restricted instrument sets to test the validity of instruments (Newey and West 1987), and test the assumptions of singly and doubly exogenous variables.

B. State Dependence and Dynamic Panels

Over the past couple of decades the panel econometrics literature has devoted significant attention to a particular form of endogeneity in the form of lagged dependent variables (Anderson and Hsiao 1982; Arellano and Bond 1991; Kiviet 1995; Blundell and Bond 1998). In labor economics dynamic models arise in a variety of situations, such as habit persistence in preferences over consumption and hours of work choices, in explicit and implicit labor contracts that cause wages to change only sluggishly to changing economic conditions, or in employers tagging workers as potentially risky if they have a prior history of unemployment. In the unemployment tagging case the very fact that a worker has been unemployed in the past has a direct effect on the chances of a future spell of unemployment, which is behaviorally distinct from a worker that is predisposed to being unemployed based on some unobserved factor (to the econometrician at least). Thus, separating state dependence from unobserved heterogeneity is of economic and econometric interest.

In the context of our wage equation example we modify the specification to admit state dependence as:

$$lnw_{it} = \alpha_i + \rho lnw_{it-1} + \beta\pi_{it} + X_{it}\gamma + u_{it} \tag{12}$$

where in this case β is the short-run effect of the fatality rate on wages and β/(1-ρ) is the long-run effect. In terms of estimation, it is important to recognize that $E[lnw_{it}\alpha_i] \neq 0$, which follows by definition because α_i is a determinant of current wages it must also be a determinant of lagged wages. Moreover, under the standard case where u_{it} is *iid*, then $E[lnw_{it}u_{it}] = 0$. In the Cornwall, et al. terminology the lagged dependent variable is singly exogenous because it is correlated with the fixed effect but not the overall model error, and thus its inclusion is no different than any other typical regressor in the model (assuming known initial conditions). Indeed, the usual time series stationarity requirement for dynamic models that $|\rho| < 1$ is not necessary provided that the number of cross sectional units (N) is large relative to the number of time units (T), though economic interpretation gets muddied (Holtz-Eakin, Newey, and Rosen 1988).

The problem with the dynamic model occurs once we first difference to eliminate the latent heterogeneity α_i

$$\Delta lnw_{it} = \rho\Delta lnw_{it-1} + \beta\Delta\pi_{it} + \Delta X_{it}\gamma + \Delta u_{it}\ , \tag{13}$$

where we now have an induced endogeneity because the MA(1) error term Δu_{it} is correlated with the change in the lagged dependent variable Δlnw_{it-1}. Identification, however, is no more difficult than the case of the endogenous fatality rate above wherein in this case we can use lag levels of the dependent variable dated at time (t-2) and earlier as instruments for the change in the lagged dependent variable. Arellano and Bond (1991) discuss how it is possible to overidentify the model by using different lags of the dependent variable for each period with a block-diagonal instrument matrix; that is, if there is a total of 5 time periods then (t-4) is an instrument in time (t-2); (t-3) and (t-4) are valid instruments in time (t-1); and (t-2), (t-3), and (t-4) are valid instruments in time t. If there are also endogenous regressors beyond the lagged dependent variable such as the fatality rate then the same rules apply as discussed in the prior section.

We have also estimated dynamic first-difference regressions based on both the simple Anderson-Hsiao just-identified IV estimator and the heavily over-identified Arellano-Bond dynamic GMM estimator, which permit separating short-run versus long-run steady state estimates.[6] Remember 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).

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

As one would then expect, the steady state estimates of VSL after the estimated three-year adjustment period are larger than the short-run estimates. The difference between the short-run and long-run VSL is about $1 million, ranging from $6 million to $7 million versus $7 million to $8 million using a standard work year. Again, the central tendency of VSL estimates is not greatly affected when panel data are used with estimators that accommodate generic endogeneity, weak instruments, measurement error, latent heterogeneity and possible state dependence.

VI. Sample Composition Dynamics

Fundamental to identification of panel data models is the relative importance of the between-group versus within-group variation. If too much of the variation is between groups then the within or first-difference estimator is not identified. On the other hand, if the bulk of variation is within groups then the random effects estimator, which is a weighted average of within and between groups variation, converges to the within estimator as the ratio of between to within variance approaches zero. Concern over sample composition dynamics is exemplified in our wage-fatal risk example, where the fatality rate is not person-specific but instead is at the industry-occupation level, implying that between-group variation may be a significant fraction of the total variation. Indeed, Kniesner, et al. (2012) demonstrated that most of the variation in aggregate fatality risk is between groups (across occupations or industries at a point in time) and not within groups (within either occupations or industries over time). However, the within-group variation in the fatality measure accounts for about one-third of the total and thus it is feasible to identify the risk parameter in the first-difference wage model.

What is less well appreciated in many labor-market studies is the underlying source of the within variation that permits identification. For example, in labor supply applications that have a focus on identifying the wage elasticity of labor supply, it is typical to take the within-group variation in wages and hours as given, although Altonji and Paxson (1992) showed that for most workers it was necessary to change jobs if they wanted to changes hours or wages in a significant way. In our wage-fatal risk example, is the within variation largely coming from workers who stay in the same job but face varying on-the-job fatal risk over time, or is it from workers who change jobs either to a safer work environment at a lower wage or to a riskier work environment at a higher wage? Kniesner, et al. (2012) show that the main source of variation identifying compensating differentials for fatal injury risk comes from workers who switch industry-occupation cells over time. That is, the within-group variation is eight times higher for job changers than job stayers, and thus job changers are key to identifying compensating differentials.

If job changers are essential to identification for this, and potentially many labor models, then an important consideration is whether the dynamics of changing sample composition is endogenous, and if so, what form the endogeneity takes. For example, if the decision to change jobs is idiosyncratic and time invariant, then any potential bias from job switchers is swept out with first differencing. Or, if the decision to change jobs is trending over time, then including a person-specific trend and double differencing as in equation (5) will eliminate the endogeneity of job changers. However, suppose that workers who switch to more dangerous jobs require a large wage increase to accept a new job that is more dangerous but workers who seek a safer job do not accept a safer job if it is accompanied by much of a wage cut. The result will be an estimated hedonic locus in a panel data set that is driven by idiosyncratic worker selection effects that change over time and thus may not be well captured by a fixed effect or trend.

The time-varying selection effects can be introduced either as selection on observables or selection on unobservables (Barnow, Cain, and Goldberger 1980; Heckman 1979). In the observables case a control function of the form $g_{it} = f(Z_{it}\eta)$ can be appended to the model to control for the probability of switching jobs, where the Z_{it} are observed covariates and the η are unknown parameters. This could be implemented in a variety of ways such as directly including g_{it} in the regression model and estimating the parameters of the selection model jointly with the fatality risk parameter, or as a two-step propensity score estimator where in the first step estimates a flexible model of job change and then append $\hat{g}_{it}$ to the wage equation.

The canonical selection on unobservables model of Heckman (1979) likewise involves a function similar to the selection on observables, but in the unobservables case g_{it} is a generalized residual. So, for example, if the first stage model is a probit then $g_{it} = \frac{\varphi(Z_{it}\eta)}{\Phi(Z_{it}\eta)}$, which is the inverse Mill's ratio. (See Wooldridge (2010, Chapter 19) for alternative specifications in the panel data context). Kniesner, Viscusi, and Ziliak (2012) develop an interactive factor model as described in Bai (2009), which for wage levels here is

$$lnw_{it} = \alpha_i + \beta\pi_{it} + X_{it}\gamma + \delta_t + u_{it} \text{ with} \tag{14}$$

$$E[\alpha_i u_{it}] = 0, \tag{15}$$

$$E[\delta_t u_{it}] = 0, \text{ and} \tag{16}$$

$$E[u_{it}|\pi_{it}, X_{it}] \neq 0 = \lambda_i(\theta_0 + \theta_1 t) + e_{it}, \tag{17}$$

where λ_i is the inverse Mills ratio of the probability of ever changing jobs. Given our specification of the conditional mean function in (17) where λ_i is a time-invariant factor loading, the first-differenced model for the selection bias corrected panel data regression estimated on the subsample job changers is

$$\Delta lnw_{it} = \beta\Delta\pi_{it} + \Delta X_{it}\gamma + \Delta\delta_t + \theta_1\lambda_i + \Delta u_{it}. \tag{18}$$

The first-stage probit model for constructing the inverse Mills ratio regresses whether the worker ever changes a job on the time-means of the variables used in the wage equation. Because the regression in (18) does not include time means, exclusion restrictions as well as nonlinearities identify the effect of the inverse Mills ratio.

Kniesner, Viscusi, and Ziliak (2012) demonstrate that among job changers the estimated VSL is robust to the inclusion of selection effects that change over time. In other words, the correlation between preferences for risk and the risk of the job are well captured by controlling for simple latent intercept heterogeneity via first-differences. The robustness obtains even when they estimate a difference-in-difference with selection correction. Although it is not possible to extrapolate the result to the wider scope of models in labor economics, we note that controlling for fixed intercept heterogeneity is useful in mitigating bias from missing data in many other labor applications (Wooldridge 2010).

VII. Summary and Policy Implications

We have offered a brief summary of some key problems confronting empirical applications in labor economics and how panel data can be utilized to robustly estimate parameters of economic interest such as the value of statistical life. Our pedagogical framework of the hedonic equilibrium model was motivated in part because obtaining reliable estimates of compensating wage differentials 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, and the lack of disaggregated, longitudinal data on fatal job risks.

The wide variation of VSL estimates in the literature also has generated concern that underlying econometric problems may jeopardize the validity of the estimates. The range for VSL in the cross-section empirical literature is extremely wide, from about \$0 million to \$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 in our empirical example is that controlling for latent time-invariant intercept heterogeneity is crucial – much more so than how one does it econometrically—so much so that it reduces the estimated VSL by as much as two-thirds to about $6 million to $10 million depending on the time-frame (short-run versus long-run) and whether or not measurement error is addressed. In short, the models that yield the $6 million to $10 million range are preferred because they control comprehensively for selection on unobservables (via fixed effects, state effects, and industry occupation effects), selection on observables, and measurement error.

Using panel data econometric methods to narrow the 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. Because of the wide range of estimates from the earlier cross-sectional research, agencies often have failed to provide any boundaries at all to the key VSL parameter in their benefit assessments. For example, in comparing the cost estimates of health and safety regulations found in Breyer (1993), 23 of the 53 policies are in the indeterminate zone (neither pass nor fail the benefit-cost test) based on the cross-section range of VSL, but this is reduced to just 2 policies with our refined range of VSL. Panel data methods confer to labor economics the dual benefits of statistical robustness with economic relevance.[7]

[7] For more discussion see Kniesner and Leeth (2009).

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Table 1: Linear Cross-Section and Panel Data Estimates of Value of Statistical Life

	Pooled OLS Estimator	Between-Groups Estimator	Random-Effects Estimator	First-Difference Estimator	Difference-in-Differences Estimator
	(1)	(2)	(3)	(4)	(5)
Annual Fatality Rate	0.0037	0.0063	0.0015	0.0013	0.0015
	(0.0013)	(0.0021)	(0.0007)	(0.0006)	(0.0006)
Implied VSL ($Millions)	15.4	25.9	6.2	5.8	6.8
	[5.3, 25.6]	[8.9, 42.9]	[0.3, 12.2]	[0.8, 10.8]	[1.1, 12.5]

Source: Calculations based on Kniesner, Viscusi, Woock, and Ziliak (2012). Standard errors are in parentheses, and 95% confidence intervals are in square brackets

Table 2. Panel Quantile Estimates of Value of Statistical Life

Quantile	No Unobserved Heterogeneity		Unobserved Heterogeneity (λ = Tuning Parameter)	
		Implied VSL (in $Million)	λ = 1.0	Implied VSL (in $Million)
0.10	0.0025	4.33	0.0020	3.46
	(0.0010)		(0.0012)	
0.25	0.0022	5.15	0.0021	4.92
	(0.0009)		(0.0011)	
0.50	0.0049	16.82	0.0022	7.55
	(0.0010)		(0.0011)	
0.75	0.0062	31.11	0.0029	14.55
	(0.0010)		(0.0010)	
0.90	0.0075	55.11	0.0030	22.04
	(0.0015)		(0.0011)	

Source: Calculations based on Kniesner, Viscusi, and Ziliak (2010). Standard errors are in parentheses, calculated from 500 bootstrap replications. The panel quantile bootstrap standard errors are obtained by sampling (with replacement) the dependent variable and regressors for each cross-sectional unit.

Table 3: Instrumental Variables Estimates of Value of Statistical Life

	First-Difference IV Estimator, t-1 and t-3 Fatality as Instruments	First-Difference IV Estimator, Lag Differenced Fatality as Instrument	First-Difference IV Estimator, t-2 and t-3 Fatality as Instruments	First-Difference IV Estimator, Lag Differenced Fatality as Instrument	First-Difference IV Estimator, t-2 and t-4 Fatality as Instruments	First-Difference IV Estimator, Lag Differenced Fatality as Instrument
	(1)	(2)	(3)	(4)	(5)	(6)
Annual Fatality Rate	0.0018	0.0018	0.0020	0.0020	0.0014	0.0013
	(0.0009)	(0.0009)	(0.0011)	(0.0011)	(0.0011)	(0.0012)
Implied VSL ($Millions)	7.6	7.8	8.7	8.5	6.4	5.7
	[-0.1, 15.2]	[0.1, 15.4]	[-0.9, 18.4]	[-1.1,18.1]	[-3.6, 16.3]	[-4.3, 15.7]
Number of Observations	4338	4338	4338	4338	3235	3235

Source: Calculations based on Kniesner, Viscusi, Woock, and Ziliak (2012). Standard errors are recorded in parentheses, and 95% confidence intervals in square brackets. Standard errors are robust to heteroskedasticity and within industry-by-occupation autocorrelation.