Value of Statistical Life Analysis and Environmental Policy: A White Paper

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1 Introduction

The U.S. Environmental Protection Agency (EPA) uses a value of statistical life (VSL) estimate to express the benefits of mortality risk reductions in monetary terms for use in benefit cost analyses of its rules and regulations. EPA has used the same central default value (adjusted for inflation) in most of its primary analyses since 1999 when the Agency updated its *Guidelines for Preparing Economic Analyses* (USEPA, 2000). Prior to the release of the *Guidelines*, EPA sought advice from the Science Advisory Board's Environmental Economics Advisory Committee (SAB-EEAC) on the appropriateness of this estimate and its derivation. In 2000, EPA also consulted with the SAB-EEAC on the appropriateness of making adjustments to VSL estimates to capture risk and population characteristics associated with fatal cancer risks.[1] Currently, the Agency engaged with the SAB Advisory Council on Clean Air Act Compliance Analysis (the Council) on appropriate approaches to valuing mortality risks in the context of the 812 Second Prospective Analysis.[2]

EPA is now in the process of revising and updating its *Guidelines* and as such we are revisiting our approach to valuing mortality risk reductions. The literature has grown considerably since EPAs default estimate was derived and several EPA-funded reports have raised issues related to the robustness of estimates emerging from the mortality risk valuation literature. Furthermore, several meta-analyses have been conducted of this literature, providing new means of deriving central, default values for consideration. EPA's goal in bringing this issue to the SAB-EEAC is to seek expert opinion and guidance regarding the most appropriate way in which to proceed in updating the VSL estimate used to assess the mortality risk reductions from environmental policy.

It is important to note that this discussion focuses exclusively on mortality risk valuation. While we recognize the importance of morbidity and co-morbidity risks, the focus of this particular White Paper is on mortality; morbidity will be addressed at a future time.

To help inform the discussion, this paper provides background on current EPA practices for valuing mortality risk reductions, briefly summarizes the findings of three cooperative agreement reports on various segments of the literature, and reviews three recent meta-analyses that derive aggregate VSL estimates. The paper concludes with charge questions for consideration and discussion by the EEAC members. Full copies of the cooperative agreement reports and the meta-analyses are included in the Appendices.

2 Current Guidance and Practice for Valuing Mortality Risks

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1 *An SAB Report on EPAs White Paper Valuing the Benefits of Fatal Cancer Risk Reductions*, #EPA-SAB-EEAC-00-013, July 27, 2000.

2 *Review of the Revised Analytical Plan for EPA's Second Prospective Analysis – Benefits and Costs of the Clean Air Act 1990-2020*, Draft Report, #EPA-SAB-COUNCIL-ACV-XXX-XX, March 5, 2004. Portions related to VSL are included as Appendix B.

Reductions in mortality risk constitute the largest quantifiable benefits category of many of EPA's rules and regulations. As such, mortality risk valuation estimates are an important input to most of the Agency's benefit-cost analyses.

EPA's *Guidelines* advise analysts to use a central VSL estimate of $4.8 million in 1990 dollars. Based on the gross domestic product (GDP) deflator this converts to approximately $6.2 million in 2002 dollars. This value is derived from 26 estimates assembled for EPA's first retrospective analysis of the Clean Air Act (USEPA, 1997). Each estimate is from a different study, with 21 of the estimates from hedonic wage studies and the remaining five derived from contingent valuation (CV) studies. The estimates range from $0.9 million to $20.9 million (2002 dollars) and the studies were published between 1976 and 1991. The estimates are fitted to a Weibull distribution that is often used in probabilistic assessments of uncertainty in EPA benefits calculations. Appendix A contains a list of the estimates used by the Agency and indicates the study from which each was derived.

Until 2003, the estimate from EPA's *Guideline*s was uniformly applied to mortality risk reductions across program offices. EPA recently used an estimate of $5.5 million (1999 dollars) in its analysis of reduced mortality from air regulations. The economic analysis for EPA's Proposed Inter-State Air Quality Rule describes the approach.

> The mean value of avoiding one statistical death is assumed to be $5.5 million in 1999 dollars. This represents a central value consistent with the range of values suggested by recent meta-analyses of the wage-risk VSL literature. The distribution of VSL is characterized by a confidence interval from $1 to $10 million, based on two meta-analyses of the wage-risk VSL literature. The $1 million lower confidence limit represents the lower end of the interquartile range from the Mrozek and Taylor (2000) meta-analysis. The $10 million upper confidence limit represents the upper end of the interquartile range from the Viscusi and Aldy (2003) meta-analysis.[3]

This approach has been considered by the Council as part of their review of the Analytic Plan for the second Clean Air Act Prospective Analysis. As noted above, the Council is currently drafting its final report on the Analytic Plan.

2.1 "Adjustments" to the Base VSL

While there are many risk and population characteristics that may affect VSL estimates, to date EPA makes few adjustments to base estimates. Based on advice from the SAB-EEAC[4] and other committees,[5] EPA analysts have adjusted the base VSL estimate to account for the effects

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[3] *Benefits of the Proposed Inter-State Air Quality Rule*, EPA 452-03-001, January 2004.

[4] An SAB Report on EPA's White Paper *Valuing the Benefits of Fatal Cancer Risk Reduction*, EPA-SAB-EEAC-00-013, July 27, 2000.

[5] *Arsenic Rule Benefits Analysis: An SAB Review,* EPA-SAB-RSAC-01-008, August

of time. Specifically, future risk reductions valued according to VSL are discounted, including risk reductions spread over any latency period and/or cessation lag. This issue is of particular importance for cancer risks, but has also been employed for mortality from particulate matter.

Because income elasticity is believed to be positive, EPA has also adjusted current VSL estimates for anticipated income growth over time. Specific elasticity estimates have varied somewhat, but have been generally based on a review of the empirical literature on cross-sectional income elasticity of WTP. Income growth has been defined as the change in per capita GDP over time and projections of GDP growth are based on estimates from the Bureau of Labor Statistics.

EPA has been advised that the costs of illness for fatal cancers may be added to VSL estimates to assess the benefits of reducing cancer mortality.[6] The empirical effect of this addition is small and to date, the Agency has incorporated it only once into its regulatory analyses.

Finally, EPA has been advised that the evidence does not support empirical adjustments for other factors that may differ between study and policy cases, and that may affect VSL, including:

- risk preferences or risk aversion;
- age;
- cross-sectional income;
- cancer premium, fear, or dread;
- baseline health status; and
- voluntariness and controllability of risk.

2.2 Sensitivity and Alternate Estimates

The *Guidelines* allow for sensitivity analysis around key risk and population characteristics that affect the value of risk reduction. The particular parameters for a given sensitivity analysis should be guided by the benefit transfer concerns for that policy context.

EPA has considered several of the factors listed above in sensitivity analyses or alternative estimates. "Alternative estimate" is generally used to describe an analysis that incorporates scientific conclusions believed to be equally valid alternatives to the primary estimate. Sensitivity analyses typically employ other points on the Weibull distribution of VSL described in the *Guidelines*. For the case of the effect of age on VSL, EPA has employed various treatments including sensitivity analysis using the value of statistical life year, empirical adjustments based on CV studies, and an alternate analysis using only stated preference literature. The recent Durbin amendment to the appropriations bill for the Agency now precludes the Agency from performing any age-based adjustments when estimating the value of

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2001.

[6] *Arsenic Rule Benefits Analysis: An SAB Review,* EPA-SAB-RSAC-01-008, August 2001 (p. 6).

mortality risk reductions to adults in most contexts.[7]

3 Robustness of Estimates from Mortality Risk Valuation Literature

In anticipation of periodically revisiting the Agency's approach to mortality risk valuation, EPA funded three studies to examine the various segments of the mortality risk valuation literature. Black et al. (2002) and Alberini (2004), provide empirical assessments of the robustness of mortality risk valuation estimates emerging from hedonic wage-risk studies and contingent valuation studies, respectively. Blomquist (2004) provides a summary of the averting behavior literature.[8] All three studies are provided in their entirety in Appendices B, C and D.

3.1 Hedonic Wage Literature

Black et al. (2002) systematically examines the robustness of hedonic wage estimates of willingness to pay for mortality risk reductions using data sets commonly used in this area of research. To perform an hedonic wage study researchers generally need information on worker characteristics, including wage, and job risk. Specifically, this study examines the roles of functional form, measurement error, and unobservable characteristics using various data sets, including data on occupational risk from the Bureau of Labor Statistics (BLS) and National Institute for Occupational Safety and Health (NIOSH) and data on worker characteristics from the Current Population Survey (CPS), Outgoing Rotation Groups of the CPS, and the National Longitudinal Survey of Youths (NLSY).

Since no large data set exists that contains both basic types of information, researchers must match observations from various sources, making decisions on how best to combine the data which are often reported at different levels of aggregation. For example, researchers can choose

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[7] Public Law 108-199, "Consolidated Appropriations Act, 2004," Section 419 reads "None of the funds provided in this Act may be expended to apply, in a numerical estimate of the benefits of an agency action prepared pursuant to Executive Order No. 12866 or section 312 of the Clean Air Act (42 U.S.C. 7612), monetary values for adult premature mortality that differ based on the age of the adult."

[8] Blomquist 2004 appears in *Review of Economics and the Household* but is based on the work emerging from the cooperative agreement.

to create either industry-based or occupation-based risk measures to match with the worker-level data, each with its own difficulties. If industry-based measures are used, different occupations within an industry receive the same risk level (e.g., a miner and secretary for a mining firm). However, occupation-based measures potentially problematic because occupation is not well classified, with employers and employees often disagreeing on occupation classification.

3.1.1 Baseline estimates

The authors begin with ordinary least squares (OLS) estimation of simple log linear hedonic wage equations for three different worker samples and using both NIOSH and BLS risk data. The covariates included in the basic regression include basic controls such as worker age, education, union status, marital status, race and ethnicity. Also included, when possible, are variables to control for workers' firm size, state of residence, and one-digit industry and occupation. Results are reported separately for men and women. The positive VSL estimates that are calculated from these basic results range from \$3.7 million to \$16.4 million. The authors raise concerns regarding variation in other working conditions that may be captured in the estimates and interpret the instability they find in their parameter estimates as evidence that the measures of job risk are correlated with the regression error. The remainder of the paper is focused on identifying the source of this instability.

3.1.2 Role of Functional Form

The authors estimate the same equations using a more flexible functional form and using non-parametric approaches. In both cases they find that the results are just as volatile. Interestingly, they also find that the estimates are somewhat larger using the more flexible functional form. They conclude that the instability is not a result of the log linear specification. They also note that their tests do not necessarily mean that the non-linear specification is correct, only that it implies the presence of other problems.

3.1.3 Measurement Error

The authors note three possible sources of measurement error:

- Low sampling variation within industry and occupation cells given the small size of some of these cells (in recognition of this problem, BLS and NIOSH suppress data when number of fatalities is low);
- Heterogeneity in the actual job risk and non-random assignment of that job risk within occupation (e.g., late night convenience store clerks tend to be male and older);
- Industry and Occupation are not measured accurately, especially at three-digit level.

After using various techniques to determine the magnitude of the measurement error, they then attempt to correct or mitigate the error with limited success. Their efforts lead them to believe that the estimates they obtain are inconsistent and should not be used in policy analysis.

3.1.4 Unobservables

Using the National Longitudinal Survey of Youth (NLSY) data, the authors explore the effect of other characteristics not typically included in hedonic wage equations and typically not available in other worker samples, such as illegal drug use and Armed Forces Technical Qualification (AFTQ) scores. They find that those who admitted using illegal drugs tended to take on more occupational risk while those with higher AFTQ scores tended to sort into safer jobs. Hence, job risk is an endogenous variable.

3.1.5 Conclusions

In short, Black et al. find that results from hedonic applications to wage-risk data are not robust and are in fact quite unstable. For many of the specifications they try, they find a negative price of risk and for others they find that small changes in the covariates or risk measure used produce large variation in the estimated price. In their attempts to identify the source of this variation, they first examine the functional form of the regression equation. Using more flexible functional forms does not alleviate the problem. Second, they find "overwhelming evidence" that the job risk measures contain measurement error and that this error is correlated with covariates commonly used in the wage equations. Studies that do not correct for these errors would likely underestimate the value of risk reductions. Finally, they provide evidence that occupation risks are correlated with other characteristics typically not provided in the data sets commonly used for this type of analysis.

The findings of Black et al. are of obvious concern to EPA given the Agency's reliance to date on the hedonic wage-risk literature in determining its central, default VSL for use in policy analysis. To the extent that hedonic estimates are unstable, questions regarding the continued use of this literature in policy applications must be addressed.

3.2 Contingent Valuation Literature

Alberini (2004) examines the robustness of estimates of willingness to pay for mortality risk reductions derived from contingent valuation data and illustrates the empirical effects of some well-known problems in the contingent valuation literature. The author selects several papers from the literature and examines the robustness of the WTP estimates under alternative assumptions regarding (i) choice of distribution for WTP; (ii) presence of contaminating responses (yea-saying, nay-saying, and random responses); (iii) treatment of zero WTP; (iv) interpretations of WTP responses; (v) endogeneity of subjective baseline risks and/or risk reductions; (iv) treatment of regressors and outliers, and (vii) sample selection bias. Each issue is examined separately for some subset of the papers for which Alberini was able to obtain data.

The five CV studies from the original 26 studies in Viscusi (1991) are of obvious interest, but the author was able to obtain data for only one of the five. Additional studies are chosen from the relatively recent literature on the basis of quality, and Alberini's judgment of the study results' applicability to environmental policy, as well as availability of data. The studies used in Alberini (2004) are: Gerking, de Haan and Schulze (1988); Johannesson and Johansson (1996);

Johannesson, Johansson, and Lofgren (1997); Persson, Norinder, Hualte, and Gralen (2001); Krupnick, et al. (2002); and Alberini, Cropper, Krupnick and Simon (forthcoming).

3.2.1 Choice of Distribution

Analyzing single-bounded responses from two studies, Alberini finds that mean estimates may depend crucially on assumptions about the underlying distribution of responses, and on the coverage of the range of possible WTP. Median WTP is far less sensitive to these factors. Alberini concludes that double-bounded questions may be preferable and that median WTP should be used rather than means.

3.2.2 Mixture Models

"Mixture models" are presented to illustrate how one could model and estimate the extent of contaminating responses to a CV survey (e.g., 'yea-saying,' 'nay saying'). The models are estimated using data from three of the studies collected. The results are interesting, but it is clear that it is difficult to reliably estimate mixture models. Alberini concludes that contaminating responses could be an important factor affecting inferences of respondent behavior, and thus questionnaires should include debriefing questions designed to identify the presence of contaminating responses in such a way that the debriefing results can be used in the statistical analysis.

3.2.3 Treatment of Zero Responses

Alberini shows that alternative interpretations of zero responses can significantly affect the estimates of mean WTP, while again estimates of the median are not substantially affected. This issue is intimately related to the choice of the underlying distribution of responses, though it is confounded somewhat by the treatment of single- vs. double-bounded responses.

3.2.4 Treatment of Extreme Responses

Alberini also examines the effects of extreme responses on WTP estimates and shows that dropping outliers can have large effects on both the mean and median WTP, although median WTP is less sensitive.

3.2.5 Endogeneity of Risk

When surveys elicit perceptions of baseline risks or risk reductions, it is important to test and correct for endogeneity between subjective risks and WTP. If not controlled for, endogeneity biases estimates of the risk coefficient and confounds scope tests. Alberini shows that endogeneity can affect inferences regarding whether it is absolute or relative risk changes that determine WTP. This is important since values for absolute risk changes are needed in order to calculate a VSL. Alberini recommends, therefore, that researchers express risks in both absolute and relative terms. The author also examines the effect of excluding implausibly large subjective risk values and finds this can also affect the results.

3.2.6 Conclusions

Although Alberini (2004) does not provide a comprehensive examination of the contingent valuation literature, her findings are nevertheless of significant consequence to the Agency. Methods for eliciting willingness to pay values for mortality risk reductions have clearly advanced with time. Her systematic examination of a number of key issues using several available datasets in the analysis of CV data, as well as the presentation and interpretation of CV results, at the very least provides a number of factors that should be considered in selecting studies on which to base any central, default VSL estimate.

3.3 Averting Behavior Literature

Although not a formal meta-analysis or a detailed statistical treatment of the averting behavior literature, Blomquist (2004) summarizes the empirical averting behavior VSL literature and provides a heuristic review of existing estimates. The author begins by presenting a basic framework for estimating VSL based on averting behavior and follows with a brief review of existing estimates.

The study finds that VSL for adults from this literature ranges from a little less than $1.7 million to $7.2 million in 2000 dollars. Making a few assumptions, Blomquist finds a simple average adult VSL of approximately $4.5 million. In the author's judgment the range of "best estimates" is about $2 million to $7.2 million, with a subjective best estimate of $4 million. Blomquist reports evidence that VSLs may be greater, or at least not less, for children than for adults, but existing studies are not conclusive on this point. Furthermore, empirical evidence on VSLs for senior citizens is limited and not conclusive.

The author then makes some broad conclusions about the averting behavior literature:

- More recent estimates are larger than those in earlier studies. This is credited to greater use of hedonic approaches rather than relying on values of time, disutility costs, etc.;
- Difficulties with individual risk perception are an issue, but not a barrier for estimating VSL from averting behaviors. This conclusion is based on (i) evidence that individual risk perceptions are correlated with expert assessments, (ii) that VSL estimates can be adjusted for risk misperception in a sensitivity analysis (and these values may actually be preferred), and (iii) VSL estimates can be informative even if they are not "perfect.";
- VSLs from averting behaviors have tended to be somewhat lower than those from hedonic wage studies. However, the difference is not great, and there is reason to believe that hedonic wage VSLs are biased upward (e.g., Shogren and Stamland, 2002);
- VSLs from averting behaviors tend to be higher than those from stated preference studies. The paper attributes much of this difference to hypothetical bias in SP studies.;
- Blomquist suggests a meta-analysis of averting behavior VSL estimates, specifically recommending that the analysis consider: base risk level, risk change, adjustment for risk perception bias, value of time, treatment of disutility or jointness in consumption, and individual characteristics.

4 Meta-Analyses of the Mortality Risk Valuation Literature

Since EPA derived the VSL estimate cited in the *Guidelines* advances have been made in the field of mortality risk valuation. There are new examinations of how context affects mortality risk valuation, as well as new hedonic wage and contingent valuation studies. Some new CV studies make use of improved risk communication devices, which have been shown to improve the validity of these estimates. Key recent work on mortality valuation includes Krupnick, et al. (2002), Eeckhoudt and Hammitt (2001), Viscusi (2004), Smith, et al. (2003), and Smith, et al. (2004). While we recognize the important contributions these and other recent papers have made directly, we focus on three recent meta-analyses that include many relatively new mortality valuation studies.

Meta-analysis is a potentially useful means of combining individual but related studies in an analytically rigorous way that accounts for individual characteristics of each study. We reviewed a number of meta-analyses for this background paper. Each was assessed as to whether it provided a viable estimate or range of estimates of VSL that the Agency could use for policy analysis. The studies by de Blaij, et al. (2000), Miller (1990), and Miller (2000) lack the level of coverage and/or statistical rigor deemed appropriate for Agency use. We review three studies, however, in more detail, as they contain broad coverage of the available literature, rigorous statistical analyses, and/or a presentation of a range of predicted VSL estimates. These studies can provide useful insights into our efforts to update the VSL estimate used in EPA analyses. The three studies we review below are Kochi, Hubbell and Kramer (2003), Mrozek and Taylor (2002), and Viscusi and Aldy (2003).[9] Summaries of each of these studies appear below, including descriptions of the criteria used to select the individual studies used, data and statistical specifications, and results. Appendix J presents a combined bibliography of all the VSL studies included in the meta-analyses considered below.

4.1 Summary of Kochi, Hubbell and Kramer[10]

Kochi, Hubbell and Kramer (2003) employ an empirical Bayes estimation method to generate

[9] The Council in their assessment of the 812 Analytic Blueprint considered these same three studies for the Second Prospective Analysis.

[10] This summary is based on the 2003 version of the analysis that accompanied the EPA's Analytic Blueprint for Second Prospective Analysis. An updated version of the study is currently under review for publication and will be provided to the SAB-EEAC. We have not thoroughly assessed differences in the two versions.

predicted VSL estimates using multiple results from both hedonic wage and stated preference studies. To identify potential studies for inclusion, the authors searched for recent work in bibliographies from previously published meta-analyses and review articles, citations from other VSL studies, web searches, and personal contacts. They collected 47 hedonic wage studies and 29 contingent valuation studies for potential inclusion in their analysis.

In deciding whether to include a study, they applied the same criteria used in Viscusi (1992), a review article of 37 studies. Viscusi employed four explicit criteria for selecting studies in his analysis:

- include only hedonic wage and contingent valuation studies; consumer market studies "...failed to provide an unbiased estimate of the dollar side of the risk-dollar tradeoff, and tend to underestimate VSL." (p. 7);
- exclude hedonic wage studies using actuarial risk data (because these data include risks other than those on the job and therefore bias the VSL estimate down);
- include only studies using a simple regression estimation approach (as opposed to a more complex estimate of the tradeoff for discounted expected life years lost);
- studies must have a minimum sample size of 100.

In addition, the following selection criteria are noted as implicit in Viscusi (1992):

- only include hedonic wage studies for general or blue-collar workers;
- only include CV studies on samples of the general population;
- only include studies from high income countries (e.g., US, UK, Japan).

These selection criteria reduced the number of studies in Kochi, et al. from 76 to 45. They use all reported VSL estimates for reduced risk of immediate death from each study, resulting in 196 estimates.

Kochi et al. re-estimated all possible VSLs and associated standard errors for each included study based on information provided in each original study, using mean values for variables.[11] Recalculations that resulted in a negative VSL were excluded from the primary analysis, but are included in a sensitivity analysis.

The authors employed Bayes estimation, which requires the assumption that each estimate used be an independent sample. As this is unlikely if multiple observations from a single study are included, the authors array the culled VSL estimates into "homogeneous subsets" by author and other characteristics. A total of 60 subsets were created in this fashion, each assumed to be independent. Once subsets were created, a representative VSL for each subset was constructed by averaging VSLs and their standard errors within the subset. Predicted VSL estimates are based on these representative VSLs.

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[11] The VSL from CV studies is calculated as WTP/(risk reduction).

This estimation method adjusts each of the representative VSLs based on within-study variability and the distribution of VSLs across studies. Smooth distributions are generated by using kernel density estimation, assuming a normal distribution for the kernel function. To test for sensitivity of the results to original valuation method, the authors separately estimate distributions for hedonic wage and contingent valuation studies. A bootstrap technique, resampling each sample of method-specific estimates 1000 times, is then applied to compare the different distributions of VSL.

4.1.1 Results

The primary results using all studies are summarized in Table 1. The table shows a mean VSL of \$5.4 million with a standard error of \$2.4 million (2000 dollars). A sensitivity analysis examining hedonic wage and CV estimates as separate sets found the hedonic wage distribution has a mean of \$2.8 million (standard error = \$1.3 million), while the hedonic wage distribution has a mean of \$9.4 million (standard error = \$4.7 million). The differences in means, medians, and interquartile ranges between the distributions are statistically significant. The sample containing U.S. studies only has a mean of \$8.5 million (standard error = \$4.9 million).

Table 1
Results of Empirical Bayes Estimates and Bootstrap Tests for Distribution Comparisons
Reproduced from Table 2 in Kochi, Hubbell, and Kramer (2003)

	Mean (million \$)	Standard Error (million \$)	Coefficient of Variation	Bootstrap Test		
				Mean	Median	Interquartile
Distribution Comparison by Evaluation Method						
Total (60)	5.4	2.4	0.4	P-value (Ho: HW = CV)		
CV (18)	2.8	1.3	0.5			
HW (42)	9.4	4.7	0.5	<0.001	<0.001	<0.008

4.1.2 Limitations

Although the study is useful for aggregate level comparisons, it does not account for the impact of specific study characteristics, including population characteristics, on VSL. Furthermore, the study gives no weight to the original authors'judgements to distinguish reasonable or preferred estimates from others, with the exception of negative VSLs. This may be statistically valid, but is troublesome because the conclusions of the authors who are most familiar with their research are lost.

It is not clear to what extent this analysis captures different specifications used across studies. If the VSLs are based on regressions with different specifications and this is not otherwise captured in the analysis, then it seems the "homogeneous groupings" are somewhat arbitrary and could be made differently. Since differences in specification are likely to significantly influence the resulting VSL estimates, the study should account for these differences in some way.

The authors also note that the results are sensitive to small VSLs with low variances. These estimates receive a great deal of weight in the empirical analysis. Removing Krupnick, et al. (2000), for example, increases the mean estimated VSL by almost 10%.

4.2 Summary of Mrozek and Taylor

Mrozek and Taylor's analysis focuses on results from hedonic wage studies only. Estimates from 47 studies were used although the authors do not specify how they selected their studies. Ultimately, 14 studies were subsequently dropped because:

- mean risk values, and in some cases also mean earnings, were not reported (6 studies);
- the risk measure confounds death and injury (1);
- observations represent industries, not individuals (1);
- study was unavailable (1);
- many variables were not reported (1);
- mean wages were incorrectly calculated (1);
- no VSL estimate was reported or obtainable (2 studies); or
- results were identical to another study (1).

As with the Kochi et al. study, multiple observations are used from each study when the original authors reported variations in model specifications or samples from which VSL estimates could be obtained. One to 28 observations are obtained from each study. Variables included in the meta-regressions are of three types: those which may influence wage/risk tradeoffs (e.g., mean hourly earnings, national unemployment rate in the year wage data was collected, mean annual risk of death); those describing the sample (e.g., if the data is from a national sample of US workers, if risk variable included a worker's self-assessment of risk, if the sample is 100 percent white collar); and methodological choices of the original researchers (e.g., if a risk-squared term is included, the number of industry categories controlled for, if at least one dummy variable describing a job characteristic was included).

The authors use weighted least squares so that each study, regardless of the number of observations drawn from it, is weighted equally. Four models are estimated, in each case the log of VSL is the dependent variable. Model 1 is the most inclusive, while model 2 eliminates observations based on samples with high risks and those using actuarial data. Model 3 further restricts the sample to the U.S. and includes a dummy variable indicating where five or more industries were controlled for in the original study. Model 4 is the same as model 3 except that it incorporates a continuous variable indicating the number of industries controlled for in the original study.

4.2.1 Results

All four models indicate a positive and significant relationship between the mean risk and VSL. The authors find this relationship to be concave - VSL estimates begin to decline when mean risk is between 1.2 to 1.67 deaths per 10,000, depending upon the model. The coefficient on earnings is positive but significant only in models 1 and 2. VSL estimates from national U.S. samples are higher than those from specialized U.S. samples and the use of NIOSH data results in higher VSL estimates than do estimates generated from BLS data.

The authors use the meta-analysis results to develop revised estimates of VSL by predicting VSL as if the original studies had all followed a set of best practice assumptions (e.g., including a risk-squared term, including at least one occupational dummy, including at least one dummy describing a job characteristic). Table 2 presents mean adjusted predictions from models 3 and 4 for five baseline risk levels ranging from 0.25 to 2 deaths per 10,000, by potential dataset (BLS or NIOSH), and by control for inter-industry differences. Estimates assuming the use of NIOSH data are higher than those assuming use of BLS data and range from \$1.35 million to \$11.7 million (1998 dollars), estimates decline for risks greater than 1.5 per 10,000. The authors conclude that the evidence best supports an estimate of \$2 million at the average occupational risk level of 0.5 per 10,000. Refining this estimate for an average worker leads to an estimate of approximately \$2.6 million (see footnote 17).

Table 2
Estimates of the Value of Statistical Life: Mean Adjusted Fitted Values[a]
Reproduced from Table 4 in Mrozek and Taylor (2002)

Risk (x 10^{-4})	Based on Model (3), Table 3		Based on Model (4), Table 3	
	< 5 Industries	**$ 5 Industries**	**0 Industries**	**7 Industries**
BLS Risk Data				
P = 0.25	\$3.82m	\$1.35m	\$2.99m	\$1.27m
	(1.39)	(0.47)	(1.12)	(0.40)
P = 0.5	\$4.73m	\$1.67m	\$3.90m	\$1.65m
	(1.64)	(0.53)	(1.44)	(0.51)
P = 1.0	\$6.25m	\$2.20m	\$5.57m	\$2.36m
	(2.36)	(0.73)	(2.22)	(0.80)
P = 1.5	\$6.78m	\$2.39m	\$6.33m	\$2.68m
	(3.02)	(0.92)	(2.83)	(1.03)
P = 2.0	\$6.05m	\$2.13m	\$5.72m	\$2.42m
	(3.09)	(0.92)	(2.83)	(1.03)
NIOSH Risk Data				
P = 0.25	\$6.59m	\$2.32m	\$5.24m	\$2.22m
	(2.62)	(1.00)	(2.08)	(0.84)
P = 0.5	\$8.16m	\$2.88m	\$6.82m	\$2.89m
	(3.17)	(1.20)	(2.72)	(1.10)
P = 1.0	\$10.8m	\$3.80m	\$9.76m	\$4.13m
	(4.57)	(1.65)	(4.18)	(1.68)

P = 1.5	$11.7m	$4.13m	$11.1m	$4.69m
	(5.65)	(1.95)	(5.21)	(2.07)
P = 2.0	$10.4m	$3.68m	$10.0m	$4.24m
	(5.57)	(1.85)	(5.06)	(1.97)

[a] Values are expressed in millions (1998 dollars). Standard errors are in parentheses.

4.2.2 Limitations

The study has been criticized in a paper by Harrison (2002) for failing to report standard errors and for the authors' choice of which estimates from each study they included. For example, the authors excluded estimates in original studies that were statistically insignificant or negative - such as the negative coefficients on the BLS variable in certain studies.

Hammitt (2002) and Krupnick (2002) each provide commentary on the Mrozek and Taylor study. Hammitt highlights several important findings. For example, Mrozek and Taylor find that failure to control for non-fatal risks is less significant than previous studies report and they confirm a common result that NIOSH data produces VSL estimates that are substantially higher than BLS data. Hammitt also highlights the importance of controlling for industry as a significant finding from Mrozek and Taylor. Hammitt notes the mixed evidence in Mrozek and Taylor concerning the use of actuarial versus perceived risk estimates, as well as the mixed results concerning pre- and post-tax dollars. Hammitt does question the Mrozek and Taylor results concerning the relationship between risk and VSL estimates. Specifically, Hammitt believes that the increase in VSL as risk increases is too large to be supported by standard models.

Krupnick (2002) focuses on the policy relevance of the Mrozek and Taylor meta-analysis. Mrozek and Taylor report a best estimate of $2 million, which is about 66 percent less than the estimate currently used by EPA in most benefit-cost analysis. While there are examples of rules that may have "failed" the benefit-cost test by using this lower estimate, Krupnick notes that there are many factors that enter into the decision-making process on a given policy, making it unlikely that this lower estimate would significantly change decision making in these cases. While Krupnick endorses the Mrozek and Taylor study, he does state that concerns with the hedonic wage literature may supplant the use of this study in policy analysis.

4.3 Summary of Viscusi and Aldy

Viscusi and Aldy conduct a review of more than 60 studies of mortality risk across 10 countries, examining a number of econometric issues, the effects of unionization on risk premiums, and the effects of age and income on VSL estimates. The analysis includes fifty-two hedonic wage studies from the U.S. and other countries selected based on the following set of criteria:

- written in English;
- published in academic journal or book;
- provides enough information to calculate a VSL.

The authors did not attempt to eliminate studies or modify the original VSL estimates. Point estimates from each study are those using the "whole sample" based on the original authors' preferred specification.

The empirical analysis drops 3 studies that did not have an income measure. It also appears that three studies that did not report mean risks were dropped, resulting in 46 studies for OLS specifications. Other specifications dropped either one or two more studies, but it is not clear which ones. Values in the final set of studies range from $0.5 million to $20.8 million. Half of the U.S.-based studies estimate a VSL from $5 to $12 million. The median estimate from the sample is about $7 million.

In the statistical analysis, the authors first replicate four other published meta-analyses, using the preferred specifications of the authors of those studies (Liu, Hammit and Liu, 1997; Miller, 2000; Bowland and Beghin, 2001; Mrozek and Taylor, 2002).[12]

Next, the paper presents original meta-analyses employing six specifications, three using OLS specifications and three robust specifications with Huber weights. The simplest specifications include only the log of income and mean risk as dependent variables; two other specifications include mean risk squared; and the most complete and robust specifications also include variables to control for the underlying data source, whether risks are subjective, whether the study included a morbidity variable, and regional, urban, industry, and occupation dummies.

4.3.1 Results

The predicted values in the study are presented in Table 3, which is adapted from Table 8 of Viscusi and Aldy (2003). Generally, predicted values for the U.S. range from $5.5 million to $7.6 million. The study notes that median predicted values were generally very close to the means.

The authors predicted mean VSL estimates by using the estimated coefficients from the meta-analysis to predict the natural logarithm of VSL for each original study. After converting log(VSL) to VSL the study-specific predicted values were averaged to get the mean estimates presented in Table 3. Confidence intervals were constructed by using the prediction error for each study from the meta-analysis regressions. Lower and upper confidence intervals for each study were averaged to produce the lower and upper confidence intervals reported below. Predicted U.S. mean values are constructed based on regression samples using all countries, but with averaging across only U.S. studies. The authors note that the confidence intervals are valid only under the assumption that the model is specified correctly.

The meta-analysis is undertaken to estimate the effects of income on VSL and the study finds

[12] Liu, Hammit and Liu (1997) and Bowland and Beghin (2001) focus on developing countries and thus are not considered in our summary.

that income elasticity for VSL ranges from about 0.5 to 0.6 across several specifications. The authors note that the 95 percent confidence interval on income elasticity never exceeded 1.0.

Table 3
Mean predicted VSL, U.S. sample
Reproduced from Viscusi and Aldy (2003)

	OLS 1	OLS 2	OLS 3	Robust w/ Huber wts	Robust w/ Huber wts	Robust w/ Huber wts
Variables	Log(Y) & mean risk	OLS1 + mean $risk^2$	Full set	Log(Y) & mean risk	OLS1 + mean $risk^2$	Full set
Mean predicted VSL (95% C.I.)	5.5 (3.8 - 8.1)	5.8 (4.1 - 8.3)	6.9 (3.1 - 16.2)	6.1 (4.6 - 8.2)	6.3 (4.8 - 8.4)	7.6 (3.0 - 19.4)

4.3.2 Limitations

While the meta-analysis results are highly consistent across specifications, the confidence intervals for the regressions that include the full set of covariates are broad because there are relatively few degrees of freedom. Moreover, the precise VSL values used for each study in the sample are not fully clear. The paper reports VSL's for each study in the analysis, but some of these are in the form of a range. Finally, the selection criteria does not include estimates from "grey" or unpublished literature.

5 Conclusion and Summary

Since 1999 EPA has relied on a central VSL estimate of $6.2 million (2002 dollars) for most of its economic analyses, which is derived from a Weibull distribution of 26 hedonic wage and contingent valuation studies of mortality risk valuation. Recently, in air regulations EPA has used an estimate of $5.5 million (2003 dollars), which is derived from recent meta-analyses. In light of additions and advances in the literature, the time is ripe for revisiting the VSL estimate(s) used in EPA policy analysis.

This background paper reports on three cooperative agreements that assess the hedonic wage, contingent valuation, and averting behavior literatures, as well as reviews three recent meta analyses of the mortality risk valuation literature.

Each of the cooperative agreements highlights areas of concern with the particular literature under investigation. Black, et al. (2003) raise concerns with the stability of hedonic wage estimates, given the large changes in results that come from slight changes in specification or choice of data. Alberini (2004) demonstrates how modifications in specification can affect results and asserts that median estimates are more stable than mean estimates, though researchers must be attuned to the impact of outliers and zero values when doing their estimation. Finally, Blomquist (2004) reviews the averting behavior literature and encourages a more thorough analysis for use in policy decisions.

We also review three recent meta-analyses of the mortality risk valuation literature. Kochi, Hubbel and Kramer (2003) use Bayesian techniques to combine contingent valuation and hedonic wage studies in a meta-analytic framework. They recalculate the original estimates to account for independence and report an estimate of $5.4 million from their studies. Both Mrozek and Taylor (2003) and Viscusi and Aldy (2004) conduct meta analyses of the hedonic wage literature. The studies differ in their selection criteria and how they use the estimates. Mrozek and Taylor report a best estimate of $2 million, while Viscusi and Aldy report a best estimate of around $6 million.

These reports and studies are informative as EPA revisits the best VSL estimate to use in policy analysis. This background paper concludes with a series of Charge Questions to guide discussion of issues confronted when using the existing mortality risk valuation literature to evaluate environmental policies.

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Charge Questions for Discussion

The charge questions are structured around a set of broad issues that define the general objectives of this review.

I. Literature support for a revision of the current Guidelines for valuing changes in fatal risk.

In 1999, the Science Advisory Board - Environmental Economics Advisory Committee reviewed the draft *Guidelines for Preparing Economic Analyses*. The *Guidelines* state that the Agency would continue to conduct periodic reviews of the risk valuation literature and revise the Guidelines accordingly, under advisement from the SAB. Though the literature has grown since the publication of the 2000 *Guidelines*, the Agency's practice of valuing changes in fatal risks has largely been unchanged. Does the literature support a revision of the current Guidelines for valuing fatal risk changes?

II. Questions on the important strengths and limitations of the available literature and how these factors be accounted for in practice.

A. The Background Paper summarizes several EPA commissioned reports that document methodological concerns underlying VSL studies that use hedonic wage equations, contingent valuation surveys, and averting behavior methods. What are the important practical lessons EPA can draw from these reports, and how should these be used to evaluate literature to be used by EPA?

B. To what extent is it scientifically appropriate for the Agency to use VSL estimates from unpublished studies and studies from developing countries in developing mortality risk valuation policy?

III. The risk valuation literature has grown substantially since the 1999 Guidelines were published. EPA has questions about what is the most scientifically appropriate way for EPA to aggregate the literature in updating its mortality risk valuation policy. There are a number of alternatives to consider:

A. Current Practice (fitting a distribution)

EPA *Guidelines* recommend using a distribution of VSL estimates based on 26 studies from the literature. A Weibull distribution was fit to the set of estimates, yielding a central estimate of approximately $6.1 million. Is this sort of "curve fitting" a preferred methodology for deriving a distribution of VSL values for use in economic analyses of EPA regulations?

B. Meta-analyses

(i) There are three widely-circulated meta-analyses of VSL estimates that are recent contributions to the literature. Is meta-analysis the preferred methodology for deriving VSL values for use in economic analyses of EPA regulations?

(ii) The white paper summarizes three widely-circulated meta-analyses of VSL estimates that appear to be generally regarded as high quality.[1] These analyses differ in their selection criteria, the scope of studies they consider, and their technical approach to combining existing VSL estimates. In general, what are the relative strengths and weaknesses of each study in regards to application to EPA policy analyses? Does one of these studies emerge as a preferred candidate for VSL estimates for EPA policy analyses?

(iii) Each of the three studies use different criteria to select estimates to include in the analysis (e.g., only HW studies, HW and CV studies). Are there particular selection criteria that should be required in any meta-analysis used by EPA for policy analysis?

(iv) Similarly, each of the studies uses different statistical techniques to calculate their VSL estimates. For example, some studies rely on regression techniques, whereas others fit a particular distribution to the data. What approach should EPA use for calculating VSL estimates for policy analysis?

(v) Each of the meta-analyses manipulates the original data to some extent. For example, some studies adjust for after-tax wages, whereas others do not. Is there a set of such manipulations that the EEAC believes to be critical for any meta-analysis? Are there some data manipulations that are generally incompatible with sound meta-analysis?

(vi) How should a quality meta-analysis handle zero or negative VSL estimates from studies that otherwise meet its selection criteria for inclusion?

(vii) If the Agency relies upon multiple meta-analyses to estimate VSL for policy analysis, how can the different meta-analyses most rigorously and appropriately be combined given that they use different statistical procedures, and overlapping, but not identical sets of studies?

C. Are there other alternatives methodologies EPA should consider for aggregate the literature in updating its mortality risk valuation policy?

[1] The three studies are Viscusi and Aldy (2003), Kochi, Hubbell and Kramer (2003), and Mrozek and Taylor (2000).

IV. The characteristics of risks and populations addressed in the VSL literature are often different from those addressed by EPA policies. The SAB has addressed some of these questions concluding that the only empirically feasible adjustments to a base VSL are (1) discounting over periods of latency and cessation lag, and (2) increasing VSL over time to account for rising real income.

A. Does the literature continue to support empirically accounting for these effects in policy analysis?

B. Does the literature support empirically accounting for other risk and population characteristics in transferring existing VSL estimates to the analysis of EPA policies?

V. Empirical analysis is always limited by data constraints. The analysis by Black, et al., for example, highlights the impact of existing data limitations in hedonic wage studies. EPA is interested in hearing from the SAB-EEAC members on how the Agency might assist research through efforts to make data more available.

A. Can useful analytical gains be made through low-cost improvements in data quality or increased data availability? What steps can EPA and other government agencies take in the short term to facilitate research through improved data quality or increased accessibility to existing data sets?

B. The EEAC recently reviewed EPA's draft Environmental Economics Research Strategy and provided advice regarding research needs for mortality valuation as part of that review. Given the additional information provided to the committee for this review, do EEAC members wish to identify any additional research needs or provide any modifications to their recent advice?

C. What do members of the EEAC see as the most fruitful, long-term strategies for overcoming the challenges of using the existing literature for environmental policy analysis?

APPENDIX A

Value of Statistical Life Estimates on Which EPA VSL Estimate is Based

VALUE OF STATISTICAL LIFE ESTIMATES (mean values in 1997 dollars)		
Study	**Method**	**Value of Statistical Life**
Kneisner and Leeth (1991 - US)	Labor Market	$0.7 million
Smith and Gilbert (1984)	Labor Market	$0.8 million
Dillingham (1985)	Labor Market	$1.1 million
Butler (1983)	Labor Market	$1.3 million
Moore and Viscusi (1988)	Labor Market	$3.0 million
Marin and Psacharopoulos (1982)	Labor Market	$3.4 million
Kneisner and Leeth (1991 - Australia)	Labor Market	$4.0 million
Cousineau, Lecroix and Girard (1988)	Labor Market	$4.4 million
Dillingham (1985)	Labor Market	$4.7 million
Viscusi (1978, 1979)	Labor Market	$5.0 million
R.S. Smith (1976)	Labor Market	$5.6 million
V.K. Smith (1976)	Labor Market	$5.7 million
Olson (1981)	Labor Market	$6.3 million
Viscusi (1981)	Labor Market	$7.9 million
RS.Smith (1974)	Labor Market	$8.7 million
Moore and Viscusi (1988)	Labor Market	$8.8 million
Kneisner and Leeth (1991 - Japan)	Labor Market	$9.2 million
Herzog and Schlottman (1987)	Labor Market	$11.0 million
Leigh and Folsom (1984)	Labor Market	$11.7 million
Leigh (1987)	Labor Market	$12.6 million
Garen (1988)	Labor Market	$16.3 million
Miller and Guria (1991)	Contingent Valuation	$1.5 million
Viscusi, Magat and Huber (1991)	Contingent Valuation	$3.3 million
Gegax et al. (1985)	Contingent Valuation	$4.0 million
Gerking, de Haan and Schulze (1988)	Contingent Valuation	$4.1 million
Jones-Lee (1989)	Contingent Valuation	$4.6 million
Derived from US EPA (1997) and Viscusi (1992).		

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Appendix B

Selected Excerpts from

Review of the Revised Analytical Plan for EPA's Second Prospective Analysis – Benefits and Costs of the Clean Air Act 1990-2020, Draft Report, #EPA-SABCOUNCIL-ACV-XXX-XX, March 5, 2004.

Excerpt 1, from page 3 of the draft report: "Value of Premature Mortality and Morbidity Associated with Reductions in Air Pollution"

Excerpt 2, from pp 54- 62 of the draft report: "12. USE OF VSL META-ANALYSES"

Excerpt 3, from pp 121-127 of the draft report: "APPENDIX D: ADDITIONAL DISCUSSION CONCERNING THE USE OF VSLS"

Value of Premature Mortality and Morbidity Associated with Reductions in Air Pollution: Uncertainty analysis with respect to Value of a Statistical Life (VSL) values requires information about the distribution of VSL estimates corresponding to risks and populations that are similar to those relevant for the CAAA. The marginal distribution of all empirical VSL estimates derived across all contexts is unlikely to be appropriate for this purpose, as is any arbitrary convenient assumption about distributional shape.

The Panel recommends a primary focus, at this juncture, on the Viscusi-Aldy estimates of VSLs based on U.S. studies. The Agency should not rely exclusively on the Kochi et al. meta-analysis, which has not yet been peer-reviewed and published.

The Council Special Panel does not support an effort by the Agency to comply with the OMB requirement for cost-effectiveness analysis by utilizing Quality-Adjusted Life Year (QALY) as the measure of effectiveness. Too many other classes of benefits besides human health benefits must be taken into consideration. A workshop on appropriate cost-effectiveness approaches for this application may be helpful, but its scope would need to be very carefully defined and the differences between cost-effectiveness analysis in the typical health context versus cost-effectiveness for specific human health benefits of the Clean Air Act (CAA) would be an important dimension of the discussion.

Concerning morbidity, the Agency should continue to use Willingness-To-Pay (WTP) estimates for morbidity values, rather than COI estimates, should these be available. Where WTP is unavailable, COI estimates can be used as placeholders, awaiting further research, provided these decisions include suitable caveats. The Dickie and Ulery study is a valuable addition to the repertoire of empirical results concerning WTP for acute respiratory illnesses and symptoms, although it is not so superior as to supercede all earlier studies.

Ecological Effects: Human health risk reductions may be the most substantial benefit from the CAAA, but they are not the only important benefit. Benefits to ecosystems and other welfare benefits such as visibility are likely to be substantial and are still receiving limited attention. The Council nevertheless recognizes substantial challenges in quantitative assessment of these benefits. The greater heterogeneity in ecosystems services makes it even more difficult to produce estimates of the benefits from their protection than for the protection of human health. The input of the new Science Advisory Board Committee on Valuing the Protection of Ecological Systems and Services (CVPESS) and a new Council Ecological Effects Subcommittee (EES) may be able to stimulate the development of greater expertise on this issue than is presently available. Ecological effects to be valued must be limited to those effects for which there is a defensible, rather than just speculative, link between air emissions and service flows. The Council strongly objects to using inappropriate or unsupported placeholder values in the absence of better information.

12. USE OF VSL META-ANALYSES

12.1. Agency Charge Questions Related to Use of VSL Meta-Analysis.

Charge Question 22: EPA's current analytic blueprint calls for an expert-judgment project on VSL determination that would produce a probability distribution over the range of possible VSL values for use in the 812 project. EPA is not sure how much priority to give to this project. A much simpler alternative would be for EPA to specify a plausible range of VSL values. One option would be to use a range bounded by $1 million (based roughly on the lower bound of the interquartile range from the Mrozek-Taylor meta-analysis) and $10 million (based roughly on the upper bound of the interquartile range of the Viscusi-Aldy meta-analysis. This range would match that reflected in EPA's sensitivity analysis of the alternative benefit estimate for the off-road diesel rulemaking. The range would then be characterized using a normal, half-cosine, uniform or triangular distribution over that range of VSL values. EPA would then ask this Committee to review this distribution. This approach could be done relatively quickly, based on the reviews and meta-analyses commissioned to date, and would allow a formal probability analysis to proceed, without suggesting that the Agency is trying to bring more precision to this issue than is warranted by the available science.

Charge Question 23: Pursuant to SAB Council advice from the review of the first draft analytical blueprint, EPA reviewed a number of meta-analyses – either completed or underway – developed to provide estimates for the value of statistical life (VSL) to be applied in the current study. EPA plans to consult with the Council (and coordinate this consultation with the EEAC) on how best to incorporate information from the Kochi et al (2002) meta-analysis, other published meta-analyses (Mrozek and Taylor and Viscusi and Aldy), and recent published research to develop estimates of VSL for use in this study. In addition, EPA plans to implement two particular adjustments to the core VSL values: discounting of lagged effects and longitudinal adjustment to reflect changes in aggregate income. Does the Council support these plans, including the specific plans for the adjustments described in chapter 8? If the Council does not support these plans, are there alternative data or methods the Council recommends?

Charge Question 31: EPA plans to work with the Council and the EEAC to develop revised guidance on appropriate VSL measures. We hope to include the Kochi et al (2002) meta-analysis, other recent meta-analysis, recent publications, and the 3 literature reviews sponsored by EPA. (A separate charge question pertaining to this element of EPA's VSL plan is presented below). In addition, EPA plans to conduct a follow-on meta-regression analysis of the existing VSL literature to provide insight into the systematic impacts of study design attributes, risk characteristics, and population attributes on the mean and variance of VSL. Does the Council support the plans described in chapter 9 for conducting this meta-regression analysis? If the Council does not support this analysis or any particular aspect of its design, are there alternative approaches which the Council recommends for quantifying the impact of study design attributes, risk characteristics, and population attributes on the mean and variance of VSL?

Charge Question 37: Does the Council support including the Kochi et al. (2002) meta-analysis as part of a larger data base of studies to derive an estimate for the value of avoided premature mortality attributable to air pollution? Are there additional data, models, or studies the Council recommends? Does the SAB think that EPA should include Kochi et al. 2003 if not accepted for publication in a peer reviewed journal by the time the final 812 report is completed?

12.2. Summary of Council Response

The Council has combined the responses to charge questions 22, 23, 31, and 37 and has provided additional discussion concerning the use of VSLs in Appendix B of this Council Report. Major summary points appear below.

- Since the Panel's initial receipt of the Analytical Plan, the plan for an expert-judgment project on VSLs has been dropped from the blueprint. The expert elicitation exercise is no longer an active portion of this charge question.

- Uncertainty analysis with respect to VSL values requires information about the distribution of VSL estimates corresponding to risks and populations that are similar to those relevant for the CAAA. The univariate distribution of all empirical VSL point estimates derived across all contexts is unlikely to be appropriate for this purpose, as is any arbitrary convenient distributional shape.

- Discounting of lagged effects is advisable, but the literature on discount rates for future financial outcomes and future health states is not clear on whether straightforward discounting using an exponential model and a common rate will be appropriate. Sensitivity analysis and caveats are recommended.

- Adjustments for future changes in aggregate income levels are being based on very limited empirical evidence and should be considered placeholder efforts at present. It would be preferable in the future if these adjustments were made in the context of a formal model of preferences and the relevant elasticities. Placeholder efforts should be clearly identified as such, and accompanied by strong caveats. The First Prospective Analysis included (in an Appendix) estimates allowing income growth. This type of analysis may be a candidate for the recommended "exploratory" or preliminary analyses discusses earlier.

- The Panel recommends a primary focus, at this juncture, on the Viscusi-Aldy estimates based on U.S. studies, although work in the direction of the Kochi et al. analysis should be encouraged. Preferably, the variance estimates should be based on the variance in the conditional expectation from the model, for a set of conditions that most closely approximate those relevant for the CAAA.

- It is certainly reasonable to expect that the Second Prospective Analysis would consider insights derived from the other VSL meta-analyses (e.g. Mrozek and Taylor, and Kochi et al.). The Council recommends that, to the extent VSL measures are developed as conditional expectations from a meta-analysis, they should rely primarily on published peer review studies. As the Council's general comments on approaches to methodological innovation imply, the meta-analyses that best serve Agency needs will not always be published.

- Continual evolution of the relevant literatures justifies development by the Agency of a more formal laboratory phase for evaluation of potential methodological innovations. A "satellite benefit-cost analysis" based on updated methodologies could serve as a forum for evaluation of new methods before these innovations are formally and widely adopted by the Agency for the Section 812 Analyses and other analyses.

12.3. Expert Judgment - VSLs

The Agency desires to bound the range of plausible VSL values between $1 million and $10 million, which seems reasonable given the state of knowledge about empirical values in different contexts. This range, however, represents the marginal distribution of VSL estimates aggregated across values that have been determined in very different contexts. The ideal VSL distribution to employ would be the conditional distribution of VSL values, derived for contexts that most closely match the risks and affected populations relevant to the CAAA. This VSL does not necessarily lie in the middle of the overall marginal distribution of empirical VSL estimates across the broad range of contexts in the literature.

Some VSL distribution is needed from which to draw alternative point values of the VSL for simulations of the effect of uncertainty about VSL values. However, the Council Special Panel does not agree with arbitrary assignment of some convenient distribution (e.g. normal, half-cosine, uniform or triangular) for the range of values. Why not compare Mrozek-Taylor versus Viscusi-Aldy meta-analyses, including the latter's re-estimates with a sample consisting of one observation per study? Use these estimates to derive an appropriate mean and variance of the relevant conditional distribution from that model "configured" for the policy analysis. The idea is to narrow the range of plausible VSL estimates to reflect more closely the risks and affected populations for the policies in question.

12.4. Adjusting for latencies, income growth?

Latency in health effects, as well as cessation lags, mean that a comprehensive assessment of mortality risk reduction benefits must take into account individual discounting. In discounting individual health effects, there remains an important question as to whether the usual convenient exponential form of discounting is an appropriate assumption, given the numerous empirical anomalies. There are also unresolved questions about the difference in discount rates concerning future health, as opposed to future financial status. While the Council concurs that future benefits need to be discounted, there is no consensus in the literature concerning how to do this. As a practical matter, pending additional research, the Agency should adopt discounting assumptions that are consistent with the rest of the Analytical Plan and include sensitivity analysis and caveats.

The Panel does not support the use of the proposed adjustment for aggregate income growth. This is arbitrary and inconsistent with VSL as a marginal rate of substitution (MRS). The Council acknowledges that, in principle, demands for environmental risk reductions (like demands for all other goods and services) are likely to vary systematically across individuals with such factors as income, age, gender, ethnicity, or a host of other variables. However, empirical evidence based upon utility-theoretic specifications has not yet been amassed to a point where there is any professional consensus as to the precise way in which demand for risk reductions varies with these factors. The Council also acknowledges methodological change without full vetting and review runs the risk of creating an appearance of manipulation. Thus, it is imperative that the Agency substantiate any adjustments before attempting to incorporate them in the Section 812 Analyses.

The Agency needs to be aware that there are some important subtleties concerning income in revealed preference derivations of the marginal rate of substitution between risk reductions and income. Income adjustments to VSLs (or equivalently to marginal rates of substitution) require very stringent approximations. While empirical evidence for income effects is substantial, it is generally derived from *ad hoc* reduced-form specifications, rather than any formal theoretical basis.

Nonetheless, it remains clear that the Agency should take into account that, over time, average real incomes are likely to grow. The Agency should continue to consider ways in which to capture overall real income growth. Unfortunately, most of the literature on income elasticities in VSLs is not based upon a framework that produces reliable estimates of what adjustments should be made in the aggregate, over time. The Council cannot support the proposed adjustments for aggregate income growth as being theoretically consistent.

Any income adjustments in the present analysis fall within the category of satellite or exploratory analyses that may be developed as supplementary to the primary analysis. As such, they would be intended to stimulate discussion and review, rather than constituting a primary component of an analysis intended to be used in evaluating a policy. In any provisional analysis, it may be possible to place bounds on the likely errors that would accompany simple approximations to likely income effects. If an adjustment of this type is considered essential even at this stage in the analytical process, the Agency should be especially prudent in qualifying it and present the results in a format that is as transparent as possible. This would include explaining in detail how any income adjustments have been accomplished and why they are deemed to be necessary.

It is worth emphasizing that as soon as the Agency begins to manipulate VSL estimates to reflect anticipated changes in real incomes, it opens the door to arguments that VSLs should also be adjusted for other long-run changes. These might include other changes in budget constraints, such as alterations to the relative prices of medical care. Or, they could include shifts in typical indicators of preferences, such as trends in the sociodemographic mix in the population (e.g. changes in the age distribution).

The Agency should also be aware that if VSLs are to be adjusted for income growth, so should be all of the other demand-based benefit measurements entertained in the Section 812 Analyses. It may be difficult to defend making income-growth adjustments only to one component on the benefits algebra.

In the longer term, consideration should be given to obtaining income-based adjustments to VSLs (or even other types of adjustments) through preference calibration techniques. These methods hold promise for generating forecasts that are consistent with the relevant elasticities (see Smith, Pattanayak, and Van Houtven, 2003).

12.5. Available meta-analyses

Three meta-analyses were discussed in EPA's evaluation of summary measures for the available VSL estimates (Mrozek and Taylor, 2002, Viscusi and Aldy, 2003, and Kochi, Hubbell, and Kramer, 2003). The studies differ in several key respects, including:

a. The number of observations included from each study;
b. The format of the observations (e.g. actual estimates, use of group means, and other transformations of the primary estimates);
c. The sample composition – U.S. studies, international, revealed and stated preference;
d. The set of independent variables used for controls (e.g. inclusion of industry effects);
e. Bayesian means versus regression summaries;
f. Published versus unpublished summaries.

The background for the charge questions tends to focus attention on the selection of a single study as a summary for developing for the Prospective Analysis "one" VSL estimate of reductions in mortality risk. However, the charge questions explicitly refer to the "systematic impacts of study design attributes, risk characteristics, and population attributes on the mean and variance of VSL." The earlier meta-analysis strategies tended to miss the opportunity to combine the insights from all studies to influence how summary measures are constructed and used. We recommend that serious consideration be given to using these insights in adapting how any meta-summary is used.

Equally important, the sensitivity of VSL estimates from meta-summary equations to the sample composition (i.e. which studies are included) and to the controls used (i.e. which study features are explicitly modeled) suggests that it would be prudent to use the resulting lessons from this research in at least three ways:

a. If one study, such as the Viscusi and Aldy (2003) meta-analysis, is selected, evaluate the sensitivity of the conditional expectation to the baseline risk and other control variables selected in measuring the conditional prediction.

b. Evaluate the variance in the conditional prediction as a function of the values for the independent variables included in the model in relation to the mean values for these variables for the sample used to estimate the model.

c. Consider the effects of inclusion or exclusion of independent variables or observations on the coefficient estimate for the risk measure. The data sets used in these studies are generally available for attempts at replication, so this type of comparison can be readily undertaken and would permit evaluation of the sensitivity of the VSL estimate to assumptions made, based on the available literature.

In general, it does not seem prudent to extend the sample to include studies for labor markets outside the U.S. The terms of employment, information about safety conditions, fringe benefits (e.g. health insurance), etc. are likely to be so different that one could not be sure that differences attributed to income or risk levels were in fact due to these variables.

12.6. Interpreting CV measures as opposed to wage-risk measures

One advantage asserted for the Kochi et al. study is the inclusion of contingent valuation (CV) evidence concerning VSLs. However, there is an important issue that has not been adequately discussed when CV results are included with revealed-preference wage-risk results concerning VSLs. The CV based measure of the VSL implicitly accepts a proportionality assumption between ex ante willingness to pay and the risk change.

The proper theoretical interpretation of the CV measures is as an *ex ante* option price for a risk change. If OP denotes the value for a risk reduction from P0 to P1 (with P1 < P0), and the P's designate the probability of death before and after the risk reduction, theory implies:

$$OP = f\,(P0\ ,\ P1\ ,\ \text{and other variables})$$

The comma between P0 and P1 implies that linear proportionality in (P0 - P1) is an approximation, not a feature implied by theory. Thus, to rewrite equation (1) as equation (2) below, where the option price associated with a risk reduction is proportional to the size of the risk reduction (as well as being a function of a number of other variables) and then to approximate VSL as in equation (3) by normalizing upon a 1.00 risk change, adds additional untested assumptions.

$$OP = (P_0 - P_1) \cdot g(\text{other vari ables})$$

$$VSL \approx \frac{OP}{(P_0 - P_1)} = g(\text{other vari ables})$$

A meta-analysis that includes CV studies to expand the range of risk changes (or the types of risks considered) will accomplish this objective. However, it also changes the summary measure from an ex ante marginal rate of substitution to a linear approximation. Unfortunately, this added condition makes it difficult to evaluate whether the resulting differences in summary results between CV and wage-risk studies should be attributed to these additional assumptions implicitly added to the model, or to the expansion in the range or types of risks.

Nevertheless, the Council recognizes that CV-based studies offer unique opportunities to examine the empirical influence of many additional factors on the resulting estimates of VSLs. Despite the potential difficulty in rendering their findings compatible with those from revealed-preference wage-risk studies, CV studies have the potential to make important contributions to our understanding of how consumers value risk reductions, and it is important to take advantage of these opportunities.

12.7. Emerging considerations

As recent unpublished research by Cameron and DeShazo seems to suggest, the terms identified in equations (1), (2), and (3) above, and other things, may well be very important to the *ex ante* option price measured for the risk change. This research is presently available only as early reports from a detailed contingent valuation study. Nonetheless, it reaffirms the notion that it may be important to evaluate the sensitivity of the conditional expectation of the VSL to the conditioning variables used in its construction.

The Council's discussion also supported efforts to refocus attention on incremental willingness to pay for an incremental risk change, rather than the traditional, but potentially confusing construct that is a VSL. The panel's discussion urged EPA to consider including a preamble on the concept that is sought as a benefit measure, its likely link to the conditions of daily living and illness preceding death, as well as to any latency and temporal issues associated with exposure and increased risk of death.

The Panel recognizes that the current state of research makes it unlikely that empirical measures can imminently be developed that reflect all of these concerns. Nonetheless, the discussion led to a consensus that the Panel should urge Agency staff to consider careful qualification and sensitivity analysis for the measure used to monetize mortality risk reductions.

12.8. Which meta-analyses to use

In general, the Council Special Panel recommends that the Kochi et al. meta-analysis should not be given any particular prominence among the alternative meta-analyses used for determining one appropriate measure to use for the VSL. There are several reasons:

a. The Kochi study is still unpublished. While it can sometimes be difficult to publish further meta-analyses when others are already in the literature, the Agency should not rely disproportionately on the Kochi study before it has been thoroughly peer-reviewed. The standards for peer-review obviously differ across journals and even across reviewers, but reliable peer-review can also be accomplished outside of the journal publication process. Both Mrozek and Taylor (2001) and Viscusi and Aldy (2003), however, have already appeared in the peer-reviewed literature.

b. There are problems in the derivation of the variance of the VSL estimates. Some appear to be typographical errors. The researchers apparently faced some problems in terms of unobserved (or unreported) covariances among parameter estimates. However, it might be possible to derive estimates of variance in mean annual wage from the current population survey (CPS) or other sources, and use this information to fill in some of the blanks. It is not clear whether one should use a predicted wage or an actual mean wage. Overall, this is a careful study but, like all meta-analyses, it needs to address the potential impact of some of its key assumptions on the results of the analysis before it is possible to assess their importance.

c. The use of author-specific means of VSL (p. H-12 to H-13) is troublesome if the different estimates have been derived from different samples.

If called upon to recommend just a single meta-analysis at this point, the Council Panel would recommend a primary focus on the Viscusi-Aldy estimates based on U.S. studies. However, as the 812 process evolves over time, the Council has recommended a commitment to Satellite or provisional analysis to test new methods in a policy relevant format. This would assure that the Agency did not miss opportunities to incorporate insights from new research as it emerges. It would also signal a commitment to understanding the full implications of methodology change before it was adopted as the "Agency Practice."

Finally, variance estimates for the VSL measures predicted for a risk context and an affected population similar to those relevant to the CAAA should be based on the variance in the conditional expectation from the model.

12.9. Unpublished meta-analyses?

The Council was asked explicitly to address the question of unpublished meta-analyses. In general, we believe a peer-reviewed study will have greater professional credibility than one that has not met this standard. The Panel has some reservations about basing an analysis with the gravity of the Second Prospective Analysis on unpublished research, but has even greater reservations about using entirely non-peer-reviewed research. Each of the available meta-analytic studies has different advantages and shortcomings so that no single study should be the sole basis for information about the distribution to be used for the VSL in the Second Prospective Analysis.

This is another reason for creating an ongoing commitment by the Agency to engage in activities that serve as laboratories for methodological developments. Based on innovations in the literature, new methods and new meta-analyses will continue to be developed and applied to policy issues. First, they should be used for evaluative purposes. Results designated as explicitly as "exploratory" can be disseminated in Agency working papers to evaluate the implications of new proposals for analysis. This process serves a role that parallels the peer review process. However, it is more focused and relevant to Agency needs because the appropriate policy context is being considered. These satellite benefit cost analyses could then provide a forum for exchange and evaluation of new methods before they are formally adopted for specific analyses that would be submitted as the Agency's official evaluation of a proposed regulation.

APPENDIX D: ADDITIONAL DISCUSSION CONCERNING THE USE OF VSLS

This appendix covers material that can be classified as "experimental" or "methods development." It emphasizes some shortcomings of existing practices with respect to VSLs. The Agency is advised to anticipate changes in the state of the art in human health benefits valuation that may be appropriate to incorporate in future 812 analyses as these updated approaches are vetted and as the justification for them becomes more widely understood.

The Council first wishes to highlight persistent conceptual problems stemming from the use of "the VSL." Normalizing WTP to a 1.00 risk reduction is arbitrary and has proven to be confusing to non-specialists and therefore open to being used in a strategically misleading fashion. As a device for combining WTP estimates based on different risk changes, any arbitrary normalization is equally appropriate and a more policy-relevant risk change would be preferable for normalization, even if this necessitates a change in traditions.

That WTP should be close to proportional to the size of the risk change has theoretical support and would be enormously convenient. However, empirical tests of this theory are very difficult with hedonic wage data and contingent valuation studies tend to produce results at odds with this assumption. More information on this important aspect of VSL implementation would be valuable.

WTP for risk reductions should be presumed to be heterogeneous across risks and individuals, unless demonstrated otherwise. It is important that the proposed meta-analyses are designed to recognize this.

Existing meta-analyses have tended to maintain the hypothesis that there exists a single immutable VSL (or a simple VSL function that depends mostly on income levels). The early Agency posture suggested that this unknown VSL merely needed to be revealed by somehow combining VSL estimates from different studies.

The studies that form the raw material for meta-analysis may be compromised to varying degrees by their subjects having had incomplete information about risk. Credible meta-analyses should address these problems as well.

The Agency should proceed cautiously in adopting the results of existing or new meta-analyses as the basis for some assumed distribution for the WTP that will be appropriate for the Second Prospective Analysis. The contexts of the constituent studies may not adequately match the policy context where the WTP is needed.

D.1. VSLs vs. Micromorts

The concept of the value of a statistical life has unnecessarily impeded clear communication with risk managers about the public's value for small changes in health risks. However, the Council acknowledges that it is not in the Agency's best interest to attempt to take the lead by proposing fundamental changes in the way economists traditionally have thought about valuing mortality risks. Such initiatives properly comes from the academic community. However, the Council wishes to draw the Agency's attention to ideas and approaches that are likely to develop in the literature over the next few years. Even without adopting a substantially different perspective on mortality risk valuation, the Agency can report mortality values in ways that are less susceptible to misinterpretation by non-experts in the constituency for the Section 812 reports. Specifically, the Agency should exercise more precision in describing and qualifying the measures of mortality risk reduction it currently uses. Whenever the concept of a VSL is introduced, the Agency should identify the VSL explicitly as a normalization relative to a particular baseline risk. The corresponding range of untransformed WTP estimates for the policy-relevant range of risk changes should be provided for comparison.

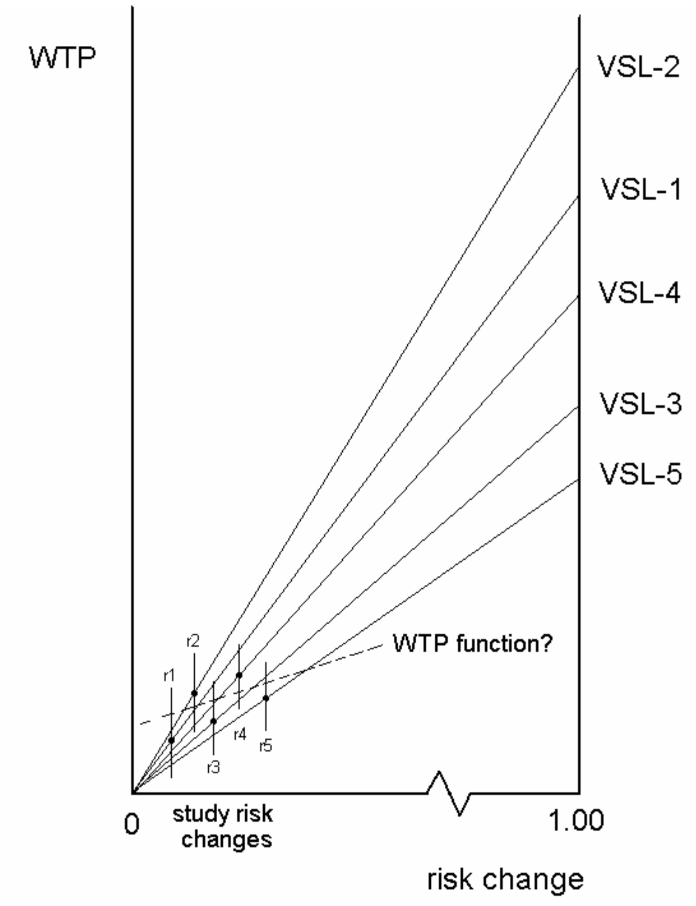
VSL is defined as the marginal rate of substitution (MRS), namely the (local) difference in income that will leave an individual equally well off in the face of a difference in mortality risk. It is well recognized in the literature that this MRS depends on baseline risk, income, and may well depend on other characteristics of the risk and the individual. The units in which this MRS is described are arbitrary (e.g., dollars per pound, pennies per ton, etc.). By focusing on "the Value of a Statistical Life," we have arbitrarily adopted as our units "dollars per 1.00 risk change."

The population WTP for a specified risk reduction is defined as the sum of individuals' WTP for the individual risk reductions. For example, if a policy change reduces fatality risk this year by Är for everyone in a population of size N, the population WTP for this change can be calculated as vN, where v is the population average WTP for a Är reduction in the chance of dying this year. This same population value is often described as the product of the average VSL and the expected number of "lives saved" by the risk reduction. Using the normalization of dollars per 1.0 risk change, VSL is defined as v / Är, and "lives saved" is equal to the expected number of deaths averted this year, i.e., N Är.

While this alternative formulation, in terms of the average VSL and the number of "lives saved," is mathematically equivalent to the population WTP (i.e., the product of the average WTP and the population size), it is potentially misleading. It suggests that the value of each "life saved" is equal to the average VSL, and that one only needs to know the expected number of "lives saved" in order to calculate population WTP. In addition to other factors, VSL is likely to depend on the size of the individual risk reduction Är, and so the population WTP for a change that "saves one life" may depend on whether the change reduces many people's risk by a small amount or reduces a small number of people's risk by a large amount.

The arbitrary choices made with respect to the normalization of VSLs unnecessarily court objections from non-specialists who confuse "The Value of a Statistical Life" (the economists' technical term for an extrapolated linear approximation to a marginal measure) with "The Value of Life" in the sense of some measure of the intrinsic value of one human life with certainty. Long ago, Ron Howard (1984) proposed the term "micromort," meaning the value of a one-in-a-million risk reduction, which would translate into one one-millionth of our usual $5-6 million VSL, or just 5 to 6 dollars. This metric would be less misleading than the VSL, but unfortunately it has never achieved currency. There is no imperative to choose a 1.00 risk change as the intervening metric for scaling. Scaling all estimates to the risk change relevant for some specific policy is just as valid, and would lead to the identical mathematical result for aggregate WTP for a risk reduction policy.

There are other potential concerns about empirical measures of WTP for risk reductions. Suppose that we are trying to combine the information about WTP for risk reductions from five different studies, each involving one particular (different) risk reduction, r1 through r5, as in the figure. (With any luck, there will be standard errors on the underlying WTP estimates, as shown, so there will be corresponding standard errors on the resulting individual studies' estimates of VSLs, although these are not depicted in the diagram.)



If we use the WTP and risk information from each study to impute the associated VSL for a 1.00 risk change, the numbers may vary widely, as shown. It is these different VSL estimates that most meta-analyses seek to "average" according to formulas of different complexity and sophistication. By taking some type of average of the five separate VSLs, we can infer an average WTP for risk reductions that controls for the different risks across studies. However, if the true WTP function tracks along the dashed line, and if the policy context concerns a risk change that is, say, slightly larger than r5, then the WTP that would be inferred from the average VSL would be an inappropriate estimate.

The individual WTP point values depicted in the diagram may also differ because of other types of heterogeneity across the contexts wherein they were derived. In that case, it would of course be inappropriate to average these results, even after normalization to a common 1.00 risk change.

VSLs are based on empirical data concerning choices in the neighborhood of very small risks and small risk differences. Outside of this domain, we can really say nothing about WTP for much larger risks and risk changes. The implicit extrapolation to a 1.00 risk change that produces a VSL is understood by specialists to be purely a convenient device to control for variations in the sizes of risk reductions across the studies that yield these estimates. Unfortunately, this is often not understood as such by non-specialists.

D.2. Proportionality

The VSL can be viewed simply as a strategy for getting around the fact that WTP from different studies corresponds to different sized risk changes. It would be inappropriate to average the individual WTP estimates without acknowledging that they apply to different risk changes. The issue of proportionality of estimated WTP for risk reduction and magnitudes of these risk reductions has been raised previously (e.g. Hammitt and Graham, 1999). Certainly, if we wish to maintain the hypothesis that there exists a single one-size-fits-all VSL that is the same for all possible risk reductions, then the estimated WTP for different risk reductions ought to be proportional to the sizes of the risk reductions in question. This constitutes a requirement for a very specific type of "scope test." However, not all empirical estimates of WTP functions produce parameters that are consistent with this requirement. Some studies show negligible effects of risk changes on WTP. Such a result that is clearly problematic for valuing mortality risks. However, other studies reveal estimates that suggest that WTP is not strictly proportional to the size of the risk change.

Stated-preference (e.g. contingent valuation) studies almost invariably show that WTP is an increasing but concave function of risk reduction. Revealed-preference studies (e.g., hedonic wage studies) typically do not tell us anything about how WTP depends on the magnitude of the risk change because we model workers as choosing jobs from a continuous set of jobs that differ in wage and risk, and typically do not have information on what jobs (and risks) and individual rejects.

For example, compensating-wage-differential estimates are based on fitting a regression model to data on individual workers' wages, occupational fatality risks, and other variables such as education and job experience that influence wages. This regression estimates how wages vary with occupational fatality risk, holding other factors constant. Each worker is assumed to prefer the job he holds to other jobs that are potentially available to him, which are characterized by the regression. Setting the independent variables equal to the worker's characteristics, the regression is interpreted as describing how the set of jobs available to him differ in wage and risk.

Many of the studies that yield WTP estimates do so for only a single common risk difference for all subjects, so there is too little information in any single study to assess the effect of the size of the risk change on WTP. Some sort of preference calibration exercise would be necessary in order to combine all of the available estimates.

<u>D.3. Heterogeneity: Context-dependent WTP</u>

Many practitioners seem to lose sight of the subtlety that the VSL is not a physical constant, like the constant of gravitation $(6.673 \pm 0.003) \times 10^{-8}\ cm^3 gm^{-1} s^{-2}$, or the mass of a hydrogen atom $(1.67339 \pm 0.0031) \times 10^{-24}$ g. Instead, VSL is an artifact of human preferences. It is based on willingness to pay for risk reduction, which depends on the marginal (dis)utility of risk and on the marginal utility of income. While it may be possible to identify some regularities across types of people in these two marginal utilities, it is conceivable that they are essentially unique to each person. Therefore, so can be the corresponding VSL.

The contexts for empirical studies concerning risk tradeoffs differ in many more ways besides just the risk change they consider. The types of risk and the characteristics of the individuals experiencing these risks can also lead to heterogeneity in WTP. If the policy context is not "in the middle" of the range of study contexts, then it can be potentially very misleading to assume that the "average VSL" implied by the range of available studies is a good measure of WTP to reduce the specific risk in the specific affected population for the policy under consideration.

The Council agrees that it is important to look at how estimated VSLs depend on characteristics of the individual (e.g., age, life expectancy), characteristics of the risk (e.g., latency, accompanying morbidity, voluntariness), and any other relevant factors. To the extent that WTP may not be a precisely proportional function of the size of the risk change, it will also be important to look more closely at the relationship between WTP estimates for different studies, concerning different specified risk changes, and to assess whether the proportionality assumption is generally tenable.

<u>D.4. Problems with Meta-analyses</u>

The meta-analysis in the Kochi paper, like many other meta-analyses, is premised on the assumption that there is a simple VSL relationship that is merely revealed with different degrees of bias and noise by different studies. At best, unfortunately, the underlying construct is probably a complex VSL function. This function has many, many arguments. VSL is known to depend on the nature of the risk (severity, latency, voluntariness, etc.) and on the attributes of the individual who is considering this risk (age, gender, health status, etc.). VSL is also likely to depend upon the manner in which the demand information behind it is elicited (from self-selected employment decisions, housing choices, stated preference surveys, etc.). If only this last source of heterogeneity existed, we might be confident that techniques for pooling VSL estimates across studies would be a sensible exercise. Unfortunately, we can be fairly confident that there is fundamental heterogeneity in preferences with respect to risk, so that there is no reason, a priori, to expect that any summary statistic across studies corresponds to any single underlying "true" VSL.

The distribution of VSLs to be "averaged" in a meta-analysis is an artifact of the range of contexts (types of risks and affected populations) analyzed in the list of studies contributing to the meta-analysis. If this distribution of contexts does not correspond to the context pertinent to the environmental policy in question, then the "meta-analysis VSL" may have little to do with people's willingness to pay the costs of this policy.

D.5. WTP and Incomplete Information

It is important to recognize two explanations for why people's empirical decisions about mortality risk may differ from conventional theory: (1.) the individuals may be ill-informed or may make mistakes (e.g., cognitive errors), and (2) the theory may be oversimplified or wrong. It is likely that most people would like to make decisions in a way that optimizes their risk reduction spending (i.e., equal marginal spending per unit risk reduction) across various domains (e.g., housing, employment choices). However, they do not do so in practice because of information limitations and well-known errors in decision making about risk.

Some published research has made an attempt to sort out which of the factors that lead to differences between perceived risk and simple theory are simply cognitive errors (e.g., susceptibility to framing effects), and which are attributes of preferences potentially meriting normative recognition (e.g., distribution of benefits and risks of activity; such as voluntariness) (see Hammitt, 2000b).

In general, economists are inclined to defer to "consumer sovereignty" in measuring the types of tradeoffs people are willing to make. In the event of misinformation or cognitive problems, however, good policy should probably over-ride consumer errors where possible and simulate what would have been consumers' WTP under similar conditions, but with complete and accurate information.

D.6. What to do in the near term

The Agency needs to verify that the distribution of risk reductions over which each meta-analysis has been estimated, and the context for these reductions, at least corresponds to the types of risk reductions relevant to the Clean Air Act and its amendments. The Panel continues to support meta-analyses of willingness to pay for risk reductions, but discourages the Agency from leaving the impression that it is searching for a single one-size-fits-all VSL. Instead, it should be a maintained hypothesis that heterogeneity matters. Heterogeneity should be ignored only if it can be shown to be inconsequential. The benefits from mortality (and morbidity) risk reduction attributed to a particular policy should be commensurate with the size and nature of the risk reduction and with the attributes of the affected populations.

It seems worth speculating that researchers' habit of talking in terms of conventional VSLs has much to do with the recent public relations problems concerning the "senior death discount." This different VSL for seniors was embodied in the alternative net benefits calculations associated with some recent analyses by the Agency. The public backlash to this differential seems to have been attributable almost entirely to the use of the VSL concept, which led the public to think that the issue at stake is the "*value* of a senior." In reality, the issue at stake is much closer to "how much money should seniors be required to pay for small risk reductions." It is essential to steer the press and the public towards the legitimacy of individual preferences and the corresponding demands (consumer sovereignty), rather than sticking with the arbitrary unit choice that expresses a marginal rate of substitution between risk changes and income as the "value of life." The word "value" is assumed by non-economists to be something intrinsic. Demand for risk reductions is not intrinsic and immutable, independent of context. It is subjective and individual, and measured differences in this demand across subpopulations and risk contexts should be honored wherever they are verifiable and based on complete information about those risks.

If WTP for small risk reductions can be shown to be approximately proportional to the size of these risk reductions over the relevant domain of the WTP function, the Panel believes it would be less inflammatory to present the marginal rate of substitution expression in terms of risk changes of a size that are pertinent to policy choices. The Panel recommends that the Agency consider converting VSL estimates into units with a less potentially misleading denominator (micromorts, millimorts, picomorts, etc.) and presenting these estimates in tandem with ordinary VSL estimates, if not in lieu of them.

APPENDIX C

How Robust Are Hedonic Wage Estimates of the Price of Risk: The Final Report

by Dan A. Black, Jose Galdo, and Liqun Liu

How Robust Are Hedonic Wage Estimates of the Price of Risk?

The Final Report

by

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I. Introduction

At least since Adam Smith's *Wealth of Nations*, economists have recognized that workers require compensation to accept the risk of death or dismemberment on the job. While this wage premium provides employers with incentives to reduce the risk on the job, the calculus of the marketplace allows workers and employers to trade the costs of reducing workplace risk against the benefits associated with the reduction.

This calculus, when applied to large numbers of workers, allows a researcher to calculate the value of a statistical life, or the wage reduction associated with reducing the expected number of deaths by one worker. As this value represents the amount of wages that workers are willing to forgo to reduce risk, the value of a statistical life appears to be a useful tool for evaluating individuals' willingness to pay for reductions in risk in other areas. Indeed, it is a measure of the price of risk. The Environmental Protection Agency (EPA) often considers regulations that both impose costs on industry and reduce the deaths from environmental contamination. While the costs may often be calculated with a great deal of accuracy, the problem for policymakers is to value the corresponding benefits. The price of risk appears to be a useful tool for such evaluations.

When basing policy on estimates of the price of risk, the precision and accuracy of the estimates become of utmost importance. Yet, Viscusi (1993), in his review of labor market studies of the value of life, reports that the majority of the estimates are in the $3 to $7 million range [in December 1990 dollars, p. 1930], and this range excludes studies that Viscusi felt were flawed. While this represents over a 133 percent variation, Viscusi correctly notes that much of the variation should be expected, as the studies used different methodologies and different samples. Workers may differ in their attitudes toward risk, and the mixes of workers in these various studies differ substantially. His review, however, leaves unanswered how much of this variation results from differences in the sample of workers, measures of job risk, and the specification of the estimating equation.

In this report, we use three data sets to estimate the price of risk: the Outgoing Rotation Groups of the Current Population Survey, the March Annual Demographic Supplement of the Current Population Survey, and the National Longitudinal Survey of Youth (1979). Labor economists frequently use these three data sets to estimate wage equations. We match these data to two sources of job risk data: the Bureau of Labor Statistics estimates from their Survey of Working Conditions and the National Institute of Occupational Safety and Health estimates from their National Traumatic Occupational Fatality survey. We then use these data to estimate the price of risk. Among our major findings are:

- First, and foremost, the estimates are quite unstable. Small changes in the specification of covariates or the risk measured used result in large variations in the estimated price of risk. Many of the estimates indicate that the price of risk is negative, which is contrary to the theoretical framework used;
- The instability of the estimates does not appear to be the result of the misspecification of the functional form of the regression function. More flexible functional forms of the regression function provide similar estimates to similarly specified OLS equations, and the more flexible functional forms also produced unstable estimates when changing the covariates or risk measures. In our view, therefore, the instability is not the result of equation misspecification;
- We find overwhelming evidence that the job risk measures contain much measurement error. Moreover, the measurement error is nonclassical, as it is correlated with covariates that are usually put into earnings of wage equations. Estimates that do not account for such measurement error may be highly attenuated, which would cause an understatement of the value of risk reduction.

There may, of course, be other biases that offset the attenuation that usually occurs with severe measurement error;

- We find some evidence that job risk is correlated with characteristics not commonly available in labor economic data sets. Using data from the NLSY, we find that job risk varies inversely with Armed Forces Qualification Test scores and varies positively with illegal drug use. This suggests that job risk may be correlated with regression error, rendering OLS estimates inconsistent.

Collectively, these findings lead us to have severe doubts about the usefulness of existing estimates to guide public policy. These estimates are so highly sensitive to the risk measure used and the specification of the wage equation that the selection of any particular value of the price of risk seems arbitrary.

The rest of this report is structured as follows. The next section contains all of the estimates. The first subsection presents the basic estimation results, while the second subsection examines the sensitivity of estimates to assumptions about functional form. The third subsection examines the extent and impact of measurement error; the fourth subsection looks at possible correlations between the job risk measures and the regression error. In the final section, we offer some brief concluding comments.

II. Estimating the Price of Risk

A. Baseline Estimates

The basic notion of hedonic models of risk is to ask the question: "All else the same, how much must I compensate a worker to accept an increase to the risk in the worker's job?" The model is conceptually very simple. Consider a worker who faces a risk of death on-the-job, denoted r^*, and is paid a wage w. Given a von Neumann-Morgenstern expected utility function, we may characterize the expected utility function of the worker as:

$$E(U) = (1 - r^*)U(w \mid X) + r^* D$$

where D is the disutility of death, $U(w \mid X)$, is the utility of earning a wage w, and X is a vector of all other factors that affect the worker's utility. Von Neumann-Morgenstern are unique up to an affine transformation so that we may add or subtract a constant of $-D$.[1] This allows us to rewrite the above equation as:

$$E(U) = (1 - r^*)u(w \mid X) \ ,$$

where $u(w \mid X) = U(w \mid X) - D$.

If we hold constant the expected level of utility, we may ask "How much must we compensate the worker in order for the worker to accept an increase in job risk?" The answer to that question in differential form is simply:

$$dE(u) = 0 = -u(w \mid X) + \frac{\partial u(w \mid X)}{\partial w} \frac{\partial w}{\partial r^*},$$

or

$$\frac{\partial w}{\partial r^*} = \frac{u(w \mid X)}{\partial u(w \mid X) / \partial w} = \varphi(w, X) > 0 \, .$$

In general, the slope of the wage-risk locus depends on the base level of utility, the vector of covariates X, and the level of wages. In general, theory offers no guidance as how to specify the $\varphi(w, X)$ function. A particularly convenient form is $\varphi(w, X) = \gamma w$ so that we have:

$$\frac{\partial w / \partial r^*}{w} = \gamma \, .$$

This form is particularly convenient because it arises from the well known Mincerian equation

$$ln(w) = X\beta + r^*\gamma + \varepsilon_i$$

[1] The variable y is an affine transformation of x if $y = a + bx$, where $b > 0$ which is required to maintain the preference ordering.

While this is the starting point for virtually all hedonic labor market studies, it is worth noting that it is based on a very strong assumption that $\varphi(w, X) = \gamma w$.

Thus, the starting point for our analysis is a wage equation of the form:

$$ln(w_i) = X_i\beta + r_i^*\gamma + \varepsilon_i \qquad (1)$$

where $ln(w_i)$ is the natural logarithm of the *ith* worker's wage, r_i^* is the measure of risk (potentially a vector), X_i is a vector covariate, (β,γ) are coefficients to be estimated, and ε_i is the error term of the regression. We assume that $Cov(X_i, \varepsilon_i) = 0$ and $Cov(r_i^*, \varepsilon_i) = 0$ (so that the risk measures and other covariates are exogenous). This form of the wage equation is what Viscusi (1993) calls the "basic approach in the literature" and admits a natural interpretation for γ as the price of risk.

We begin our analysis by estimating equation (1) with Ordinary Least Squares (OLS). We report estimates with three different samples of workers: the Annual Demographic Survey (or March) Current Population Survey (March CPS), the Outgoing Rotation Groups of the Current Population Survey (ORG CPS), and the 1979 panel of the National Longitudinal Survey of Youths. The March CPS and the ORG CPS are nationally representative samples of workers, while the NLSY is a rich panel data set of individuals aged 14 to 21 in 1979. We describe the data sets in more detail in the Appendix B.

There are two major sources of government-reported job risk: the Bureau of Labor Statistics (BLS) estimates from their Survey of Working Conditions and the National Institute of Occupational Safety and Health (NIOSH) estimates from their National Traumatic Occupational Fatality survey. The NIOSH data provide one-digit occupation or industry mortality rates by state. While the BLS data contain counts of deaths by three-digit occupation or industry codes, they do not provide any regional variation. The risk measures have their own distinct costs and benefits. The BLS data, available from 1995 to 2000, contain very detailed measures of the annual number of deaths, but the data suppression procedure requires at least 5 deaths in a cell before the number of deaths is reported.[2] Thus, there are a substantial number of missing values for these data. The use of annual data may be subject to a great deal of sampling error associated with the annual fluctuation in the number of deaths. Moreover, these data only provide the counts of the number of deaths in each industry or occupation. To make these a rate, it is necessary for researchers to estimate the number of workers in an industry or occupation. To estimate these numbers, we use both the March CPS data and the ORG CPS data, but undoubtedly this estimation generates measurement error in our risk measures. Finally, the BLS data assume that job risk is a constant across the country, which clearly is not the case.

The NIOSH data provide fatality rates by one-digit industry or occupation codes by state. It reports 5-year averages from 1981 to 1985, 1986 to 1990, and from 1991 to 1995. Thus, it does not require the researcher to estimate the number of workers in an industry or occupation cell, allows the job risks measure to vary by state, and smoothes much of the sampling variation by using the 5-year average. The use of the 5-year average and the coarser one-digit industry or occupation codes by state reduces, but does not eliminate, the problem of missing values because of data suppression. On the other hand, these data treat police officers and dental assistants as having the same job risk as both are in the one-digit "service worker" occupation. The use of 5-year averages, while smoothing the sampling variation, may miss important time series variation.

Combining the two data sources, we estimate our models for the ORG CPS and March CPS using the BLS data for the years 1995 to 2000.[3] For the NIOSH data, we use the ORG CPS and March CPS for the years 1985 to 1995 and the NLSY data for the years 1986 to 1993. For both the BLS data and the NIOSH data, we estimate separate equations for each sex and use both the occupation and industry risk measure. We limit the sample to workers who are aged 25 to

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[2] The BLS suppresses reports with fewer than five deaths.
[3] We choose not to estimate the NLSY data with the BLS data because of the relatively small sample sizes of the NLSY and the irregular sampling of the NLSY in the late 1990s.

60 inclusive. Because theory provides little guidance as to the exact specification of the X_i we provide several different specification of the vector. For the CPS data sets, our basic specification includes a quartic in the worker's age, a vector of dummy variables that control for education level, union status, marital status, and a vector of variables of controls for the worker's race and ethnicity. We also add additional controls for the worker's firm size, which is not consistently available for the ORG CPS data so it is not used for this data set. In subsequent specifications, we include controls for state of residence, and then one-digit industry and occupation. For the NLSY, the basic specification also includes experience, tenure, and worker's Armed Forces Qualification Test (AFQT) scores.

While all of these covariates have been used in countless wage equations, our use of the state and one-digit industry and occupation variables warrants some discussion. Unfortunately, for the BLS data our measures of job risk are ultimately assigned by the worker's three-digit industry or occupation. There is a long history in labor economics of adding industry and occupation variables as covariates in wage equations. Indeed, in the 1980s and early 1990s there was a very visible strand of literature (e.g., Krueger and Summers 1988) that examined whether the payment of above equilibrium wages could be detected from these controls. This literature has documented very large earnings differentials across industries and occupations that were not explained by the type of covariates we include in our analysis. While a portion of these wage differentials may reflect differences in job risk, undoubtedly a substantial portion of these wage differentials reflect other unobserved features of jobs. Using industry and occupation covariates, therefore, sweeps out a good deal of unobserved heterogeneity. The costs of such controls, however, are that they remove much of the variation in our job risk measures. Of course, a similar problem arises in our use of the NIOSH data when we include both state and one-digit industry and occupation variables. In our view, the inclusions of such controls is crucial to control for such unobserved heterogeneity.

While we use both industry-based and occupation-based risk measures, we would be remiss if we did not comment on the relative merits of the two risk measures. At first glance, the use of the industry measure seems inappropriate. After all, this measure assigns the same job risk to a secretary in the coal mining industry as to the coal miner, clearly overstating the secretary's level of job risk and understating the coal miner's job risk. In contrast, the use of occupational risk would combine the job risk of a secretary in the coal mining industry with a secretary in the insurance industry, presumably a pair with a much more homogeneous job risk. Yet, this line of argument is deceiving. Mellow and Sider (1983) document that industry is measured more accurately than occupation, a point to which we return below.

Finally, for Table 1, we report four specifications. The first specification contains a set of basic controls that are found in most wage equations. While we try to make the various data sets have similar specifications, these basic controls differ across data sets because of difference in the covariates that are available (e.g., the ORG CPS data do not consistently contain a firm size measure and the NLSY data contain the AFQT score of recipients). The second specification contains a vector of dummy variables that control for the worker's state of residence. This allows us to control for some differences in the cost of living, state taxes, worker's compensation programs, and other state programs that may confound the estimates. In the third specification, we add either industry or occupation controls depending on whether we are using the industry or occupation risk measure. When using the industry risk measure, we add one-digit occupation controls, and when using the occupation risk measure, we add one-digit industry controls. (We seek to identify the source of parameter instability with this specification.) The last specification contains both industry and occupation controls.

With this rather long introduction, in Panels 1 through 10 of Table 1A, we present our estimates of equation (1) for men; t-statistics are reported in parentheses. Panel 1 presents the estimates using the March CPS matched to the NIOSH industry risk measure. Each of the estimates provides a positive estimate of the impact of job risk on wages, although the estimates are highly variable. The inclusion of state of residence controls more than doubles the estimated price of risk, the inclusion of the one-digit occupation controls reduces the estimate by more than 40 percent, and the inclusion of the industry controls reduces the coefficient by more than 60 percent.

We may use the estimates to construct a value of a statistical life, although we report the values only when they are positive as the theory predicts. Assume that a worker has wage and salary earnings of \$35,000, about the mean in 1994 for men (\$34,137). We may take the estimates in Panel 1 of Table 1A, divide them by 100,000 (to normalize them back to deaths per 100,000 workers so that we obtain an estimate of the price of risk, γ), and then calculate value of a statistical life using the formula $\$35,000 \times (e^{\gamma} - 1) \times 100,000$ where γ again is the estimated price of risk. In Panel 1, the estimated value of a statistical life varies from about \$3.7 million to \$16.4 million. Half of the estimated values of a statistical life are within Viscusi's range , which is \$3.3 to \$7.8 million when inflation adjusted to 1994, but the estimates vary by over 440 percent.

The range of estimates from Panel 1, of course, gives us pause about the quality of the estimates. Unfortunately, there is strong reason to suspect that these estimates may be capturing variations in working conditions other than simple job safety. Literally hundreds of wage studies have used industry dummies as covariates in earnings or wage equations. While these dummies undoubtedly capture variation in job risk, they also capture variation in other working conditions that are not measured in most commonly used data sets. To the extent that other working conditions covary with job risk, the estimates in Panel 1 are biased. We interpret the instability of the parameter estimates in Panel 1 as evidence of the measure of job risk being correlated with the regression error, or $cov(r_i^*, \varepsilon_i \mid X) \neq 0$. From a theoretical perspective, this is hardly surprising. Wealthier workers tend to buy safer jobs. One suspects that they also purchase cleaner jobs, with better hours and nicer offices. Given that we observe only a modest fraction of the characteristics that affect the worker's productivity and only one measure of job-related amenities (job risk), it is hardly surprisingly that we have biased estimates.

Determining the nature of the bias, however, is more difficult. To see why, suppose that the true model of wage determination is:

$$ln(w_i) = X_i\beta + r_i^*\gamma + Z_i b + \varepsilon_i^{'} \qquad (1')$$

where Z_i is a vector of job disamenities and $b > 0$ by assumption. Because wealthier workers prefer jobs with fewer disamenities, or job amenities are a normal good, we expect that $\text{cov}(r_i^*, \varepsilon_i^{'}) < 0$ and $\text{cov}(Z_i, \varepsilon_i^{'}) < 0$ so that more productive workers have jobs with fewer disamenities. If we mistakenly estimate equation (1) rather than equation (1'), it is straightforward to show that:

$$\hat{\gamma}^{OLS} = \frac{\gamma\,\text{var}(r_i^* \mid X_i) + \text{cov}(r_i^*, Z_i \mid X_i)b + \text{cov}(r_i^*, \varepsilon_i^{'} \mid X_i)}{\text{var}(r_i^* \mid X_i)}.$$

As we expect that job disamenities covary positively ($\text{cov}(r_i^*, Z_i \mid X_i) > 0$) and as $b > 0$, the second term in the numerator causes us to overestimate the price of risk, but as $\text{cov}(r_i^*, \varepsilon_i^{'} \mid X_i) < 0$, the third term in the numerator tends to causes us to underestimate the price of risk. Which bias dominates is, of course, not known. Obviously, the inclusion of one-digit industry and occupation controls imperfectly controls for some of the missing unobservables.

In Panel 2 of Table 1A, we repeat the exercise for the NIOSH occupation measure of job risk. Contrary to the prediction of the theory, three of the four estimates are negative and significant! Again, the magnitudes of the estimates are quite unstable, again suggesting that job risk covaries with the unobservables (ε_i) in the wage equation. This is quite a disconcerting result. The NIOSH occupation job-risk measure is constructed from the same data sources as the

NIOSH industry job-risk measure. Moreover, one expects that the occupation measure of job risk might be a more accurate measure of job risk than industry-based measures.

In Panels 3 and 4, we estimate equation (1) using the BLS measures of job risk and the March CPS from 1995 to 2000. While the BLS job risk measure uses a much more disaggregated measure of industry or occupation (3-digit as opposed to one-digit) to assign job risk, the BLS measure ignores spatial variation in job risks. The BLS estimates are also based on annual fatalities, which tend to be volatile, especially for relatively small industries or occupations. The BLS-based estimates are quite unstable. For instance, using the industry-based measures, the estimates range from -188 to 239, and the estimates are quite precisely estimated.

Economists have long been concerned about biases in estimates of the return to schooling that results from the sorting of higher ability workers into higher levels of schooling. This "ability bias" could cause economists to severely over-estimate the returns to schooling; see Card (1998, 2001) for excellent reviews. We worried that our estimates suffered from similar ability bias. If low-ability workers were being sorted into dangerous jobs (so that $\text{cov}(r_i^*, \varepsilon_i' \mid X_i) < 0$), our estimates would be biased downward. In an attempt to assess how severe such a problem might be, we took a page from the returns-to-schooling literature and found a data set that contains test scores for respondents. While test scores tend to be an imperfect measure of ability, our hope was that the use of test scores as a covariate might remove much of the ability bias.

Toward that end, we used the National Longitudinal Survey of Youth 1979 data. Because respondents took the Armed Forces Qualification Test (AFQT), we have measures of the respondents' aptitude. The AFQT is a set of 10 tests. We first demeaned test scores, conditioning on the age of the respondents at the time they took the test. We then took the first two principal components of the demeaned test scores as our measures of aptitude and added the test score measures to our set of basic controls. In Panels 5 and 6, we estimate equation (1) with our NLSY sample using data from 1986 to 1993 and the NIOSH job risk measures. To our disappointment, the results are reasonably similar to the results from the corresponding two panels of Table 1A. While there are some modest differences, it is important to note that others have found variation arising from the use of different survey instruments and respondents in different locations. Moreover, the estimates using the NIOSH occupation-based job risk measures remain negative and significant. The three positive estimated values of a statistical life are outside of Viscusi's range of $3.3 to $7.8 million, and two are over $20 million.

Finally, for men we also estimate equation (1) using the Outgoing Rotation Group (ORG) data from the CPS and both the NIOSH and BLS job risk measures. Unfortunately, the ORG data does not contain a consistent measure of firm size, but the data do afford larger samples and have a measure of wages that is superior to the March data. In particular, the March data requires the researcher to impute the value of the wage by dividing earnings last year by usual weekly hours and weeks worked. As a result, the earnings may be from a variety of different jobs last year, not just in the job measured. Moreover, the March data requires workers to accurately recall last years earnings, the usual hours of work, and the number of weeks worked last year. One suspects that each of these questions is subject to considerable measurement error. In contrast, the ORG ask about the wages on the major job last week, which appears to provide much better wage estimates. When looking at hourly wage, there are mass points at the minimum wage, whole dollar amounts, and numbers divisible by five and ten as one might expect from hourly wages.

The results are reported in Panels 7 through 10 of Table 1A. Unfortunately, the results are just as disappointing as the results from the other data sets. Of the 16 different estimates, nine are negative and statistically significant. Of the remaining seven estimates, four are outside of Viscusi's range of $3.3 to $7.8 million.

Historically, the value of a statistical life literature has focused on the job risk of men. While we point out below that there are some sensible reasons for concentrating on men, we did not want to ignore the wage hedonic for female workers. In Table 1B, we replicate our estimates for women. Generally, the qualitative findings are quite similar for men and women. The estimates of the price of risk for women are quite volatile, just as they are for men. We will not test our reader's patience with a complete description of the results in Table 1B, but we will use estimates from Panel 1 to construct value of life estimates for women, ignoring the negative and statistically insignificant result in column 5. Using these estimates, the range of estimates in Panel 1 imply value of life estimates that range from $9.7 to $17.6 million, assuming that we evaluate the women's value of life at an earnings of $35,000 as well.[4] As in the estimates we have for men, there is an extremely wide variation in the price of risk.

While we have demonstrated the large variations in the estimated coefficients for the job risk measure, we have not demonstrated how unusual this variability is in the estimation of wage equations. To illustrate how stable coefficients usually are, in Table 2 we report the estimated returns to a bachelor's degree relative to a high school degree using the same specification as appears in Panel 8 of Tables 1A and 1B. There is a long history in labor economics of estimating such returns to education; see Card (1998) for an excellent review. It is generally recognized that when estimating the returns to schooling, one does not want to condition on the worker's occupation because a better occupation is a part of the way in which schooling raises earnings, but to keep our results comparable with Panel 8, we also provide results that condition on occupation. The estimated coefficients for both men and women are remarkably consistent. Unless one conditions on occupation, the results never differ by more than 10 percent. Even when conditioning on occupation, the coefficients fall by less than 40 percent.

Finally, while we have reported a large number of specifications, we wanted to provide some guidance as to our preferred specification. To us, there is no doubt that the final

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[4] In these data, women earn about 60 percent of what men earn. One could, therefore, reduce these estimates by 40 percent to evaluate the value of life at the mean earnings of women.

specification that contains the basic controls, the controls for state of residence, and the industry and occupation controls is superior to the other specifications. For reasons that we explained above, we think the ORG CPS data provide a better wage measure than the March CPS data. Similarly, while we think the richness of the NLSY data is valuable, the limited sample sizes and the limited age range, we again favor the ORG data. Finally, while the BLS data's three-digit disaggregation is very appealing, the use of a single national number looses much variation that is extremely useful for identification so we favor the use of the NIOSH data, although we remain agnostic as to whether one should use the occupation or industry risk measure.

In summary, the evidence from the estimates presented in Table 1 lead us to draw five conclusions:

- First, and foremost, estimation of equation (1) produces quite unstable estimates of the price of risk. Small changes in the specification of covariates result in large variations in the estimated price of risk;
- Second, estimates from the men's and women's sample appear to provide reasonably similar estimates of the price of risk. As a result, we will concentrate our discussion on the results for men, but we will continue to present the results for women so that the interested reader may compare differences in estimates between the sexes;
- Third, many of our estimates are within generally accepted bounds for estimates of the value of life. For instance, for men 6 of our estimates are within Viscusi's range of \$3.3 to \$7.8 million. Other estimates are quite similar to other reports in the literature. For instance, our estimate using the NIOSH occupation risk data with the March CPS data with controls for both industry and occupation (\$0.6 million) is virtually the same as Kniesner and Leeth's (1991) estimate of \$0.7 million using CPS data and the industry NIOSH data;

- Fourth, both industry and occupation controls substantially affect the estimated coefficients, even when using the "opposite" measure of risk. Henceforth, we report only the specifications that have both industry and occupations;
- Fifth, despite the more disaggregated industry and occupation categories that the BLS uses in the construction of their job risk measures, we find no evidence that the BLS data are superior to the NIOSH data. As a result, we will focus our discussion on the NIOSH data because these data afford us a longer time horizon. Again, we will continue to report estimates using the BLS data so that the interested reader may compare the NIOSH-based estimates to the BLS-based estimates.

Thus, we now attempt to find reasons for the instability of these estimates.

Generally, we think there are three reasons to explain the instability of the estimates found in Table 1. First, we may be suffering from a bias resulting from the correlation of the job risk measure and the regression error, or $\text{cov}(r_i^*, \varepsilon_i' \mid X_i) \neq 0$. This would result in standard endogeneity bias. Second, our functional form in equation (1) could be incorrect. The results of Heckman et al. (1998) and Heckman, Ichimura, and Todd (1997, 1998) can be interpreted as finding that the misspecification of the functional form of the conditional mean functions accounted for a great deal of the heterogeneity in estimates of the impact of job training programs. Third, because the measures of job risk are imperfect, the resulting measurement error might result in significant biases.

The concern regarding $\text{cov}(r_i^*, \varepsilon_i' \mid X_i) \neq 0$ proves to be a difficult nut to crack. Unlike most recent advances in applied microeconomics, our estimates of the price of risk have focused on the equilibrium relationship between risk and wages. Many of the recent advances in applied microeconomics have focused on finding "natural experiments" that induce exogenous variation in the variable of interest (job risk in our case) and examine how agents react to that random variation. For instance, Angrist (1990) uses the draft lottery during the Vietnam Era to access the impact of military service on earnings. Because of exogenous variation in military service that the draft lottery induces, Angrist is able to obtain a consistent estimate of the impact of military service on earnings. Similarly, Angrist and Evans (1998) document that variation in the gender composition of a woman's first two children affects the likelihood that the woman will have additional children, with women whose first two children are the same gender being more likely to have an additional child. Because the gender composition of a woman's offspring is uncorrelated with her productivity in the labor market, Angrist and Evans are able to obtain estimates of the impact of children on labor supply. See Angrist and Krueger (1999) for an excellent discussion of the recent advances in applied microeconomics.

Unfortunately, such truly exogenous variation appears very difficult to obtain in labor market studies of the price of risk. One might hope that government policies may be used for such variation. For instance, Evans, Farrelly, and Montgomery (1999) use the imposition of workplace bans on smoking, which implicitly increase the cost of smoking, to examine the impact of such mandates on smoking. Government safety regulations might appear to provide similar types of natural experiments. However, while the Occupational Safety and Health Administration (OSHA) has implemented several policies to reduce workplace risk, Viscusi (1981) notes that in the first six years of OSHA's existence workplace accident rates declined less than 16 percent, and most of this decline was the result of the changing industry structure. Indeed, Kniesner and Leeth's (1995) review of the literature suggests that OHSA has had no measurable impact on worker safety. Thus, it appears unlikely that government safety regulations will provide sufficient variation to measurably affect the price of risk.

Similarly, one might hope that technological advances would provide natural experiments. Indeed, there are technological advances that have had large impacts on workplace safety (e.g., the introduction of long-wall coal mining), but these technological advances also affect the demand for labor and the skill mix of labor in the industry or occupation. This makes it extremely difficult to distinguish the impact of demand changes on wages from the impact of reduction in risk on wages. Hence, technological advances do not appear to be legitimate natural experiments. In the near future, we intend to explore an idea that Bill Evans suggested to us: the use of regional variation in the improvement of auto travel safety resulting from the introduction of air bags and improved enforcement of drunk driving laws. Unfortunately, results from this project are months away and so we must rely on our use of equilibrium variation in job risk. We can, however, address the issues of the proper functional form and the role of measurement error. In the next two subsections, we address these two issues in some detail.

B. The Role of Functional Form and Support

Again, consider the standard hedonic wage equation:

$$ln(w_i) = X_i\beta + r_i^*\gamma + \varepsilon_i . \tag{1}$$

The equation makes three strong assumptions that may do violence to the data. First, it assumes that the researcher knows the appropriate vector of covariates (X_i, r_i^*). Second, it assumes the coefficients (β, γ) are constants, rather than functions or random vectors. Thus, the impact of risk on log wages is the same for a 45-year-old black male accountant as for a 27-year-old white male high school graduate working in the oil fields of Texas. Third, it assumes a log-linear relationship between the wage and the covariates. As Angrist and Krueger (1999) emphasize, the use of OLS estimation may provide very misleading estimates if these assumptions are incorrect.

For the ORG CPS and March CPS data, our sample size is sufficiently large and our covariates X_i are of sufficiently low dimension that we may employ the cell-matching or frequency estimator. We may for the *kth* cell consider the estimation of the equation:

$$ln(w_{ik}) = \alpha_k + r_i^* \gamma + \varepsilon_{ik} \tag{2}$$

where w_{ik} is the *ith* worker's wage, α_k is a constant to be estimated, and ε_{ik} is the regression error. For the NLSY, the data are a bit sparser so we recoded the test scores variables into deciles, transformed the experience and tenure variables, originally measured in months of work, into years, and combined some of the post-high school variables.

The estimation of equation (2) allows for a complete set of interactions for every variable included in X_i and imposes no linearity restriction. Hence, we can interpret equation (2) as being estimates of :

$$ln(w_i) = g(X_i) + r_i^* \gamma + \varepsilon_i . \tag{2'}$$

In this model, the function $g(X_i)$ is a nuisance parameter. We continue to make the assumption that the job risk parameter, r_i^*, enters the equation linearly and that it remains additively separable, in the logarithmic form, from the other covariates, X_i. Finally, if equation (1) represents the true form of the model, the estimation of equation (2) will provide consistent estimates of the price of risk, γ.

In Panels 1 through 8 of Table 3A, we report the results of the estimation of equation (2) for men and in Panels 1 through 8 of Table 3B we report the results for women as well. Taken as a whole, the volatility of the results in Table 3 are remarkably consistent with corresponding volatility of the results in Table 1. The estimates continue to be highly volatile when changing the specification of the vector of covariates, X_i. There is perhaps some evidence that the estimates are somewhat larger when using the flexible functional form. In our view, this suggests that instability of the estimates is not a result of the log-linear specification of the vector of covariates, X_i.

Of course, even this specification assumes that the relationship between the logarithm of wages and job risk is linear. Viscusi (1981) reports substantial variation in estimates of the value of life by quartiles of the distribution of job risk. For instance, the implied value of life for workers in the first quartile of fatality risk is \$5 million while the implied value for workers in the fourth quartile is only \$2.8 million.

To examine whether there are substantial nonlinearities in the wage-risk locus, we next divide jobs into risk deciles and then estimate the equation:

$$\ln(w_i) = X_i\beta + r_{di}\gamma + \varepsilon_i \tag{3}$$

where r_{di} is the risk decile of the ith worker and γ is now a vector of coefficients to be estimated. The use of these discrete cells allows us to trace out any nonlinearity. In Panels 1 through 10 of Table 4A, we report the results of the estimation of equation (3) for men and in Panel 1 through 10 of Table 4B for women.

The results show a highly nonlinear relationship between the wages and risk levels. For instance, focusing on the last column of Panel 8 of Table 4A, initially there is an increase in wages as risk levels with the coefficient on the second to the fourth deciles remaining positive. The estimated coefficient then remain approximately zero as job risk increases. Nor are these results unusual. A quick review of all the panels in Tables 3A and 3B shows a consistent lack of a monotonic relationship between job risk and wages. Of course, interpreting this nonmonotonicity is difficult. It may be the result of the misspecification of equation (1); perhaps significant interactions between the job risk measures and the covariates, X_i, have not been modeled. Alternatively, this nonmonotonicity may be indicative of a covariance between the job risk measures and the regression error, or $\mathrm{cov}(r_i^*, \varepsilon_i \mid X_i) \neq 0$.

If the nonmonotonicity is the result of the exclusion of relevant interaction terms, we may rely on nonparametric regressions to produce estimates of the relationship between job risk and wages without making any functional form assumptions. Exploiting recent advances in applied microeconometrics, we next estimate the wage-risk locus nonparametrically. We use the propensity score matching estimator of Rosenbaum and Rubin (1983); see Heckman, Ichimura, and Todd (1997, 1998) and Smith and Todd (2000) for a discussion of propensity score estimates and examples of their use. Until recently, propensity score matching has been limited to cases in which the variable of interest was binary. For case job risk, this would require the division of jobs into risky and safe classifications, a much too restrictive formulation in our view. Fortunately, Imbens (1999) and Lechner (2000) have shown that propensity score matching extends to finite numbers of alternatives.

We divide jobs into K risk categories (for quintiles, $K = 5$). Let the wage of the *ith* worker in the *jth* risk category, Y_{ij}, be given by:

$$Y_{ij} = g_j(X_i) + \varepsilon_{ij} \qquad j = 1, 2, \ldots K \tag{4}$$

where X_i is a vector of characteristics that determines earnings and ε_{ij} is again the error term. The function $g_j(\cdot)$ is an unknown function that determines wages. We may define the price of risk:

$$p_{ijk}(X_i) = Y_{ij} - Y_{ik}\,, \tag{5}$$

which is the cost per hour of moving the worker from the *jth* risk class to the *kth* risk class.

The fundamental problem is that we observe either Y_{ij} or Y_{ik} but never observe both. We estimate the "missing" wage using nonparametric methods, a nearest neighbor estimator. After estimating the missing wages, the price of risk is:

$$\hat{p}_{ijk}(X_i) = Y_{ij} - \hat{Y}_{ik} \tag{6}$$

where $\hat{Y}_{ik}$ is the estimated missing wage. Given these individual prices of risk, the average price of risk for moving from the *jth* to the *kth* risk category may be calculated as

$$\hat{p}_{jk} = \frac{\sum_{i=1}^{N_j} \hat{p}_{ijk}(X_i)}{N_j}. \tag{7}$$

The nonparametric estimation of the price of risk avoids making any assumptions about the functional form of the $g_j(\cdot)$ and allows the price of risk to vary across individuals. Thus, moving from the first to the second decile of risk may have a different price than moving from fourth to the fifth decile.

As Heckman et al. (1998) emphasize, there is an added benefit to nonparametric estimation: it forces researchers to confront the "support problem," which is most easily seen when considering the cell-matching estimator but similarly exists for the propensity score estimator. For instance, suppose researchers wish to know the price of moving a 55-year-old white male with a bachelor's degree from the first decile of risk to the tenth decile of risk. The researchers may well find there are no 55-year-old white males with a bachelor's degree in the tenth decile of risk. The data simply will not allow researchers to calculate that price of risk because nonparametric estimation relies on matching workers across the various categories of risk. Of course, with parametric regression such as those in equation (1), we can extrapolate outside the range of the data. Extrapolation outside the range of the data, however, is simply identification by functional form assumption.

While "propensity score" matching may appear quite abstract, the intuition behind the estimator is really quite simple. We wish to compare someone in a "risky job" with someone in a "safe job," but clearly we want to make sure the individuals are comparable. One could try to match workers exactly on the X vector, but for many workers there may not be anyone with an exact match. The basic idea of nonparametric regression is to find someone "similar" without requiring an exact match. The genius of the Rosenbaum and Rubin's (1983) result is that, if the appropriate assumptions hold, we may match workers only on the estimated probability that they are in a risky job. This greatly simplifies the matching and can lead to faster rates of convergence. See Smith (2000) for an excellent non-technical introduction.

In Panels 1 through 10 of Tables 5A and 5B, we provide nearest neighbor estimates for our nonparametric approach. Nearest neighbor estimates simply match people in the *jth* group to the person who is "closest" to them in the *kth* group. Thus, as we are matching on the person's propensity score, for person i in the *jth* group we observe their propensity score, s_{ij}^{0}, find the person in the *kth* group whose propensity score is the closest to s_{ij}^{0}, and use this person's wage as the missing counterfactual. To guarantee that the match is of reasonable quality, we apply a caliper of 0.01, so if the difference between the treatment group observation and nearest neighbor from the comparison group exceeds 0.01, the observation is discarded. We match on the most exhaustive set of covariates for each of the three data sets that we use: the CPS March Demographic Supplement, the CPS ORG data, and the NLSY. The fifth quantile is the group with the highest risk jobs and the first quantile is the group (with the lowest risk jobs).

Several features of the estimates are of interest. First and foremost, the estimates vary as much as the OLS regression estimates vary. Many of the estimates of compensation necessary to take on added risk are negative. Even when the estimates are positive, the estimates do not monotonically increase in risk as we move to higher risk quintiles. Looking across data sets, we see large movements in the estimated comparison group wage. For instance, focusing on the first column of Table 5A, the wages of the first quantile (the comparison group) vary from \$10.99 to \$8.19. This difference shows the differences in who gets placed in the quantile and who gets matched to an observation in the treatment group (the higher risk group) across the differing data sets.

Collectively, the results in Tables 3 through 5 provide some evidence that the variability of the parametric estimates do not appear to be the result of any restrictive assumptions imposed by the parametric representation of equation. The problem of the parameter variability, therefore, would seem to rest elsewhere. Of course, this does not mean that the log-linear form of the wage equation traditionally used in this literature is correct. Rather, it simply documents that other problems with the estimation that appear in parametric and nonparametric estimates. In the next section, we explore the role of measurement error in the estimation of the price of risk and find evidence that this measurement error may well be the source of at least some of the volatility.

C. The Role of Measurement Error

The quality of estimates is necessarily limited by the quality of measurement. No matter how sophisticated the theoretical and econometric models, data of poor quality may still provide estimates of poor quality. In the next section, we suggest why the data from the BLS and NIOSH, while providing extremely accurate measures of the aggregate job risk in the United States, may not provide accurate estimates of the job risk of those workers in our sample.

There are essentially three problems in our measurement of job risk. First, because we divide workers into industries or occupations–some of which are quite small–we may have considerable sampling variation within these industry and occupation cells. Both the BLS and NIOSH data recognize this problem and suppress data when the number of fatalities is too low, but this inherent sampling variation creates measurement error. Second, within occupations, there may be a great deal of heterogeneity in the actual job risk and the assignment of that job risk may be extremely nonrandom. For instance, employers may assign male and older clerks at convenience stores evening and late night hours when the risk of holdup–and injury during the robbery–are particularly high and assign female and younger clerks daytime hours. Because we only measure the aggregate job risk of convenience stores clerks, however, this would result in our overestimating the job risk of young and female clerks and underestimating the job risk of older and male clerks. Finally, because we need to assign workers to an industry or occupation, the quality of our measurement is limited to the quality of the data on industry and occupation assignment. The best available evidence (e.g,, Mellow and Sider, 1983) is that industry and occupation–especially at the three-digit level–are not measured accurately.

1. Documenting the Magnitude of the Measurement Error

If the researcher could measure (X_i, r_i^*) perfectly, Ordinary Least Squares (OLS) estimation of equation (1) would provide consistent and efficient estimates of the parameters (β, γ), assuming the functional form of the conditional mean function was properly specified and the covariates (X_i, r_i^*) are orthogonal to the error term. Unfortunately, there are numerous reasons to suggest that the measure of job risk (r_i^*) is mismeasured and perhaps mismeasured badly. First, government fatality reports are inherently an estimate of job risk: they are realizations of a random variable. For instance, suppose there are N_k workers in the *kth* industry (or occupation) category, and each of these workers is subjected to a risk, r_k^*. Unfortunately for the researcher, the government's tally of deaths in the *kth* category is not exactly equal to the expected number of deaths, $r_k^* N_k$. Rather, the government's tally is equal to the random variable D_k. Using the random variable D_k, the researcher constructs an estimate of r_k^* as $r_k = D_k / N_k$. While $E(r_k) = r_k^*$, it is almost certain that $r_k \neq r_k^*$. Thus, let $r_k = r_k^* + \eta_k$, where η_k is the measurement error associated with the variable r_k.

A simple example illustrates the problem. Suppose there are 400,000 workers in a particular industry or occupation (a relatively large 3-digit industry or occupation, or a large one-digit industry or occupation at the state level) and every worker faces a 5 in 100,000 chance of an on-the-job death, a little larger than the national average in 1995, which was 4.3 per 100,000 workers. If we randomly simulate the number of deaths for five years, we obtain 23, 24, 26, 24, and 11 deaths for a mean death rate of 5.4 per 100,000, very close to the true mean of 5. The standard deviation, however, 1.4, or about 30 percent of the true mean. If we consider an industry or occupation of only 100,000 workers, we get deaths of 3, 3, 9, 8, and 6 for a mean of 5.8, which again is not too far off the true mean of 5. The standard deviation of this sample, however, is 3.4 which over 60 percent of the true mean.

This argument can be formalized. If we assume that the risk of death in an industry or occupation, r_k^*, is the same for all individuals, the number of deaths in an industry or occupation is distributed binomially, with mean $n_k r_k^*$ (where n_k is the number of workers in the industry or occupation) and variance $n_k r_k^*(1-r_k^*)$. This implies that our estimate of the death rate has a mean of r_k^* (so that the estimates are unbiased) and has variance $r_k^*(1-r_k^*)/n_k$. A commonly used measure of the precision of the estimate is the coefficient of variation, which is simply the ratio of the standard deviation to the mean. In our case, the coefficient of variation is for the death rate in an industry or occupation is simply $(1-r_k^*)^{1/2}/(r_k^*)^{1/2}(n_k)^{1/2}$. The smaller r_k^* the larger the coefficient of variation, and, of course, the probability of an on-the-job fatality is very small even for extremely dangerous occupations. Thus, there is intrinsically a lot of sampling variation when trying to estimate rare events such as on-the-job fatalities.

Past studies have indicated that job risk differs by firm size, region, and worker characteristics. Thus, when we make the further substitution for the *ith* worker's risk (who is in the *kth* industry/occupation class) that $r_i^* = r_k$, we are undoubtedly introducing measurement error. Thus, let:

$$r_k = r_i^* + \nu_{ik} \tag{8}$$

where ν_{ik} represents the measurement error associated with using r_k as a proxy for r_i^*.

The measurement error undoubtedly attenuates the estimates of the coefficient, γ. Indeed Hausman (2001) terms this the "iron law of econometrics." From an empirical standpoint the relevant question is "How severe is attenuation bias that results from the measurement error ν_{ik}?" Fortunately, we have up to four reports on the level of job risk that we may use to determine the extent of the measurement error.

To see why, consider two measures of job risk:

$$r_{1i} = r_i^* + \nu_{1i} \tag{9}$$

$$r_{2i} = r_i^* + \nu_{2i} \tag{10}$$

where r_i^* is the true measure job risk, ν_{ji} is the measurement error associated with the jth measure of job risk, and r_{ji} is the jth observed measure of job risk. The covariance of the two measures is simply:

$$Cov(r_{1i}, r_{2i}) = Var(r_i^*) + Cov(\nu_{1i}, r_i^*) + Cov(\nu_{2i}, r_i^*) + Cov(\nu_{1i}, \nu_{2i}) \tag{11}$$

and the variances of the two measure are:

$$Var(r_{1i}) = Var(r_i^*) + 2\,Cov(\nu_{1i}, r_i^*) + Var(\nu_{1i}) \tag{12}$$

$$Var(r_{2i}) = Var(r_i^*) + 2\,Cov(\nu_{2i}, r_i^*) + Var(\nu_{2i}) \tag{13}$$

which provides us with six unknown parameters and three equations. It also demonstrates why it is essentially impossible to make much progress on the problem in this form: the system is underidentified.

Fortunately, we may follow Griliches (1986) and assume that our measurement error is classical. That is, we may assume $Cov(\nu_{1i}, r_i^*) = Cov(\nu_{2i}, r_i^*) = Cov(\nu_{1i}, \nu_{2i}) = 0$, which reduces our three-equation system to:

$$Cov(r_{1i}, r_{2i}) = Var(r_i^*) \tag{14}$$

$$Var(r_{1i}) = Var(r_i^*) + Var(\nu_{1i}) \tag{15}$$

$$Var(r_{2i}) = Var(r_i^*) + Var(v_{2i}) . \tag{16}$$

Of course, with additional covariates, we need to make the additional assumptions that $Cov(v_{1i}, X_i) = 0$ and $Cov(v_{2i}, X_i) = 0$ so that the measurement errors are uncorrelated with covariates. Because we have up to four measures of job risk, the classic errors-in-variables model has empirical content: the covariance of any two measures of risk should have precisely the same covariance as any other two measures.

At this point, it is useful to present a convenient decomposition for OLS regressions. Yule (1907) has shown that the estimation of equation (1) with OLS is equivalent to the following. Estimate:

$$\ln(w_i) = X_i\, b + \varepsilon_i' \tag{17}$$

and recover the residual from the equation, which we denote $\ln(w_i)'$. Then, estimate:

$$r_i = X_i\, \delta + u_i' \tag{18}$$

and recover the residual from the equation, which we denote r_i'. We may then estimate the equation:

$$\ln(w_i)' = r_i'\gamma + \varepsilon_i'' \tag{19}$$

The estimation of equation (19) will yield precisely the same estimate of γ as the OLS of γ from equation (1).

Exploiting Yule's decomposition, our three equations system of covariances would simply become:

$$Cov(r_{1i}, r_{2i} \mid X_i) = Var(r_i^* \mid X_i) \tag{20}$$

$$Var(r_{1i} \mid X_i) = Var(r_i^* \mid X) + Var(v_{1i} \mid X) = Var(r_i^* \mid X) + Var(v_{1i}) \tag{21}$$

$$Var(r_{2i} \mid X_i) = Var(r_i^* \mid X_i) + Var(v_{2i} \mid X_i) = Var(r_i^* \mid X_i) + Var(v_{2i}) \tag{22}$$

where $Var(v_{1i} \mid X) = Var(v_{1i})$ and $Var(v_{2i} \mid X_i) = Var(v_{2i})$ by the assumptions that $Cov(v_{1i}, X_i) = 0$ and $Cov(v_{2i}, X_i) = 0$. As $Var(r_i^*) \geq Var(r_i^* \mid X)$, the addition of covariates must always reduce the signal-to-noise ratio or $Var(r_i^* \mid X) / \left(Var(r_i^* \mid X) + Var(v_{ji})\right) < Var(r_i^*) / \left(Var(r_i^*) + Var(v_{ji})\right)$. Because job risk varies with the observable characteristics, X, conditioning on the observable characteristics removes the variation in the risk measures that are correlated with X. If the measurement error is uncorrelated with the measurement error, as in the case of classical measurement error model, then the inclusion of covariates leaves the variance of the measurement error unaffected and reduces the variation in actual job risk, r_i^*. Thus, the addition of covariates increases the attenuation bias associated with the measurement error: the inclusion of covariates, while necessary to control for the heterogeneity in workers and jobs, removes information

In Table 6, we present the correlation and Yulized residual correlations for the various job risk measures. We use data from the 1995 CPS, including both the ORG and March Supplement. The results are quite depressing. The raw correlation before conditioning on any covariates is modest at best, ranging from 0.53 to 0.30. As the correlation differs in magnitude, we have at least some evidence that the measurement error is not classical. When we condition on the full set of covariates, the correlations range from 0.41 to 0.02! The inclusion of both the state controls and the industry and occupation controls in particular reduces the correlation among the various measures. (Notice that in absence of measurement error the correlations should be 1.)

In Table 7, we produce the full range of Yulized residual covariance, which in turn may be used to construct any estimate desired. The OLS estimates of the price of risk are simply the ratio of the risk measure covariance with the wage measure, divided by the variance of the risk measure. Similarly, we may form any IV estimate desired by dividing the covariance of a risk measure and the wage measure by the covariance of the two risk measures. Thus, the ratio of the variance of the risk measure to its covariance with the other risk measure provides a measure of the magnitude of the attenuation bias resulting from measurement error in the job risk measures. The ratios of the variance-to-covariance are large, particularly for the BLS occupation measure, suggesting that OLS estimates in Table 1 are substantially attenuated. Of course, the *negative* measures of job risk are substantially attenuated as well. Moreover, notice that the covariances of the logarithm of wages and the various job risk measures are quite different and often of the opposite sign, which forces us to conclude that the measurement error is nonclassical.

A second manner in which we might find evidence of measurement error is to compare the estimates of fatality rates for subgroups of the population that we derive using our data with the fatality rates for these subgroups that NIOSH compiles. Because of the aggregation bias that exists in our data, we may see substantial differences between our estimates and the NIOSH Census of occupational deaths. Our data assign the mean risk rate to everyone within an occupation regardless of age, race, and sex, but that may be incorrect. Even if we limit ourselves to a homogeneous occupation in the same industry, there may be substantial differences in risk, say, between a police officer in Washington, DC, and one in Larned, Kansas (population, 4,236). Yet, we suspect that the policeman in Larned is more likely to be white than the officer in Washington. Similarly, even within the city of Washington, we suspect that younger and male police officers may be given somewhat more dangerous assignments than more senior and female officers. Such aggregation bias may add substantially to the measurement error.

The actual situation is much more complicated. Industry and occupation are very poorly measured, even in carefully collected data sets such as the CPS. For instance, using a CPS supplement that interviewed both employees and their employers, Mellow and Sider (1983) document that employer and employee agree on three-digit industry codes only 84.1 percent of the time. Even for the broader one-digit industry codes, the rate of agreement is only 92.3

percent. The situation for occupation codes is even worse. Employee and employer agree only 57.6 percent of the time about the three-digit code and only 81.0 percent of the time for one-digit codes. Thus, there is a substantial degree of measurement error in the industry and occupation measures. Mellow and Sider document that for the sample in which both firm and worker agree on the three-digit industry code, the estimated price of risk for non-fatal accidents is 50 percent higher than for the sample as a whole.

In Table 8, we depict aggregate fatality rates from the NIOSH census for workers by race, sex, age categories, industry, and occupation. These compilations provide the level of job risk by each of these categories. We then match our various measures of job risk to the CPS and attempt to replicate the NIOSH census by aggregating the CPS data over the observed characteristics. The results indicate a substantial amount of error in our measures of job risk. The job risk that black Americans face is substantially underestimated in our measure and the job risk that white Americans face is overestimated. Similarly, we substantially overestimate the job risk that women face and underestimate significantly the job risk that men face. Generally, our estimates of the job risk of blacks are understated, although the BLS occupation job risk overstates the fatality rate for almost every other group. This can result because the job risk measures are suppressed if an occupation or industry cell has too few deaths to be disclosed. We overestimate the risk to younger workers and underestimate the risk to older workers. Finally, there are some large discrepancies across industry and occupation divisions as well.

In Table 9, we exploit the time series of data available from the NIOSH. We compare the aggregate fatality rate that NIOSH calculates each year to the implied fatality rates when we match the NIOSH industry and occupation fatalities rates to the ORG CPS. Two features of the data are very distinctive. First, the matched data always understate the level of job risk. Second, because the NIOSH data are updated every five years, the matched data are too volatile in the years that the new estimates are released and do not show a sufficient time series variation to match the aggregate time series trend.

The form of this measurement error is particularly troublesome. We find the measurement error is correlated with various covariates that are typically included in wage or earnings equations. Given the state of the measurement error literature, this correlation makes it impossible to recover unbiased parameter estimates of the price of risk. Moreover, given that we find convincing evidence that the measurement error is correlated with observable factors that affect wages (the covariates), we expect that the measurement error is probably correlated with unobservable factors that affect wages (the regression error). Such complex correlations among the job risk, the covariates, and the regression error make it impossible to obtain consistent estimates of the price of risk. We simply need better data.

2. Attempts to Correct or Mitigate the Measurement Error

In this section we explore three different methods to help mitigate the impact of the measurement error. We pursue these corrections with trepidation. Given the form of the measurement error that we described in the previous section, the estimates we present in this section are inconsistent.

We begin by considering a simple means of increasing the signal-to-noise ratio in data with multiple reports. Consider:

$$\overline{r_i} = \frac{\sum_{j=1}^{4} r_{ji}}{4} \tag{23}$$

or the simple average of the four job risk measures. This does not reduce the mean bias of the measurement error because we continue to use all of the available measures of job risk. Thus, if we systematically understate the job risk of African Americans in each of our four risk measures, the average of these risk measures will continue to understate the job risk of African Americans.

It will in general reduce the variance associated with the measurement error, unless error terms are perfectly correlated. In Tables 10A and C, we estimate our equation using data from both the ORG and March data for men, while in Tables 10B and D we repeat the exercise for women. The estimates are again highly sensitive to the specification of the equation and are often negative.

In a recent paper, Lubotsky and Wittenberg (2001) considered how to construct an index from the multiple reports. If we let b_j for $j \in \{1,2,3,4\}$ be the OLS estimates from entering all the noise measures of job risk in the same, we simply construct:

$$b^* = \sum_{i=1}^{4} \frac{\text{cov}(r_i, \ln(w))}{\text{cov}(r_1, \ln(w))} b_i \tag{24}$$

where $\text{cov}(r_i, \ln(w))$ is the covariance between the ith risk measure and the natural logarithm of wages. Lubotsky and Wittenberg show that b^* is a lower bound on the price of risk when the measurement error is uncorrelated with the regression error ($\text{cov}(\nu_i, \varepsilon) = 0$) and the measurement error is uncorrelated with the true measure of job risk ($\text{cov}(r^*, \nu_i) = 0$). Importantly, the Lubotsky and Wittenberg result allows the various measurement errors to be correlated across job risk measures, or $\text{cov}(\nu_j, \nu_i) \neq 0$. While we have used the first job risk measure to normalize the estimate, this is arbitrary. Lubotsky and Wittenberg term the estimator in equation (24) the "post hoc" estimator. The post hoc estimator is essentially a weighted average of the individual coefficients where the weights are determined by the relative strength of the covariance between the individual risk measure and the dependent variable.

In Table 11A and 11C, we present the "post hoc" estimator for men using 1995 for both the ORG and March data; Tables 11B and 11D repeats the exercise for women. In each case, the "post hoc" estimator provides negative estimates of the price of risk. Thus, the "post hoc" estimator does nothing to correct the fundamental problem with these estimates.

Finally, we consider the classic solution to a measurement error problem: Instrumental Variables estimation. As we have multiple reports, we may easily find an instrument if we are willing to assume, in addition to the assumption of Lubotsky and Wittenberg, that $\operatorname{cov}(\nu_j, \nu_i) = 0$. In Table 12, we produce our IV estimates of the price of risk using the NIOSH job risk data and the March and ORG data for men. Our selection of these data is not random. We picked a set of data where the covariances between the Yulized residual of wage and the Yulized residual of the job risk measures are positive for both measure of job risk. We use the most extensive set of controls. The resulting IV estimates are quite variable. The IV estimates range from being the same magnitude as the OLS estimates to larger by a factor of 10.

Thus, the IV estimates illustrate the potential attenuation that may plague the OLS estimates. As Black, Berger, and Scott (2000) and Kane, Rouse, and Staiger (1999) emphasize, however, these estimates may be biased away from zero if there is a negative covariance between the measurement error and the true value of job risk. Moreover, Shogren and Stamland (2002) document that the failure to account heterogeneity in the skill to avoid accidents may cause us to overestimate the price of risk. Thus, while the presence of measurement error that we have documented and Hausman et al.'s (1991) "iron law of econometrics" suggest that current estimates of the price of risk are severely attenuated, other biases may cause us to overestimate the price of risk.

Thus, we conclude with the same caveat that we began with: We believe that the estimates reported in this section are inconsistent. Thus, one should not use these estimates for setting policy. It is important, however, to note that existing estimates may suffer from substantial attenuation bias to the extent that they have not controlled for measurement error in job risk measures.

D. The Role of Unobservables

One common concern in the use of observational data is the possibility that unobservable factors might confound estimates. In the context of equation (1), the concern is that the risk measure might be correlated with other factors that affect earnings. In this section, we exploit the richness of the NLSY data to see if such concerns are justified.

Our approach is to use the 1984 survey of the NLSY to examine whether job risk is correlated with other factors observed in the NLSY data that are not commonly observed in other data sources. In Table 13, we depict the correlation between job risk and education, illegal drug use, and AFQT score. Education is, of course, generally observed in most data sets that labor economists use and it is highly correlated with our measures of drug use. Few data sets, however, contain information on respondents' illegal drug use, but one suspects that illegal drug use may be correlated with unobservables that affect wages. We find that illegal drug users do take more risk than those who do not (admit) take illegal drugs. Similarly, we find that those who score higher in the Armed Forces Qualification Test have safer jobs.

Our findings are similar to Hersch and Viscusi (2001). They document that cigarette smokers assume more job risk than nonsmokers and receive less of a compensating differential for their injuries. Similarly, we find that individuals willing to undertake the risk associated with taking illegal drugs or who are less able to score highly on standardized tests also take on higher job risk. In the case of the AFQT scores, there is little doubt that the correlation between job risk and the omitted variable biases the estimate of the price of risk in models without standardized test scores. Numerous models have documented that standardized test scores are highly correlated with wages. Hence, the failure of the data sets such as the CPS to have these measures and the correlation we document suggest that job risk is an endogenous variable. While the interpretation of the correlation between drug use and job risk is a bit more problematic, it suggests that there are unobservable characteristics that affect the propensity to take drugs and assume job risk, which affect wages.

With panel data, there is another approach that may be used to attempt to control for unobservable factors that might be confounding our estimates of the price of risk. If one is willing to assume that the form of the wage equation is:

$$ln(w_{it}) = X_{ii}\beta + r_{it}^{*}\gamma + \alpha_i + \varepsilon_{it}, \tag{25}$$

we may allow for a correlation between the individual fixed-effect, α_i, and the measure of job risk, r_{it}^{*}. Thus, we are essentially assuming that any unobservable characteristics that affect both job risk and wages are time invariant. While this is clearly a restrictive assumption, if it is true then the use of a fixed-effect model would eliminate the bias from these unobservable factors.

In Tables 14A and 14B, we provide estimates of the price of risk using equation (25) and data from the NLSY for men and women. Unfortunately, if not surprisingly, the results are disappointing. For men, the estimates are generally negative and not significant. For women, the estimates are generally positive (as the theory requires), but the estimates are generally not statistically significant. Collectively, the fixed-effects estimates do not appear to provide credible estimates of the price of risk. In our view, this is hardly surprising. It is generally recognized that fixed-effects models exacerbate the problems of measurement error (see Griliches 1986), and these data contain a great deal of measurement error.

III. Conclusion

The existing estimates of the price are largely based on methodologies similar to those we have used in this study: the application of OLS to log linear wage equations. Several of our estimates appear to be consistent with that literature. Unfortunately, these estimates are not robust. Changes in the specification of the equation or changes in the job risk measure can result in very large changes in the estimated price of risk.

In our attempts to explain why the estimates are so unstable, we find evidence that the functional form of the regression equation has little impact on the estimates. Using more flexible functional forms, we found the estimates were similar to the corresponding log linear regression traditionally used in the literature. In addition, the more flexible methods also produced highly unstable estimates when we changed the covariates or altered the risk measure.

We did find, however, compelling evidence that there was a great deal of measurement error in the various measures of job risk. Because we have multiple measures of job risk, we may look at the correlation among the various measures of job risk. The correlation is seldom above 0.5 and the inclusion of richer sets of covariates lowers those correlations. In addition, there appears to be a systematic bias that is correlated with many of the covariates that labor economists often include in wage equations. The existing measurement error literature provides little guidance in how to correct for such nonclassical measurement error. This finding argues strongly for better efforts in data gathering.

Finally, we find some evidence that the assignment of job risk is correlated with the regression error. Because many standard data sets such as the CPS contain only modest sets of covariates, evidence from the NLSY suggests that job risk is correlated with standardized test scores and illegal drug use, data elements that are not typically available to researchers. Thus, it seems quite likely that job risk measures may be endogenous in many wage equations.

V. Appendix

Appendix A

We initially intended to use the Panel Study of Income Dynamics (PSID) as our panel data for this project. Ultimately, we decided that the National Longitudinal Study of Youth (NLSY) was a better data set because it contained standardized test scores for respondents. The initial results with the PSID, however, were quite similar to the results from the two CPS data sets and the NLSY. In Table A1, we provide estimates for men using specifications similar to Table 1 of the report. Again, the estimates are very sensitive to specification and are often negative.

Table A1. Estimated Price of Risk for Male Workers,
1985-1992 PSID Data and NIOSH Risk Data

A. Industrial Risk			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-200	-273	467
	(-3.31)	(-4.19)	(3.88)
B. Occupation Risk			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-958	-1152	433
	(-17.46)	(-19.19)	(5.63)

Note: The dependent variable is the natural log of the worker's real wage. For basic regression, the independent variables include a quartic in the worker's age, a vector of dummy variables that control for the worker's education, a dummy variable indicating whether the worker is black/white, a dummy variable indicating whether the workers are under union contract or not, and marital status. There are 25,971 observations in the men's regression. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses.
Source: Authors' calculations.

Appendix B

1. March CPS Data

The Annual Demographic Survey or March Supplement Survey, sponsored jointly by the U.S Bureau of Labor Statistics and the U.S Census Bureau, is one of the primary sources of annual income. Using a selected random sample of some 50,000 households from all 50 states and the District of Columbia, the Annual Demographic Survey gives a detailed analysis of geographical mobility, education attainment, work experience, annual income and poverty status of persons 15 years old and over.

In our study we constrained the data to individuals between 25 and 60 years old. The dependent variable is the log of hourly wages which is based on labor income from non-farm workers and non-self-employees. We divided the annual labor income by the total number of weeks times the hours per week worked during the year. We did not used allocated or inputting information for earnings, hours or weeks. In addition, we did not give any special treatment to top-coding observations.

We use the standard industrial and occupational classification to build 1-digit occupation (11 major groups) and 1-digit industry (10 major groups) categories. Based on these major categories we merge the March CPS data with the NIOSH risk data for each worker in the sample. In the case of BLS risk data that is given at 3-digit industry and occupational level, we merge it with the March CPS data using the 3-digit level for both industry and occupational categories.

The remaining independent variables used in the regressions are:

- Age: We use a quartic in age;
- Race/Ethnicity: 5 dummy variables for white, blacks, Asian, Hispanic and others;

- Education: 9 dummy variables for the highest completed years of education: less than junior high school, junior high school, some high school, high school, some college, college degree, master degree, professional degree, and Ph.D. degree;
- Marital Status: 4 dummy variables for married, single, widowed, divorced; the excluded
- Firm Size: 6 dummy variables for firms with less than 10 workers, 10-25 workers, 26-99 workers, 100-499 workers, 500-1000 workers, and more than 1000 workers,;
- State fixed effects: 51 dummy variables for each state in U.S, including the District of Columbia. ;
- Industry/Occupation: Dummy variables for 10 industrial categories and 11 occupational ones;

The mean values and standard deviations for the variables used in the regressions are in the Appendix tables.

2. NLYS Data

The NLSY is a nationally representative sample of 12,686 young men and women who were 14-21 years old when they were first surveyed in 1979. These individuals were interviewed annually through 1994 and are currently being interviewed on a biennial basis.

In our regression model, the panel dataset is extracted from 1986 to 1993, and the observations are aged from 25 to 60. Then the panel dataset is merged with NIOSH risk data. In the basic control, the independent variable is the log hourly wage, and the independent variables include age, union status, experience, tenure status, marital status, education background, firm size, and test score. The data are constructed as follows:

- *Lwage*: log of average real wage (adjusted by each year's price index) on all jobs held during the year. There are two variables used to construct wages: total income from wages and salary in the past calendar year and number of hours worked in the past calendar year. The wage variable is equal to total wage income divided by total hours worked;

- *Union*: indicates whether any job held during the year was covered by a collective bargaining agreement. The union indicator is equal to one if wages are covered by collective bargaining on any of the five jobs;
- *Experience*: total months the respondent has been employed since age 16;
- *Tenure*: total months the respondent has worked for the current employer;
- *Education*: highest grade or year of education completed;
- *AA (BA) degree:* dummy variable indicating the respondent has a 2-year (4-year) college degree as of each year interview;
- *Marital Status*: indicates by three dummy variables, which are married with spouse present, married without spouse present, never married;
- *Industry and Occupation*: information on industry of primary job (CPS) and all other jobs are available in every year in the sample. We use the standard industrial and occupational classification based on the Census data of the corresponding years to build 1-digit occupation (11 major groups) and 1-digit industry (10 major groups) categories;
- *AFQT*: AFQT (Armed Forces Qualification Test) is a test used by the military to judge whether an applicant is suitable for military service. The test was given to NLSY respondents;
- *Firm Size*: a dummy variable indicating whether the employees of a firm are greater than 1,000 or not.

3. ORG Data

The Outgoing Rotation Groups are of the Current Population Survey (CPS) which is a monthly survey of about 50,000 households. Each household entering the CPS is in the survey for 4 consecutive months, out for 8, and then returned for another four months before leaving the sample permanently. Since 1979 only households in months 4 and 8 have been asked their usual weekly earnings and usual weekly hours. These are the outgoing rotations groups that are put together into a single Outgoing Rotation Group file. Hence, an individual appears only once in any file year, but may reappear in the following year.

In our study we constrained the data to individuals between 25 and 60 years old. The dependent variable is the log of hourly wages which is based on labor income from non-farm workers and non-self-employees. We divided the weekly labor income by the number of hours worked per week. We did not consider allocated or inputting information for earnings or hours. Again, we did not give any special treatment to top-coding observations.

We use the standard industrial and occupational classification to build 1-digit occupation (11 major groups) and 1-digit industry (10 major groups) categories. Based on these major categories we merge the ORG data with the NIOSH risk data for each worker in the sample. In the case of BLS risk data that is given at 3-digit industry and occupational level, we merge it with the March CPS data using the 3-digit level for both industry and occupational categories.
The rest of independent variables used in the regressions are constructed in a similar manner to those from the March Supplement. The mean values and standard deviations for the variables used in the regressions are in the appendix tables.

4. NIOSH Risk Data

NIOSH risk data is constructed by the National Institute for Occupational Safety and Health (NIOSH), the Federal agency responsible for conducting research and making recommendations for the prevention of work-related disease and injury. It collects the death certificates from all 50 states and the District of Columbia in answer to the need for a comprehensive enumeration of workers who sustain a fatal work-related injury. The fatality rates were calculated as deaths per 100,000 workers. Rates were not calculated for categories with less than three fatalities or less than 20,000 employees.

The NIOSH risk containing both industry and occupation one-digit level job risks, which varies across state was merged to March CPS, NLSY and ORG data by state and one-digit industry/occupation categories according to the following scheme: data from 1983-1985 was merged to NIOSH 1985, data from 1986-1990 was merged to NIOSH 1990, and data 1991-2000 was merged to NIOSH 1995.

5. BLS Risk Data

The BLS Risk data is based on the Census of Fatal Occupational Injuries which has been conducted in all 50 States and the District of Columbia by the Bureau of Labor Statistics, as part of the BLS occupational safety and health statistics program. Information about each fatality is obtained by cross-referencing source documents (death certificates, workers' compensation records and reports to federal and state agencies). The Census gives the number of fatalities at 3-digit Industry and Occupational level for each cell with at least 5 deaths per year.

We build the BLS industry and occupational fatality risk rate as the ratio between the number of fatalities in each 3-digit industry and occupational category and the weighted number of workers in each cell. The weighted number of workers in each 3-digit category was extracted from the March CPS Data and ORG Data. Finally the risk rate was expressed as:

$$Risk_i = \frac{\# Fatalities_{ij}}{\# Workers_{ij}} * 100,000$$

for i= industry, occupation.

j=3-digit cells.

Appendix B Table 1. Mean Values of 1993 NLSY Data

Variables	Male	Female
Hourly Real Wage	9.54 (6.39)	7.87 (4.26)
Marital Status:		
Never Married	0.43 (0.50)	0.44 (0.50)
Married Spouse Present	0.44 (0.50)	0.44 (0.50)
Other	0.13 (0.34)	0.12 (0.32)
Job Risk		
Industry Risk	4.76 (5.05)	3.07 (3.70)
Occupation Risk	6.50 (7.72)	4.95 (7.70)
Age	32 (2.21)	32 (2.23)
Background		
Experience	11.30 (5.50)	10.52 (3.15)
Union	0.28 (0.45)	0.25 (0.43)
Tenure	7.46 (20.73)	6.37 (18.78)
Firm Size	0.61 (0.49)	0.59 (0.49)
Education		
AFQT	10.64 (2.66)	9.93 (2.29)
Less high school	0.02 (0.14)	0.01 (0.07)
Some high school	0.09 (0.28)	0.04 (0.20)
High school		
Some college	0.29 (0.45)	0.35 (0.48)
Associate Degree	0.09 (0.29)	0.13 (0.34)
Bachelor Degree	0.16 (0.37)	0.17 (0.37)
Master Degree	0.06 (0.23)	0.09 (0.29)
PhD Degree	0.02 (0.13)	0.01 (0.08)
Professional Degree	0.02 (0.14)	0.02 (0.15)
Race		
White	0.52 (0.50)	0.50 (0.50)
Black	0.28 (0.45)	0.31 (0.46)
Hispanic	0.20 (0.40)	0.19 (0.39)
Number of Observations	1,210	1,166

Appendix B Table 2. Mean Values of 1995 March CPS and 1995 ORG Data

	1995 March CPS Data		1995 ORG Data	
Variables	**Male**	**Female**	**Male**	**Female**
Hourly real wage	10.42	7.84	10.09	7.80
	(8.39)	(12.3)	(6.05)	(5.02)
Age	40.05	40.09	39.91	40.11
	(9.43)	(9.37)	(9.36)	(9.26)
Marital Status				
Married	0.713	0.655	0.710	0.638
	(0.45)	(0.47)	(0.45)	(0.48)
Widowed	0.004	0.023	0.004	0.024
	(0.06)	(0.15)	(0.06)	(0.15)
Divorced	0.109	0.175	0.108	0.181
	(0.31)	(0..38)	(0.31)	(0..38)
Never married	0.172	0.145	0.176	0.155
	(0.37)	(0.35)	(0.38)	(0.36)
Education				
Less junior high school	0.030	0.018	0.019	0.011
	(0.17)	(0.13)	(0.13)	(0.10)
Some high school	0.076	0.062	0.065	0.053
	(0.26)	(0.24)	(0.24)	(0.22)
High school	0.322	0.343	0.328	0.338
	(0.46)	(0.47)	(0.46)	(0.47)
Some college	0.181	0.198	0.184	0.199
	(0.38)	(0.39)	(0.38)	(0.39)
College degree	0.189	0.181	0.196	0.193
	(0.39)	(0.38)	(0.39)	(0.39)
Master	0.067	0.064	0.071	0.071
	(0.25)	(0.24)	(0.25)	(0.25)
Ph.D.	0.015	0.010	0.016	0.007
	(0.12)	(0.10)	(0.12)	(0.08)
Race/Ethnicity				
White	0.855	0.833	0.852	0.829
	(0.34)	(0.37)	(0.35)	(0.37)
Black	0.076	0.100	0.083	0.112
	(0.26)	(0.30)	(0.27)	(0.31)
Asian	0.033	0.034	0.035	0.034
	(0.17)	(0.18)	(0.18)	(0.18)
Hispanic	0.128	0.108	0.076	0.061
	(0.33)	(0.31)	(0.26)	(0.24)
Firm Size				
< 10 workers	0.140	0.139	----------	----------
	(0.34)	(0.34)		
[10-25] workers	0.092	0.08	----------	----------
	(0.29)	(0.27)		
[25-99]workers	0.143	0.127	-----------	-----------
	(0.35)	(0.33)		
[100-499] workers	0.153	0.158	-----------	-----------
	(0.36)	(0.36)		
[500-1000] workers	0.058	0.069	----------	----------
	(0.23)	(0.25)		
> 1000 workers	0.407	0.423	----------	----------
	(0.49)	(0.49)		
No. of observations	25,681	24,814	65,365	63,063

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Table 1A. Estimated Price of Risk for Male Workers

Panel 1. March CPS and NIOSH Industry Risk: 1985-1995*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	210	467	280	106
	(12.83)	(26.33)	(15.3)	(4.78)
Value of Statistical Life (in millions of dollars)	7.4	16.4	9.8	3.7

*There are 266,534 observations in the regressions.

Panel 2. March CPS and NIOSH Occupation Risk: 1985-1995*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Industry	no	no	yes	yes
Occupation	no	no	no	yes
Risk/100,000	-262	-188	-90.5	19.47
	(-19.28)	(-13.01)	(-4.56)	(1.18)
Value of Statistical Life (in millions of dollars)	----	----	----	0.7

*There are 266,534 observations in the regressions.

Panel 3. March CPS and BLS Industry Risk: 1995-2000*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	-84.5	7.73	187	-188
	(-3.76)	(0.35)	(7.77)	(-6.79)
Value of Statistical Life (in millions of dollars)	----	0.3	6.6	----

*There are 102,411 observations in the regressions.

Panel 4. March CPS and BLS Occupation Risk: 1995-2000*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	-12.3	-5.17	-25.2	18.1
	(-2.15)	(-0.91)	(-4.46)	(3.24)
Value of Statistical Life (in millions of dollars)	----	----	----	0.6

*There are 102,411 observations in the regressions.

Table 1A cont. Estimated Price of Risk for Male Workers

Panel 5. NLSY and NIOSH Industry Risk: 1986-1993*				
Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	281	673	754	-290
	(3.28)	(6.82)	(7.34)	(-5.59)
Value of Statistical Life (in millions of dollars)	9.8	23.6	26.5	----
*There are 20,338 observations in the regressions.				

Panel 6. NLSY and NIOSH Occupation Risk: 1986-1993*				
Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	-253	-228	-290	-145
	(-5.19)	(-4.33)	(-5.59)	(-2.14)
Value of Statistical Life (in millions of dollars)	----	----	----	----
*There are 20,338 observations in the regressions.				

Panel 7. ORG and NIOSH Industry Risk: 1985-1995*				
Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	160	359	464	84.2
	(20.39)	(42.75)	(55.72)	(8.01)
Value of Statistical Life (in millions of dollars)	5.6	12.6	16.3	2.9
*There are 550,119 observations in the regressions.				

Panel 8. ORG and NIOSH Occupation Risk: 1985-1995*				
Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	-400	-338	-365	105
	(-54.27)	(-43.13)	(-46.06)	(10.58)
Value of Statistical Life (in millions of dollars)	----	----	----	3.7
*There are 550,119 observations in the regressions.				

Table 1A cont. Estimated Price of Risk for Male Workers

Panel 9. ORG and BLS Industry Risk: 1995-2000*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	-126	-22.7	170	-121
	(-9.36)	(-3.51)	(12.16)	(-6.90)
Value of Statistical Life (in millions of dollars)	----	----	6.0	----

*There are 242,109 observations in the regressions.

Panel 10. ORG and BLS Occupation Risk: 1995-2000*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	-117	-83.8	-126	35.1
	(-16.06)	(-10.78)	(-17.82)	(4.39)
Value of Statistical Life in millions of dollars)	----	----	----	1.2

*There are 242,109 observations in the regressions.

Table 1B. Estimated Price of Risk for Female Workers

Panel 1. March CPS and NIOSH Industry Risk: 1985-1995*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	276	501	340	-31.9
	(10.77)	(18.91)	(12.4)	(-0.99)
Value of Statistical Life (in millions of dollars)	9.7	17.6	11.9	----

*There are 250,354 observations in the regressions.

Panel 2. March CPS and NIOSH Occupational Risk: 1985-1995*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	-359	-262	-128	-59.1
	(-14.61)	(-10.29)	(-7.56)	(-2.07)
Value of Statistical Life (in millions of dollars)	----	----	----	----

*There are 250,354 observations in the regressions.

Panel 3. March CPS and BLS Industry Risk: 1995-2000*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	33.8	87.0	19.7	-292
	(0.87)	(2.27)	(4.81)	(-5.78)
Value of Statistical Life (in millions of dollars)	1.2	3.0	0.7	----

*There are 102,411 observations in the regressions.

Panel 4. March CPS and BLS Occupation Risk: 1995-2000*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	-5.06	-4.92	-2.60	94.8
	(-0.22)	(-0.21)	(-0.11)	(4.10)
Value of Statistical Life (in millions of dollars)	----	----	----	3.3

*There are 102,411 observations in the regressions.

Table 1B cont. Estimated Price of Risk for Female Workers

Panel 5. NLSY and NIOSH Industry Risk: 1986-1993*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	129	716	700	-334
	(1.15)	(5.67)	(5.34)	(-1.79)
Value of Statistical Life (in millions of dollars)	4.5	25.1	24.6	----

*There are 19,272 observations in the regressions.

Panel 6. NLSY and NIOSH Occupation Risk: 1986-1993*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	-547	-290	-250	48.20
	(-5.28)	(-2.61)	(-2.25)	(0.25)
Value of Statistical Life (in millions of dollars)	----	----	----	1.7

*There are 19,272 observations in the regressions.

Panel 7. ORG and NIOSH Industry Risk: 1985-1995*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	293	438	415	-2.63
	(25.54)	(37.08)	(36.67)	(-0.19)
Value of Statistical Life (in millions of dollars)	10.3	15.4	14.6	----

*There are 556,532 observations in the regressions.

Panel 8. ORG and NIOSH Occupation Risk: 1985-1995*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	-291	-231	-133	33.6
	(-22.71)	(-17.41)	(-10.17)	(2.16)
Value of Statistical Life (in millions of dollars)	----	----	----	1.2

*There are 556,532 observations in the regressions.

Table 1B cont. Estimated Price of Risk for Female Workers

Panel 9. ORG and BLS Industry Risk: 1995-2000*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	-55.10	-24.3	97.6	---
	(-2.30)	(-1.03)	(4.14)	(-11.01)
Value of Statistical Life (in millions of dollars)	----	----	3.4	----

*There are 246,904 observations in the regressions.

Panel 10. ORG and BLS Occupation Risk: 1995-2000*

Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Occupation	no	no	yes	yes
Industry	no	no	no	yes
Risk/100,000	-130	-109	-68.1	56.7
	(-5.55)	(-4.82))	(-3.18)	(2.52)
Value of Statistical Life (in millions of dollars)	----	----	----	2.0

*There are 246,904 observations in the regressions.

Note: 1. The dependent variable is the natural log of the worker's real wage. For the basic regression, the independent variables include a quartic in the worker's age, a vector of dummy variables that control for the worker's education, a vector of dummy variables indicating whether the worker is Hispanic, Asian, African American, or other race, and a dummy variable indicating whether the worker is under a union contract or not, and dummy variables for the worker's marital status. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses. The pooled data sets (1985-1995, 1995-2000, 1986-1993) are used to estimate the time-specific fixed-effect (within-group estimate).
2. In NLSY data, the independent variables for the basic regression include a quartic in the workers' age, education level, union, working experience, tenure status, AFQT scores, race/ethnicity categories, and dummy variables for the worker's marital status. .
Source: Authors' calculations

Table 2. Estimated Returns to a Bachelor's Degree for ORG Data, 1995

Panel 1. Male workers-ORG 1995*				
Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Industry	no	no	yes	yes
Occupation	no	no	no	yes
BA Degree	0.397	0.386	0.385	0.240
(relative to high school degree)	(62.56	(47.53)	(47.63)	(27.86)
*There are 51,659 observations in the regressions.				
Panel 2. Female workers-ORG 1995*				
Basic Controls	yes	yes	yes	yes
State	no	yes	yes	yes
Industry	no	no	yes	yes
Occupation	no	no	no	yes
BA Degree	0.479	0.459	0.444	0.277
(relative to high school degree)	(76.68)	(57.72)	(56.24)	(33.68)
*There are 53,291 observations in the regressions.				

Note: The dependent variable is the natural log of the worker's real wage. For the basic regression, the independent variables include a quartic in the worker's age, a vector of dummy variables that control for the worker's education, a vector of dummy variables indicating whether the worker is Hispanic, Asian, African American, or other race, a dummy variable indicating whether the worker is under a union contract or not, and dummy variables for the worker's marital status. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses.

Table 3A. Semi-parametric Estimated Price of Risk for Male Workers

Panel 1. March CPS and NIOSH Industry Risk: 1985-1995*

Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	493	212	47.6
	(18.84)	(12.13)	(0.14)
Value of Statistical Life (in millions of dollars)	17.3	7.4	1.7

*There are 266,535 observations in the regression.

Panel 2. March CPS and NIOSH Occupation Risk: 1985-1995*

Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-235	-276	-377
	(-10.44)	(-19.03)	(-13.94)
Value of Statistical Life (in millions of dollars)	----	----	----

*There are 250,354 observations in the regression.

Panel 3. March CPS and BLS Industry Risk: 1995-2000*

Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-70.8	-43.8	-218
	(-2.88)	(-1.08)	(-1.14)
Value of Statistical Life (in millions of dollars)	----	----	----

*There are 112,416 observations in the regression

Panel 4. March CPS and BLS Occupation Risk: 1995-2000*

Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-11.91	-7.13	376
	(-1.86)	(-0.53)	(2.97)
Value of Statistical Life (in millions of dollars)	----	----	13.2

*There are 102,411 observations in the regression.

Table 3A cont. Semi-parametric Estimated Price of Risk for Male Workers

Panel 5. ORG and NIOSH Industry Risk: 1985-1995*			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	151	323	451
	(19.23)	(35.65)	(7.36)
Value of Statistical Life (in millions of dollars)	5.3	11.3	15.8
*There are 546,210 observations in the regressions.			
Panel 6. ORG and NIOSH Occupation Risk: 1985-1995*			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-403	-399	450
	(-54.45)	(-46.42)	(7.31)
Value of Statistical Life (in millions of dollars)	---	---	15.8
*There are 546,210 observations in the regressions.			
Panel 7. ORG and BLS Industry Risk: 1995-2000*			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-142	-94.4	-339
	(-10.44)	(-6.25)	(-7.21)
Value of Statistical Life (in millions of dollars)	---	---	---
*There are 242,109 observations in the regressions.			
Panel 8. ORG and BLS Occupation Risk: 1995-2000*			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-123	-120	13.60
	(-17.20)	(-14.44)	(0.66)
Value of Statistical Life (in millions of dollars)	---	---	0.5
*There are 242,109 observations in the regressions.			

Note: There is a fixed-effect in each regression for each combination of the independent variables. The basic regression includes age, education level, and race/ethnicity category. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses.
Source: Authors' calculations.

Table 3B. Semi-parametric Estimated Price of Risk for Female Workers

Panel 1. March CPS and NIOSH Industry Risk: 1985-1995*

Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	689	291	330
	(16.19)	(10.57)	(0.44)
Value of Statistical Life (in millions of dollars)	24.2	10.2	11.6

*There are 250,354 observations in the regression.

Panel 2. March CPS and NIOSH Occupation Risk: 1985-1995*

Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-337	-377	-73.7
	(-7.14)	(-13.94)	(-0.27)
Value of Statistical Life (in millions of dollars)	----	----	----

*There are 243,958 observations in the regression.

Panel 3. March CPS and BLS Industry Risk: 1995-2000*

Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	78.7	95.4	-526
	(1.81)	(1.28)	(-1.47)
Value of Statistical Life (in millions of dollars)	2.8	3.3	----

*There are 108,324 observations in the regression.

Panel 4. March CPS and BLS Occupation Risk: 1995-2000*

Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	7.64	48.1	-116
	(0.32)	(1.41)	(-0.24)
Value of Statistical Life (in millions of dollars)	0.3	1.7	----

*There are 84,534 observations in the regression.

Table 3B cont. Semi-parametric Estimated Price of Risk for Female Workers

Panel 5. ORG and NIOSH Industry Risk: 1985-1995*			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	272	386	187
	(23.68)	(30.37)	(1.61)
Value of Statistical Life (in millions of dollars)	9.5	13.5	6.6
*There are 556,532 observations in the regressions.			
Panel 6. ORG and NIOSH Occupation Risk: 1985-1995*			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-297	-251	-90.5
	(-23.07)	(-0.76)	(-0.76)
Value of Statistical Life (in millions of dollars)	----	----	----
*There are 556,532 observations in the regressions.			
Panel 7. ORG and BLS Industry Risk: 1995-2000*			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-77.60	-22.9	-432
	(-3.21)	(-0.85)	(-5.60)
Value of Statistical Life (in millions of dollars)	----	----	----
*There are 242,109 observations in the regressions.			
Panel 8. ORG and BLS Occupation Risk: 1995-2000*			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-222	-191	180
	(-7.64)	(-5.89)	(2.64)
Value of Statistical Life (in millions of dollars)	----	----	6.3
*There are 242, 109 observations in the regressions.			

Note: There is a fixed-effect in each regression for each combination of the independent variables. The basic regression includes age, education level, race/ethnicity category, and marital status dummy variables. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses.
Source: Authors’ calculations.

Table 4A. Estimated Price of Risk for Males by Decile of Risk

Panel 1. March CPS and NIOSH Industry Risk: 1985-1995*

Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	-0.05	-0.02	0.05
[0.9, 1.4]	(-7.54)	(-2.89)	(0.81)
Third	-0.08	-0.02	-0.01
[1.4, 1.7]	(-10.40)	(-4.48)	(-1.50)
Fourth	-0.02	0.01	0.01
[1.7, 2.1]	(-4.35)	(2.08)	(1.59)
Fifth	-0.08	-0.00	-0.00
[2.1, 2.6]	(-11.66)	(-1.24)	(-0.13)
Sixth	0.03	0.07	0.02
[2.6, 3.2]	(4.62)	(10.48)	(2.86)
Seventh	-0.02	0.08	0.02
[3.2, 4]	(-4.09)	(11.88)	(2.64)
Eighth	0.01	0.10	0.02
[4, 6.4]	(1.88)	(15.88)	(2.56)
Ninth	0.06	0.15	0.03
[6.4, 12.5]	(11.44)	(23.36)	(3.81)
Tenth	0.01	0.16	0.05
[> 12.5]	(1.98)	(24.76)	(4.43)

Table 4A cont. Estimated Price of Risk for Males by Decile of Risk

Panel 2. March CPS and NIOSH Occupation Risk: 1985-1995

Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	0.12	0.05	0.01
[0.6, 0.9]	(15.14)	(6.07)	(1.41)
Third	0.11	0.07	0.00
[0.9, 1.4]	(14.80)	(8.70)	(0.11)
Fourth	0.08	0.04	0.03
[1.4, 1.9]	(11.13)	(5.95)	(3.52)
Fifth	0.08	0.06	0.00
[1.9, 2.5]	(10.99)	(7.41)	(1.04)
Sixth	0.02	0.01	0.01
[2.5, 3.2]	(3.10)	(1.62)	(1.12)
Seventh	0.00	0.03	0.01
[3.2, 4.1]	(0.14)	(4.10)	(1.15)
Eighth	0.07	0.09	0.01
[4.1, 6.8]	(10.31)	(11.15)	(1.20)
Ninth	0.07	0.07	0.01
[6.8, 11.9]	(10.18)	(9.03)	(1.40)
Tenth	-0.05	-0.03	0.01
[> = 11.9]	(-7.79)	(-3.96)	(1.40)

Table 4A cont. Estimated Price of Risk for Males by Decile of Risk

Panel 3. March CPS and BLS Industry Risk: 1995-2000			
Basic Control	yes	yes	yes
State	yes	yes	yes
Industry/Occupation	no	no	yes
Second	-0.01	-0.01	0.01
[0.52, 0.67]	(-1.53)	(-1.07)	(1.08)
Third	0.05	0.05	0.01
[0.67, 0.90]	(5.22)	(5.33)	(1.42)
Fourth	0.07	0.07	0.00
[0.90, 1.30]	(7.71)	(7.33)	(0.17)
Fifth	0.05	0.04	0.00
[1.30, 1.98]	(5.53)	(1.64)	(0.63)
Sixth	-0.01	-0.01	-0.04
[1.98, 2.83]	(-1.83)	(-2.08)	(-5.15)
Seventh	-0.05	-0.04	-0.06
[2.83, 4.03]	(-5.51)	(-5.23)	(-7.21)
Eighth	-0.00	-0.00	-0.04
[4.03, 6.51]	(-0.77)	(-0.19)	(-4.28)
Ninth	0.06	0.07	-0.04
[6.51, 13.05]	(7.11)	(8.02)	(-4.65)
Tenth	0.02	0.03	-0.08
[> 13.05]	(2.74)	(3.89)	(8.35)

Table 4A cont. Estimated Price of Risk for Males by Decile of Risk

Panel 4. March CPS and BLS Occupation Risk: 1995-2000

Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	-0.26	-0.26	-.017
[0.0, 0.37]	(-8.28)	(-8.19)	(-5.59)
Third	-0.00	-0.04	-0.00
[0.37, 0.86]	(-0.25)	(-0.47)	(-0.33)
Fourth	0.02	0.02	0.05
[0.86, 1.26]	(2.84)	(2.81)	(6.15)
Fifth	0.02	0.02	0.01
[1.26, 2.03]	(2.91)	(2.52)	(2.28)
Sixth	0.19	0.19	0.09
[2.03, 2.61]	(25.09)	(24.99)	(11.74)
Seventh	0.01	0.01	0.01
[2.61, 3.58]	(1.92)	(1.69)	(1.59)
Eighth	0.01	0.01	0.02
[3.58, 5.51]	(1.53)	(1.74)	(3.26)
Ninth	0.01	0.01	0.07
[5.51, 15.19]	(1.35)	(2.04)	(9.52)
Tenth	-0.05	-0.03	0.09
[> 15.19]	(-7.04)	(-5.37)	(10.69)

Table 4A cont. Estimated Price of Risk for Males by Decile of Risk

Panel 5. NLSY and NIOSH Industry Risk: 1986-1993

Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	-0.06	-0.03	0.02
[1, 1.4]	(-2.48)	(-0.95)	(0.79)
Third	-0.12	-0.11	-0.02
[1.4, 2]	(-4.02)	(-3.78)	(-0.68)
Fourth	-0.04	-0.03	0.01
[2, 2.1]	(-1.40)	(-0.88)	(0.37)
Fifth	-0.09	0.01	0.05
[2.1, 2.7]	(-3.09)	(0.50)	(1.62)
Sixth	0.02	0.08	0.04
[2.7, 3.4]	(1.21)	(2.98)	(1.25)
Seventh	-0.06	0.06	0.07
[3.4, 4.6]	(-2.62)	(2.29)	(2.00)
Eighth	0.03	0.09	0.01
[4.6, 7.1]	(1.18)	(3.41)	(0.27)
Ninth	0.05	0.13	0.00
[7.1, 12.3]	(2.23)	(4.92)	(-0.05)
Tenth	0.02	0.14	-0.02
[> 12.3]	(0.85)	(4.89)	(-0.42)

Table 4A cont. Estimated Price of Risk for Males by Decile of Risk

Panel 6. NLSY and NIOSH Occupation Risk: 1986-1993

Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	0.09	0.02	0.00
[0.6, 1]	(3.03)	(0.62)	(-0.16)
Third	0.08	0.06	0.03
[1, 1.5]	(2.64)	(1.92)	(0.86)
Fourth	0.07	0.03	-0.01
[1.5, 2.1]	(2.41)	(0.92)	(-0.44)
Fifth	0.04	0.05	0.01
[2.1, 2.7]	(1.46)	(1.68)	(0.21)
Sixth	0.06	0.05	0.04
[2.7, 3.7]	(1.97)	(1.71)	(0.96)
Seventh	0.05	0.06	0.02
[3.7, 4.6]	(1.77)	(2.04)	(0.57)
Eighth	0.16	0.14	0.01
[4.6, 7.9]	(5.86)	(4.99)	(0.28)
Ninth	0.07	0.09	0.01
[7.9, 16.2]	(2.54)	(3.13)	(0.12)
Tenth	-0.04	-0.04	-0.03
[> 16.2]	(-1.49)	(1.39)	(-0.50)

Table 4A. Estimated Price of Risk for Males by Decile of Risk

Panel 7. ORG and NIOSH Industry Risk: 1985-1995

Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	-0.06	-0.04	-0.02
[0.9, 1.4]	(-22.24)	(-14.02)	(-6.96)
Third	-0.08	-0.03	-0.01
[1.4, 1.7]	(-22.90)	(-9.23)	(-4.97)
Fourth	-0.03	-0.01	-0.01
[1.7, 2]	(-11.96)	(-6.12)	(-3.35)
Fifth	-0.07	-0.01	-0.02
[2, 2.5]	(-24.74)	(-4.70)	(-6.95)
Sixth	0.01	0.04	-0.01
[2.5, 3.2]	(3.97)	(15.61)	(-3.07)
Seventh	-0.01	0.06	-0.01
[3.2, 4]	(-6.11)	(22.32)	(-3.97)
Eighth	0.01	0.09	-0.01
[4, 6]	(5.63)	(30.19)	(-5.09)
Ninth	0.03	0.11	-0.01
[6, 11.4]	(10.81)	(37.21)	(-4.33)
Tenth	-0.00	0.10	-0.01
[> 11.4]	(-0.90)	(36.03)	(-3.66)

Table 4A cont. Estimated Price of Risk for Males by Decile of Risk

Panel 8. ORG and NIOSH Occupation Risk: 1985-1995

Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	0.13	0.07	0.01
[0.6, 0.9]	(38.56)	(18.43)	(3.86)
Third	0.13	0.09	0.00
[0.9, 1.3]	(36.21)	(22.95)	(2.48)
Fourth	0.06	0.03	0.01
[1.3, 1.8]	(17.91)	(8.95)	(3.14)
Fifth	0.04	0.03	-0.00
[1.8, 2.4]	(11.66)	(8.76)	(-0.20)
Sixth	0.06	0.03	0.00
[2.4, 2.9]	(17.55)	(10.28)	(0.30)
Seventh	0.00	0.00	-0.00
[2.9, 3.9]	(2.07)	(1.77)	(-0.29)
Eighth	0.04	0.04	-0.00
[3.9, 6.5]	(13.83)	(11.19)	(1.44)
Ninth	0.05	0.04	-0.01
[6.5, 10.9]	(17.12)	(12.07)	(-2.83)
Tenth	-0.06	-0.07	-0.00
[> 10.9]	(-20.02)	(-20.40)	(-0.03)

Table 4A cont. Estimated Price of Risk for Males by Decile of Risk

Panel 9. ORG and BLS Industry Risk: 1995-2000

Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	0.04	0.03	0.03
[0.47, 0.6]	(7.33)	(4.83)	(4.54)
Third	0.08	0.06	0.02
[0.6, 0.8]	(13.36)	(10.05)	(4.33)
Fourth	0.06	0.05	0.00
[0.8, 1.1]	(11.88)	(8.31)	(-0.56)
Fifth	0.00	-0.02	-0.03
[1.1, 1.7]	(0.02)	(-4.44)	(-5.59)
Sixth	-0.01	-0.02	-0.05
[1.7, 2.3]	(-1.87)	(-4.51)	(-9.93)
Seventh	-0.01	-0.02	-0.04
[2.3, 3.5]	(-1.55)	(-3.56)	(-7.07)
Eighth	0.02	0.01	-0.03
[3.5, 5.7]	(5.25)	(3.15)	(-6.30)
Ninth	0.05	0.05	-0.03
[5.7, 11.8]	(10.41)	(8.37)	(-5.35)
Tenth	0.05	0.04	-0.04
[> 11.8]	(10.32)	(7.74)	(-9.73)

Table 4A cont. Estimated Price of Risk for Males by Decile of Risk

Panel 10. ORG and BLS Occupation Risk: 1995-2000			
Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	0.05	0.05	0.05
[0, 0.3]	(4.43)	(3.89)	(4.25)
Third	-0.04	-0.05	0.01
[0.3, 0.7]	(-7.58)	(-8.71)	(2.44)
Fourth	-0.06	-0.06	0.00
[0.7, 1]	(-12.22)	(-11.31)	(-1.21)
Fifth	0.02	0.01	0.00
[1, 1.7]	(4.50)	(3.02)	(0.48)
Sixth	0.01	0.00	0.00
[1.7, 2.5]	(3.25)	(0.94)	(0.02)
Seventh	0.08	0.08	0.01
[2.5, 3.4]	(19.32)	(17.45)	(3.28)
Eighth	0.02	0.01	0.00
[3.4, 5.3]	(6.23)	(2.74)	(1.94)
Ninth	-0.06	-0.05	0.01
[5.3, 13.9]	(-14.17)	(-11.92)	(3.80)
Tenth	-0.04	-0.04	-0.01
[> 13.9]	(-12.84)	(-10.95)	(-2.86)

Table 4B. Estimated Price of Risk for Females by Decile of Risk

Panel 1. March CPS and NIOSH Industry Risk: 1985-1995			
Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	-0.08	-0.03	-0.01
[0.9, 1.4]	(-15.64)	(-6.52)	(-1.89)
Third	-0.09	-0.04	-0.01
[1.4, 1.7]	(-15.66)	(-6.56)	(-1.44)
Fourth	-0.10	-0.05	-0.02
[1.7, 2.1]	(-20.82)	(-8.66)	(-3.16)
Fifth	-0.20	-0.08	-0.04
[2.1, 2.6]	(-32.61)	(-12.48)	(-5.53)
Sixth	-0.08	-0.03	-0.00
[2.6, 3.2]	(-12.21)	(-5.18)	(-0.05)
Seventh	-13	-0.03	-0.00
[3.2, 4]	(-22.40)	(-4.53)	(-0.05)
Eighth	-0.03	0.04	-0.01
[4, 6.4]	(-5.19)	(5.82)	(-1.82)
Ninth	0.02	0.12	-0.07
[6.4, 12.5]	(3.72)	(16.56)	(-0.62)
Tenth	0.00	0.12	-0.01
[> 12.5]	(0.63)	(14.13)	(-0.81)

Table 4B cont. Estimated Price of Risk for Females by Decile of Risk

Panel 2. March CPS and NIOSH Occupation Risk: 1985-1995			
Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	0.07	0.02	0.00
[0.6, 0.9]	(13.27)	(3.98)	(1.08)
Third	0.05	0.04	0.00
[0.9,1.4]	(9.47)	(6.48)	(1.08)
Fourth	-0.01	-0.03	-0.00
[1.4, 1.9]	(-1.97)	(-4.85)	(-0.01)
Fifth	-0.03	-0.03	-0.02
[1.9,2.5]	(-6.11)	(-5.43)	(-3.18)
Sixth	-0.10	-0.10	-0.02
[2.5, 3.2]	(-16.14)	(-15.10)	(-2.69)
Seventh	-0.10	-0.07	-0.02
[3.2, 4.1]	(-17.25)	(-10.95)	(-2.58)
Eighth	-0.06	-0.00	-0.03
[4.1, 6.8]	(-9.29)	(-1.16)	(-3.32)
Ninth	-0.02	-0.02	-0.04
[6.8, 11.9]	(-2.51)	(-3.02)	(-3.27)
Tenth	-0.11	-0.10	-0.04
[> 11.9]	(-13.30)	(-11.17)	(-3.20)

Table 4B cont. Estimated Price of Risk for Females by Decile of Risk

Panel 3. March CPS and BLS Industry Risk: 1995-2000

Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	0.08	0.08	0.07
[0.52, 0.67]	(12.07)	(13.01)	(12.31)
Third	0.14	0.14	0.11
[0.67, 0.90]	(21.24)	(20.77)	(17.91)
Fourth	0.15	0.14	0.10
[0.90, 1.30]	(22.13)	(20.71)	(14.68)
Fifth	0.07	0.06	0.07
[1.30, 1.98]	(10.10)	(8.93)	(9.52)
Sixth	0.02	0.02	0.02
[1.98, 2.83]	(3.80)	(2.80)	(3.62)
Seventh	-0.03	-0.04	-0.01
[2.83, 4.03]	(-4.57)	(-5.23)	(-0.17)
Eighth	0.00	0.08	0.00
[4.03, 6.51]	(1.13)	(0.89)	(0.42)
Ninth	0.10	0.09	0.01
[6.51, 13.05]	(10.32)	(10.20)	(1.66)
Tenth	0.13	0.13	-0.01
[> 13.05]	(12.05)	(12.74)	(-0.94)

Table 4B cont. Estimated Price of Risk for Females by Decile of Risk

Panel 4. March CPS and BLS Occupation Risk: 1995-2000			
Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	0.02	0.02	0.00
[0.0, 0.37]	(3.26)	(2.76)	(0.70)
Third	0.06	0.05	0.06
[0.37, 0.86]	(8.56)	(8.30)	(9.68)
Fourth	0.07	0.06	0.08
[0.86, 1.26]	(9.31)	(8.88)	(11.26)
Fifth	-0.00	-0.00	0.03
[1.26, 2.03]	(-0.34)	(-1.28)	(5.17)
Sixth	0.14	0.13	0.06
[2.03, 2.61]	(17.02)	(16.00)	(7.55)
Seventh	0.04	0.03	0.04
[2.61, 3.58]	(4.77)	(3.92)	(4.83)
Eighth	-0.05	-0.05	-0.01
[3.58, 5.51]	(-6.12)	(-6.49)	(-1.74)
Ninth	-0.11	-0.11	-0.04
[5.51, 15.19]	(-9.65)	(-10.21)	(-3.48)
Tenth	-0.49	-0.04	0.02
[> 15.19]	(-3.42)	(-2.95)	(1.70)

Table 4B cont. Estimated Price of Risk for Females by Decile of Risk

Panel 5. NLSY and NIOSH Industry Risk: 1986-1993

Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	-0.06	0.01	0.00
[1, 1.4]	(-2.90)	(0.41)	(-0.14)
Third	-0.11	-0.08	-0.05
[1.4, 2]	(-4.49)	(-2.94)	(-1.67)
Fourth	-0.09	-0.06	-0.05
[2, 2.1]	(-4.16)	(-2.13)	(-1.74)
Fifth	-0.13	-0.02	-0.02
[2.1, 2.7]	(-5.23)	(-0.63)	(-0.48)
Sixth	0.00	0.03	-0.01
[2.7, 3.4]	(-0.04)	(1.17)	(-0.39)
Seventh	-0.12	-0.02	-0.03
[3.4, 4.6]	(-4.98)	(-0.60)	(-0.89)
Eighth	-0.04	0.03	-0.08
[4.6, 7.1]	(-1.69)	(0.96)	(-1.87)
Ninth	-0.03	0.09	-0.11
[7.1, 12.3]	(-1.11)	(3.02)	(-2.20)
Tenth	-0.03	0.10	-0.15
[> 12.3]	(-1.13)	(2.84)	(-2.54)

Table 4B cont. Estimated Price of Risk for Females by Decile of Risk

Panel 6. NLSY and NIOSH Occupation Risk: 1986-1993

Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	0.03	-0.01	0.00
[0.6, 1]	(1.61)	(-0.53)	(0.18)
Third	-0.06	-0.05	0.00
[1, 1.5]	(-2.82)	(-1.96)	(0.15)
Fourth	0.03	0.00	0.07
[1.5, 2.1]	(1.42)	(0.00)	(2.43)
Fifth	-0.09	-0.07	0.02
[2.1, 2.7]	(-3.85)	(-2.92)	(0.66)
Sixth	-0.08	-0.07	0.10
[2.7, 3.7]	(-3.06)	(-2.68)	(2.53)
Seventh	-0.12	-0.10	0.04
[3.7, 4.6]	(-5.29)	(-3.92)	(1.15)
Eighth	-0.08	-0.06	0.02
[4.6, 7.9]	(-3.03)	(-1.90)	(0.46)
Ninth	-0.17	-0.13	-0.05
[7.9, 16.2]	(-5.00)	(-3.72)	(-0.78)
Tenth	-0.14	-0.11	-0.01
[> 16.2]	(-4.53)	(-3.49)	(-0.19)

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Table 4B. Estimated Price of Risk for Females by Decile of Risk

Panel 7. ORG and NIOSH Industry Risk: 1985-1995

Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	-0.07	-0.03	-0.09
[0.9, 1.4]	(-33.87)	(-14.93)	(-4.04)
Third	-0.08	-0.04	-0.02
[1.4, 1.7]	(-32.41)	(-15.09)	(-0.78)
Fourth	-0.08	-0.05	-0.01
[1.7, 2]	(-35.28)	(-19.39)	(-4.68)
Fifth	-0.14	-0.07	-0.01
[2, 2.5]	(-59.60)	(-26.85)	(-7.10)
Sixth	-0.09	-0.04	-0.00
[2.5, 3.2]	(-36.87)	(-16.77)	(-1.56)
Seventh	-0.11	-0.03	-0.00
[3.2, 4]	(-45.25)	(-13.07)	(0.88)
Eighth	-0.03	0.01	-0.01
[4, 6]	(-11.74	(6.44)	(-2.90)
Ninth	0.01	0.09	-0.00
[6, 11.4]	(4.92)	(29.32)	(-2.06)
Tenth	0.03	0.09	0.00
[> 11.4]	(11.79)	(31.06)	(1.24)

Table 4B cont. Estimated Price of Risk for Females by Decile of Risk

Panel 8. ORG and NIOSH Occupation Risk: 1985-1995			
Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	0.07	0.02	0.01
[0.6, 0.9]	(35.64)	(9.15)	(1.23)
Third	0.05	0.03	0.00
[0.9, 1.3]	(22.30)	(12.80)	(0.07)
Fourth	-0.01	-0.03	0.00
[1.3, 1.8]	(-4.78)	(-12.33)	(0.47)
Fifth	-0.05	-0.04	-0.02
[1.8, 2.4]	(-17.97)	(-14.22)	(-7.67)
Sixth	-0.03	-0.05	-0.02
[2.4, 2.9]	(-14.02)	(-19.33)	(-7.74)
Seventh	-0.06	-0.07	-0.02
[2.9, 3.9]	(-28.18)	(-29.44)	(-7.98)
Eighth	-0.06	-0.04	-0.02
[3.9, 6.5]	(-24.23)	(13.76)	(-6.96)
Ninth	0.01	-0.00	-0.03
[6.5, 10.9]	(3.19)	(-2.43)	(-6.82)
Tenth	0.01	-0.03	-0.00
[> 10.9]	(6.33)	(-11.90)	(-0.54)

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Table 4B cont. Estimated Price of Risk for Females by Decile of Risk

Panel 9. ORG and BLS Industry Risk: 1995-2000

Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	0.11	0.11	0.09
[0.4, 0.6]	(29.23)	(25.73)	(23.16)
Third	0.20	0.18	0.14
[0.6, 0.8]	(55.69)	(45.41)	(36.06)
Fourth	0.17	0.15	0.10
[0.8, 1.1]	(46.19)	(35.90)	(26.06)
Fifth	0.06	0.04	0.06
[1.1, 1.7]	(15.79)	(9.90)	(14.25)
Sixth	0.05	0.03	0.03
[1.7, 2.3]	(11.67)	(7.68)	(7.89)
Seventh	0.07	0.05	0.02
[2.3, 3.5]	(15.78)	(11.00)	(5.84)
Eighth	0.08	0.07	0.02
[3.5, 5.7]	(17.79)	(14.20)	(4.36)
Ninth	0.11	0.10	0.01
[5.7, 11.8]	(21.08)	(17.11)	(2.78)
Tenth	0.13	0.11	0.01
[> 11.8]	(40.42)	(30.63)	(4.62)

Table 4B cont. Estimated Price of Risk for Females by Decile of Risk

Panel 10. ORG and BLS Occupation Risk: 1995-2000			
Basic Control	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Second	0.00	0.00	0.01
[0, 0.3]	(1.21)	(0.51)	(2.88)
Third	0.01	0.01	0.06
[0.3, 0.7]	(3.63)	(3.08)	(16.15)
Fourth	0.03	0.03	0.05
[0.7, 1]	(7.97)	(7.37)	(12.62)
Fifth	0.03	0.02	0.02
[1, 1.7]	(7.98)	(5.67)	(5.64)
Sixth	0.02	0.01	0.01
[1.7, 2.5]	(4.96)	(2..37)	(3.04)
Seventh	0.09	0.09	0.03
[2.5, 3.4]	(20.63)	(18.51)	(6.72)
Eighth	0.07	0.05	0.04
[3.4, 5.3]	(15.72)	(10.72)	(8.93)
Ninth	-0.04	-0.04	0.01
[5.3, 13.9]	(-6.82)	(-6.39)	(1.97)
Tenth	-0.01	-0.01	-0.02
[>13.9]	(-3.02)	(-5.38)	(-7.74)

Note: 1. The NIOSH and BLS industry and occupation risk data are divided into 10 deciles. The dependent variable is the natural log of the worker's real wage. For the basic regression, the independent variables include a quartic in the worker's age, a vector of dummy variables that control for the worker's education, a vector of dummy variables indicating whether the worker is Hispanic, Asian, African American, or other race, a dummy variable indicating whether the worker is under a union contract or not, and marital status. Workers are aged 25 to 60 inclusive. T-statistics are given in parenthesis. Cut-off points are given in brackets.

2. In NLSY data, the independent variables for the basic regression include a drop in the workers' age, educational level, union coverage, working experience, tenure status, AFQT test scores and race/ethnicity categories.

Source: Authors' calculation

Table 5A. Propensity Score Matching Estimators: Nearest-Neighbor Matching* for Males 1993 CPS Outgoing Rotation Data, 1993 March CPS Data, 1995 NIOSH Risk Data, 1994 BLS Risk Data, 1993 NLSY Data

Panel 1. March CPS and NIOSH Industry Risk				
	Quintile 5 vs. 1	**Quintile 4 vs. 1**	**Quintile 3 vs. 1**	**Quintile 2 vs. 1**
Mean real wage of matched treatment group	10.55	10.18	10.86	10.85
Mean real wage of matched comparison group	10.99	11.60	11.03	13.42
Treatment Effect	**-0.44**	**-1.42**	**-0.19**	**-2.57**
	(-0.15)	(-0.71)	(-0.56)	(-0.72)
Panel 2. ORG and NIOSH Industry Risk				
	Quintile 5 vs. 1	**Quintile 4 vs. 1**	**Quintile 3 vs. 1**	**Quintile 2 vs. 1**
Mean real wage of matched treatment group	9.28	9.54	9.86	9.89
Mean real wage of matched comparison group	9.58	10.21	10.82	11.30
Treatment Effect	**-0.29**	**-0.66**	**-0.95**	**-1.41**
	(-1.47)	(-3.03)	(-4.24)	(-6.86)
Panel 3. NLSY and NIOSH Industry Risk				
	Quintile 5 vs. 1	**Quintile 4 vs. 1**	**Quintile 3 vs. 1**	**Quintile 2 vs. 1**
Mean real wage of matched treatment group	9.20	8.86	10.19	8.52
Mean real wage of matched comparison group	10.53	10.79	8.69	10.66
Treatment Effect	**-1.33**	**-1.93**	**1.50**	**-2.14**
	(-0.45)	(-1.30)	(1.46)	(-1.36)
Panel 4. March CPS and NIOSH Occupation Risk				
	Quintile 5 vs. 1	**Quintile 4 vs. 1**	**Quintile 3 vs. 1**	**Quintile 2 vs. 1**
Mean real wage of matched treatment group	8.67	10.53	11.68	13.56
Mean real wage of matched comparison group	8.86	9.42	9.72	11.39
Treatment Effect	**-0.18**	**1.11**	**1.95**	**2.17**
	(-0.30)	(1.94)	(2.83)	(2.56)

*Nearest-neighbor matching with a caliper 0.01

**Table 5A cont. Propensity Score Matching Estimators: Nearest-Neighbor Matching* for Males
1993 CPS Outgoing Rotation Data, 1993 March CPS Data, 1995 NIOSH Risk Data, 1994 BLS Risk Data, 1993 NLSY Data**

Panel 5. ORG and NIOSH Occupation Risk

	Quintile 5 vs. 1	Quintile 4 vs. 1	Quintile 3 vs. 1	Quintile 2 vs. 1
Mean real wage of matched treatment group	7.72	9.54	11.10	11.98
Mean real wage of matched comparison group	8.37	9.06	9.65	10.95
Treatment Effect	**-0.64**	**0.47**	**1.37**	**1.03**

Panel 6. NLSY and NIOSH Occupation Risk

	Quintile 5 vs. 1	Quintile 4 vs. 1	Quintile 3 vs. 1	Quintile 2 vs. 1
Mean real wage of matched treatment group	7.74	9.44	10.03	11.02
Mean real wage of matched comparison group	8.19	9.40	9.24	10.57
Treatment Effect	**-0.45**	**0.04**	**0.79**	**0.45**
	(-0.69)	(0.05)	(1.16)	(0.53)

Panel 7. March CPS and BLS Industry Risk

	Quintile 5 vs. 1	Quintile 4 vs. 1	Quintile 3 vs. 1	Quintile 2 vs. 1
Mean real wage of matched treatment group	10.99	9.85	10.55	13.23
Mean real wage of matched comparison group	9.77	9.63	10.14	11.30
Treatment Effect	**1.21**	**0.22**	**0.41**	**1.93**
	(2.97)	(0.69)	(1.38)	(3.27)

Panel 8. ORG and BLS Industry Risk

	Quintile 5 vs. 1	Quintile 4 vs. 1	Quintile 3 vs. 1	Quintile 2 vs. 1
Mean real wage of matched treatment group	8.35	8.83	10.13	11.57
Mean real wage of matched comparison group	8.39	9.18	9.75	10.92
Treatment Effect	**-0.03**	**-0.34**	**0.38**	**0.64**
	(-0.19)	(-2.13)	(2.24)	(3.66)

*Nearest-neighbor matching with a caliper 0.01

Table 5A cont. Propensity Score Matching Estimators: Nearest-Neighbor Matching* for Males 1993 CPS Outgoing Rotation Data, 1993 March CPS Data, 1995 NIOSH Risk Data, 1994 BLS Risk Data, 1993 NLSY Data

Panel 9. March CPS and BLS Occupation Risk

	Quintile 5 vs. 1	Quintile 4 vs. 1	Quintile 3 vs. 1	Quintile 2 vs. 1
Mean real wage of matched treatment group	9.76	11.47	12.89	11.35
Mean real wage of matched comparison group	9.71	11.30	11.89	11.73
Treatment Effect	**0.05**	**0.17**	**1.00**	**-0.38**
	(0.90)	(0.26)	(0.85)	(-0.46)

Panel 10. ORG and BLS Occupation Risk

	Quintile 5 vs. 1	Quintile 4 vs. 1	Quintile 3 vs. 1	Quintile 2 vs. 1
Mean real wage of matched treatment group	8.84	10.77	10.93	10.84
Mean real wage of matched comparison group	9.06	10.29	10.41	10.74
Treatment Effect	**-0.22**	**0.48**	**0.53**	**0.10**
	(-1.65)	(3.25)	(3.37)	(0.55)

*Nearest-neighbor matching with a caliper 0.01
Source: Authors' calculations.

**Table 5B. Propensity Score Matching Estimators: Nearest-Neighbor Matching* for Females
1993 CPS Outgoing Rotation Data, 1993 March CPS Data, 1995 NIOSH Risk Data, 1994 BLS Risk Data, 1993 NLSY Data**

Panel 1. March CPS and NIOSH Industry Risk				
	Quintile 5 vs. 1	**Quintile 4 vs. 1**	**Quintile 3 vs. 1**	**Quintile 2 vs. 1**
Mean real wage of matched treatment group	8.35	7.57	7.30	8.07
Mean real wage of matched comparison group	7.82	8.08	7.83	8.31
Treatment Effect	**0.53**	**-0.50**	**-0.52**	**-0.23**
	(2.17)	(-1.75)	(-2.16)	(-0.74)
Panel 2. ORG and NIOSH Industry Risk				
	Quintile 5 vs. 1	**Quintile 4 vs. 1**	**Quintile 3 vs. 1**	**Quintile 2 vs. 1**
Mean real wage of matched treatment group	7.87	6.74	6.96	7.46
Mean real wage of matched comparison group	7.54	7.44	7.44	8.01
Treatment Effect	**0.33**	**-0.69**	**-0.47**	**-0.54**
	(1.54)	(-5.08)	(-3.28)	(-4.33)
Panel 3. NLSY and NIOSH Industry Risk				
	Quintile 5 vs. 1	**Quintile 4 vs. 1**	**Quintile 3 vs. 1**	**Quintile 2 vs. 1**
Mean real wage of matched treatment group	8.35	6.97	7.50	8.71
Mean real wage of matched comparison group	5.22	9.74	8.97	8.04
Treatment Effect	**3.13**	**-2.77**	**-1.47**	**0.67**
	(3.49)	(-1.67)	(-1.46)	(0.64)
Panel 4. March CPS and NIOSH Occupation Risk				
	Quintile 5 vs. 1	**Quintile 4 vs. 1**	**Quintile 3 vs. 1**	**Quintile 2 vs. 1**
Mean real wage of matched treatment group	6.10	7.42	8.75	9.74
Mean real wage of matched comparison group	6.69	7.07	7.62	8.77
Treatment Effect	**-0.58**	**0.34**	**0.72**	**0.96**
	(-2.72)	(1.55)	(2.14)	(2.09)

*Nearest-neighbor matching with a caliper 0.01

Table 5B cont. Propensity Score Matching Estimators: Nearest-Neighbor Matching* for Females
1993 CPS Outgoing Rotation Data, 1993 March CPS Data, 1995 NIOSH Risk Data, 1994 BLS Risk Data, 1993 NLSY Data

Panel 5. ORG and NIOSH Occupation Risk				
	Quintile 5 vs. 1	**Quintile 4 vs. 1**	**Quintile 3 vs. 1**	**Quintile 2 vs. 1**
Mean real wage of matched treatment group	5.99	6.80	8.42	8.88
Mean real wage of matched comparison group	6.87	7.17	8.15	7.79
Treatment Effect	**-0.88**	**-0.37**	**0.27**	**1.08**
	(-1.58)	(-0.74)	(0.31)	(2.07)
Panel 6. NLSY and NIOSH Occupation Risk				
	Quintile 5 vs. 1	**Quintile 4 vs. 1**	**Quintile 3 vs. 1**	**Quintile 2 vs. 1**
Mean real wage of matched treatment group	5.99	6.80	8.42	8.88
Mean real wage of matched comparison group	6.87	7.17	8.15	7.79
Treatment Effect	**-0.88**	**-0.37**	**0.27**	**1.08**
	(-1.58)	(-0.74)	(0.31)	(2.07)
Panel 7. March CPS and BLS Industry Risk				
	Quintile 5 vs. 1	**Quintile 4 vs. 1**	**Quintile 3 vs. 1**	**Quintile 2 vs. 1**
Mean real wage of matched treatment group	7.88	7.46	8.00	8.04
Mean real wage of matched comparison group	7.10	6.82	7.61	8.06
Treatment Effect	**0.78**	**0.64**	**0.39**	**-0.02**
	(-4.62)	(0.87)	(1.04)	(-0.08)
Panel 8. ORG and BLS Industry Risk				
	Quintile 5 vs. 1	**Quintile 4 vs. 1**	**Quintile 3 vs. 1**	**Quintile 2 vs. 1**
Mean real wage of matched treatment group	7.19	6.23	7.31	7.91
Mean real wage of matched comparison group	7.34	7.28	7.71	7.86
Treatment Effect	**-0.14**	**-1.04**	**-0.39**	**0.04**
	(-0.92)	(-5.53)	(-4.13)	(0.31)

*Nearest-neighbor matching with a caliper 0.01

Table 5B cont. Propensity Score Matching Estimators: Nearest-Neighbor Matching* for Females 1993 CPS Outgoing Rotation Data, 1993 March CPS Data, 1995 NIOSH Risk Data, 1994 BLS Risk Data, 1993 NLSY Data

Panel 9. March CPS and BLS Occupation Risk

	Quintile 5 vs. 1	Quintile 4 vs. 1	Quintile 3 vs. 1	Quintile 2 vs. 1
Mean real wage of matched treatment group	8.07	8.07	7.34	8.41
Mean real wage of matched comparison group	7.98	7.12	7.43	8.06
Treatment Effect	**0.09**	**0.94**	**-0.09**	**0.34**
	(0.29)	(3.68)	(-0.24)	(1.17)

Panel 10. ORG and BLS Occupation Risk

	Quintile 5 vs. 1	Quintile 4 vs. 1	Quintile 3 vs. 1	Quintile 2 vs. 1
Mean real wage of matched treatment group	7.27	7.46	7.25	8.01
Mean real wage of matched comparison group	7.08	6.73	6.87	7.25
Treatment Effect	**0.19**	**0.72**	**0.37**	**0.75**
	(2.18)	(6.84)	(3.40)	(-3.58)

Note: *Nearest-neighbor matching with a caliper 0.01. The NIOSH and BLS industry and occupation risk data are divided into 5 quintiles. The logit model was used to calculate the probability in each risk quintile versus risk quintile 1. For NLSY data the independent variables include a quartic in the worker's age, education level, experience, tenure, test scores, union, and dummy variables indicating firm size, marital status, industry and occupation status, and race/ethnicity categories. For CPS Outgoing Rotation data, the independent variables include age, age quartic, educational level, firm size, race/ethnicity, marital status, and state. The probabilities are used as propensity scores to get the nearest-neighbor estimations. Here, 0.01 is the nearest-neighbor matching standard.
Source: Authors' calculations.

Table 6. Correlations and OLS Residual Correlations for Male Workers
1995 CPS Outgoing Rotations Data, CPS March Data, BLS and NIOSH Risk Data

A. March CPS Data				
Basic Controls	no	yes	yes	yes
State	no	no	yes	yes
Industry/Occupation	no	no	no	yes
Correlations				
NIOSH Ind / NIOSH Occ	0.50	0.48	0.39	0.41
NIOSH Ind / BLS Ind	0.47	0.40	0.41	0.02
NIOSH Ind / BLS Occ	0.29	0.24	0.25	0.02
NIOSH Occ / BLS Ind	0.36	0.31	0.34	0.04
NIOSH Occ / BLS Occ	0.38	0.34	0.35	0.04
BLS Ind / BLS Occ	0.44	0.39	0.38	0.22
B. CPS Outgoing Rotation Data				
Basic Controls	no	yes	yes	yes
State	no	no	yes	yes
Industry/Occupation	no	no	no	yes
Correlations				
NIOSH Ind / NIOSH Occ	0.53	0.43	0.32	0.28
NIOSH Ind / BLS Ind	0.48	0.45	0.45	0.06
NIOSH Ind / BLS Occ	0.30	0.27	0.26	0.05
NIOSH Occ / BLS Ind	0.37	0.33	0.31	0.07
NIOSH Occ / BLS Occ	0.40	0.38	0.38	0.09
BLS Ind / BLS Occ	0.43	0.40	0.39	0.22

Note: The residual correlations are based on the OLS regression of the risk variable on a set of independent variables. The basic controls are dummy variables for age, age quartic, education, race, ethnicity, union coverage, and marital status. After estimating the residuals for each regression, we estimated the residual correlations for each set of regressions. The number of observations for the 1995 CPS Outgoing Rotations data and 1995 March CPS data are 51,140 and 25,237, respectively.
Source: Authors' calculations.

Table 7. Covariances and Variances of Residual Estimates for Male Workers
1995 CPS Outgoing Rotation Data, NIOSH Risk Data, and BLS Risk Data

Panel 1. NIOSH Industry / NIOSH Occupation

	Basic Controls	State	Ind/Occ
VAR (Lnwage)			
VAR (NIOSH Ind)	0.24	0.23	0.21
VAR (NIOSH Occ)	39.92	33.91	14.80
COV (NIOSH Ind, NIOSH Occ)	45.71	38.57	17.09
COV (Lnwage, NIOSH Ind)	18.43	11.56	4.51
COV (Lnwage, NIOSH Occ)	0.06	0.15	0.01
R2 Lnwage on X	-0.23	-0.13	0.02
R2 NIOSH Ind on X	0.27	0.29	0.37
R2 NIOSH Occ on X	0.03	0.21	0.64
	0.09	0.24	0.66

Panel 2. NIOSH Industry / BLS Industry

	Basic Controls	State	Ind/Occ
VAR (Lnwage)	0.25	0.24	0.21
VAR (NIOSH Ind)	47.67	39.58	18.20
VAR (BLS Ind)	59.36	58.09	35.65
COV (NIOSH Ind, BLS Ind)	24.45	21.72	1.53
COV (Lnwage, NIOSH Ind)	0.09	0.18	0.02
COV (Lnwage, BLS Ind)			
R2 Lnwage on X	0.26	0.28	0.36
R2 NIOSH Ind on X	0.04	0.21	0.63
R2 BLS Ind on X	0.06	0.08	0.43

Panel 3. NIOSH Industry / BLS Occupation

	Basic Controls	State	Ind/Occ
VAR (Lnwage)	0.24	0.24	0.21
VAR (NIOSH Ind)	39.59	32.60	13.75
VAR (BLS Occ)	154.35	152.86	115.10
COV (NIOSH Ind, BLS Occ)	21.58	18.90	2.02
COV (Lnwage, NIOSH Ind)	0.04	0.13	0.01
COV (Lnwage, BLS Occ)	-0.23	-0.18	0.06
R2 Lnwage on X	0.30	0.32	0.41
R2 NIOSH Ind on X	0.03	0.20	0.66
R2 BLS Occ on X	0.04	0.05	0.29

Table 7 cont. Covariances and Variances of Residual Estimates for Male Workers
1995 CPS Outgoing Rotation Data, NIOSH Risk Data, and BLS Risk Data

Panel 4. NIOSH Occupation / BLS Industry

	Basic Controls	State	Ind/Occ
VAR (Lnwage)	0.25	0.24	0.21
VAR (NIOSH Occ)	50.14	42.08	19.02
VAR (BLS Ind)	59.06	57.83	35.39
COV (NIOSH Occ, BLS Ind)	18.31	15.62	1.99
COV (Lnwage, NIOSH Occ)	-0.23	-0.14	0.02
COV (Lnwage, BLS Ind)	-0.07	-0.03	-0.06
R2 Lnwage on X	0.26	0.28	0.36
R2 NIOSH Occ on X	0.09	0.24	0.65
R2 BLS Ind on X	0.06	0.08	0.43

Panel 5. NIOSH Occupation / BLS Occupation

	Basic Controls	State	Ind/Occ
VAR (Lnwage)	0.25	0.24	0.21
VAR (NIOSH Occ)	59.95	51.18	23.63
VAR (BLS Occ)	175.53	173.78	131.13
COV (NIOSH Occ, BLS Occ)	39.72	36.50	5.11
COV (Lnwage, NIOSH Occ)	-0.23	-0.14	0.02
COV (Lnwage, BLS Occ)	-0.21	0.16	0.05
R2 Lnwage on X	0.26	0.28	0.36
R2 NIOSH Occ on X	0.09	0.24	0.65
R2 BLS Occ on X	0.05	0.06	0.29

Panel 6. BLS Industry / BLS Occupation

	Basic Controls	State	Ind/Occ
VAR (Lnwage)	0.25	0.24	0.21
VAR (BLS Ind)	67.78	66.45	42.19
VAR (BLS Occ)	175.02	173.33	130.92
COV (BLS Ind, BLS Occ)	43.91	42.56	16.43
COV (Lnwage, BLS Ind)	-0.08	-0.04	-0.06
COV (Lnwage, BLS Occ)	-0.21	-0.16	0.05
R2 Lnwage on X	0.26	0.28	0.36
R2 BLS Ind on X	0.06	0.08	0.43
R2 BLS Occ on X	0.05	0.05	0.28

Source: Authors' calculations.

Table 8. Comparison of Job Risk Estimates from Matched Data to NIOSH Rates
1995 CPS Outgoing Data, 1995 NIOSH Data, and 1995 BLS Data

Demographics	NIOSH	Matched Data			
		NIOSH Industry	BLS Industry	NIOSH Occupation	BLS Occupation
Sex					
Male	7.30	4.95	6.05	5.29	7.53
Female	0.70	2.86	2.44	2.35	2.06
Race					
White	4.20	3.96	4.34	3.84	5.16
Black	4.60	4.00	4.13	4.33	5.08
Other	3.90	3.67	4.33	3.80	4.71
Age Group					
25-34	3.70	3.95	4.45	3.99	5.49
35.44	3.90	4.06	4.44	3.94	5.17
45.54	4.50	3.86	4.01	3.68	4.67
55.64	6.10	3.76	4.08	3.89	4.83
Industry Division					
Ag/For/Fishing	15.50	15.78	22.77	12.90	19.39
Mining	25.40	23.94	25.56	10.00	18.23
Construction	13.10	12.72	12.10	7.96	14.43
Manufacturing	3.60	3.63	4.10	4.39	4.49
Trans/Comm/PU	10.30	10.32	10.59	6.48	11.71
Wholesale Trade	3.50	3.62	5.84	5.11	6.00
Retail Trade	2.40	2.87	3.37	3.86	4.08
Finance/Insurance	1.20	1.08	1.37	2.17	2.49
Services	1.50	1.52	1.61	2.49	2.17
Public Admin	5.80	4.51	—	2.39	6.23
Occupation Division					
Exec/Admn/Mgr	2.50	3.55	3.55	2.40	2.52
Prof/Spec	1.40	2.48	1.66	1.40	1.24
Tech/Support	3.60	3.07	2.29	3.50	1.89
Sales	2.70	2.94	3.71	2.97	3.96
Clerical	0.70	3.73	3.25	0.63	0.81
Services	2.50	2.50	2.69	2.57	4.10
Farm/For/Fish	16.60	10.84	18.76	17.01	21.70
Crafts	8.00	6.85	7.23	7.89	9.92
Mach Operators	3.50	3.79	3.99	3.23	3.04
Transport	17.80	7.73	12.39	18.11	19.85
Laborers	10.40	6.05	7.63	10.51	11.93

Source: Authors' calculations.

Table 9. Comparison of Job Risk Estimates from Matched Data to NIOSH Rates
1985-1995 CPS Outgoing Rotation Data and NIOSH Risk Data

	NIOSH	Matched Data	
year		NIOSH Industry	NIOSH Occupation
1985	5.80	5.34	5.23
1986	5.10	4.61	4.51
1987	5.20	4.62	4.50
1988	5.00	4.57	4.51
1989	4.80	4.53	4.48
1990	4.60	4.52	4.45
1991	4.50	4.03	3.99
1992	4.30	3.99	3.96
1993	4.40	4.01	3.95
1994	4.40	3.97	3.93
1995	4.30	3.94	3.89

Source: Authors’ calculations.

Table 10A. Estimated Price of Risk for Male Workers
1995 CPS Outgoing Rotation Data, 1995 NIOSH Risk Data, and 1995 BLS Risk Data

A. Risk Measure I			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-315	-145	40.4
	(-8.53)	(-2.92)	(0.58)
B. Risk Measure II			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-376	-214	39.0
	(-9.86)	(-4.20)	(0.54)

Note: The dependent variable is the natural log of the worker's real wage. For basic regression, the independent variables include a quartic in the worker's age, a vector of dummy variables that control for the worker's education, a vector of dummy variables indicating whether the worker is Hispanic, Asian, African American, or other race, a dummy variable indicating whether the workers are under union contract or not, and marital status. There are 52,143 observations in the men's regressions. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses. The risk measure I is equal to the simple mean of valid risk measures of NIOSH industry risk, NIOSH occupational risk, BLS industry risk, and BLS Occupational risk. The risk measure II is the simple mean of the same risk categories, but adding zeros to the missing values.
Source: Authors' calculations.

Table 10B. Estimated Price of Risk for Female Workers
1995 CPS Outgoing Rotation Data, 1995 NIOSH Risk Data, and 1995 BLS Risk Data

A. Risk Measure I			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-120	93.40	-60.70
	(-1.97)	(1.30)	(-0.74)
B. Risk Measure II			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-345	82.0	-38.60
	(-4.06)	(0.75)	(-0.25)

Note: The dependent variable is the natural log of the worker's real wage. For basic regression, the independent variables include a quartic in the worker's age, a vector of dummy variables that control for the worker's education, a vector of dummy variables indicating whether the worker is Hispanic, Asian, African American, or other race, a dummy variable indicating whether the workers are under union contract or not, and marital status. There are 53,819 observations in the women's regression. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses. The risk measure I is equal to the simple mean of valid risk measures of NIOSH industry risk, NIOSH occupational risk, BLS industry risk and BLS occupational risk. The risk measure II is the simple mean of the same risk categories, but adding zeros to the missing values.
Source: Authors' calculations.

Table 10C. Estimated Price of Risk for Male Workers
1995 March CPS Data, 1995 NIOSH Risk Data, and 1995 BLS Risk Data

A. Risk Measure I			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-120	73.0	191
	(-2.40)	(1.41	(2.73)
B. Risk Measure II			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-174	24.70	197
	(-3.32)	(0.46)	(2.66)

Note: The dependent variable is the natural log of the worker's real wage. For basic regression, the independent variables include a quartic in the worker's age, a vector of dummy variables that control for the worker's education, a vector of dummy variables indicating whether the worker is Hispanic, Asian, African American, or other race, a dummy variable indicating whether the workers are under union contract or not, and marital status. There are 25,621 observations in the men's regression. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses. The risk measure I is equal to the simple mean of valid risk measures of NIOSH industry risk, NIOSH occupational risk, BLS industry risk, and BLS occupational risk. The risk measure II is the simple mean of the same risk categories, but adding zeros to the missing values.
Source: Authors' calculations.

Table 10D. Estimated Price of Risk for Female Workers
1995 March CPS Data, 1995 NIOSH Risk Data, and 1995 BLS Risk Data

A. Risk Measure I			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	31.60	124	-56.10
	(0.35)	(1.36)	(-0.51)
B. Risk Measure II			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-129	91.80	37.30
	(-1.07)	(0.75)	(0.21)

Note: The dependent variable is the natural log of the worker's real wage. For basic regression, the independent variables include a quartic in the worker's age, a vector of dummy variables that control for the worker's education, a vector of dummy variables indicating whether the worker is Hispanic, Asian, African American, or other race, and a dummy variable indicating whether the workers are under union contract or not. There are 24,758 observations in the women's regression. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses. The risk measure I is equal to the simple mean of valid risk measures of NIOSH industry risk, NIOSH occupational risk, BLS industry risk, and BLS occupational risk. The risk measure II is the simple mean of the same risk categories, but adding zeros to the missing values.
Source: Authors' calculations.

Table 11A. “Post Hoc” Risk Indicator in a Multiple Regression for Male Workers
1995 CPS Outgoing Rotation Data, 1995 NIOSH Risk Data, and 1995 BLS Risk Data

Basic Regression	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
NIOSH Industry Risk/100,000	845	1118	172
	(18.25)	(23.13)	(2.63)
NIOSH Occupational Risk/100,000	-723	-527	75.20
	(-18.37)	(-13.11)	(1.37)
BLS Industry Risk/100,000	-231	-301	-297
	(-6.25)	(-8.20)	(-7.92)
BLS Occupational Risk/100,000	59.60	29.90	62.40
	(3.03)	(1.54)	(3.27)
Estimated “Risk” Effect/100,000	-7652	-5991	-237

Note: The dependent variable is the natural log of the worker’s real wage. For basic regression, the independent variables include a quartic in the worker’s age, a vector of dummy variables that control for the worker’s education, a vector of dummy variables indicating whether the worker is Hispanic, Asian, African American, or other race, a dummy variable indicating whether the workers are under union contract or not, and marital status. There are 215,365 observations in the men’s regression. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses. The “post hoc” risk indicator is extracted from the coefficients of the industry and occupational risk used simultaneously in the multiple regression. The occupational risk coefficient is weighted by the ratio of the bivariate covariance between the natural log of real wage and the occupational risk, to the covariance between log of real wage and the industry risk. The units of the “post hoc” estimator are given in terms of industry risk.
Source: Authors’ calculations.

Table 11B. “Post Hoc” Risk Indicator in a Multiple Regression for Female Workers 1995 CPS Outgoing Rotation Data, 1995 NIOSH Risk Data, and 1995 BLS Risk Data

Basic Regression	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
NIOSH Industry Risk/100,000	489	600	7.28
	(7.34)	(9.00)	(0.10)
NIOSH Occupational Risk/100,000	-548	-368	53.70
	(-8.45)	(-5.69)	(0.70)
BLS Industry Risk/100,000	-526	-538	-498
	(-7.09)	(-7.38)	(-6.05)
BLS Occupational Risk/100,000	102	72.70	244
	(1.34)	(0.97)	(3.20)
Estimated “Risk” Effect/100,000	-2287	-1809	-575

Note: The dependent variable is the natural log of the worker’s real wage. For basic regression, the independent variables include a quartic in the worker’s age, a vector of dummy variables that control for the worker’s education, a vector of dummy variables indicating whether the worker is Hispanic, Asian, African American, or other race, a dummy variable indicating whether the workers are under union contract or not, and marital status. There are 182,453 observations in the women’s regression. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses. The “post hoc” risk indicator is extracted from the coefficients of the industry and occupational risk used simultaneously in the multiple regression. The occupational risk coefficient is weighted by the ratio of the bivariate covariance between the natural log of real wage and the occupational risk, to the covariance between log of real wage and the industry risk. The units of the “post hoc” estimator are given in terms of industry risk.
Source: Authors’ calculations.

Table 11C. "Post Hoc" Risk Indicator in a Multiple Regression for Male Workers*
1995 CPS Outgoing Rotation Data, 1995 NIOSH Risk Data, and 1995 BLS Risk Data

Basic Regression	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
NIOSH Industry Risk/100,000	630	863	154
	(18.84)	(24.85)	(3.59)
NIOSH Occupational Risk/100,000	-561	-350	105
	(-17.62)	(-10.66)	(2.41)
BLS Industry Risk/100,000	-270	-322	-329
	(-8.77)	(-10.55)	(-10.06)
BLS Occupational Risk/100,000	42.90	12.70	118
	(2.45)	(0.74)	(6.92)
Estimated "Risk" Effect/100,000	-5192	-3810	-115

Note: *Adding zero value for missing risk categories. The dependent variable is the natural log of the worker's real wage. For basic regression, the independent variables include a quartic in the worker's age, a vector of dummy variables that control for the worker's education, a vector of dummy variables indicating whether the worker is Hispanic, Asian, African American, or other race, a dummy variable indicating whether the worker is under union contract or not, and marital status. There are 353,950 observations in the men's regression. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses. The "post hoc" risk indicator is extracted from the coefficients of the industry and occupational risk used simultaneously in the multiple regression. The occupational risk coefficient is weighted by the ratio of the bivariate covariance between the natural log of real wage and the occupational risk, to the covariance between log of real wage and the industry risk. The units of the "post hoc" estimator are given in terms of industry risk.
Source: Authors; calculations.

Table 11D. "Post Hoc" Risk Indicator in a Multiple Regression for Female Workers*
1995 CPS Outgoing Rotation Data, 1995 NIOSH Risk Data, and 1995 BLS Risk Data

Basic Regression	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
NIOSH Industry Risk/100,000	425	540	1.23
	(10.66)	(13.41)	(0.03)
NIOSH Occupational Risk/100,000	-506	-247	37.40
	(-10.11)	(-5.45)	(0.63)
BLS Industry Risk/100,000	-423	-428	-359
	(-7.62)	(-7.48)	(-5.95)
BLS Occupational Risk/100,000	259	214	534
	(4.08)	(3.43)	(8.44)
Estimated "Risk" Effect/100,000	-1287	-907	-134

Note: *Adding zero value for missing risk categories. The dependent variable is the natural log of the worker's real wage. For basic regression, the independent variables include a quartic in the worker's age, a vector of dummy variables that control for the worker's education, a vector of dummy variables indicating whether the worker is Hispanic, Asian, African American, or other race, a dummy variable indicating whether the worker is under union contract or not, marital status. There are 337,530 observations in the women's regression. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses. The "post hoc" risk indicator is extracted from the coefficients of the industry and occupational risk used simultaneously in the multiple regression. The occupational risk coefficient is weighted by the ratio of the bivariate covariance between the natural log of real wage and the occupational risk, to the covariance between log of real wage and the industry risk. The units of the "post hoc" estimator are given in terms of industry risk.
Source: Authors' calculations.

Table 12. IV Estimation for Estimated Price of Risk

Panel 1: March CPS and NIOSH Risk: Males, 1985-1995				
	OLS Estimation	**OLS Estimation**	**IV Estimation**	**IV Estimation**
Basic Controls	yes	yes	yes	yes
State	yes	yes	yes	yes
Industry/Occupation	yes	yes	yes	yes
	Industry	Occupation	Industry	Occupation
Risk/100000	107	20.34	101	285
	(4.72)	(1.72)	(1.22)	(4.71)
Instrument	no	no	yes Occupation Risk	yes Industry Risk
Panel 2: ORG and NIOSH Risk: Males, 1985-1995				
	OLS Estimation	**OLS Estimation**	**IV Estimation**	**IV Estimation**
Basic Controls	yes	yes	yes	yes
State	yes	yes	yes	yes
Industry/Occupation	yes	yes	yes	yes
	Industry	Occupation	Industry	Occupation
Risk/100000	84.0	105	293	201
	(7.75)	(10.58)	(10.50)	(13.80)
Instrument	no	no	yes Occupation Risk	yes Industry Risk

Note: For all regressions, the dependent variable is the natural log of the worker's real wage. For the basic regression, the independent variables include a quartic in the worker's age, a vector of dummy variables that control for the worker's education, a vector of dummy variables indicating whether the worker is Hispanic, Asian, African American, or other race, a dummy variable indicating whether the worker is under a union contract or not, and marital status. Workers are aged 25 to 60 inclusive. T-statistics are given in parentheses. The Instrumental Variable (IV) estimation was performed using for the variable industry risk the instrument occupation risk, and for the variable occupation risk the instrument industry risk.
Source: Authors' calculations.

Table 13. Correlation of NIOSH Job Risk Measures and Air Force Qualification Test (AFQT) Score, Illegal Drug Use, and Education, NLSY Data

Panel A. NIOSH Industry / NIOSH Occupation and Other variables	Education	AFQT Score (Age demeaned)	Illegal drug use
Industry job risk	-0.15	-0.10	0.09
Occupation job risk	-0.26	-0.21	0.05
Panel B. Residual Correlations			
Industry job risk	0.02	-0.01	0.06
Occupation job risk	0.01	-0.03	-0.00

Notes: Illegal drug use data are from 1984 and include heroine, cocaine, and marijuana/hashish. In 1984, respondents were aged 19 to 26 inclusive. AFQT-job risk and education-job risk correlations are from 1990 when respondents were aged 25 to 32 inclusive.

Source: Authors' calculation.

Table 14A. Fix-Effect Estimated Price for Male Workers
1986 to 1993 NLS Data and NIOSH Risk Data

A. Industrial Risk			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-47.00	-19.40	-110
	(-0.41)	(-0.17)	(-0.65)
Mean Risk	6.18	6.18	6.18
B. Occupation Risk			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	-78.90	-74.70	5.46
	(-1.51)	(-1.42)	(-0.08)
Mean Risk	7.66	7.66	7.66

Table 14B. Fix-Effect Estimated Price for Female Workers
1986 to 1993 NLS Data and NIOSH Risk Data

A. Industrial Risk			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	266	288	83.20
	(1.75)	(1.87)	(0.39)
Mean Risk	3.61	3.61	3.61
B. Occupation Risk			
Basic Controls	yes	yes	yes
State	no	yes	yes
Industry/Occupation	no	no	yes
Risk/100,000	123	116	74.70
	(1.11)	(1.05)	(0.39)
Mean Risk	3.64	3.64	3.64

Note: For the basic regression, the independent variables include a quartic in the worker's age, education level, union, experience, tenure status, test scores, race/ethnicity category, and marital status. Workers are aged 25 to 60. T-statistics are given in parentheses. There are 20,338 observations in the male regressions and 19,272 observations in the female regressions. There is a fixed-effect for individual in each specification.
Source: Authors' calculations.

APPENDIX D

Robustness of VSL Estimates from Contingent Valuation Studies

By Anna Alberini

Robustness of VSL Estimates from Contingent Valuation Studies

by

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WILLINGNESS TO PAY FOR MORTALITY RISK REDUCTIONS: THE ROBUSTNESS OF VSL FIGURES FROM CONTINGENT VALUATION STUDIES

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1. INTRODUCTION

Reductions in risk of death are arguably the most important benefit underlying many health, safety, and environmental legislative mandates. For example, in two recent analyses of the benefits of U.S. air quality legislation, *The Benefits and Cost of the Clean Air Act, 1970-1990* (US EPA, 1997) and *The Benefits and Cost of the Clean Air Act, 1990-2010* (US EPA, 1999), over 80 percent of monetized benefits were attributed to reductions in premature mortality.

In quantifying the benefits of policies that save lives, Viscusi (1993) recommends a range of Values of a Statistical Life (VSLs) from \$3 to 7 million (1990 dollars) based on a review of labor market and other studies. The US Environmental Protection Agency (USEPA) uses a VSL of \$6.1 million (1999 \$) in its base analyses.[1] This value was derived by the Agency using values from 26 studies of mortality risk valuation. The majority of these studies are compensating wage studies that use observed workplace risk-income tradeoffs to infer the VSL. Only five of the twenty-six estimates are from stated-preference studies that elicit directly willingness to pay for a specified risk reduction (Gerking et al., 1988; Jones-Lee et al., 1985, Miller and Guria, 1991, Gegax et al. 1985) or risk-risk and risk-dollar tradeoffs using a variant of conjoint questions (Viscusi et al., 1991a).

These contingent valuation (CV) studies, however, are broader in scope than hedonic wage studies, in that the risks they value are not limited to workplace risks. Moreover, in principle the method of contingent valuation offers greater flexibility than other approaches to measuring money-risk tradeoffs, suggesting that it is important to examine the VSL figures produced by CV surveys.

Contingent valuation is a valuation technique that directly asks individuals to report information on their willingness to pay for an improvement in environmental quality, health or safety, or in the provision of a public good. This technique can and has been applied to both public and private goods. A change in the risk of death experienced by an individual, for example, is a public good if the risk reduction is delivered by a public

[1] Recently, the Agency has employed alternate estimates in several of its analyses: \$3.7 million based solely on the five stated preference estimates as well as a range of \$1-\$10 million based on meta-analytic results focused on hedonic wage studies.

program, such as an environmental or transportation safety program, but a private good if the risk reduction is delivered by an action or product (e.g., carbon monoxide detector) privately purchased and used by an individual.

In conjoint choice surveys, respondents are asked to state which they prefer between two commodities (or policy packages) described by a set of attributes. One of the attributes is usually the price of the good, or the cost of providing a government program. Because they are based on what individuals say they would do under specified, but hypothetical, circumstances, both contingent valuation and conjoint choice are examples of stated-preference methods for obtaining WTP for a commodity.

Contingent valuation has several advantages over other methods for measuring the value that people place over reductions in mortality risks. For example, in CV surveys respondents are generally told explicitly what the baseline risks and the risk reductions are. This is in sharp contrast with most compensating wage and other consumer studies, where it is assumed that individuals' perceived risks are equal to their objective risks. Moreover, the survey sample can be created to include persons of all ages, environmental exposures, and health status, whereas in labor market studies the population being studied is typically working males in their prime.

In contingent valuation surveys, changes in small probabilities have proven to be a very difficult commodity to value. Probabilities and risks must be explained to the respondents in the first place. Respondents may find it difficult to grasp that many risks can be avoided or reduced, but at a cost. Moreover, the risk changes to be valued are usually very small, and may be dismissed as meaningless by the respondents. It is, therefore, not surprising that many some CV surveys about reductions in mortality risks result in numerous zero WTP responses, and that the WTP amount announced by respondents sometimes fail to increase with the size of the risk reduction as predicted by economic theory (Hammitt and Graham, 1999).

Statistical modeling of the WTP responses is further complicated by the fact that the underlying distribution of WTP has long and hard-to-nail-down tails, and that respondents with positive WTP must be distinguished from those respondents who hold no value at all for the risk reduction. This raises concerns about the robustness of these studies' estimates of mean and median WTP, and of the estimated relationships between WTP and individual characteristics such as income, age, education, and health status of the respondent. These relationships are used to test the internal validity of the WTP responses, and can potentially be used for benefit transfer purposes.

The purpose of this research is three-fold. The original goal of the research was to obtain the original survey data on which the five stated-preference estimates of VSL are based and re-analyze them to check the data quality and examine the robustness of the econometric estimates of VSL with respect to a variety of criteria (described below). The purpose of these analyses was to find out if alternative analyses and statistical models of the WTP data would have resulted in largely different estimates of WTP/VSL.

Second, we searched the recent literature on mortality risk valuation using stated-preference studies, examining carefully the survey materials and questionnaires, the risk reduction scenarios presented to the respondents, the wording and the nature of the payment questions, and the sample of respondents, seeking to draw lessons that could be used in interpreting results and estimates of VSL and in guiding future stated-preference studies about value of mortality risk reductions. Summaries of these studies are offered in Appendix A to this report. The questionnaires and our comments on their structure and quality are offered in Appendices B and C, respectively.

Third, for some of these papers or articles—those where the program delivering the risk reduction, the population surveyed, and the quality of the study itself suggest that results would be interesting and could be applicable to environmental policy—we obtained the original datasets from the authors and econometrically re-analyzed the WTP responses to assess the robustness of the estimates of WTP/VSL. Results are reported throughout this report.

It should be emphasized that the analyses conducted here are not meta-analyses of the VSL figures produced by stated preference studies. The purpose of this research is to examine the studies one by one, and not to uncover the across-study relationship between WTP and characteristics of the study design, the populations being surveyed, and the risk reductions being valued.

Briefly, we find that:

- Estimating mean WTP using the data from one-shot dichotomous choice questions can be problematic. Depending on the distribution assumed for WTP, mean WTP was either negative, or positive but implausibly large.

- When the analysis was conditional on covariates, we found that the relationship between WTP and one important covariate—the age of the respondent—was not robust to the procedure used for computing mean WTP.

- We recommend using dichotomous-choice CV questions with follow-up, even though the latter are not incentive compatible, to refine information about WTP and nail down the tails of the distribution of WTP.

- Median WTP is a robust and conservative welfare estimate.

- Debriefing questions should be included to uncover respondent failure to comprehend various aspects of the risk reduction scenario, yea-saying, nay-saying and completely random responses.

- In one of the studies we examined, we found that those respondents who reported a relatively high WTP for their income were probably persons who misunderstood the timing of the payments. Very high WTP relative to a person's income could also be due to income mismeasurement or failure to give the budget constraint proper consideration.

- When respondents are asked to estimate their own subjective risks and/or risk reductions, it is important to check whether WTP and subjective risks are endogenous. In one of the two examples presented in this report, we found that accounting for endogeneity of risks and WTP improved the sensitivity of WTP to the size of the risk reduction, which is an important internal validity criterion.

- We recommend that researchers express risk reductions in both absolute *and* relative terms. For example, they may say that the risk reduction is "5 in 10000. This represents a 30% reduction in your risk of dying."

- We endorse the practice of showing the respondents one's risk of death for a specific cause (e.g., traffic accidents) in the context of the risk of dying for all causes, and for other specific causes.

- Comparison of the visual aids used in various studies suggests that it is best to keep the visual depiction of risk as simple as possible.

The remainder of this report is organized as follows. Chapter 2 describes possible criteria to assess the econometric robustness of the estimates of WTP and VSL. Chapter 3 describes the studies that were identified for this work and the availability of data and questionnaires. Chapter 4 describes the studies for which we were able to obtain the original datasets.

The second part of the report is more empirical. Chapter 5 examines the importance of the assumptions about the distribution of WTP and of the formulae used to compute mean WTP in the context of (single-bounded) dichotomous-choice data. It also compares alternate welfare statistics, such as mean and median WTP. These issues are also examined in the context of analyses conditional on covariates, such as age.

Chapter 6 discusses outliers, and Chapter 7 possible sources of "contamination" of the responses, such as yea-saying, nay-saying, and completely random responses, presents mixture models. Attempts to estimate mixture models using maximum likelihood methods are presented for situations when the researcher suspects that such response patterns may exist, but does not have information from other survey responses that can be used to identify which respondents engage in such response behaviors. Chapter 8 focuses on alternative interpretations of the WTP responses, including zero responses and continuous versus interval data.

Chapter 9 focuses on the possible endogeneity of (subjective) risk and WTP, and explores how treating these variables as endogenous can affect scope tests and the issue of whether absolute or relative risk reductions drive WTP. Chapter 11 discusses sample selection issues, and chapter 12 discusses the main lessons learned from examining the questionnaires that were made available to us. Conclusions and recommendations are presented in Chapter 13.

Appendix A contains summaries of selected papers. Appendix B contains the questionnaire used in selected papers. Appendix C provides summaries and comments on the questionnaires. Appendix D contains research reports by the authors of the three of the four original studies.

2. Possible Robustness Criteria

A. Data Quality Checks

The results from stated preference surveys are only as good as the data from which they are generated. There are several basic checks that help ensure data quality, including, for example, regressions that test internal validity of the WTP responses. We examine responses from several contingent valuation surveys eliciting WTP for mortality risk reduction to see if they satisfy basic requirements suggested by economic theory.

When the CV survey is conducted using the dichotomous-choice format,[2] for example, the percentage of "yes" responses to the payment question should decline with the bid amount. Figure 2.1 reports the percentage of "yes" responses to the payment question observed in a survey of US residents, where two independent subsamples of respondents were asked to report information about their WTP for risk reductions of different size. The figure shows that the percentage of "yes" responses declines regularly with the bid amount, ranging from 73% at the lowest bid amount ($70) to 35% at the highest bid amount ($725) for a risk reduction of 5 in 1000.

It is also important to check that the bid amounts assigned to the respondents in the survey cover a reasonable portion of the range of possible WTP values. For example, Alberini (1995a, 1995b) shows that when the distribution of WTP is assumed to be symmetric and the statistic of interest is mean/median WTP, placing the bids on one side of the median and/or too far away from the center of the distribution may result in a significant loss of efficiency of the estimates of mean/median WTP. Cooper (1993) emphasizes the importance of covering the entire range of possible WTP values.[3]

In much recent empirical work, WTP is assumed to follow an asymmetric distribution, such as the log normal or the Weibull. Failure to present respondents with bid amounts nicely spread over the possible range of WTP values, however, can seriously impair the researcher's ability to obtain stable estimates of the parameters of the distribution.

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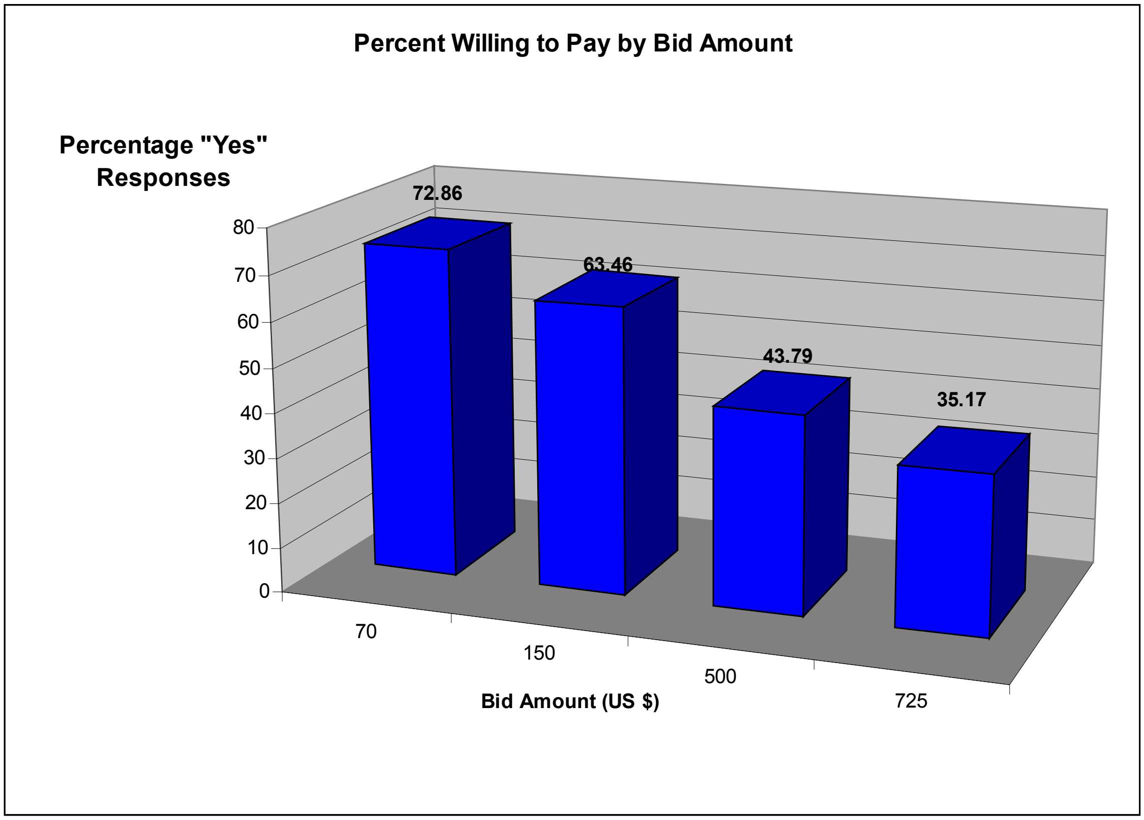
[2] In a dichotomous-choice contingent valuation survey, respondents are asked to state whether or not they would purchase the good to be valued, or vote in favor or against a proposed government program, if the cost to their household was $X. If the respondent is in favor of the program, or says he would buy the good, then his WTP exceeds $X. If the respondent declines to buy the good, or votes against the program, then WTP must be less than the dollar amount X. The dollar amount, $X, is generally termed the bid value, and is varied across respondents. Binary response econometric models are then fit to the responses to this payment question, and estimates of mean or median WTP are usually obtained exploiting the properties of the distribution WTP is assumed to follow (see, for example, Cameron and James, 1987).

[3] Care should be taken, however, to avoid bid values that are implausibly small or large. The responses to the WTP questions for such amounts might reflect the loss of credibility of the scenario, rather than the true respondents' preferences.

Figure 2.1 Percent of "yes" responses by bid value: US Study (Alberini et al, forthcoming).



In much recent empirical work, WTP is assumed to follow an asymmetric distribution, such as the log normal or the Weibull. Failure to present respondents with bid amounts nicely spread over the possible range of WTP values, however, can seriously impair the researcher's ability to obtain stable estimates of the parameters of the distribution.

B. Choice of distribution for WTP.

In their report of contingent valuation surveys eliciting non-use values for Prince William Sound in Alaska, Carson et al. (1995) show that the estimates of both mean and median WTP from dichotomous choice CV survey data can be very sensitive to the distributional assumption about WTP. This suggests that alternative distributional assumptions should be explored for the data from existing CV surveys. In particular, we wish to see what happens when we move away from logit or probit models of the responses to

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[4] Care should be taken, however, to avoid bid values that are implausibly small or large. The responses to the WTP questions for such amounts might reflect the loss of credibility of the scenario, rather than the true respondents' preferences.

dichotomous choice payment questions used by many researchers, as these models imply that WTP is allowed to be negative.

We also wish to investigate the effect of using alternate procedures for computing mean WTP, holding the distribution of latent WTP the same. We extend this research question to the situation when the researcher is interested in estimating mean (median) WTP conditional on certain covariates of interest.

To elaborate on this latter point, willingness to pay for a mortality risk reduction is usually regressed on individual characteristics, including income, education, age and gender to test internal validity of the WTP responses. In addition to checking whether the results of the study are credible, these regressions also seek to answer questions related to the use of the VSL figures in policy analyses.

For example, there has been much recent interest in whether WTP for a risk reduction, and hence the VSL, is lower for elderly persons, reflecting their fewer remaining life years. Because economic theory does not offer unambiguous predictions about the relationship between VSL and age, the answer to this question is an empirical issue, and it is important to see if conclusions about the shape of the relationship between age and WTP depend on the procedure used for computing mean/median WTP.

C. Outliers

Collett (1991) defines as outliers “observations that are surprisingly far away from the remaining observations in the sample,” and points out that such values may occur as a result of measurement errors, execution errors (i.e., use of faulty experimental procedure), or be legitimate, if extreme, manifestations of natural variability.

Outliers with Respect to the Dependent Variable. Lanoie et al. (1995) explicitly consider respondents whose WTP amounts are disproportionately large relative to the rest of the sample. They identify three influential observations in their sample of workers of the Montreal area. When these observations are removed from the sample, the VSL estimated from the CV component of their study drops from \$22-27 million to \$15 million (1995 Can. Dollars).

Textbook presentations of the outlier problem sometimes recommend plotting the dependent variable of the regression against a regressor of interest to identify outliers through visual inspection, but it is clear that the responses to dichotomous-choice WTP questions do not easily lend themselves to such a treatment. In dichotomous-choice CV studies, WTP is not directly observed, suggesting that the formal definition of an outlier might be modified to denote an observation such that a “yes” response to the payment question predicted by the model was observed when the predicted probability of “yes” is very low, or a “no” response was observed when the probability of a “no” is very low (Copas, 1988).

Outliers in the Independent Variables. We also wish to examine the robustness of the estimates of WTP and VSL with respect to the presence of individuals who report (i) high values for certain independent variables, (ii) or high WTP amounts relative to the level of certain independent variables.

An example of (i) may occur when individuals are asked to estimate their own subjective risk and risk reductions. It is important to check whether WTP is sensitive to respondents with large self-assessed baseline risks or risk reductions, as these may signal failure to comprehend probabilities.

An example of (ii) is given by respondents whose announced WTP amounts are large relative to their income. Our interest in this question is motivated by the fact that in many CV surveys about environmental quality, researchers expect WTP to be a relatively small fraction of the respondent's income. This expectation has led them, in some cases, to exclude from the usable sample those respondents whose WTP is greater than, say, 5% of income.

When dealing with reductions in mortality risks there is no particular reason to believe that WTP should be a small fraction of income, but researchers sometimes do limit their regression analyses to those persons whose WTP for a mortality risk reduction is a relatively small proportion of household income. For example, in Persson et al. (2001) attention is restricted to those respondents whose WTP for a risk reduction in the coming year is less than 5% of annual household income.

Large WTP amounts relative to income may affect the income elasticity of WTP, which is important for benefit transfer purposes and when one wishes to predict WTP for the population. Moreover, large WTP amounts relative to one's income may signal a problematic WTP response. For example, the respondent may have failed to consider his or her income constraint. To identify outliers and assess their impact on the estimates of mean and median WTP, one might, therefore, consider excluding from the sample respondents whose implied WTP values exceed specified fractions of their income (e.g., 5%, 10% or 25% percent) and examining how the estimates of mean and median WTP, and income elasticity of WTP, change.

Outliers, continues Collett (1991), sometimes arise in the presence of *mixtures* of populations. We discuss mixtures of populations in the next section.

D. Discrete Mixtures.

Analyses of dichotomous-choice CV data rely on the assumption that respondents answer "yes" to a dichotomous choice payment question if their WTP amount is greater than the bid, and "no" when their WTP amount is less than the bid. It seems possible, however, that in some cases the sample might be "contaminated" with responses that do not abide by the economic paradigm.

Examples of such contaminating responses include "yea-saying," "nay-saying," and completely random responses. Yea-saying implies that the respondent answers "yes" with probability 1, regardless of the bid amount. By contrast, nay-saying implies that the respondent answers "no" with probability 1, regardless of the bid amount. When the responses are completely random, the respondent answers "yes" with probability 0.5, and "no" with probability 0.5, regardless of the bid value. This behavior is equivalent to letting the response to the payment question depend on the outcome of a coin flip.

Yea-saying behavior is possible, for example, when the respondent wishes to please the interviewer, or hopes that by answering affirmatively to the payment question the survey will be terminated soon. Nay-saying behavior, on the other hand, might be observed when the scenario is couched in terms of a public program, and the respondent dislikes certain aspects of government programs, even though, privately, he might attach a positive value to the good or environmental quality improvement provided by the program. It is also possible that respondents exhibit nay-saying behaviors when they are opposed to new taxes, and/or when they fear they are committing to something that they do not fully understand.

Finally, completely random responses might be due to complete confusion about the scenario, failure to understand the commodity being valued, no interest in the survey, and/or poorly written questions or survey materials. Completely random responses might also result from a data entry error, in which case, however, the problem arises for reasons other than the respondent's behavior.

Because CV surveys eliciting WTP for mortality risk reduction must present respondents with probabilities, which are difficult for many people to process, and with scenarios that are sometimes difficult to grasp, there would seem to be room for these undesirable response effects in these studies. While it is possible, in some cases, to identify yea-sayers, nay-sayers and completely random responses by making judicious use of debriefing questions and interviewer observations, in other studies that use dichotomous choice payment questions it is not easy or possible to say whether the response to the payment question is legitimate or is due to one of these contaminating behaviors.

From the statistical point of view, when there is no data "separation" the presence of contaminating responses can be addressed by specifying a (discrete) mixture of distributions.[5] In this report, for the sake of simplicity it is assumed that the observed sample responses come from a mixture with two components. Let the first component of the mixture be a well-behaved distribution of WTP with cdf $F(\bullet)$, while the second component of the mixture is yea-saying behavior. Let α be the probability of yea-saying behavior, while $(1-\alpha)$ is the probability of announced responses that are consistent with true WTP amounts. When a "yes" response is observed, then the contribution to the likelihood is

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[5] Known data separation is said to occur when the researcher knows exactly which of the two population the respondent belongs to.

(2.1) $$\Pr(yes_i) = (1-\alpha)\cdot\Pr(WTP_i > B_i) + \alpha\cdot 1 = (1-\alpha)\cdot(1-F(B_i;\theta)) + \alpha$$

where B is the bid amount, while the contribution to the likelihood by an observed "no" response is:

(2.2) $$\Pr(no_i) = (1-\alpha)\cdot\Pr(WTP_i \le B_i) = (1-\alpha)\cdot F(B_i;\theta)\,.$$

Equations (2.1) and (2.2) are, therefore, different from the typical contributions to the likelihood in statistical models of dichotomous choice responses, the difference arising from having to account for the fact that an observed "yes" has a probability (1-α) of being a genuine "yes" and α of being the result of yea-saying behavior.

When yea-saying exists and is not adequately accounted for, the estimated survival curve of WTP (i.e., 1 minus the cdf of WTP, which traces out the percentage of respondents willing to pay any given bid amount) lies above the true survival curve (see Figure 2.2). This will lead to overestimating both mean and median WTP.

Similarly, if the second of the two discrete components of the mixture was "nay-saying," the appropriate contributions to the likelihood would be:

(2.3) $$\Pr(yes_i) = (1-\alpha)\cdot\Pr(WTP_i > B_i) = (1-\alpha)\cdot(1-F(B_i;\theta))\,,$$

and

(2.4) $$\Pr(no_i) = (1-\alpha)\cdot\Pr(WTP_i \le B_i) + \alpha\cdot 1 = (1-\alpha)\cdot F(B_i;\theta) + \alpha\,.$$

The estimated survival function of WTP will, therefore, lie below the true curve, which will result in underestimating mean and median WTP.

Finally, in the presence of completely random responses, the contributions to the likelihood are:

(2.5) $$\Pr(yes_i) = (1-\alpha)\cdot\Pr(WTP_i > B_i) + \alpha\cdot 0.5 = (1-\alpha)\cdot(1-F(B_i;\theta)) + 0.5\alpha$$

(2.6) $$\Pr(no_i) = (1-\alpha)\cdot\Pr(WTP_i \le B_i) + \alpha\cdot 0.5 = (1-\alpha)\cdot F(B_i;\theta) + 0.5\alpha\,.$$

The estimated survival curve will be below the true curve for bid amounts lower than the median, will cross the true curve at the median (since the probability of a "yes" is 0.5 for both legitimate responses and random responses) and will be above it for bid amounts greater than median WTP (see Figure 2.3).

The mixing probability α must be estimated by the method of maximum likelihood. It is also possible to make α a function of covariates, such as gender, age, education and attitudinal variables. As α is a probability, it is useful to specify a logit or probit link for α: $\alpha_i = \Phi(\mathbf{x}_i\gamma)$.

E. Alternative interpretations of the responses to the WTP questions

In the Gerking et al. study, respondents were asked to circle the amount on a payment card that best matched their willingness to pay. The WTP responses were treated as if they were on a continuous scale, although the correct interpretation is that an individual's WTP falls between the amount he or she picked on the payment card and the next highest amount (Cameron and Huppert, 1988). Re-specifying and re-estimating the likelihood function accordingly could result in different estimates of mean WTP, and in different regression coefficients. Presumably, the differences should depend on how broad the intervals around true WTP are, which in turn depends on how far apart the dollar amounts on the payment card are spaced, and on the underlying distribution of WTP (Cameron, 1987).

Another response interpretation issue examined in this report is the fact that, especially when the mortality risk reductions being valued are very small, many people state that they are not willing to pay anything at all to obtain the risk reduction. To our knowledge, the literature has handled this problem in three possible ways. The first is a tobit model, which has been used in some studies employing open-ended questions to elicit WTP (Gerking et al., 1988).

Second, the tobit model has also been adapted to the dichotomous choice context, in which case it has been sometimes referred to as the "spike" model (Kriström, 1997; Krupnick et al., 2002). Finally, in studies employing dichotomous choice questions with follow-ups, researchers have ignored respondents' final announcements that they were not willing to pay anything at all, and have simply assumed that these persons' WTP amounts lie between 0 and the lowest bid amount stated to the respondent in the follow-up payment question (Alberini et al., forthcoming). It is important to find out how these alternative approaches affect the final estimates of mean and median WTP.

F. Endogenous Regressors

Contingent valuation studies eliciting WTP for mortality risk reductions have sometimes asked respondents to evaluate their own baseline mortality risks (Gerking et al., 1988; Persson et al., 2001) and/or the risk reductions attainable if certain measures are taken or policies are passed (Johannesson et al., 1991).[6] WTP is then regressed on baseline risk and/or the risk reduction.

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[6] In the Gerking et al. study, for example, respondents were asked to place their own occupation on a risk ladder, and to subsequently report their WTP (WTA) for reducing (increasing) risk by one notch. In the Persson et al. survey, respondents were first asked to subjectively estimate their own risk of dying in a road-traffic accident, after being told what the risk was for a 50-year-old person. They were then asked to report their WTP for a reduction of 10, 30, 50 or 99 percent in the risk of dying in a road-traffic accident. Finally, Johannesson et al. (1991) contacted patients at a health care center in Sweden,

In studies conducted in this fashion, one expects WTP to increase with the size of the absolute risk reduction, and, ideally, to be strictly proportional to the size of the risk change (Hammitt and Graham, 1999). Before one sets out to test hypotheses about the coefficient of the risk reduction, however, it is important to establish if WTP and self-assessed risks (or risk reductions) are econometrically endogenous with one another. This happens, for example, when these variables share common unobservable individual characteristics.

Coefficient estimates based on OLS or maximum likelihood estimation that treat risk as exogenous will be biased, resulting in incorrect inference about the relationship between WTP and risk, and in biased estimates of the VSL. To address this problem, it is necessary to specify an additional equation relating respondent-assessed baseline risks to respondent characteristics and other exogenous factors that serve as instruments, and to estimate two systems of simultaneous equations, one for self-assessed risks (or risk reductions) and one for WTP.

G. Sample Selection Bias.

If the propensity to participate in a mortality risk survey depends on unobservable individual characteristics that also influence WTP for risk reductions, then the estimates of WTP may be affected by sample selection bias. To correct for it, it is necessary to specify and estimate two econometric equations. The first is a probit equation that predicts the probability of participating in the survey as a function of individual characteristics. Let P* denote propensity to participate, a continuous but latent variable:

(2.7) $$P_i^* = \mathbf{z}_i\gamma + \eta_i$$

with **z** a vector of individual characteristics, γ a vector of coefficients, and η a normally distributed error term with mean zero and variance equal to one. Let P be a binary indicator that takes on a value of 1, denoting participation in the survey, if P* is greater than zero, and zero otherwise.

The second equation explains WTP as a function of a vector of individual characteristics **x** and experimental treatments exogenously assigned to the respondent (e.g., the size of the risk reduction to be valued):

(2.8) $$WTP^* = \mathbf{x}_i\beta + \varepsilon_i\,,$$

where η and ε are correlated, their covariance being equal to σ. Because WTP is observed only for those persons who participated in the survey, one estimates

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asking them to assess their subjective risks of death due to hypertension and their subjective risk reduction associated with a medical intervention.

$$(2.9) \qquad WTP \mid P^{*} > 0 = \mathbf{x}_i \beta + \sigma \frac{\phi(\mathbf{z}_i \gamma)}{\Phi(\mathbf{z}_i \gamma)} + \text{error} .$$

In practice, this system of equations can be estimated in two stages. The first stage is a probit predicting the probability of participating in the survey. The estimated coefficients are used to build the Mills' ratio term $\phi(\mathbf{z}_i \hat{\gamma}) / \Phi(\mathbf{z}_i \hat{\gamma})$ to be included in the WTP equation. This is a limited-information maximum likelihood estimation (LIML) approach.

Once the two-stage estimation procedure is completed, mean WTP is estimated (assuming normally distributed WTP) as $\bar{x}\hat{\beta}$. Notice that the estimate of β is biased unless one explicitly includes the correction term $\phi(\mathbf{z}_i \hat{\gamma}) / \Phi(\mathbf{z}_i \hat{\gamma})$ in the WTP equation. It should also be noted that correct implementation of the two-stage estimation procedure requires that the standard errors in the second stage be corrected using the formulae provided in Murphy and Topel (1985). (Alternatively, the system of equations can be estimated by full-information maximum likelihood.)

It is clear that to estimate the probit model of participation information about the survey participants is necessary, as well as information about those persons who were sent questionnaires or otherwise solicited to participate in the survey, but declined to. With mail surveys, Cameron et al. (1999) suggest saving the addresses and zipcodes of all individuals who were sent questionnaires and imputing to those persons who do not return the completed questionnaire the characteristics (such as median income, percentage of college-educated adults, percent of home ownership, etc.) from the Census of the residents of his or her zipcode. This procedure assumes that an individual is much like his or her [avoid he or she; his or her…pick one and go with it] neighbors. With phone surveys, it might be possible to ask some questions of the person who answers the telephone, and to obtain some information about him or her, even if he or she elects not to continue the survey.

Figure 2.2. Effect of yea-saying.

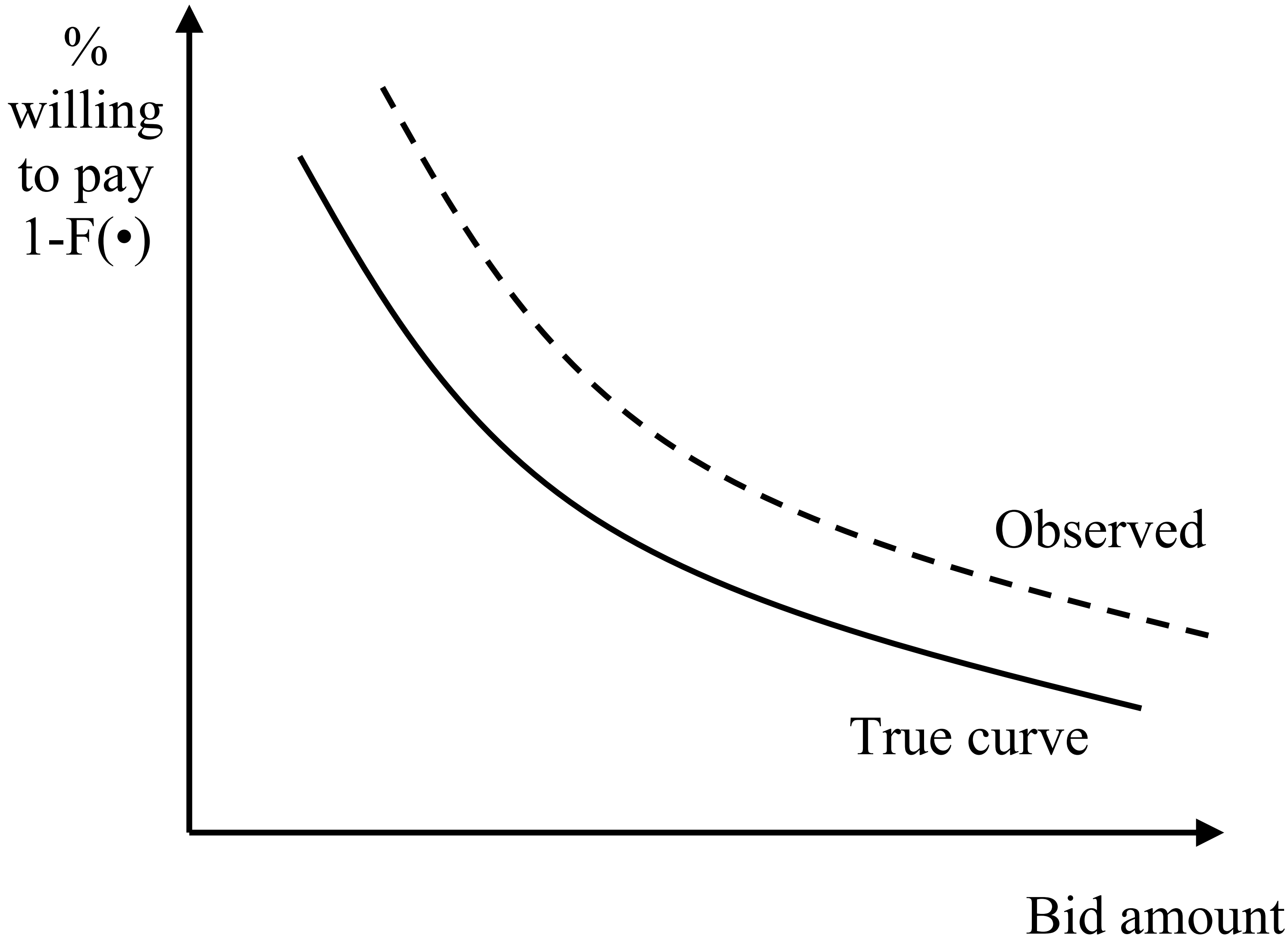
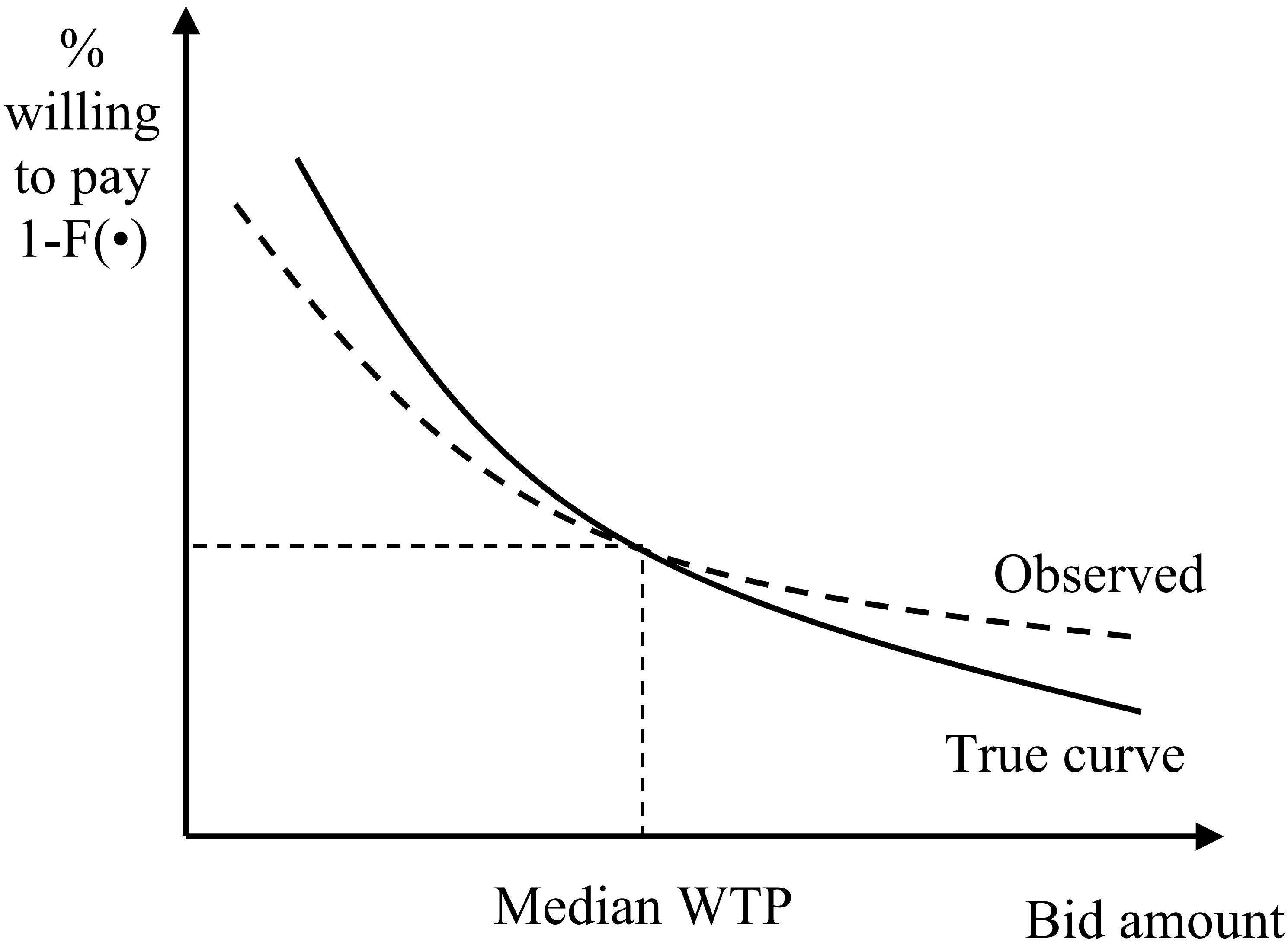


Figure 2.3. Effect of Completely Random Responses.



3. Datasets Requested and Request Status

One of the goals of this research project was to examine the robustness of the VSL estimates from CV studies used by the US EPA in its policy analyses. The five estimates—corresponding to four studies—are listed in table 3.1, which also presents a succinct description of these studies and their VSL figures.

Table 3.1. Original Studies: Description, VSL figures, and Data Availability

Study	Description
Gerking, Shelby et al. (1988), "The Marginal Value of Job Safety: A Contingent Valuation Study," *Journal of Risk and Uncertainty*, 1(2), 185-200	Mail survey (national sample) asking respondents to report wages, occupation, other respondent characteristics. Respondents are asked to identify their job on a risk ladder, then to report WTP (WTA) for a reduction (increase) by one step on the ladder. VSL = $2.66 million (based on WTP) VSL used by EPA: $4.1 million (1997 $)* Data and questionnaire available.
Gegax, Douglas, Shelby Gerking and William Schulze (1985), Valuing Safety: Two Approaches, in Experimental Methods for Assessing Environmental Benefits, Volume IV. Report prepared for the U.S. EPA, Office of Policy Analysis under Assistance Agreement #CR811077-01.	Same survey as above. The questionnaire includes questions about income, type of occupation and industry, perceived risks of various injuries and deaths in the workplace, experience, etc. The responses to these questions are used to estimate a compensating wage equation. VSL = $2.136 million (based on WTP) VSL used by EPA: $4.0 million (1997 $)* Data and questionnaire available.
Miller, T. and J. Guria (1991), *The Value of A Statistical Life in New Zealand*, Wellington, New Zealand: Land Transport Division, New Zealand Ministry of Transport.	In-person survey (national sample) asking respondents a mix of contingent valuation, contingent behavior, and other choice questions (which city they would live in). VSL = NZ $1.893 (average of all questions) VSL used by EPA: $1.5 million (1997 $)* Data and questionnaire not available.
Viscusi, W. Kip, Wesley A. Magat and Joel Huber (1991a), *Issues in Valuing Health Risks: Applications of Conjoint Valuation*	Uses risk-risk and risk-money tradeoffs to infer the value attached to three diseases potentially associated with environmental

and Conjoint Measurement to Nerve Disease and Lymphoma, Draft report to EPA, Office of Policy, Planning and Evaluation, under Assistance Agreement CR# 815455-01-1 and 814388-02.	exposures: - peripheral neuropathy [a nerve Disease]; → VSL = $1.6 million. - curable lymphoma (chance of dying 10%) → VSL = $2.5 million - terminal lymphoma (chance of dying 100%) → VSL = $4.0 million VSL used by EPA: $3.3 million (1997 $)* Data no longer exist; questionnaire not available.
Jones-Lee, Michael W. (1989), *The Economics of Safety and Physical Risk*, Oxford, Great Britain: Basil Blackwell.	National sample, mix of choice and CV questions. VSL used by EPA: $4.6 million (1997 $)* Questionnaire not available. Declined to obtain the data.

* As reported in US Environmental Protection Agency (2000), Guidelines for Preparing Economic Analyses, Office of the Administrator, EPA Report 240-R-00-003, Washington, DC, September.

We approached the authors of three of the four original stated-preference studies to supply the data collected through their surveys. Only Dr. Gerking was able to provide the dataset used for his report to the US EPA (Gegax et al. 1985) and for his 1988 article; data for a number of the remaining studies were not available: Drs. Viscusi and Huber no longer have the data supporting their 1991 study. Ted Miller also said that he did not have the data from his 1991 study co-authored with Guria. Regarding the last stated-preference study, the one by Dr. Jones-Lee, we declined to obtain and work with these data, due to data quality concerns.

As the data for the older studies were lacking, we selected a number of recent articles estimating the VSL using contingent valuation surveys, and requested the authors to share their data, questionnaires, reports and programs, and any other useful supporting materials for this exercise.

The studies that we identified as potentially interesting were as follows:

(i) Johannesson et al. (1991): This study focuses on persons with high blood pressure. These persons may be more susceptible to the effects of certain pollution exposures, such as particulate matter in the air and heavy metals;

(ii) Lanoie et al. (1995): The authors' goals were similar to those of the Gerking et al. study, in that they wished to compare wage-risk tradeoffs in the workplace with stated WTP for a risk reduction. The risks presented to the respondents were of the correct magnitude;

(iii) Johannesson et al. (1997):This study focuses on the relationship between age and VSL;

(iv) Johannesson and Johansson (1996): This study focuses on lifetime extensions to be experienced in the future, rather than risk reductions;

(v) Persson et al. (2001): This study was carefully conducted study in the context of road transportation safety, and

(vi) Corso et al. (2001): This paper explores whether failure of WTP to increase and/or be proportional with the size of the risk reduction is due to poor understanding of probabilities on the part of the respondents, and if this can be addressed with appropriate visual aids.

(vii) In addition, the data from surveys in Canada and the US based on similar survey instruments (Krupnick et al., 2002, and Alberini et al., forthcoming) are available to us. The latter two studies examine the relationship between VSL and age and health status, and elicit WTP for future risk reduction, seeking to estimate the implicit discount rate(s) of the respondents (Alberini et al., 2004).

Table 3.2 summarizes these more recent studies, along with the status of the data and questionnaires. In terms of the approach for eliciting WTP, and hence the econometric models, these studies include both one-shot dichotomous choice WTP questions (Johannesson et al., 1996, 1997), dichotomous choice questions with follow-ups (Krupnick et al., and Alberini et al.), and the open-ended format (Lanoie et al., Persson et al.). The mode of administration included mail surveys (Persson et al.), telephone surveys (Johannesson et al., 1996, 1997), combination telephone-mail-telephone (Corso et al.), in-person interviewing (Lanoie et al.) and self-administered computer questionnaires (Krupnick et al., Alberini et al.).

One caveat is in order. None of these studies explicitly refers to the environmental exposure context. However, Desaigues et al. (2003), in discussing an application of the Krupnick et al. questionnaire in France, argue that perhaps this is the only recent mortality risk CV study that can be applied in a straightforward fashion to the context of air pollution. We also wish to emphasize that in all of the studies listed in table 3.2, the risk reduction to be valued by the respondent is of a private nature, and is delivered by a hypothetical medical intervention or product, or (in the case of road transportation risks) by an unspecified safety device (Persson et al.) or by side-impact airbags (Corso et al.).

Table 3.2. Recent Mortality WTP Studies and Data Availability.

Authors and article	**Availability of Data and Questionnaire**
Johannesson, Magnus, Bengt Jonsson, and Lars Borquist (1991), "Willingness to Pay for Antihypertensive Therapy—Results for a Swedish Pilot Study," *Journal of Health Economics*, 10, 461-474.	Data no longer exist.
Lanoie, Paul, Carmen Pedro and Robert Latour (1995), "The Value of a Statistical Life: A Comparison of Two Approaches," *Journal of Risk and Uncertainty*, 10. 235-257.	Questionnaire available; data no linger exist.
Johannesson, Magnus and Per-Olov Johansson (1996), "To Be, or Not to Be, That is the Question: An Empirical Study of the WTP for an Increased Life Expectancy at an Advanced Age," *Journal of Risk and Uncertainty*, 13, 163-174.	Data are available. Data received: ADVAGEFILE.SAV (SPSS dataset), converted into ADVAGE.SD2 (SAS dataset). Basic analyses in ADVAGE DATAPREP.SAS. Questionnaire available (in Swedish and English translation).
Johannesson, Magnus, Per-Olov Johansson, and Karl-Gustav Lofgren (1997), "On the Value of Changes in Life Expectancy: Blips versus Parametric Changes," *Journal of Risk and Uncertainty*, 15, 221-239.	Data are available. Data received: VSLFILE.SAV (SPSS dataset), converted into VSLFILE.SD2 (SAS dataset). Basic analyses in VSLFILE DATAPREP.SAS. Questionnaire available (in Swedish and English Translation).
Persson, Ulf, Anna Norinder, Krister Hjalte and Katarina Gralen (2001), "The Value of a Statistical Life in Transport: Findings from a New Contingent Valuation Study in Sweden," *Journal of Risk and Uncertainty*, 23(2), 121-134.	Data are available. Data received: ENKELTOTALLA.SAV (SPSS dataset), converted to SAS (ENKELTOTALLA.SAS7BDAT). SAS program for data analysis is DATAPREP2.SAS. Questionnaire available (in Swedish and English translation).
Corso, Phaedra S., James K. Hammitt, and John D. Graham (2001), "Valuing Mortality-Risk Reduction: Using Visual Aids to Improve the Validity of Contingent Valuation," *Journal of Risk and Uncertainty*, 23(2), 165-184	Questionnaire available. First author declined to supply the data because she has not completed the analysis.
Krupnick, Alan, Anna Alberini, Maureen Cropper, Nathalie Simon, Bernie O'Brien, Ron Goeree, and Martin Heintzelman (2002), "Age, Health, and the Willingness to Pay for Mortality Risk Reductions: A Contingent Valuation Survey of Ontario	Data available. Self-administered computer questionnaire available upon request.

Residents," *Journal of Risk and Uncertainty*, 24, 161-186.	
Alberini, Anna, Maureen L. Cropper, Alan Krupnick, and Nathalie Simon (forthcoming), "Does the Value of a Statistical Life Vary with Age and Health Status? Evidence from the U.S. and Canada," *Journal of Environmental Economics and Management*.	Data available. Self-administered computer questionnaire available upon request.

4. Description of the Studies

In this section, we briefly describe the studies we use to examine the importance of various econometric modeling issues in deriving the VSL. A summary of these studies and their VSL figures is displayed in table 4.1.

Table 4.1. Mortality Risk Studies Re-analyzed in this Report.

Study	Description and VSL
Gerking, Shelby et al. (1988), "The Marginal Value of Job Safety: A Contingent Valuation Study," *Journal of Risk and Uncertainty*, 1(2), 185-200 Gegax, Douglas, Shelby Gerking and William Schulze (1985), Valuing Safety: Two Approaches, in Experimental Methods for Assessing Environmental Benefits, Volume IV. Report prepared for the U.S. EPA, Office of Policy Analysis under Assistance Agreement #CR811077-01.	Mail survey (national sample) asking respondents to report wages, occupation, other respondent characteristics. Respondents are asked to identify their job on a risk ladder, then to report WTP (WTA) for a reduction (increase) by one step on the ladder. VSL = $2.66 million.
Johannesson, Magnus, and Per-Olov Johansson (1996), "To Be, or Not to Be, That is the Question: An Empirical Study of the WTP for an Increased Life Expectancy at an Advanced Age," *Journal of Risk and Uncertainty*, 13, 163-174.	Telephone survey of Swedes aged 18-69. Respondents are told what their chance of surviving to age 75 is, and then are asked DC questions about WTP for a medical intervention that increases their life expectancy at age 75 by one year. VSL = $101,000.
Johannesson, Magnus, et al. (1997), "On the Value of Changes in Life Expectancy: Blips versus Parametric Changes," *Journal of Risk and Uncertainty*, 15, 221-239.	Telephone survey of Swedes aged 18-74. DC questions about WTP for 2 in 10,000 reduction in their risk of dying (from all causes). VSL ≅ $4.5 million.
Persson, Ulf, et al. (2001), "The Value of a Statistical Life in Transport: Findings from a New Contingent Valuation Study in Sweden," *Journal of Risk and Uncertainty*, 23(2), 121-134.	Mail survey in Sweden. Elicits WTP for X% reduction in the risk of dying in a road-traffic accident. Subjective baseline risks. VSL = $2.84 million (based on WTP for 2 in 100,000 risk reduction).
Krupnick, Alan, et al. (2002), "Age, Health, and the Willingness to Pay for Mortality Risk Reductions: A Contingent Valuation Survey of Ontario Residents," *Journal of Risk and Uncertainty*, 24, 161-186.	Survey of persons aged 40-75 years in Hamilton, Ontario. Self-administered computer questionnaire, centralized facility.

	VSL = Can $1.2 to 2.8 million.
Alberini, Anna, et al. (forthcoming), "Does the Value of a Statistical Life Vary with Age and Health Status? Evidence from the U.S. and Canada," *Journal of Environmental Economics and Management*.	US national survey conducted over Web-TV. VSL =$700,000 to $1.54 million (based on 5 in 1000 risk reduction)

A. Johannesson et al. 1996 study

The first of the two data sets provided by Magnus Johannesson was that used in his 1996 paper co-authored with Per-Olov Johannsson "To Be, or Not to Be, That Is the Question: An Empirical Study of the WTP for an Increased Life Expectancy at an Advanced Age." [Full citations are provided several times in tables. Don't think you need to include here.]In this study, a random sample of the population of 18-69 year-olds in Sweden was interviewed over the telephone in June 1995. Respondents were told that a person of their age and gender had a probability of X of surviving until age 75. Respondents were told that on average a 75-year old lives for an additional ten years, and were asked to consider a medical treatment that increases expected remaining life at age 75 by another year. Respondents were then asked whether they would choose to buy this treatment if it cost [FILL] SEK and has to be paid for this year.

The purpose of the study is, therefore, to elicit WTP for a future life extension, and the elicitation method is a single-bounded dichotomous choice payment question.[7] There were a total of 6 bid values: 100, 500, 1000, 5000, 15000 and 50000 SEK, with one US dollar equivalent to 7.25 SEK at the time of the study.

A total of 2013 respondents agreed to participate in the survey. The probabilities of survival until age 75 varied with the respondent age and gender, but the length of the extension of the expected remaining life at age 75 was the same—one year—for all respondents.

Table 4.2 reports the number of respondents assigned to each bid value and the percentage of "yes" responses to each bid value. Clearly, median WTP is between 100 and 500 SEK. In practice, the bid design covers only the upper tail of the distribution of WTP, raising questions about the quality of the estimates of mean and median WTP, if the researchers wishes to fit a binary response model assuming that the distribution of WTP is skewed.

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[7] The original questionnaire also queried respondents about how confident they felt about their responses to the payment question. Specifically, as shown in Appendix B, people who answered "yes" to the dichotomous choice question were asked whether they felt "totally certain" or "fairly certain" that they would buy the treatment at the stated cost. The dataset supplied by Dr. Johannesson includes a variable, WTPCONS, an indicator that takes on a value of one if and only if the respondent was absolutely sure that he would pay, and zero otherwise.

Table 4.2. Bid Design and "Yes" Response Rates in Johannesson and Johansson (1996).

Bid value (1995 SEK)	Number of respondents	Percent willing to pay the bid
100	342	53.22
500	337	38.58
1000	325	31.38
5000	327	22.63
15000	352	13.64
50000	330	9.09

The dataset also contains the following variables:

- Respondent age,
- household size,
- education (1=less than high school, 2=high school, 3=university),
- personal monthly pre tax income, and
- sex.

Descriptive statistics for these variables are reported in Table 4.3.

Table 4.3. Descriptive Statistics of the Data. Johannesson and Johansson (1996).

Variable (variable description)	Number of valid observations	Mean	Std deviation	Min	Max
PRICE (bid)	2013	11894	17667	100	50000
WTP (response to the payment question)	2013	0.2811	0.4496	0	1
AGE	2013	42.11	14.05	18	69
HSIZE (size of the household)	2007	2.6253	1.2973	1	7
EDU (categorical variable for educational attainment)	2007	2.0842	0.7883	1	3
PINC (personal income) [8]	1927	14397.51	9122.38	0	99000

[8] Household income was supposed to be included in the dataset, but the authors of the article found something wrong with that variable in the file, as it had missing values for all households with more than one member (Magnus Johannesson, personal communication, April 2002).

In addition to producing estimates of WTP based on a binary logit model, Johannesson and Johansson separate the data by age group of the respondent, and fit separate logit models to each of the subsamples derived in this fashion. They then predict mean WTP for each age group, and trace out the profile of (mean) WTP over age, concluding that WTP for an extension in life expectancy past a certain age increases with age.

B. Johannesson et al. 1997 study

The second dataset supplied by Dr. Johannesson was used in the paper "On the Value of Changes in Life Expectancy: Blips versus Parametric Changes," and was co-authored with Per-Olov Johansson and Karl-Gustaf Lofgren.

The authors report on a telephone survey of a representative sample of the population of Sweden of ages 18-74. The final sample is comprised of 2029 individuals, for a response rate of 83 percent. The survey was conducted from September to November 1996.

Respondents were told that X out of 10000 people of their gender and age would die during the next year.[9] They were also asked to assume that a preventative and painless treatment were available that would reduce by 2 in 10000 the risk of dying in the next year, but have no effects thereafter.

The bid levels used in this study were 300, 500, 1000, 2000, 5000 and 10000 SEK. Table 4.4 shows the percentage of respondents assigned to each of the bid values and the percentage of "yes" responses at each bid level.

Table 4.4 Experimental Design and "Yes" Response Rates in the Johannesson, Johansson and Lofgren study, 1997.

Bid amount	Number of respondents	Percentage "yes" responses
300	405	51.36
500	410	44.63
1000	196	37.76
2000	406	36.70
5000	410	34.10
10000	201	28.83

[9] The baseline risk of death over the next year was 10, 30, 70, and 200 for males in the age groups 18-39, 40-49, 50-59, and 60-69, respectively. For females, the baseline risk values were 5, 20, 40, and 100. All baseline risks are out of 10000.

The percentages of "yes" responses follow a pattern similar to that of Table 4.3, in that they (i) decline monotonically with the bid amount, (ii) imply only limited coverage of the range of WTP values, and (iii) imply that median WTP is between 300 and 500 SEK.

Johannesson et al. estimate mean WTP (and hence VSL) by age group. They obtain a inverted-U shape profile of the relationship between WTP and age.

Descriptive statistics of the other variables included in the dataset provided by Dr. Johannesson are shown in Table 4.5.

Table 4.5. Descriptive Statistics of the data. Johannesson et al. study, 1997.

Variable (variable description)	Number of valid observations	Mean	Std deviation	Min	Max
PRICE (bid)	2029	2658.95	2976.25	300	10000
WTP (response to the payment question)	2028	0.3999	0.4900	0	1
AGE	2027	45.08	15.21	18	75
HSIZE (size of the household)	2026	2.60	1.37	1	9
EDU (categorical variable for educational attainment)	2019	2.02	0.80	1	3
PINC (personal income, thou. SEK)	1929	14.24	8.08	0	99
QOL (Quality of life, 1=lowest quality of life, 10=highest quality of life)	1990	7.34	1.77	1	10
HINC (household income, thou. SEK)	1692	24.49	12.86	0	99
SEX (1=male)	2029	0.4785	0.4996	0	1

C. Gerking et al. study (1988)

The dataset submitted by Dr. Gerking matches almost perfectly the sample detailed in the Gerking et al. (1988) article.[10] This survey was conducted by mail. Questionnaires were sent to 3000 households representative of the population of the US, and to 3000 households drawn from US counties with a high representation of risky industries.

Starting from a total of 969 returned questionnaire, Gerking et al.'s usable sample excludes the 11 persons who failed to report WTA or WTP amounts, restricting attention

[10] These same data were used in Gegax et al. 1985.

to 459 observations for WTA, and 499 for WTP. Gerking et al. further exclude from the sample (i) the three respondents who are retired or unemployed, and (ii) the 58 respondents who did not indicate their initial risk level. The final sample is comprised of 904 observations, 428 for WTA (q6type=1) and 476 for WTP (q6type=2). Descriptive statistics for this cleaned sample are displayed in table 4.6.

Table 4.6. Descriptive statistics for the Gerking et al. data.

(a) Willingness to Accept Subsample

variable	N	Mean	Std. Dev.	Minimum	Maximum
WTA	428	1871.15	2215.36	0	6001.00
education	379	15.11	2.89	11	23
Age	427	41.80	11.87	19	71
income	378	28,369.96	21166.12	28	200,000
Black (dummy)	427	0.02	0.15	0	1
Hispanic	428	0.007	0.08	0	1
Male	427	0.85	0.35	0	1
Union member	424	0.27	0.44	0	1
Subjective risk	409	0.00065	0.00054	0.00025	0.00225
Objective risk	428	0.00007	0.00009	0	0.00064
Lives in central city (dummy)	426	0.14	0.35	0	1
Lives in suburban area (dummy)	428	0.54	0.50	0	1
Lives in rural area (dummy)	428	0.31	0.46	0	1

(b) Willingness to Pay Subsample

variable	N	Mean	Std. Dev.	Minimum	Maximum
WTA	476	678.60	1554.85	0	6001
education	404	14.94	2.65	11	23
Age	470	40.80	11.70	20	75
income	415	27,686.17	20,944.37	0	250,000
Black (dummy)	468	0.03	0.18	0	1
Hispanic	476	0.02	0.16	0	1
Male	470	0.82	0.38	0	1
Union member	467	0.29	0.45	0	1
Subjective risk	466	0.0008	0.00049	0.0005	0.0025
Objective risk	476	0.000076	0.00010	0	0.00064
Lives in central city (dummy)	470	0.17	0.37	0	1
Lives in suburban area (dummy)	476	0.55	0.50	0	1
Lives in rural area (dummy)	476	0.27	0.45	0	1

D. Krupnick et al studies

The Krupnick et al. studies (Krupnick et al., 2002; Alberini et al., forthcoming) were conducted in Hamilton, Ontario in Spring 1999, and in the US in August 2000. Respondents in the Canada study were recruited by random digit dialing and asked to go to a centralized location to take the survey, which was self-administered using the computer. Respondents for the US study, which was nationwide, were selected among members of a panel of households developed by Knowledge Networks, were solicited to participate in the survey via e-mail, and took the survey via Web-TV.

The purpose of these two studies was three-fold. First, the researchers wished to examine the relationship between age and WTP, which is important for environmental policy purposes, as many environmental programs save primarily the lives of older people. Second, they wished to examine the relationship between health status and WTP. This is also important for policy purposes, as some agencies have argued in favor of the use of Quality Adjusted Life-Years (QALY), a concept widely used in medical decisionmaking where values are adjusted for quality of life, which is presumably lower for chronically ill people. Third, they wished to examine the matter of latency.[11]

[11] By latency, these authors refer to a risk reduction that takes place in the future, but for which investment must be made now.

In both studies, the survey begins by asking for the respondent's age and gender, inputs required for the purpose of showing the respondent age- and gender-specific baseline risks of dying. The questionnaire then asks whether the respondent suffers from heart disease, chronic respiratory illness, has high blood pressure, and has or has had cancer.[12] Respondents are also asked whether family members suffered from these illnesses.[13]

The survey continued with a simple probability tutorial and with a probability quiz and a probability choice question intended to test their comprehension of probabilities. This part of the survey introduced the risk communication device used throughout the survey, i.e., a grid with 1000 squares, where white squares represent survival and red squares represent death.

People were then shown the risk of dying for the average person of their age and gender over the next ten years. Risks were expressed as X per 1000. People were also shown the increase in the risk of dying for different age groups, and were subsequently informed that measures can be taken to reduce these risks. Subjects were given examples of actions that can be taken to reduce risks, along with qualitative information about the expensiveness of such actions.

In the contingent valuation scenario respondents were asked to consider a reduction in the risk of dying, shown graphically by changing the corresponding number of squares from red to blue, delivered by a hypothetical product. If the product had to be taken for 10 years, and paid for every year, to secure the risk reduction, the questionnaire continued, would the respondent be willing to pay $Y per year for that product? Based on the response to this question, respondents were queried about a higher dollar amount (if yes) or a lower dollar amount (if no). Those respondents who answered "no" twice were asked whether they were willing to pay anything at all, and, if so, how much.

Respondents valued a total of three risk reductions. Two of them would begin immediately and take place over the next 10 years, while the last was to be experienced at age 70. (The latter risk reduction was valued only by respondents of age 60 and younger.) The graphical presentation emphasized the overlap between the timing of the payments and the timing of the risk reduction (for the immediate risk reductions), or the delay

[12] The questions about chronic illnesses questions were more detailed and precise in the US study. Respondents were asked if they had even been diagnosed by a health care professional as having coronary disease, high blood pressure, asthma, chronic bronchitis, emphysema, high blood pressure, other heart disease, and if they had ever had a myocardial infarction (a heart attack), a stroke, and ever been diagnosed with cancer.

[13] The presence of these illnesses in the family may influence the respondents' acceptance of the risks stated to them in the survey, their subjective probabilities of surviving to age 70, and their subjective expected lifetime, thereby affecting their willingness to pay figure. Familiarity with these illnesses may also have an independent effect on WTP. In the US, people were also asked whether their natural parents were alive, and if so, how old they were (or at what ages they died).

between them (for the future risk reduction). Clearly, in this survey respondents value a private risk reduction.

As shown in Table 4.7, the survey was administered using a split sample approach with respondents randomly assigned to one of two waves and the order in which the risk reductions were shown to the respondents varying across waves. This allows an external scope test between WTP for the 5 in 1000 risk reduction (wave 1) and WTP for the 1 in 1000 risk reduction (wave 2), in addition to allowing one to perform internal scope tests.

Table 4.7. Experimental Design in the Krupnick et al. (2001) and Alberini et al. (forthcoming) studies

Risk reduction to be valued	Wave 1	Wave 2
I	5 in 1000 over 10 years	1 in 1000 over 10 years
II	1 in 1000 over 10 years	5 in 1000 over 10 years
III	5 in 1000 over 10 years, starting at age 70	5 in 1000 over 10 years, starting at age 70

Bid amounts for both studies are shown in table 4.8.

Table 4.8. Bid Values in the Krupnick et al. (2001) and Alberini et al. (forthcoming) studies.

	Initial bid	If yes	If no
US (2000 US dollars)	70	150	30
	150	500	70
	500	725	150
	725	1000	500
Canada (1999 Canadian dollars)	100	225	50
	225	750	100
	750	1100	225
	1100	1500	750

Figures 4.1 and 4.2 display the percentages of “yes” responses to the initial payment questions by country and by risk reduction. Three patterns emerge from these figures. First, respondents behave as expected, in that the percentage of “yes” responses declines with the bid amount. This is true for both risk reductions and both countries. Second, the proportion of “yes” responses is lower for the smaller risk reduction at all bid values, implying that WTP for the smaller of the two risk reductions should be smaller. Third, there is a considerable similarity between the two countries, which suggests that WTP figures may be statistically indiscernible once one accounts for the exchange rage and/or purchasing power parity.

Descriptive statistics of the Canadian and US samples are shown in table 4.9 and 4.10.

Table 4.9. Krupnick et al. Canada study, Cleaned Data,* Wave 1. N=616.

Variable	N	Mean	Standard Deviation	Minimum	Maximum
Age	616	53.97	10.21	40.00	75.00
Male dummy	616	0.47	0.50	0	1
Education	616	13.75	2.98	0	18.00
Annual household Income (Can $)	605	58923.14	36401.88	12000	175000
Chronic illness**	616	0.41	0.49	0	1

* Respondents who failed the probability quiz and prefer to be person with higher risk of dying are excluded from the sample.
** Any of cardiovascular diseases, high blood pressure, asthma, chronic bronchitis or emphysema, other chronic illnesses.

Table 4.10. Alberini et al. US data, Cleaned data,* Wave 1.

Variable	N	Mean	Standard Deviation	Minimum	Maximum
Age	571	55.66	11.45	40	80
Male dummy	567	0.48	0.50	0	1
African-American dummy	571	0.09	0.29	0	1
Education	569	13.07	2.35	8	21
Annual household Income (US $)	498	53338.35	30645.31	5000.00	130000
Chronic illness**	567	0.51	0.50	0	1

* Respondents who failed the probability quiz and prefer to be person with higher risk of dying are excluded from the sample.
** Any of cardiovascular diseases, high blood pressure, asthma, chronic bronchitis or emphysema, other chronic illnesses.

Krupnick et al. use a “spike model” (i.e., the mixed continuous/interval data version of a tobit) to model the responses to the payment questions in the Canada study, but Alberini et al. (forthcoming) use only the responses to the initial and follow-up questions and estimate a double-bounded model of WTP based on the Weibull distribution. This model does not admit zero WTP responses. Even those respondents who announced they were not willing to pay anything at all are ascribed a positive WTP amount between zero and the lowest bid they were queried about.[14]

[14] In the Canada study, zero WTP responses were reported by 19.5% of wave 1 for the 5 in 1000 risk reduction, and 36.8% of wave 2 for the 1 in 1000 risk reduction. The corresponding relative frequencies for the US are 22.0% and 37.7%.

WTP increases significantly with the size of the risk reduction and is lower for the future risk reduction. The Canada study finds that WTP is about 30% lower for the oldest age group (70 year-olds and older). A decline of similar magnitude—but statistically insignificant—is also seen with the US data. The data from these studies also suggest that there is no particular reason to believe that people with chronically impaired health are willing to pay less for a risk reduction: In Canada, for example, people with cancer are actually willing to pay *more*. Finally, WTP for a future risk reduction is lower than WTP for a contemporaneous risk reduction, implying discount rates of about 8 percent in the Canada and 4.5 percent in the US.

Figure 4.1. Percent of "yes" responses by bid value: Canada study

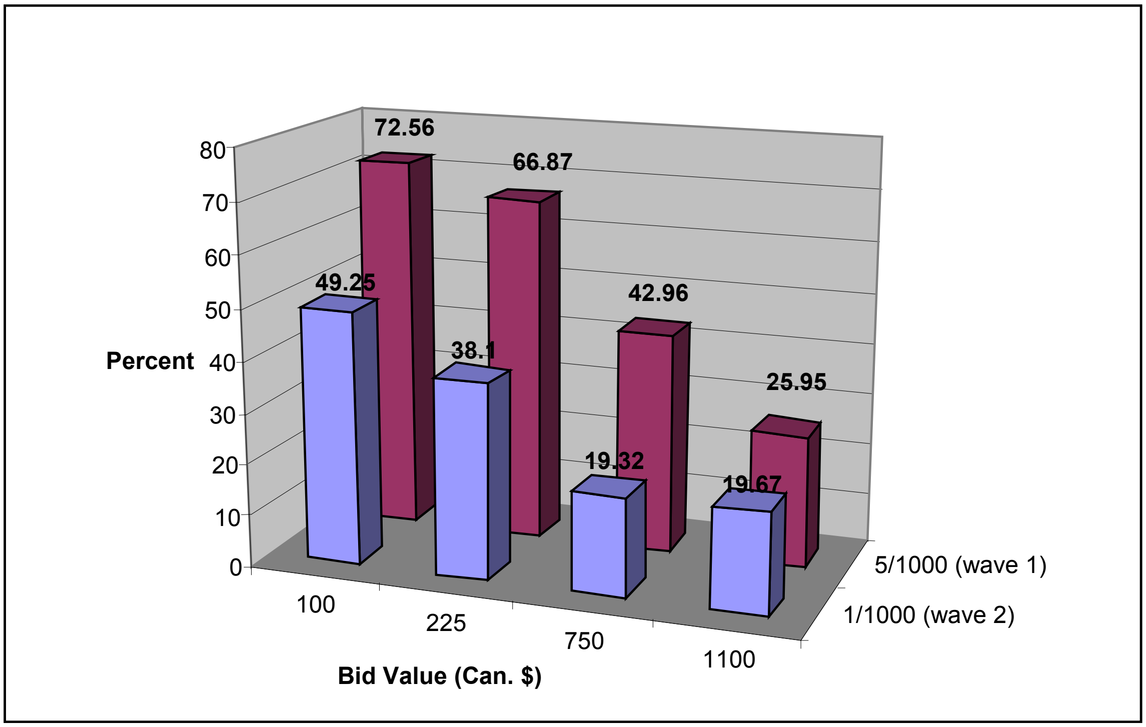
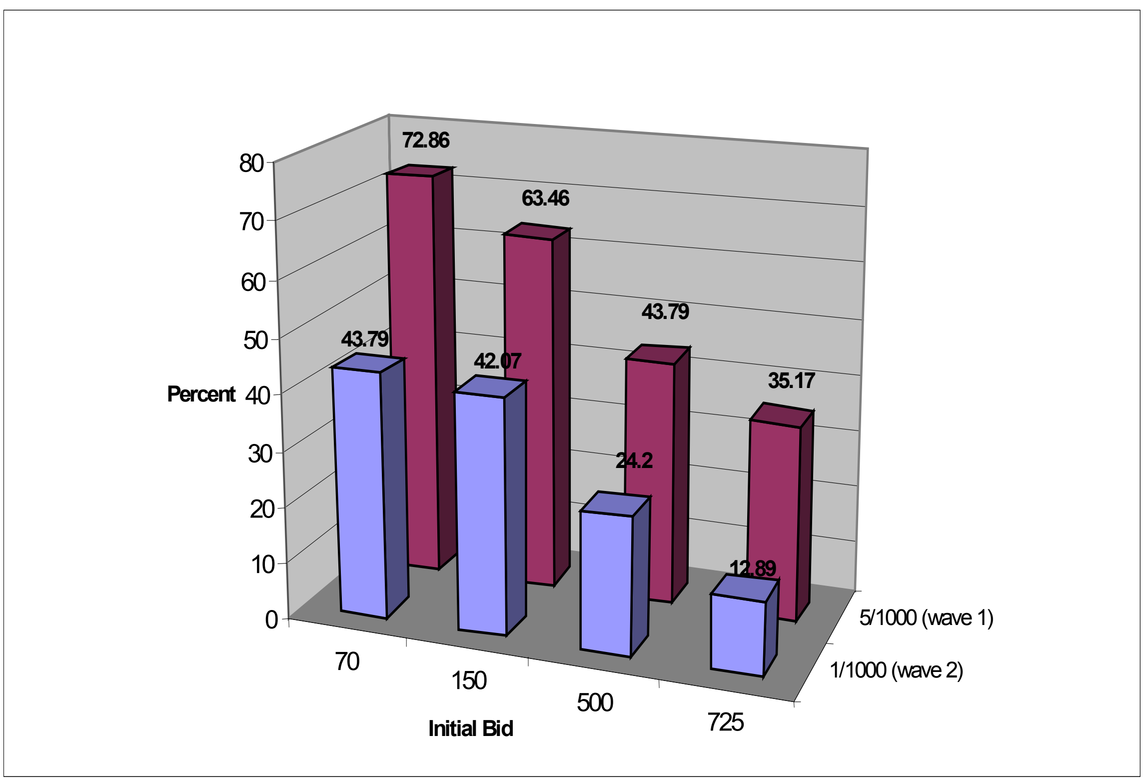


Figure 4.2. Percent of "yes" responses by bid value: US Study.



E. Persson et al. study

Persson et al (2001) report on a mail survey eliciting WTP for reductions in the risk of dying in a road-traffic related fatality. The survey was conducted in Sweden in Spring of 1998. Questionnaires were mailed to a representative sample of Swedes of ages 18-74. The first mailing (N=5650) took place in March 1998. The recipients of this mailing were sent two reminders, and a total of 2884 questionnaire were returned, for a response rate of about 51%. The researchers also sent a total of 2645 "drop out" questionnaires, 659 of which were filled out and returned. The purpose of these "drop out" questionnaires was to investigate possible self-selection of respondents into the final sample.

Two major versions of the questionnaire were created. The first version focuses on the risk of dying in a road traffic accident, while the second focuses on the risk of experiencing non-fatal injuries. The article, however, focuses on the 935 completed questionnaires about fatal risks.

The survey instrument (see Appendix B) begins with questions about the respondent's gender, age, household composition, and access to a car. It continues with questions attempting to establish the respondents' behaviors in terms of traffic safety, such as use of helmets when bicycling and seat belts when driving. Detailed questions about driving a car, riding a bicycle, walking and using public transit follow. At the end of this section, respondents are asked if they were ever injured in a traffic incident, whether this accident was in the last year, and whether any family members have ever been in an accident.

In order to assess health status, the respondents were shown a thermometer-like scale with values ranging from 0 to 100, where 0 represents the worst possible condition and 100 represents the best possible condition. Respondents were asked to indicate on the scale the number corresponding to their current health.

The next section of the questionnaire introduces the risk of dying, the risk communication device being a grid of squares. Risks are expressed as X in 100,000, and as an example people are shown the risks of dying for various causes (all causes, heart disease, stomach or esophagial cancer, traffic accident) for a 50-year-old. Respondents are then asked to assess their subjective risk of dying for any cause, and to report their WTP for a reduction in this risk in an open-ended format.

The risk reduction is expressed as a proportion (10%, 30%, or 50%, depending on the questionnaire version; see table 4.11) of the baseline risk. The risk reduction is a private commodity (safety equipment and preventive health care) and is valid for one year at the time. A reminder of the respondent's budget constraint is provided.

The questionnaire moves on to the risk of dying in a road-traffic accident. The respondent is asked to assess his or her own risk of dying in a road-traffic accident, after giving due consideration to age, miles driven, and care taken with driving. The risk reduction is private, and is delivered by safety equipment, and subject to the same limitations, caveats and open-ended format as the previous WTP question. It should be kept in mind that respondents who were previously asked to value 10% and 50% reductions in their risk of dying for any cause are also asked to value 10% and 50% reductions, respectively, in their risk of dying in a traffic accident. Those respondents that were previously assigned a 30% reduction in the risk of dying for any cause are asked to consider 30% or 99% reductions in their risk of dying in a traffic accident, with random assignment to the 30% or 99% risk reduction (see table 4.11).

Table 4.11. Experimental design for the mortality risk version of the Persson et al. study (2001).

	Proportional reduction in the risk of dying in a traffic accident			
	10% risk reduction	30% risk reduction	50% risk reduction	99% risk reduction
Mailings (N=3050)	250	2300	250	250
Returned questionnaires (N=960)	112 (11.67 of usable sample) (44.8% of mailings)	566 (58.96% of usable sample) (24.60% of mailings)	181 (18.85% of usable sample) (72.4% of mailings)	101 (10.52% of usable sample) (40.4% of mailings)

Persson et al. investigate sample selection issues, finding that their sample has higher income, education, and miles driven per year than both the population of Sweden and the people that did not return the questionnaire but filled out the “drop out” card. Access to a car and gender in the sample are roughly the same as for the population at large, but women decline to fill out the questionnaire more often than men.

Descriptive responses of the sample with valid WTP observations for mortality risk reductions are displayed in table 4.12.

Table 4.12 Descriptive statistics, Persson et al. study (N=977: respondents who answer the mortality risk WTP questions.)

Variable	Valid observations	Mean	Standard deviation	Minimum	Maximum
Male (dummy)	977	0.56	0.49	0	1
Age	976	43.25	14.29	17	74
Income (SEK)	953	146,855.72	82,969.70	7,194.24	603,448.28
DEGRISK (subjective mortality risk in road traffic accident)	960	65.62	1614.81	0	50,000
kilometers driven in a car per year	943	14,075.4	7,639.9	4,999.37	27,495
Ever been injured in an accident (dummy)	977	16.94	37.53	0	1
High school education (dummy)	977	0.45	0.50	0	1
College education (dummy)	974	0.17	0.47	0	1

Persson et al. extrapolate WTP to the population by using the estimated income elasticity of WTP, but their econometric models do not explicitly control for self-selection into the sample using information from those respondents who filled the "drop out" card and did not return the questionnaire. We were not able to obtain these additional data from them.

To compute WTP and the VSL, since the baseline risk is different for different respondent, Persson et al run regressions relating (log) WTP to baseline risks, the size of the risk reduction, access to a car, experience with accidents, income and age. This results in estimates of VSL that vary with the size of the absolute risk reduction, a result that is in contrast with the assumption made by many agencies and practitioners that there is a single VSL.

F. Summary

This chapter has presented the studies that will be examined in more detail in the remainder of this report. Table 4.13 shows in which chapters the data from the various studies are analyzed econometrically to investigate the robustness of WTP with respect to various criteria.

Table 4.13. Data analyses in the remainder of the report.

Study	Chapters (topic of the analysis)
Johannesson et al. (1997)	Chapter 5 (sensitivity of welfare estimates to distributional assumptions for WTP, procedure for computing mean); Chapter 6 (identification and removal of outliers)
Johannesson and Johannesson (1996)	Chapter 5, Appendix (sensitivity of welfare estimates to distributional assumptions for WTP, procedure for computing mean); Chapter 7 (discrete mixtures)
Gerking et al. (1988)	Chapter 8 (interpretation of WTP responses); Chapter 9 (endogeneity of risk and WTP) Mentioned in chapter 10 (self-selection into the sample)
Krupnick et al. (2002)	Chapter 7 (discrete mixtures); Chapter 8 (interpretation of WTP responses)
Alberini et al. (forthcoming)	Chapter 6 (outliers); Chapter 7 (discrete mixtures); Chapter 10 (self-selection into the sample)
Persson et al. (2001)	Chapter 6 (outliers); Chapter 9 (endogeneity of risks and WTP); Mentioned in chapter 10 (self-selection into the sample)

5. Sensitivity of Welfare Estimates to the Distribution of WTP

In this section, we examine the issue of the sensitivity of the WTP estimates to the assumptions about the distribution of WTP and the procedure used for computing the welfare statistics. As the analysis and modeling of the data rest on these assumptions when information about WTP is elicited using dichotomous choice payment questions, we use the Johannesson et al. (1997) dataset. (All calculations and estimation runs are repeated with the Johannesson and Johansson (1996) data. Results for the latter dataset are reported in an Appendix to this chapter.)

A. Basic Data Checks and Changing the distribution of WTP

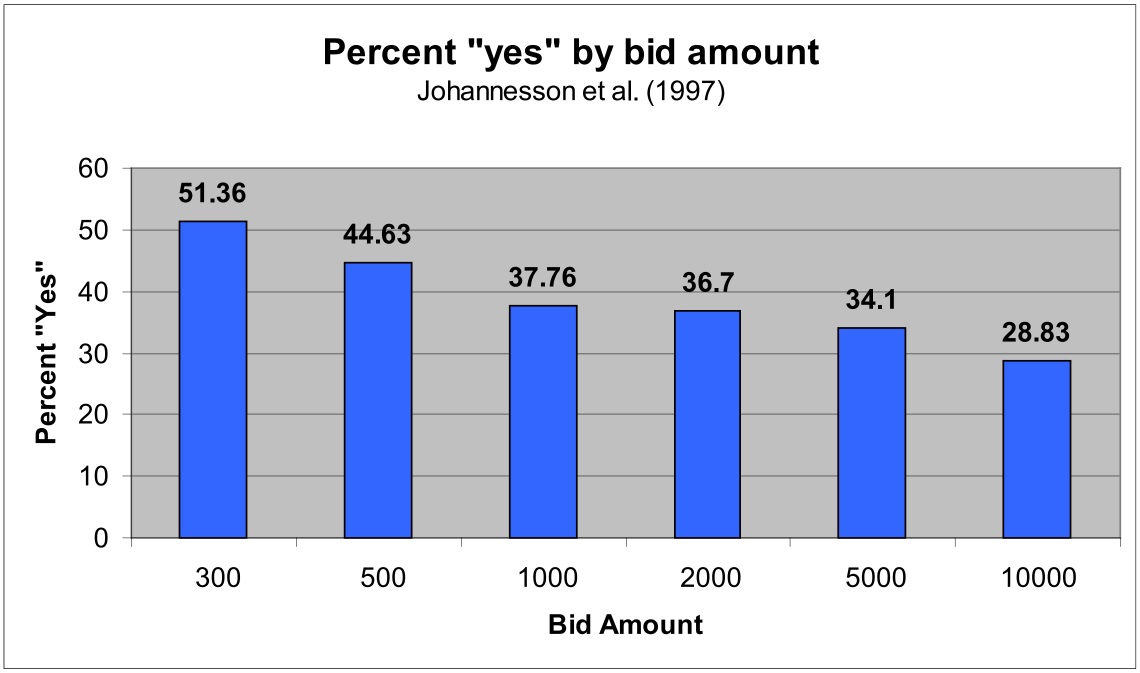
In this section, we examine the sensitivity of the estimates of WTP (and hence VSL) based on dichotomous choice data to the WTP distribution and to the procedure used by the researcher in computing mean WTP. To illustrate the consequences of assumptions and procedures, we used the data in Johannesson et al. (1997). Johannesson et al. surveyed Swedes aged 18-74 over the telephone about their WTP for a 2-in-10,000 reduction in their risk of dying over the next year.

The payment question reads as follows: “It is estimated that X(Y) men (women) out of 10,000 in the same age as you will die during the next year. Assume that you could participate in a preventive and painless treatment which would reduce the risk that you will die during the next year, but has no effects beyond that year. The treatment reduces the risk of your dying during the next year from X(Y) to X-2 (Y-2) out of 10,000. Would you at present choose to buy this treatment if it costs SEK I?”

Respondents were given two response categories, “yes” and “no.” The bid values ranged between 300 and 10,000 SEK (about $40 to $1400, implying VSL values of $200,000 to $7 million). Johannesson et al. estimate mean WTP to be 6300 SEK, or about $900.

When dichotomous choice questions are used, it is important that (i) the percentage of “yes” responses decline with the bid amount, and that (ii) the bids cover a reasonably wide portion of the range of WTP values. As shown in Figure 5.1, the percentage of “yes” responses declines from 51.36% at the lowest bid amount, 300 SEK, to 28.83% at the higher bid amount 10,000 SEK, satisfying the first of these two requirements. The figure also implies that the bids barely hit median WTP, and cover mostly the upper tail of the WTP range, failing to satisfy requirement (ii), and raising concerns about the stability of the WTP estimates of WTP. Median WTP should be between 300 and 500 SEK.

Figure 5.1.



B. Changing the Distribution of WTP

We use the data from Johannesson et al. to fit binary-response models of WTP based on four alternative WTP distributions. These distributions are the normal, logistic, lognormal and Weibull, which are commonly used to represent latent WTP in empirical analyses of dichotomous choice CV data. Mean and median WTP are derived directly from the estimated parameters of the binary-response models, using the properties of the distributions being used.[15] Results are reported in table 5.1.

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[15] For example, if it is assumed that WTP follows the normal (logistic) distribution, one fits a probit (logit) model, regressing a dummy for the "yes" or "no" responses to the payment question on the intercept and the bid. As shown by Cameron and James (1987), mean/median WTP is equal to $-\alpha/\beta$, where α is the intercept and β is the coefficient on the bid in the probit (logit) model. We use the delta method (see Cameron, 1991) to obtain the standard error around mean WTP. Using a symmetric distribution implies that mean WTP is equal to median WTP, a requirement that is relaxed when the Weibull and log normal distributions are used. In the case of the Weibull distribution, the probability that a respondent says "yes" to a dichotomous choice payment question is Prob(yes|B_i)= $\exp\left\{-\left(\frac{B_i}{\sigma}\right)^{\theta}\right\}$, and the probability that the respondent answers "no" is one minus this quantity. Mean WTP is equal to $\sigma\cdot\Gamma(1/\theta+1)$, and median WTP is $\sigma\cdot(-\ln(0.5))^{1/\theta}$.

The most surprising result of table 5.1 is that the estimates of mean and median WTP are *negative* when the normal and logistic distributions are used. In fact, the model based on the normal distribution predicts that 54% of the respondents will have negative WTP values. Using the Weibull and lognormal distributions, which admit only non-negative values of WTP and fit the data better, circumvents this problem, but results in a large discrepancy between median and mean WTP, and in very large estimates of mean WTP.[16]

Using the Weibull distribution, for example, mean WTP is two orders of magnitude greater than median WTP. The median WTP amount predicted by the two distributions is similar (239 and 250 SEK for Weibull and lognormal, respectively), but less than what would be inferred by examining the responses to the payment questions, and less than the smallest bid value offered to the respondents in the study, which raises doubt about the fit of the estimated survival curve.

Based on the log likelihood function, the binary response model based on the lognormal distribution has the best fit; the probit and logit models fare much worse. As mentioned, however, choosing to work with the lognormal distribution for WTP, results in implausibly large estimates of mean WTP, a result that does not change when one turns to the Weibull distribution.[17]

When latent WTP is a lognormal, Prob(yes|B_i)= $\Phi(\mu/\sigma - \ln B_i/\sigma)$, where μ is the mean of the logarithmic transformation of WTP, and σ is its the standard deviation. Mean WTP is $\exp(0.5\sigma^2 + \mu)$, and median WTP is equal to $\exp(\mu)$.

[16] This is probably due to two concurrent factors. The first is that the estimate of mean WTP depends crucially on the upper tail of the distribution of WTP, corresponding to high bid values. The second is that in this study the bid placement is unbalanced, with virtually all bid amounts on the right of the median. In a Monte-Carlo simulation exercise, I found that when the distribution of the underlying WTP variable is skewed, using a design that covers only a limited portion of the range of WTP (e.g., all the bid values are to the left of the median, or to its right) can bring biases on the estimates of mean/median WTP and is potentially grossly inefficient. The problem appears to be particularly severe when the underlying distribution of WTP has a relatively large variance.

[17] An alternative measure of fit frequently used with binary data model is the percentage of correctly predicted observations. This percentage is equal to 61.3% for probit, logit, and the binary model corresponding to the lognormal distribution for latent WTP, and about 60.6% for the Weibull model. These percentages are disappointing low, when compared with researchers' expectations for binary data regressions, although probably not very different from binary regressions based on CV data on environmental quality and amenities.

Table 5.1. Mean and Median WTP for various distributional assumptions (Johannesson et al. study, 1997)

	Normal (probit model)	Logistic (logit model)	Weibull	Lognormal
Mean WTP	-2096.08	-2007.75	2,894,292	Infinity
Median WTP	-2096.08	-2007.75	238.39	254.30
Log L	-1349.19	-1349.10	-1344.01	-1343.84

Clearly, these WTP figures are very different from those reported by Johannesson et al. (1997), because the latter rely on a completely different procedure for estimating mean WTP. They start with fitting a logit model of the responses, which assumes that WTP follows the logistic distribution and implicitly admits negative WTP values, but mean WTP is computed as the area under the survival curve over the *positive* WTP values, i.e., from 0 to infinity. When WTP is a logistic variate, this area is equal to $\left(-1/\beta\right)\ln\left[1+\exp(\alpha)\right]$, where α is the intercept and β is the slope of the logit model. Formally,

$$(5.1) \qquad \int_0^{+\infty} \Pr(WTP > B)dB = \int_0^{+\infty} \frac{\exp\{(\alpha+\beta B)\}}{1+\exp\{(\alpha+\beta B)\}}dB = \frac{1}{\beta}\int_0^{+\infty}\frac{(\beta)\exp\{(\alpha+\beta B)\}}{1+\exp\{(\alpha+\beta B)\}}dB$$

$$= \frac{1}{\beta}\left[\ln(1+\exp\{(\alpha+\beta B)\}\right]_0^{+\infty} = \left(-\frac{1}{\beta}\right)\cdot\ln(1+\exp(\alpha)),$$

where β is negative. Expression (5.1) is analogous to mean WTP from a tobit model, except that Johannesson et al. did not estimate a tobit model, and their sample does not contain zeros.

Regarding the fact that estimated median WTP is less than the smallest bid assigned to the respondents in the survey, it is useful to compare the predicted probabilities of a "yes" response for various bid levels under different distribution assumptions, as we do in table 5.2.

Table 5.2. Probabilities of "yes" to the bid amounts (Johannesson et al. study, 1997).

Bid amount (SEK)	Relative frequency of "yes" responses in the sample	Probability of "yes" predicted by normal model	Probability of "yes" predicted by Weibull model	Probability of "yes" predicted by lognormal model
300	51.36	45.23	48.78	49.00
500	44.63	44.84	45.88	45.87
1000	37.76	43.85	41.88	41.68
2000	36.70	41.89	37.81	37.58
5000	34.15	36.14	32.43	32.37
10000	28.36	27.27	28.42	28.64

Table 5.2 shows that all of the three parametric models used (probit, Weibull and lognormal) fit the data poorly at the lowest bid values. They all predict median WTP to be less than 300 SEK, although inspection of the empirical relative frequencies of the "yes" responses suggests than median WTP should be between 300 and 500 SEK.

C. Changing the procedure for estimating mean WTP

In table 5.3, we experiment with alternative approaches for calculating mean WTP, focusing on four procedures. The first procedure follows Cameron and James (1987), fitting a probit or logit model and computing mean WTP as

(5.2) $$m_1 = -\alpha / \beta.$$

The second is the procedure used by Johannesson et al., who fit a logit model but effectively disregard the portion of the distribution corresponding to negative values. As explained, with the logistic distribution this results in the following expression:

(5.3) $$m_2 = (-1/\beta)\ln[1+\exp(\alpha)].$$

Our third procedure relies on the fact that mean WTP is the area under the survival curve, i.e., $[1-F(\alpha+\beta y)]$. In earlier applications of the CV method, researchers estimated mean WTP by computing the area under the survival curve until the largest bid amount offered in the survey (e.g., 10,000 SEK in the Johannesson et al. study). Our third estimate of mean WTP is thus:

(5.4) $$m_3 = \int_0^{B_{\max}} [1-F(\alpha+\beta y)]dy.$$

Finally, Chen and Randall (1997) and Creel and Loomis (1998) describe semiparametric approaches for estimating mean WTP. Specifically, they propose to estimate m_3 as in equation (5.4), but improve the fit of F(•) through adding terms such as the sine and cosine transformations of the bid and of other regressors in its argument, in the spirit of fast Fourier transforms approximations.[18] The argument of F(•), therefore, becomes:

$$z = \mathbf{x}\beta + \sum_{\alpha=1}^{A}\sum_{j=1}^{J}\left[u_{j\alpha}\cos(j\mathbf{k}_{\alpha}s(\mathbf{x})) - v_{j\alpha}\sin(j\mathbf{k}_{\alpha}s(\mathbf{x}))\right], \tag{5.5}$$

where **x** is a vector that includes the bid and determinants of WTP. For a subset, or all, of these variables (the dimension of this subset being A), we introduce a scaling function s(**x**). This scaling function subtracts the minimum value of **x**, divides the result by the maximum value of **x** (thus forcing the rescaled variables to be between zero and 1), and then multiplies by (2π-0.00001). For this rescaling function to be possible, there must be at least three distinct values for **x**, which rules out applying this transformation to dummy variables. The quantities **k** and *u* are vectors of indices and parameters to be estimated, respectively. Chen and Randall (1997) and Creel and Loomis (1998) suggest that for most dichotomous choice CV survey applications it is sufficient to consider J=1, which simplifies *z* to:

$$z = \mathbf{x}\beta + \sum_{\alpha=1}^{A}\left[u_{\alpha}\cos(s(\mathbf{x})) - v_{\alpha}\sin(s(\mathbf{x}))\right]. \tag{5.6}$$

We apply the semiparametric approach defined by equation (5.6) to the Johannesson et al. data, and compute mean WTP, m_4, by integrating the survival function under the curve up to the largest bid used in the study. We choose F(•) to be the standard logistic and standard normal cdf, respectively, in two alternate runs, and specify *z* to include the intercept, the bid, and its sine and cosine transformations (after rescaling):

$$\int_0^{B_{\max}}\left[1 - F(\alpha + \beta y + \delta\sin(y') + \gamma\cos(y'))\right]dy\,, \tag{5.7}$$

where $y' = (2\pi - 0.00001)\cdot\left[\dfrac{y - B_{\min}}{B_{\max}}\right]$, where $B_{\min}$ and $B_{\max}$ are the smallest and largest bid values used in the survey.

The results from these alternative calculations of mean WTP are shown in table 5.3. The table shows that the largest jump in estimated mean WTP occurs when going from m_1—which yields a negative mean WTP—to approaches m_2-m_4, which restrict integration to the positive semiaxis (or a portion of it). Within the latter, however, the estimates of mean WTP are within about 10% of one another, regardless of using exact expressions or numerical integration, and normal or logistic F(•).

[18] Chen and Randall also consider polynomial terms in the variables **x**.

Table 5.3. Different procedures for computing mean WTP. Johannesson et al. (1997) data.

Approach	Distribution F( )	Mean WTP (in SEK)
Cameron and James (1987): m_1	Logistic	-2007
Johannesson et al., closed-form expression: m_2	Logistic	6849
Numerical integration of the survival function up to max bid: m_3	Logistic	6485
Numerical integration of the survival function up to max. bid: m_3	Normal	6319
Creel and Loomis (1998) semiparametric approach: m_4	Logistic; logit model with bid, sin(bid) and cos(bid)	6472
Creel and Loomis (1998) semiparametric approach: m_4	Normal; probit model with bid, sin(bid) and cos(bid)	6254

C. Focus on the regressors

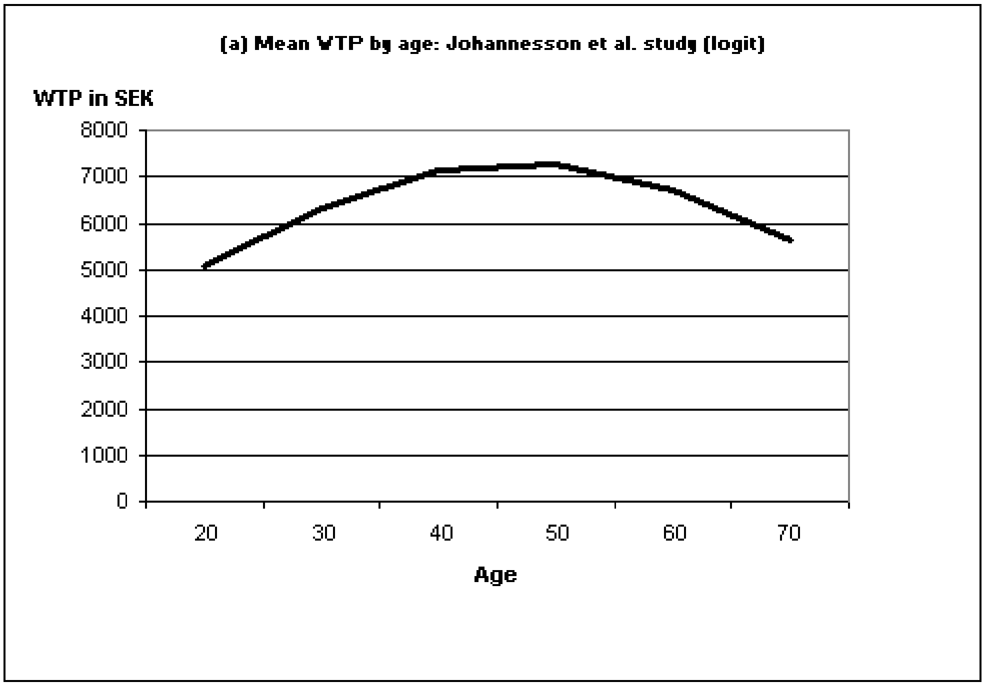
Would similar results be obtained in the situation where regressors are included in the model, and mean WTP is calculated for specific values of the regressors? This question is appropriate, for example, when seeking to answer the question of how WTP for a risk reduction varies with age. In policy analyses, some observers have suggested that older people should be willing to pay less for a risk reduction, and hence their VSL should be lower, reflecting their fewer remaining life years. Economic theory, however, does not offer unambiguous predictions about the effect of age on WTP (Alberini et al., forthcoming).[19]

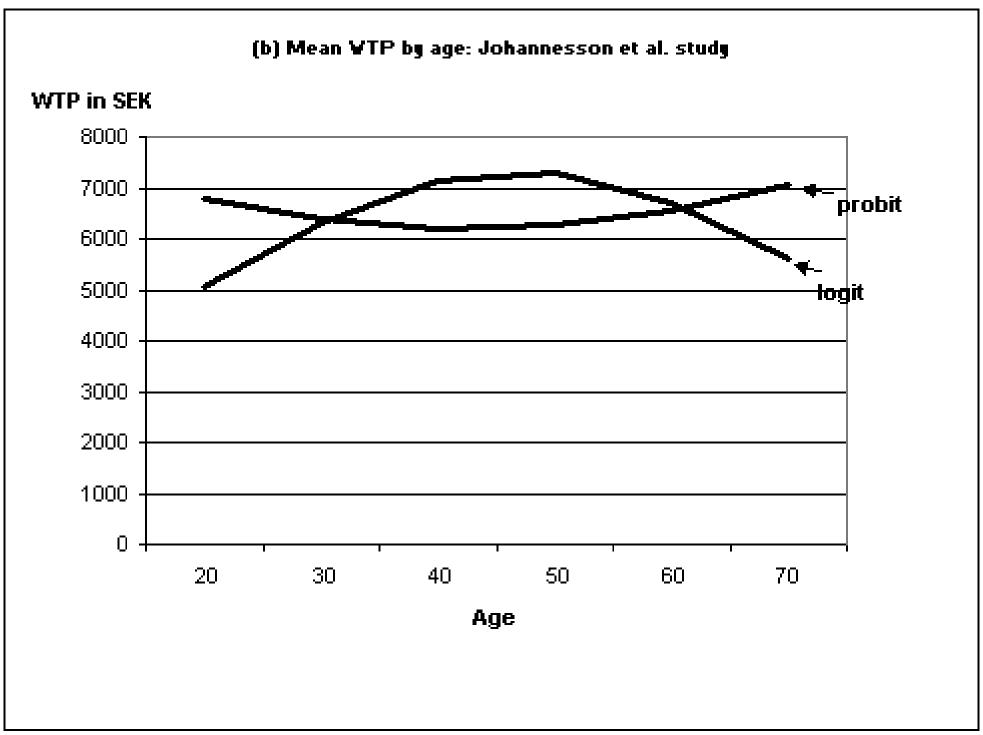
Johannesson et al. run a logit regression that includes age and age squared, plus gender and education dummies, income and the respondent's quality-of-life rating, and report finding a quadratic relationship between age and WTP that peaks when the individual is about 40-50 years old. To check the sensitivity of these results to the procedure used in the calculation, we ran logit and probit models with the same regressors, and predicted mean WTP at different ages using approaches m_2, m_3, and m_4 above.

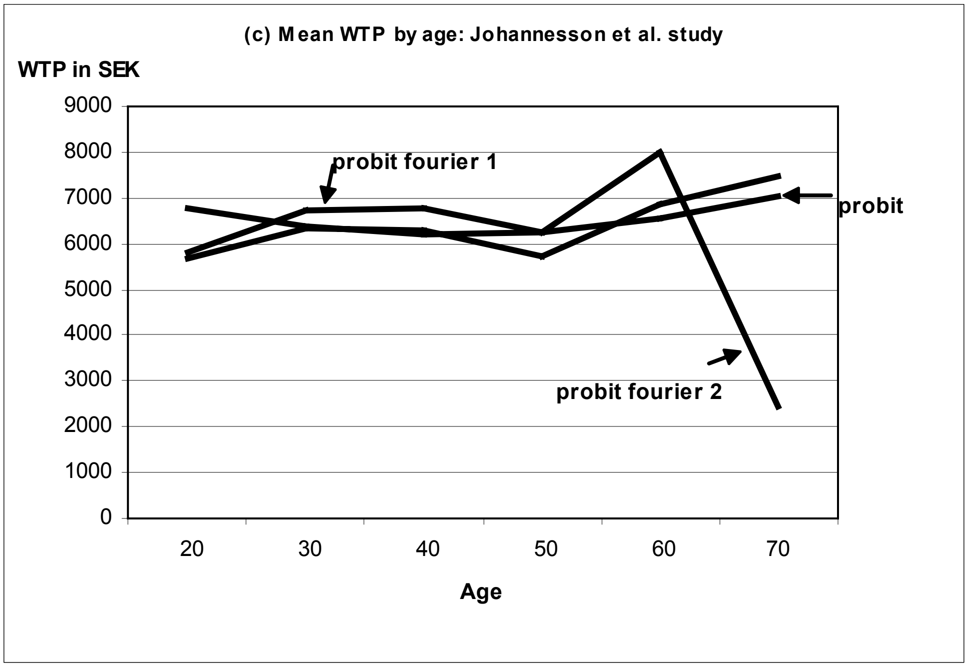
[19] Because a large proportion of the lives saved appear to be those of the elderly, there has been much recent debate whether the VSL should be lower for the elderly to reflect their fewer remaining life years. In the US, the Office of Management and Budget recently repudiated making such adjustment for age, on the grounds of insufficient evidence that VSL is lower for elderly persons (Skrzycki, 2003; Office and Management and Budget, 2003).

The results of these calculations are shown in Figure 2, panels (a)-(c). Panel (a) shows that using m_2 with the logit distribution—the same formula used by Johannesson et al.—confirms their findings: the relationship between age and WTP is an inverted U that peaks at age 50. However, when the logit model is replaced by a probit model, the relationship changes entirely, becoming a U-shaped curve.

Figure 5.2.


(a) Mean WTP by age: Johannesson et al. study (logit)
WTP in SEK
8000
7000
6000
5000
4000
3000
2000
1000
0
20
30
40
50
60
70
Age


(b) Mean WTP by age: Johannesson et al. study
WTP in SEK
8000
7000
6000
5000
4000
3000
2000
1000
0
20
30
40
50
60
70
Age
probit
logit



Legend: Logit or probit = logit or probit that integrate (1-cdf) from zero to infinity;
Fourier 1 = bid, age and age squared + fourier terms for bid, age and age squared
Fourier 2 = bid, age and age squared, income and quality of life, sex and education dummy, plus fourier terms for bid, age and age squared. In Fourier 1 and 2 integration is to the largest bid amount.

When applying the semiparametric approach, m_4, we experiment with two specifications. The first fits a probit model with the intercept, the bid, age and age squared, and their sine and cosine transformations (after rescaling). The second further includes gender and education dummies, income and the quality-of-life rating, and the sine and cosine transformations of all continuous variables (after rescaling). The predictions shown in Figure 1, panel (c) refer to a male (SEX=1) with high school education (DEDU=1). Panel (c) suggests that there is no easily discernible pattern, and that claims about the quadratic relationship are not robust, and may simply be an artifact of restrictive assumptions.

We further investigate this matter by considering a simpler model where the only covariates are age and age squared, switching to a lognormal distribution for WTP, and predicting median WTP for specific ages, which should result in more conservative estimates.[20] The results are shown in table 5.4. Two major points emerge from this table.

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[20] Specifically, I assume that log WTP= $\mathbf{x}_i\beta + \varepsilon_i$, with ε a normally distributed error term, and x a vector of covariates. This is an accelerated life model. The mapping between the latent WTP and the observed response to the payment question is, as usual, that the respondent answers “yes” if his or her unobserved WTP amount (log WTP) is greater

First, using median WTP, which is a robust lower-bound estimate of welfare change, results, as expected, in much lower WTP and VSL estimates. Second, the lognormal model results in a quadratic, inverted-U relationship between age and WTP with the highest WTP at age 40. However, the lognormal model and the use of median WTP suggest that the curvature of the relationship is *much sharper* than that predicted by the Johannesson et al. approach.[21] For example, the WTP of a 70-year-old for a reduction in risk of 2 in 10,000 is only 90 SEK, or about 20% of the WTP predicted for a 40-year-old person (440 SEK). By contrast, in Johannesson et al. the WTP of a 70-year-old is only about two-thirds that of a 40-year old.

Table 5.4. The relationship between age and WTP for a risk reduction: lognormal WTP and median WTP v. Johannesson et al. logit and truncated mean WTP.

Age	Johannesson et al, 1997.		Alternative calculation using log normal WTP.	
	Mean WTP in SEK	Implied VSL in million SEK	Median WTP in SEK	Implied VSL in million SEK
20	6100	30.3	137.18	0.672
30	6900	34.6	307.31	1.505
40	7200	36.1	440.77	2.160
50	6900	34.3	404.75	1.983
60	6000	29.8	237.97	1.166
70	4600	23.3	89.57	0.439

While inference about the general *shape* of the relationship between age and WTP is confirmed by the alternative calculations, this exercise also illustrates that the extent to which WTP changes with age depends crucially on the assumptions underlying the model.

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than the bid (log bid), and "no" otherwise. The corresponding binary data model is a probit where log bid enters as an additional regressor. Formally, $\Pr(I_i = 1) = \Phi(x_i\beta/\sigma - \log B_i/\sigma) = \Phi(x_i\alpha + \gamma \log B_i)$, where I is an indicator that takes on a value of one if the response is "yes." The estimated α coefficients from probit model (3) are 0.3005 (intercept), 0.0293 (age), -0.00034 (age squared), while γ is pegged at -0.1525.

[21] The logit regression controls for age, and finds evidence of an inverted-U shaped relationship between age and WTP. The mean WTP values for persons of various ages are computed as $(-1/\beta)\ln[1+\exp(\alpha_j)]$, where j denotes age in years, $\alpha_j = \alpha_0 + \alpha_1 \cdot j + \alpha_2 \cdot j^2$, and the αs are the coefficients from the logit regression.

Conclusions

- This section examine the effects of distributional assumptions, choice of WTP welfare statistic, and procedure used for calculating such a statistic, when the VSL is estimated from dichotomous choice WTP responses. Using the data from the Johannesson et al. (1997) study as an example, I show that changing the distributional assumption can result in dramatic changes in VSL.

- While treating WTP as a normal or logistic variate may result in negative estimated mean WTP figures, distributions of WTP defined over the positive semiaxis can, in some cases, produce estimates of mean WTP that are, depending on the data and the distribution used, orders of magnitude larger than median WTP. These problems are often seen in dichotomous choice contingent valuation surveys, and are not specific to the mortality risk context.

- It is useful to compare alternative calculations of mean WTP. For example, this section examined (i) the logistic closed-form expression for the area under the survival function, (ii) numerical integration, (iii) numerical integration to the largest bid, (iv) semiparametric estimation of the survival function plus numerical integration to the largest bid, finding that, with the Johannesson et al. data, they give similar results when covariates are not included. By contrast, the functional form of the relationship between a key covariate—the age of the respondent—is extremely sensitive to the procedure used, to the point that not much can be said about the relationship between age and WTP, which Johannesson et al. previously claimed to be quadratic.

- Median WTP is a robust welfare statistic.

In the next section, I examine whether detection of potential outliers mitigates the problems mentioned above.

Appendix.

Johannesson and Johannesson (1996) observe "yes" or "no" responses to a dichotomous choice question about a life expectancy extension of one year at the age of 75, and fit a logit model to these responses. The model assumes that WTP follows the logistic distribution, and posits that:

(A.1) $\Pr(Yes \mid B_i) = \Pr(WTP_i > B_i) = \exp(\alpha + \beta \cdot B_i) / [1 + \exp(\alpha + \beta \cdot B_i)]$

where $\alpha = \mu / \sigma$, $\beta = -1/\sigma$, μ and σ are the location and scale parameter of WTP, and B denotes the bid amount assigned to the respondent. Here, μ is both mean and median WTP.

Johannesson and Johansson estimate mean WTP using the formula $(-1/\beta)\cdot\ln[1+\exp(\alpha)]$, obtaining a figure of 8787 SEK.

Table A.1 presents the results of re-estimating binary data models of the responses to the payment questions under various distributional assumptions for latent WTP.

Table A.1. Mean and median WTP for different distributional assumptions about WTP. Johannesson and Johansson study (1996).

	Distribution			
	Normal (range: -∞ to +∞)	Logistic (range: -∞ to +∞)	Weibull (range: 0 to +∞)	Log normal (range: 0 to +∞)
Mean WTP (standard error)	-14222.35 (2362.35)	-11360.09 (2057.25)	50574.41 (27658.11)	2,092,389 (2,699,730)
Median WTP (standard error)	Same as mean WTP	Same as mean WTP	119.41 (32.93)	138.67 (29.69)
Log L	-1121.70	-1118.86	-1079.87	-1079.41

Table A.1 shows that symmetric distributions like the normal and logistic result in *negative* estimates of mean and median WTP, μ. If WTP is assumed to follow the Weibull or log normal distribution, both of which restrict WTP to be positive, mean and median WTP are, of course, positive. As is usually the case with these distributions, mean WTP is much greater than median WTP. Median WTP—a conservative but robust welfare statistic—is *almost two orders of magnitude* smaller than the estimate of mean WTP in Johannesson and Johansson (1996). By contrast, the estimates of mean WTP for the Weibull and log normal models are much larger than the figure obtained by Johannesson and Johansson, 8787 SEK.

Table A.1 also suggests that the log normal distribution provides the best fit, with the Weibull a close second. Mean WTP is, however, extremely large in the Lognormal model. Even for the Weibull model it is very large, showing that the estimates of mean WTP depend crucially on the distributional assumption made by the researcher. The lognormal and Weibull model produce similar estimates of median WTP (SEK 139 and 119, respectively).

To get a sense of the goodness of fit afforded by the various models, in table A.2 the relative frequencies of the "yes" responses are compared with the probabilities predicted by the various binary data models.

Table A.2. Probabilities of "yes" to the bid amounts (Johannesson and Johansson study, 1996).

Bid amount (SEK)	Percentage of "yes" responses	Probability of "yes" predicted by normal model	Probability of "yes" predicted by Weibull model	Probability of "yes" predicted by lognormal model
100	53.22	38.73	52.94	51.18
500	38.58	38.42	38.54	39.09
1000	31.38	38.04	32.69	33.74
5000	22.63	35.03	20.80	21.80
15000	13.64	27.95	14.40	14.68
50000	9.09	9.95	9.08	8.45

The probit model (corresponding to the assumption that WTP is normally distributed) estimates median WTP to be less than 100SEK, although the sample frequencies of "yes" responses imply that media WTP should be between 100 and 500 SEK. Moreover, it predicts virtually identical probabilities of a "yes" response for bid amounts between 100 and 1000SEK. The Weibull and lognormal models give predictions that are closer to the actual frequencies, and clearly outperform the probit and logit models.

6. Outliers

In this section, we investigate the effect of outliers on the estimates of WTP. Collett (1991) defines as outliers "observations that are surprisingly far away from the remaining observations in the sample," and points out that such values may occur a result of measurement errors, execution error (i.e., use of a faulty experimental procedure), or be a legitimate, if extreme, manifestation of natural variability.

In this section, we first examine criteria for considering a WTP response an outlier. Next, we examine WTP responses that could be considered outliers because of their WTP/income ratio. These observations are potentially suspect, and removing them could improve the fit of the binary data model, and/or result in a thinner upper tail of the observed distribution of WTP. We use the Johannesson et al., Alberini et al., and Persson et al. data to illustrate issues arising when identifying and excluding outliers. Additional examinations of the outliers problem are conducted in chapter 9, where we examine the effect of including and excluding observations with disproportionately large self-assessed baseline risks, and in section 7, where we discuss discrete mixtures containing nay-sayers, yea-sayers, and completely random responses to the payment question.

A. Outliers and Binary Data

How are outliers defined when the variable of interest is binary, as with the responses to dichotomous choice contingent valuation questions? Copas (1988) defines as an outlier an observation for which we predict a low (high) probability of a one (zero), but we do observe a one (zero).

What "high" and "low" means, of course, remains to be defined, and we use the Johannesson et al. (1997) data to check (i) how many observations could be classified as outliers according to several alternative cutoff levels, and (ii) by how much mean WTP would change if these outliers were excluded from the sample. Specifically, we wish to see how for how many observations the predicted probability of a "yes" was less than 0.05, 0.10, etc., but the response to the payment question was a "yes." The predicted probability of a "yes" is based on Johannesson et al.'s logit regression of the "yes" or "no" response indicator on respondent age, age squared, income, an education dummy, and a quality of life rating subjectively reported by the respondent in the interview:

(6.1) $$\hat{p}_i = \exp(x_i\hat{\alpha} + \hat{\beta} \cdot B_i)/[1 + \exp(x_i\hat{\alpha} + \hat{\beta} \cdot B_i)],$$

where $\hat{\alpha}$ and $\hat{\beta}$ are estimated coefficients, $\mathbf{x}$ is a vector of regressors, and B is the bid assigned to respondent i. For ease of comparison, we use the same procedure for estimating mean WTP as in Johannesson et al.'s work.[22] Results are displayed in table 6.1.

Table 6.1. Outliers in the Johannesson et al. data (based on logit regression, n=1660). All WTP figures in SEK.

Definition of outlier	How many?	Mean WTP (Johannesson et al. procedure)	Weibull: Mean WTP (in bold) and median WTP	Lognormal: Mean WTP (in bold) and median WTP
No outliers identified	None	6732 SEK	**2.894 million** 238	∞ 254
Prob(yes)≤ 0.05 and yes observed	None	6732 SEK	**2.894 million** 238	∞ 254
Prob(yes) ≤0.10 and yes observed	None	6732 SEK	**2.894 million** 238	∞ 254
Prob(yes) ≤0.20 and yes observed	5	6141 SEK	**1.150 million** 302	∞ 314
Prob(yes) ≤0.25 and observed	26	4846 SEK	**193,481** 338	**155 million** 345
Prob(yes) ≤0.30 and yes observed	59	3767 SEK	**36,114** 369	**1.821 million** 372

Table 6.1 shows that outliers according to the Copas' definition were found only when the cutoff for identifying an outlier was set to 0.20 or higher. When the cutoff is set to 0.25, for example, a total of 26 people would be considered outliers, and dropping them from the usable sample would reduce mean WTP to 4846 SEK—roughly a 40% reduction. An even larger decline in mean WTP would be observed if we dropped those respondents whose predicted likelihood of a "yes" response is less than or equal to 0.30, but were still observed to say "yes."

Excluding outliers from the usable sample—even just a few at a time—also has a large effect on the welfare statistics resulting from the Weibull and lognormal models. Although the mean WTP figures remain implausibly large, they decrease by orders of magnitude when the outliers are excluded. By contrast, median WTP rises, implying that the upper tail of the distribution is thinner after the outliers have been removed.[23] This

[22] We remind the reader that Johannesson et al. fit the logit model corresponding to equation (6.1), then compute mean WTP by integrating the area under $\hat{p}_i$ between zero and infinity. This integral has a closed-form expression, which is equal to $(-1/\hat{\beta})*\ln[1+\exp(\hat{\alpha})]$.

[23] We also checked what would happen if we changed the distributional assumption about WTP. If we replace the bid amount with log bid, which follows from the assumption that WTP is a loglogistic, there would 3 outliers when the cutoff is 0.20, 29 when it is 0.25,

suggests that the exclusion of outliers defined in this way has an effect similar to the removal of "yea-sayers" (see section 7).

One would expect that when an outlier is defined as an observation with a high predicted probability of a "no," but an actual "no" observed, would produce the opposite effects of WTP. However, we could not identify many outliers defined in this fashion in the Johannesson et al. data. Only when the cutoff was 0.40, in fact, were we able to find 13 potentially suspect observations.

B. Outliers with Respect to Income

Income is an important independent variable to include included in regressions relating WTP to individual characteristics of the respondent. There are several reasons why researchers regress WTP on household (or personal) income. First, this is a common practice for testing the internal validity of the WTP responses, as theory suggests that WTP for mortality risk reductions should be positively associated with income. Second, there is much interest in the income elasticity of WTP for the purpose of predicting WTP at specified levels of income within the sample, or for benefit transfer purposes.[24]

Measuring income from surveys of individuals is, however, problematic. If, as it is sometimes suggested, income is a variable observed with a random observation error, and a regression is run that relates WTP on income (plus other variables), one would expect the coefficient on income to be biased towards zero. The income elasticity of WTP would, therefore, be underestimated.[25]

Measuring income is particularly difficult for certain persons, such as those with income that tends to fluctuate between one year and the next (the self-employed, or workers in highly seasonal industries, like construction) and for the elderly living on retirement and investment incomes, who sometimes fail to include social security among their sources of income. It is not unusual for retired person to report very low incomes, even if their wealth is very large. For this reason, consumer expenditure surveys like the University of Michigan's Survey of Consumer Finances inquire about social security payments, ownership of land and homes, and money in retirement and savings accounts to get a better sense of the wealth of individuals. Answering the income question is also difficult for college students.

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and 29 when the cutoff is 0.30. The effect on mean WTP is qualitatively similar to that shown in table 6.1.

[24] It is recognized, however, that knowing the income elasticity of WTP in a cross-sectional sample sense does not answer the important policy question of whether VSL should change over time, as income grows and the tradeoffs people are prepared to make between income and risk reductions change.

[25] I would like to thank Trudy Cameron for raising this issue.

Another frequently encountered problem is that many people fail to answer the income question altogether, which results in many missing values. Researchers with extensive experiences in the design and administration of surveys (Richard T. Carson, personal communication) report that income is often the variable that tends to have the highest item non-response rate in a CV surveys. In the Johannesson et al. study, for example, 86 out of 2028 (4.24%) individuals failed to report any information about their personal or household income. In the Alberini et al. US study, 12.78% percent of wave 1 skipped the income question, whereas 2.9% of the Persson sample failed to answer the income question.[26]

Individuals may also intentionally misrepresent their income. For example, relatively wealthy individuals may deliberately underreport their income, while at the same time announcing a relatively large WTP amount, and other individuals may intentionally overstate their income. These tendencies can sometimes be uncovered by comparing these subjects' education with reported income.

In contingent valuation surveys about environmental quality or other public goods, researchers expect WTP to be a small fraction of the respondent's income. This expectation has led them, in some cases, to exclude from the sample respondents whose implied WTP is greater than, say, 5% of the respondent's income. With reductions in one's own risk of dying, there is no particular reason to believe that WTP *should* be a small proportion of income, but it seems appropriate to check for respondents whose announced WTP is a relatively large proportion of income, and to examine how robust the estimate of mean WTP is to excluding these respondents from the sample. Respondents with very high announced WTP relative to income may have failed to give proper consideration to their budget constraint, may have intentionally misrepresent their income, or may have miscalculated their income.

We use the data from the mortality risk survey conducted in the US by Alberini et al. (forthcoming) to investigate this matter. Results from estimating mean WTP after excluding respondents with implied WTP greater than a given percentage of income are shown in Table 6.3. We vary this percentage from 25 (the least stringent criterion) to 2.5 (the most stringent criterion), showing how doing so excludes from a minimum of 68 to a maximum of 133 respondents (almost one-third of the sample). As in Alberini et al., the WTP estimates in table 6.3 are based on a Weibull interval-data model that combines the responses to the initial WTP questions and to the follow-up questions in a double-bounded fashion. Table 6.3 shows that mean and median WTP do decline as we exclude more observations from the sample, but the change is within 10-12% of the original figures.

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[26] These percentages are actually reasonable when compared with other CV studies on different topics. For example, we recently participated in the design and administration of a CV survey about coastal erosion and the Lagoon of Venice, which was conducted over the telephone. The item non-response rate for household income was about 31%.

Table 6.3
Outliers with respect to income. Alberini et al. US Survey
WTP for 5 in 1000 risk reduction, wave 1, cleaned sample*

	Exclude if…	N	Mean WTP ($)	Median WTP ($)	Income elasticity of WTP
Least stringent	(all sample)	551	752.84 (88.37)	346.21 (28.45)	0.16
	WTP ≥ 25% of household income	483	755.56 (90.84)	362.38 (31.97)	0.16
	WTP ≥ 10% of household income	477	747.53 (90.02)	355.14 (29.24)	0.29
	WTP ≥ 5% of household income	458	719.25 (89.21)	339.33 (30.02)	0.52
Most stringent	WTP ≥ 2.5% of household income	418	678.39 (91.64)	302.26 (28.67)	0.92

* Excludes those who failed the probability quiz and the probability choice.

By contrast, what *does* change dramatically is the income elasticity of WTP, a key quantity when one wishes to (i) extrapolate study results to the general population, (ii) focus on the economically disadvantaged, and (iii) attempt benefit transfers to other countries or locales where income levels are different. As shown in table 6.3, income elasticity of WTP is 0.16 when the full sample is used, 0.29 when persons whose implied WTP amount is greater than 10% of household income are excluded, 0.52 when we exclude persons whose WTP is greater than 5% of household income, and, finally, 0.92 when the most stringent criterion is used.

This suggests that predictions for how WTP changes as income changes would vary dramatically, depending on which of these "cleaned" sample, and the corresponding estimates, one opts for.

Further investigation reveals that the 65 respondents who violated the most stringent exclusion criteria were slightly older than the remainder of the sample, but not significantly so (average ages were 57 and 54, t statistic of the null of no difference = 1.38), significantly less educated than the remainder of the sample (11.75 years of schooling v. 13.3, t statistic = 6.27), and reported much lower annual household income than the rest of the sample (sample averages: $17,942 v. 56,151, t statistic 22.18).[27] [28]

[27] The median annual household income is $17,500 and $55,000, respectively.

[28] It is possible that these respondents miscalculated or intentionally underreported their income. I regressed log income on age, age squared, education and the gender dummy for the full sample, and used the results of this regression to compute predicted income. For

Moreover, they were twice as likely to indicate, in the debriefing section of the survey, that they had misunderstood the timing of the payment (27% of this group versus 13% of the remainder of the sample, with a t statistic of 4.66).

Next, we turn to the Persson et al. data. One respondent in Persson et al.'s sample reports a WTP amount that is 83% of annual household income. Fortunately, the rest of the sample is more reasonable: Ninety-nine percent of the sample holds a WTP amount for reducing fatal auto accident that is equal to or less than 12.5% of household income. In their analysis, Persson et al. discard from the usable sample observations such that WTP accounts for more than 5% of annual household income. This loses 29 observations.

Table 6.4 displays mean WTP for the full sample, and when persons with relatively high WTP/income ratios are excluded from the usable sample. This table shows that while median WTP remains the same for the various exclusion criteria, mean WTP jumps from 1875 to 2778 SEK when we reinstate into the sample those respondents whose WTP was more than 5% of household income. This is a 50% increase in WTP.

Table 6.4. Persson et al. study. Effect of excluding observations with large annual WTP/household income ratios. All values in 1998 SEK.

Exclusion criterion	Number of observations in the sample	Sample average WTP	Sample Median WTP
None	637	2778	1000
Respondents with zero income but positive WTP	637	2778	1000
WTP greater than 50% of household income (least stringent)	636	2635	1000
WTP greater than 25% of household income	633	2163	1000
WTP greater than 20% of household income	632	2151	1000
WTP greater than 12.5% of household income	631	2143	1000
WTP greater than 10% of household income	629	2134	1000
WTP greater than 5% of household income (most stringent)	618	1875	1000

Table 6.5 displays the income elasticity of WTP when observations where WTP accounts for a relatively large share of household income are omitted from the sample. As explained in detail in section 9, we estimate a system of simultaneous equation for log

the 65 respondents with high WTP/income ratio, income predicted on the grounds of education, gender and age was always larger than reported income.

baseline risk and log WTP. The right-hand side of the WTP equation include the logarithmic transformation of the absolute risk reduction, log miles traveled in a car in a year, a dummy accounting for previous injuries sustained in a car accident, log age, log age squared, two education dummies, and dummies for the size of the household in various age groups. Table 6.5 shows that income elasticity of WTP doubles when we move from the sample created with the least restrictive criterion to the most stringent criterion. It remains, however, relatively low (0.28).

Table 6.5. Persson et al. study. Effect of excluding observations with large WTP/household income ratio on the income elasticity of WTP. 2SLS estimation, dependent variable: log WTP for risk of dying in a road traffic accident.

Exclusion criterion with respect to income	Number of observations	Income elasticity of WTP	Standard error
None	514	0.1475	0.1136
Respondents with zero personal income but positive WTP	514	0.1475	0.1136
WTP greater than 5% of household income	501	0.2850	0.1109
WTP greater than 10% of household income	509	0.2264	0.1139
WTP greater than 12.5% of household income	510	0.1937	0.1129
WTP greater than 20% of household income	511	0.1668	0.1126
WTP greater than 25% of household income	512	0.1418	0.1119
WTP greater than 50% of household income	514	0.1475	0.1136

Observations with missing baseline risk and missing WTP, observations with baseline risk smaller than 1 in 100,000, observations with WTP less than 1. Other regressors in the WTP equation: log degrisk, log riskmd, log miles traveled in a car, previously injured in a traffic accident (dummy), log age, log age squared, two education dummies, dummies for household members. Coefficients of log degrisk and log riskmd are restricted to be equal.

Conclusions

- Outliers alter WTP and VSL to an extent that depends on the data and on the definition of outlier used. Researchers should inspect their data for outliers and report estimates for the full sample as well as after outliers are excluded.
- There is no unambiguous criterion for considering one’s WTP as “large” relative to this person’s income. However, researchers should check how the estimates of WTP and other coefficients of interest are affected by including and excluding from the usable sample those respondents whose announced WTP is high relative to income.

- These observations could be the result of inaccurate calculation of income on the part of the respondent, or inattention to other details of the risk reduction scenario, as shown by the example based on the Alberini et al. data.
- To limit the measurement error for income, it might be useful to remind respondents about considering all relevant sources of income when answering to give the income. (Retired persons, for example, could be instructed to include social security and other transfer payments, while students could be instructed on how to regard their own income versus their parents'.)
- It may also be helpful to phrase the income question in a manner consistent with the frequency of wages, paychecks, or work contracts typical of the population being surveyed. For example, Lanoie et al (1995), who were interviewing Canadian workers at their workplace, queried them in terms of income per week. Most US surveys inquire about annual income, and most European surveys inquire about monthly after-tax income.
- Outliers and observations with disproportionate WTP to income ratios may occur because the respondent misunderstands the scenario. This reinforces the concept that it is useful to include debriefing questions at the end of the questionnaire to uncover possible misinterpretation of the scenario on the part of the respondent.

7. UNDESIRABLE RESPONSE EFFECTS: MIXTURES

A. Mixtures with Yea-saying, Nay-saying, and Random Answers

Contingent valuation studies about mortality risk reduction rely crucially on the respondent's comprehensions of the risk and risk reductions being valued. This raises the question whether, in spite of visual aids and practice questions about risks, some respondents remain confused about the commodity being valued, and their answers to the payment questions might be affected by undesirable response effects.

Carson (2000) describes three types of undesirable response effects that may occur in dichotomous choice CV surveys. The first is yea-saying, the phenomenon where a respondent answers "yes" to the bid question with probability 1, regardless of the bid amount. This may be done in an effort to please the interviewer, or in hopes to terminate the interview sooner.

By contrast, nay-saying is said to occur when the respondent answers "no" with probability 1, regardless of the bid amount. Respondents engaging in nay-saying may dislike new public programs and new taxes, or might be afraid to commit to something they do not fully understand.

It is also possible that, when queried about risk reductions, some people give completely random responses, answering "yes" to the payment question with probability 0.5 (and hence, "no" with probability 0.5), regardless of the bid amount. Completely random responses may be due to confusion about the scenario, failure to understand the commodity being valued, no interest in the survey, poorly written survey questions or survey materials, or simply a data entry error.

In practice, not all respondents in a contingent valuation survey will be subject to these undesirable effects. In this section, we therefore consider discrete mixtures to accommodate for this possibility. For simplicity, attention is restricted to discrete mixtures with two components, where a small fraction of the sample (α) is affected by one of these undesirable response effects, while the remainder of the sample answers the payment questions in the usual fashion (i.e., saying "yes" if latent WTP is greater than the bid, and "no" otherwise).[29] The econometrician's problem is that—unless respondent

[29] The dichotomous choice approach is currently the most widely used format for eliciting information about the respondent's willingness to pay for an improvement in environmental quality or reduction in the risk of death in contingent valuation surveys. In the single-bounded variant of the dichotomous choice approach, respondents were asked one payment question. In recent years, however, researchers have increasingly resorted to the so-called double-bounded approach in hopes of refining information about the respondent's WTP amount. The standard statistical model of the responses to dichotomous choice questions assumes that the respondent's WTP amount is a draw from a specified distribution of WTP (e.g., normal, logistic, Weibull), and that the respondent answers "yes" ("in favor of the plan") if WTP amount is greater than the bid, and "no"

or interviewer debriefs are used—it is not possible to tell which component of the mixture the respondent is drawn from.

In this chapter, we describe statistical models of mixtures of responses to dichotomous choice payment questions in contingent valuation (CV) surveys and apply these models to the data from four CV surveys about individual willingness to pay for reductions in mortality risks.

B. Likelihood Functions

In this section, we describe the contribution to the likelihood function in the presence of discrete mixture and single-bounded WTP data. We assume throughout this section that for observations from the non-degenerate component, latent WTP follows the Weibull distribution. This distribution may, of course, be replaced by any other suitable distribution.

When there is a discrete mixture with random responses, only (1-α)100% responses out of the n observations available are draws from a Weibull distribution of WTP, while the remaining $\alpha \cdot 100$% is comprised of persons whose response reflects the outcome of a coin flip. Unfortunately, we do not know which respondent is which, as is typical with mixtures of populations when the sample separation is unobserved, so that the probability of observing a "yes" response to the payment question is:

(7.1) $$\mathrm{pr}(\mathrm{yes} \mid B) = (1-\alpha)\cdot\left(1-F(B;\theta,\sigma)\right)+\alpha\cdot 0.5$$

where F( ) is the Weibull cdf with parameters θ and scale σ, and B is the bid amount. The probability of observing a "no" is:

(7.2) $$\mathrm{pr}(\mathrm{no} \mid B) = (1-\alpha)\cdot\left(F(B;\theta,\sigma)\right)+\alpha\cdot 0.5\,.$$

These expressions can be specialized to the Weibull cdf to obtain:

(7.3) $$\mathrm{pr}(\mathrm{yes} \mid B) = (1-\alpha)\cdot\exp\left(-\left(\frac{B}{\sigma}\right)^{\theta}\right)+\alpha\cdot 0.5\text{, and}$$

(7.4) $$\mathrm{Pr}(\mathrm{no} \mid B) = (1-\alpha)\cdot\left(1-\exp\left(-\left(\frac{B}{\sigma}\right)^{\theta}\right)\right)+\alpha\cdot 0.5$$

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otherwise. The associated statistical model is a binary response model (or an interval data model if respondents are asked a follow-up question), and mean and median WTP are computed from the estimated parameters, exploiting the properties of the distribution of WTP (Cameron and James, 1987).

When a sample of well-behaved sample of respondents is "contaminated" with a small percentage, α, of nay-sayers, an observed "no" response could be due to a legitimately low WTP amount, relative to the bid level, or to the fact that the respondent is a nay-sayer. Formally,

(7.5) $$\mathrm{pr}(\mathrm{no} \mid B) = (1-\alpha)\cdot\left(F(B;\theta,\sigma)\right) + \alpha\cdot 1, \text{ and}$$

(7.6) $$\mathrm{pr}(\mathrm{yes} \mid B) = (1-\alpha)\cdot\left(1 - F(B;\theta,\sigma)\right).$$

In the case of the Weibull distribution, these contributions to the likelihood are simplified to:

(7.7) $$\mathrm{pr}(\mathrm{no} \mid B) = (1-\alpha)\cdot\left[1-\exp\left(-\left(\frac{B}{\sigma}\right)^{\theta}\right)\right] + \alpha\cdot 1 \text{ and}$$

(7.8) $$\mathrm{pr}(\mathrm{yes} \mid B) = (1-\alpha)\cdot\exp\left(-\left(\frac{B}{\sigma}\right)^{\theta}\right).$$

Finally, when the contaminating population is a population of yea-sayers, an observed "yes" response could come from a respondent who holds an underlying, positive WTP amount greater than the bid level B, or from a yea-sayer. The probability of observing a "yes" is:

(7.9) $$\mathrm{pr}(\mathrm{yes} \mid B) = (1-\alpha)\cdot\left(1 - F(B;\theta,\sigma)\right) + \alpha\cdot 1,$$

since a yea-sayer answers "yes" with probability one, regardless of the bid level that has been assigned to him or her. Only persons with genuinely low WTP amounts provide "no" answers:

(7.10) $$\mathrm{pr}(\mathrm{no} \mid B) = (1-\alpha)\cdot\left(F(B;\theta,\sigma)\right).$$

Again, the final expressions of the contribution to the likelihood in the presence of yea-saying and Weibull distribution of WTP are:

(7.11) $$\mathrm{pr}(\mathrm{yes} \mid B) = (1-\alpha)\cdot\exp\left(-\left(\frac{B}{\sigma}\right)^{\theta}\right) + \alpha\cdot 1, \text{ and}$$

(7.12) $$\mathrm{pr}(\mathrm{no} \mid B) = (1-\alpha)\cdot\left[1-\exp\left(-\left(\frac{B}{\sigma}\right)^{\theta}\right)\right].$$

D. Application

In this section, we apply the mixture models with completely random responses, yea-saying and nay-saying to the data collected through three CV surveys about mortality risk reductions.

The first study was conducted by Krupnick et al. (2001) in Canada using a self-administered computer instrument. The second study is the US version of the Canada contingent valuation survey, the data being collected using Web-TV from a panel that is supposed to representative of the US population for age, gender, race and income (Alberini et al., forthcoming). Although each respondent valued three risk reductions that differed for size and timing, in this report attention is restricted to the 5 in 1000 risk reduction from wave (subsample) I and to the “yes” or “no” responses to the initial and follow-up payments questions.

Table 7.1 presents the percentage of “yes” responses to the initial bid amount for the Canada and the US studies. In table 7.2 I present the results of the mixture models for Canada using only the responses to the initial payment questions (single-bounded data). Table 7.3 refers to mixtures where equations (7.1)-(7.11) have been amended to accommodate the responses to the initial payment questions and to the follow-ups (double-bounded data) for Canada. Table 7.4 refers to the US study data, focusing on the single-bounded data because models using the initial and follow-up responses were poorly behaved.

Table 7.1. Percentage of yes responses to the initial payment questions in the Krupnick et al. (2001) and Alberini et al. (2002) studies. Risk reduction of 5 in 1000, wave I.

Bid amount (2000 US dollars)	Percentage Yes Responses	
	Canada study	US Study
70	71.52	68.84
150	66.87	62.50
500	42.14	41.06
725	25.81	34.78

Three caveats are in order. The first is that because α, the probability of the mixing component, should be non-negative and less than one, we programmed my likelihood functions so as to impose that $\alpha = \Phi(\delta)$, where Φ is the cdf of the standard normal, and δ is a parameter to be estimated. However, the optimization routine (GAUSS MAXLIK) often encountered convergence problems, with δ tending to extremely small negative numbers (which implies that $\Phi(\delta)$ tends to zero). To circumvent this problem, we dropped the requirement that $\alpha = \Phi(\delta)$, and switched to the constrained maximum likelihood procedure in GAUSS, forcing the routine to consider only values of α between 0 and 1 ($0 \le \alpha \le 1$).

The second is that after estimating the mixing component α, mean and median WTP are computed using the parameters of the well-behaved component of the mixture, i.e., the Weibull distribution. (In other words, the degenerate component is "filtered out" when computing mean and median WTP.) The third is that we produce standard errors based on bootstrapping draws from the asymptotic distribution of the estimated coefficients, but these are very similar to those obtained by than using the delta method (Cameron, 1991).

Table 7.2 shows that if one ignores the possibility of a discrete mixture (first column), and uses only the responses to the initial payment questions, mean WTP from the Canada study is quite large (1177 Canadian dollars for a risk reduction of 5 in 1000 over 10 years). This is, in fact, over twice as large as median WTP (Can \$446). Allowing for the possibility that some people may provide completely random answers (second column) reduces mean WTP to Can \$595, but keeps median WTP relatively close to the previous estimate (Can \$551), as expected (see Figure 2.1). One problem, however, is that the estimate of α is very large, and implies that virtually half of the population would be expected to be answering the payment questions in a completely random fashion. Clearly, this is not plausible, since the answers to the debrief questions suggested that people had paid attention to various aspects of the risk reduction scenario and for the most part accepted it.

The third column of the table suggests that about a quarter of the sample might consist of nay-sayers. Accounting for their presence, and focusing on the distribution of the well-behaved responses to the payment questions, slightly lowers the estimate of mean WTP, and dramatically raises median WTP. The two are now very close (Can \$969 v. 859). The standard errors of the estimates around mean WTP were omitted from the second and the third columns of table 7.3 because they were implausibly large, the results of some very large draws from the asymptotic distribution of the vectors of parameter estimates. Finally, there is no evidence of yea-saying, and α for this model is pegged at zero.

One would expect the mixture models based on double-bounded data to be better behaved. Combining the responses to the initial and follow-up questions refines information about WTP, and should make it easier to detect "bulges" in the frequencies of the responses to the payment questions that suggest possible departure from the conventional model. Indeed, table 7.3 shows that there is no longer any evidence of yea-saying and nay-saying, but the probability of a completely random response continues to be implausibly large. It is, in fact, even larger than when single-bounded models were used. Despite an estimated α of 0.66, neither mean or median WTP are affected. The former is within 5 dollars of the estimate from the traditional model, the latter only 8 cents smaller than its counterpart from the conventional model.

This finding is in sharp contrast with the answers to debriefing questions and the good internal consistency and validity shown by study participants, raising doubts about the ability of single-bounded models of WTP to capture the correct proportion of subjects engaging in degenerate response mechanisms. No evidence of any mixing component is found in the US study, where α is zero for all of the discrete mixtures we examined.

Table 7.2. Estimation results for the Canada mortality risk reduction survey based on single-bounded data. Standard errors in parentheses.

	No mixture	Mixture with completely random responses	Mixture with nay- sayers	Mixture with yea-sayers
θ	0.6159 (0.0681)	1.8785 (1.2810)	1.5975 (0.6040)	0.6159 (0.0681)
σ	808.6715 (90.50)	669.75 (76.06)	1081.29 (81.63)	808.6715 (90.50)
α	--	0.5108 (0.1306)	0.2644 (0.0501)	--
Log L	-391.34	-390.15	-389.34	-391.34
Mean WTP ($)	1176.94	594.55	969.57	1176.94
Bootstrap Std error around mean WTP	306.55	N/A	N/A	306.55
Median WTP ($)	445.99	551.04	859.61	445.99
Bootstrap Std error around median WTP	44.02	146.05	134.10	44.02

Table 7.3. Estimation results for the Canada mortality risk reduction survey based on Double-bounded data. Standard errors in parentheses.

	No mixture	Mixture with completely random responses	Mixture with nay sayers	Mixture with yea-sayers
θ	0.6274 (0.0285)	0.1921 (0.0829)	0.6274 (0.0285)	0.6274 (0.0285)
σ	580.77 (35.28)	1123.13 (326.60)	580.77 (35.28)	580.77 (35.28)
α	--	0.6577 (0.0448)	0.00	0.00
Log L	-1077.14	-988.60	-1077.14	-1077.14
Mean WTP ($)	826.41	831.24	826.41	826.41
Bootstrap Std error around mean WTP	70.85	70.92	70.85	70.85
Median WTP ($)	323.83	323.75	323.83	323.83
Bootstrap Std error around median WTP	20.96	21.10	20.96	20.96

The third study we use to experiment with mixture models is Johannesson and Johansson (1996). This study was conducted in Sweden, respondents were interviewed over the telephone and no visual aids were used. Respondents were told that the probability of surviving until age 75 was X, that the average 75-year old survives for 10 more years, and were queried about their willingness to pay for a medical treatment that would increase their expected life expectancy past age 75 by another year. The elicitation approach is a dichotomous choice question with no follow-ups. Table 7.4 shows the percentage of yes responses to the payment question.

Table 7.4. Percentage of "yes" responses to the payment questions in Johannesson and Johansson (1996).

Bid amount (SEK)	Percentage Yes Responses
100	53.22
500	38.58
1000	31.38
5000	22.63
15000	13.64
50000	9.09

Table 7.5 Estimation results for the Johannesson and Johansson 1996 study. Standard errors in parentheses. N=2013.

	No mixture	Mixture with random coin flipping	Mixture with nay sayers	Mixture with yea-sayers
θ	0.2118 (0.0145)	0.2539 (0.0537)	0.2118 (0.0145)	0.2118 (0.0145)
σ	673.88 (114.55)	557.96 (133.59)	673.88 (114.55)	673.88 (114.55)
α	--	0.1039 (0.0990)	--	--
Log L	-1079.87	-1079.57	-1079.87	-1079.87
Mean WTP (SEK)	50,574.65	12,110.20	50,574.65	50,574.65
Bootstrap Std error around mean WTP	34,759	N/A	34,759	34,759
Median WTP (SEK)	119.44	132.00	119.44	119.44
Bootstrap Std error around median WTP	30.47	36.73	30.47	30.47

As shown in columns three and four of table 7.4, we do not find any evidence of nay-saying or yea-saying. Column two suggests that about 10 percent of the population might provide completely random responses. This is a much more reasonable figure than in the previous studies. Correcting for this lowers the estimate of mean WTP, which was originally very high (over 50,000 SEK, or about 6000 US dollars), bringing it town to

12,000 SEK. However, chapter 5 suggests that mean WTP cannot be reliably estimated using the data from this study, unless one is prepared to make restrictive assumptions about the distribution of WTP and/or the calculation of the mean. Median WTP, on the other hand, is only minimally affected (132 SEK versus 119 SEK in the traditional model).

Our own research on discrete mixtures with dichotomous-choice CV responses, however, suggests that it is difficult to estimate in the mixing components in a reliable fashion. In Alberini and Carson (2001), Monte Carlo simulation methods suggest that with single-bounded dichotomous choice responses are used, α is often overestimated, despite the fact that for a considerable fraction of the replications α is pegged at zero. Using double-bounded CV responses generally improves the performance of the mixture models, and does a reasonable job identifying α. However, this is so only when the distribution of the non-degenerate component of the mixture is correctly guessed by the researchers. These simulations also suggest that the estimated α frequently captures a poorly chosen distribution for the non-degenerate component of the mixture. Moreover, being able to correctly estimate α depends crucially on assuming the correct mixture (e.g., that there truly is yea-saying, as opposed to another form of "contamination").

In practice, we suspect that mixtures with different components are likely to coexist in a sample, but identifying them is extremely difficult, unless well-crafted debriefing questions are included in the questionnaire to assist the researcher in uncovering potentially troublesome observations. For example, the NOAA Panel on Contingent Valuation recommend that questions be asked to find out why a respondent answered "yes" or "no" to the (dichotomous-choice) payment questions. In addition, debriefing questions should be asked at the end of the questionnaire to find out if the respondent has understood all aspects of the scenario.

Conclusions.

- Discrete mixtures can be used to accommodate for nay-saying, yea-saying, and completely random responses in dichotomous choice CV surveys.
- It seems likely that the samples from many studies would simultaneously include more than one of these undesirable response effects.
- Experimentation with various datasets, however, suggests that unless the sample separation is known, it is difficult to identify reliably the components of a mixture. This suggests that researchers should include questions about the reasons for the "yes" or "no" responses to the payment questions, and include debriefing questions to make sure if the respondent had understood the good being valued and various aspects of the scenario.
- Median WTP is robust to the presence of individuals who answer the payment questions in a completely random fashion, but is not robust to the presence of yea-saying and nay-saying.

8. Interpretation of the WTP Responses: Continuous and Interval Data, and Zero WTP

In this section, we focus on issues of interpretation of the responses to the payment questions, which in turn defines their treatment in the statistical model. We begin with the issue of zero WTP responses, followed by the continuous v. interval-data treatment of WTP observations pinpointed by the respondent on a payment card.

As previously mentioned, the questionnaire used by Krupnick et al. (2002) in Canada uses the dichotomous choice approach with one follow-up question. Those respondents who answered "no" to the initial and follow-up question were asked if they would pay anything all for the product that reduced their risk of dying, and, if so, how much.

In the analysis of the WTP responses for the 5 in 1000 risk reduction from wave 1, Krupnick et al. focus on a "cleaned" sample that had demonstrated basic probability comprehension,[30] and report that almost 20 percent of the sample was not willing to pay anything at all for the risk reduction. Their sample, therefore, contains a mix of zero WTP responses, and continuous and interval data. They adapt a tobit model to this mix of response types, obtaining a variant of the so-called "spike" model (Kriström, 1997). Formally,

(8.1)

$$\log L = \sum_{i\in\Im_0} \log[1-\Phi(-\mu/\sigma)] + \sum_{i\in\Im_C} \log \frac{1}{\sigma}\phi\left(\frac{WTP_i-\mu}{\sigma}\right) + \sum_{i\in\Im_{DB}} \log\left[\Phi\left(\frac{WTP_i^U-\mu}{\sigma}\right)-\Phi\left(\frac{WTP_i^L-\mu}{\sigma}\right)\right]$$

where $\Im_0$ is the subset of respondents with zero WTP, $\Im_C$ includes all respondents who report continuous WTP amounts (denoted as WTP_i), and $\Im_{DB}$ includes all respondents with YN, NY, and YY responses. WTP^U and WTP^L are the upper and lower bounds of the interval around the true WTP amounts for these respondents.[31]

Mean WTP is $\int_0^\infty \left[1-\Phi\left(\frac{x-\mu}{\sigma}\right)\right]dx$. Although this is similar to the formula that was used by Johannesson and Johansson (1996) and Johannesson et al. (1997), it should be emphasized that the two econometric models, and the WTP responses on which they are

[30] Specifically, the cleaned sample excludes those respondents who failed the probability quiz and the probability choice question the first time. The probability quiz asks respondents to tell which of two people, A and B, has a greater chance of dying, if A's risk is 5 in 1000 and B's is 10 in 1000. The probability choice question asks respondents to consider the same two people, and to tell which person they would rather be. A total of 611 respondents out of 630 in wave 1 met this selection criterion.

[31] If a respondent answers YY to the payment questions, then the lower bound of the interval around WTP is the follow-up bid, while the upper bound is infinity.

based, are completely different. In the latter two studies, the researchers did *not* observe any zero WTP responses, but simple "yes" and "no" to a one-shot payment question. There was no reason to assume that WTP should be negative, but the researchers used a distributional assumption that admits negative values, only to later discard the negative range of the distribution of WTP when calculating mean WTP.

With a cleaned sample of 616 observations, 19.64% being zero WTP responses, the spike model produces an estimate of mean WTP equal to 597.72, with a standard error around mean WTP equal to 27.09. The log likelihood function is -1363.47.

In subsequent work, Alberini et al. (forthcoming) ignore the responses to the final round of open-ended questions, and use only the responses to the initial and follow-up payment questions. This produces a double-bounded, interval-data model. Table 8.1 reports mean and median WTP under various distributional assumptions for WTP from double-bounded models. Of the models shown in table 8.1, the Weibull is the best in terms of fit, based on the log likelihood function, with the lognormal a close runner-up.

Table 8.1. Results from double-bounded models for the Canada study, Krupnick et al. (2002). All figures in 1999 Canadian dollars. N=616 (cleaned sample).

	Normal	Lognormal	Weibull
Mean WTP (Standard Error)	556.23 (26.37)	1087.61 (129.10)	712.42 (75.94)
Median WTP (Standard Error)	556.23 (26.27)	367.53 (24.20)	414.42 (25.28)
Log likelihood function	-845.55	-789.75	-788.27

Comparison with the spike model indicates that the estimate of mean WTP produced by the normal double-bounded is not very different from that of the spike normal based on the normal distribution. The Weibull double-bounded model results in a mean WTP figure that is about 1.7 times median WTP, and, as expected, the difference between mean and median WTP is even more pronounced when WTP is assumed to be lognormal. The mean WTP from the lognormal model is almost twice as large as that from the spike model.

Despite Alberini et al.'s decision to ignore the zero WTP responses and ascribe a positive WTP to these respondents, it would seem that those subjects who reported zero WTP in this questionnaire probably meant to do so: after all, they were specifically queried about this in the course of the survey. This suggests that one should consider a third alternative to the spike model and to double-bounded models, namely a mixture with two components where the sample separation is known. Specifically, for $(1-\alpha)\cdot 100\%$ of the sample WTP is identically equal to zero. For the remaining $\alpha\cdot 100\%$ of the sample WTP is assumed to follow the Weibull distribution. Mean WTP is, therefore, $0\cdot\alpha + (1-\alpha)\cdot MWTP$, where MWTP is mean WTP for the non-degenerate component of the Weibull. Since a double-bounded model estimates the latter to be Can \$813.21 (s.e.

46.54), mean WTP in the full sample is Can $653.49 (37.39). Clearly, this figure is between the figure produced by the spike model and that from the procedure that ignores the presence of zero WTP responses.

In the Gerking et al study (1988), the respondents were asked to circle dollar amounts shown on a payment card. Gerking et al. treat these responses as continuous, except for those corresponding to the figure of $6001 (the largest sum on the payment card) and more. Cameron and Huppert (1988) argue that payment cards imply interval-censored observations, and that a respondent's true WTP amount lies between the figure he circled on the payment card and the next amount. For example, if the respondent has circled $100 on the payment card on page 4 of the questionnaire, it is assumed that true WTP lies between $100 and $120, the next amount on the card. Following this reasoning, if the respondent has circled $0, then the lower bound of the interval around WTP is zero, while the upper bound of the interval is $20. We wish to check if this interpretation of the responses would result in large changes in the welfare statistics.

Table 8.2 reports the results of a tobit model analogous to that used by Gerking et al. (and hence treats zero WTP responses as true zeros), and of a Weibull and a lognormal interval-data models following Cameron and Huppert's argument that amounts circled on a payment cards imply interval-censored observations.

Table 8.2 Welfare statistics for WTP data, Gerking et al. study, 1988. N=476.

	Sample moments or order statistics	Double tobit model, most obs. Treated as continuous	Interval data and Weibull distribution	Interval data and lognormal distribution
Mean WTP	678.60 (71.26)	599.02	2330.13 (748.83)	31910.03 (22843)
Median WTP	20 (118.87)		28.60 (7.26)	28.09 (5.83)

As expected, the two interval-data models based on distributions of WTP defined on the positive real axis result in large mean WTP figures, but agree with each other and with the sample median about median WTP. That the estimates of mean WTP from, say, the Weibull model is much larger than that from the tobit model (almost four times as large) is in sharp contrast with the results from the Krupnick et al. study, where mean WTP from the Weibull, double-bounded model was only about 1.3 times mean WTP from the spike/tobit model. This suggests that the spike versus Weibull model disparities are likely to depend on the specific set of data, and are difficult to quantify exactly.

When we tried the mixture of zero WTP and a Weibull distribution, which we estimate assuming that observations are continuous, except for $6001 and higher, the latter produces an estimate of mean WTP equal to $1443.14 (s.e. 161.60). Since 47.40% of the sample indicates that they would not pay anything at all for the risk reduction, the mixture model yields (1-0.4790)·1443.14=751.73, with a standard error of 84.18.

Conclusions

- If the CV questionnaire elicits WTP using the payment card method, the responses should be treated as interval data (Cameron and Huppert, 1988).
- Follow-up questions should be included to find out if a respondent who answers "no" to the dichotomous choice payment questions or circles the number zero on a payment card truly means that he is not willing to pay anything for the risk reduction.
- In the presence of zero WTP responses, (the interval-data variant of) tobit models and mixtures with sample separation known are reasonable alternatives for handling the data.

9. Sensitivity of WTP to Risks: Endogeneity of WTP and Subjective Risks

Economic theory suggests that willingness to pay for a mortality risk reduction should increase with the size of the risk reduction. Moreover, under general assumptions, WTP should be strictly proportional to the size of the risk reduction (Hammitt and Graham, 1999). Although these requirements seem straightforward, a recent survey of the literature (Hammitt and Graham, 1999) finds that out of 25 empirical stated-preference studies conducted over the previous twenty years, (i) the majority fails to detect a statistically significant relationship between WTP and the size of the risk reduction, whether internal or external scope tests are used, and that (ii) proportionality is often violated.[32, 33]

WTP is also expected to increase with baseline risks, although when the risks are very small the effect of baseline risk is probably negligible (Hammitt and Graham, 1999). Another important question is whether people respond to the absolute or relative risk reductions (Baron, 1997).

Clearly, checking these effects and answering the question about proportional or absolute risk reduction require that, when the size of the risks are varied to the respondents, regressions be run that relate WTP to risks and risk changes. In some surveys, the researchers ask the respondents to assess their own baseline risks and/or the risk reductions that would be obtained through certain behaviors or by using certain products (Johannesson et al., 1991; Persson et al., 2001). These regressions must, therefore, check for the possible endogeneity of subjective risks and WTP. Such endogeneity arises when both WTP and risks share common unobservable individual factors, and, if left

[32] An internal test implies that the same respondent is asked to value risk reductions of different size. An external test implies that different respondents are asked to value risk reductions of the same size. Since the answers to earlier valuation questions may provide implicit cues to the respondent about the value to place on later risk reductions, it is generally felt that external scope tests are more demanding, and hence a CV study would be judged as of higher quality if the researchers can demonstrate that WTP satisfies the external scope requirement. Carson (2000), in commenting on the guidelines set by the NOAA Panel on Contingent Valuation, points out that with most environmental commodities there is no special need to demonstrate the WTP is sensitive to scope, but considers mortality risk reductions an exception to this claim due to the cognitive difficulty associated with small probabilities. Hammitt and Graham (1999) use the term "weak scope" to refer to the requirement that WTP increase with the size of the risk reduction, reserving the expression "strong scope" for the requirement that WTP be proportional with the size of the risk reduction.

[33] Hammitt and Graham (1999) discuss a number of possible reasons why WTP fails to increase with the sizes of the risk reduction, and/or violates proportionality. One obvious reason is that people do not comprehend probabilities. Corso et al. (2001) check which types of visual aids can help people process probabilities, with the end result that WTP satisfies the weak and strong scope effects.

unaccounted for, may result in invalid inference about scope and/or absolute v. relative risk reductions.[34]

In this section we demonstrate tests of endogeneity of WTP with baseline risks and absolute risks, respectively, using the data from the Gerking et al. (1988) study and the Persson et al. (2001).

A. Endogeneity of Subjective Risks in the Gerking et al study

In the Gerking et al study, respondents were asked to report their subjectively assessed baseline risks, on the grounds that WTP for a reduction in workplace-related risks should depend on perceived risks. The risk ladder used for this exercise expressed risks as X per 4000 workers, and the risk reduction to be valued was, for all respondents, 1 in 4000 from the baseline risks. The purpose of this section is to examine whether baseline risks, when small, truly do not influence WTP for a mortality risk reduction, as argued by Hammitt and Graham (1999). Because the risk reduction did not vary across respondents, it is not possible to test for sensitivity of WTP to the size of the risk change.

This implies that a possible model of WTP is described by the following equation:

(9.1) $$\log WTP_i = \mathbf{x}_i \beta + B_i \delta + \varepsilon_i ,$$

where **x** is a vector of individual characteristics thought to influence WTP, *B* is the respondent's subjective annual workplace risk, β and δ are unknown coefficients. The error ε is assumed to be normally distributed.

In equation (9.1), we refer to log WTP, where WTP is the respondent's latent willingness to pay, because I treat the responses to the payment question as interval data for WTP, following Cameron and Huppert (1988). To illustrate, if the respondent has circled $100 on the payment card on page 4 of the questionnaire, it is assumed that true WTP lies between $100 and $120, the next amount on the card. If the respondent has circled $0, then the lower bound of the interval around WTP is zero, while the upper bound of the

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[34] The coefficient on the risks in the WTP regression is biased downward if the correlation between subjective risks and WTP is positive. However, the same result would hold if the respondents are *asked* to value objective risk reductions, but replace them with *different* risk reductions of their own invention because they do not believe the effectiveness of a proposed product or policy, or because they did not accept the baseline risks as their own ("I am a better driver than the average"). If the researcher regressed WTP on the objective risks, the latter would be interpreted as a variable observed with an error, and the results of error-in-variables regressions would apply. This relates to one of Hammitt and Graham's possible reasons for the failure of WTP to increase with risk: that the risk valued by the respondents are *not* the risks stated to them in the survey by the researcher.

interval is \$20. Baseline risks B are interpreted as a continuous variable. Testing if baseline risk matters requires testing if δ=0.

B and log WTP are endogenous if *B* is correlated with the error term in equation (9.1). This may be the case if both *B* and WTP are influenced by idiosyncratic, individual-specific factors that remain unobserved to the researcher. If so, any estimation technique that treats the right-hand side variables of (9.1) as exogenous (e.g., OLS or tobit) will give biased and inconsistent estimates.

To remedy this problem, we specify an auxiliary equation where *B* is explained as a linear combination of instruments **z**, plus an error term. Formally,

(9.2) $$B_i = \mathbf{z}_i \gamma + \eta_i .$$

We assume that the error term in (9.2), η, is normally distributed. If WTP and *B* are endogenous, the covariance between ε and η is different from zero, and the two error terms are jointly normally distributed:

(9.3) $$\begin{bmatrix} \varepsilon_i \\ \eta_i \end{bmatrix} \sim \text{i.i.d.}\, N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}; \begin{bmatrix} \sigma_1^2 & \sigma_{12} \\ \sigma_{12} & \sigma_2^2 \end{bmatrix} \right).$$

The distributional assumption in (3) implies that, conditional on subjective baseline risks, log WTP is equal to $\mathbf{x}_i \beta + B_i \delta + \eta_i \lambda + e_i$, where

(9.4) $$\eta_i = B_i - \mathbf{z}_i \gamma ,$$

i.e., the error term in the equation for baseline risk,

(9.5) $$\lambda = \sigma_{12} / \sigma_2^2 ,$$

and the error term *e* is a normal with mean zero and variance v^2 equal to $\sigma_1^2 - \sigma_{12}^2 / \sigma_2^2$.

The joint distribution of the WTP responses and baseline risk can, therefore, be expressed as the product of the marginal for baseline risk (which is a normal), times the above described conditional distribution, which is also a normal. If the lower and upper bounds of the interval around a respondent's true WTP amount are denoted as $\mathrm{WTP^L}$ and $\mathrm{WTP^H}$, the respondent's contribution to the likelihood function is:

(9.6) $$\ell_i = \frac{1}{\sigma_2} \phi\left(\frac{B_i - \mathbf{z}_i \lambda}{\sigma_2} \right) \times \left[\Phi\left(\frac{WTP_i^H - m_i}{v} \right) - \Phi\left(\frac{WTP_i^L - m_i}{v} \right) \right] ,$$

where $m_i = \mathbf{x}_i\beta + B_i\delta + \eta_i\lambda$. Endogeneity can be tested by testing whether the covariance, σ_{12}, is equal to zero, or λ is equal to zero.

Table 9.1 below reports the results of one maximum likelihood estimation run based on equation (9.6). For identification purposes, at least one excluded exogenous variable in the vector **z** is not included in **x**. The vector **z**, the instruments for subjective baseline risks, includes (i) objective baseline risk based on industry and occupation, (ii) the union status of the respondent, (iii) dummies for whether the respondent works in the manufacturing sector and has a blue collar occupation, and (iv) education. Experience was also attempted, but the model failed to converged, and so the specification in table 9.1 omits this variable.

The **x** regressors include income, age dummies, gender, education, race, ethnicity, and residence in urban or suburban area. Objective risk, therefore, is the variable that is excluded from **x** and included in **z** for identification. Regarding other variables, **x** includes income, but income is not included in **z**. Education is common to both **x** and **z**.

Table 9.1, top panel, shows that subjective risk is well predicted by objective risk (ORISK), but that the former is not an unbiased estimate of the other: the intercept (CONST) is different from zero, and the coefficient on objective risk implies that a change of one in the objective risk does not translate into a change of one in the subjective risk. (It should be kept in mind that the scale of objective and subjective risks is very different. This study is affected by a large discrepancy between the risks the respondents is asked to look at, and pick his or her own subjective risk out of, which are, on average, $7.4*10^{-4}$ for this sample, and objective risks, which are only $7.5*10^{-5}$. Subjective and objective risks, therefore, differ by an order of magnitude. Both objective and subjective risks were rescaled through multiplying them by 1000 in the regression run shown in table 9.1. The change in the scale of the objective and subjective risks is absorbed into the regression coefficient on objective risks.)

Union members (UNION) and persons with blue collar occupations (BLUE) tend to report higher subjective risks than the others, and the effect of education (EDUC, measured in years of schooling) is insignificant, as is the effect of working in the manufacturing sector (MFG dummy).

WTP for the risk reduction increases with income (significant at the 10% level), and is higher among blacks and lower among persons of age between 40 and 50 years. The coefficients of the other variables were not statistically significant.

Importantly, the correlation coefficient between the errors of two equations, ρ, is insignificant, implying that baseline risk can be treated as exogenous in the WTP equation. Indeed, δ itself is insignificant, implying that baseline risks do not affect WTP: in other words, the amount of money that respondents were prepared to sacrifice to improve by one step on the risk ladder does *not* depend on which particular step of the ladder they start from. This provides support for Hammitt and Graham's claim (1999).

To check if these results are sensitive to the use of a semi-log model of WTP, and my interpretation of the responses to the payment questions (which follows Cameron and Huppert, 1988), I experimented with alternative models that specified a linear model for WTP and interpreted the data to be on a continuous scale. I estimated this model both by two-stage least squares and double-tobit (as in Gerking et al.). While this approach was able to identify more significant determinants of subjective risks (e.g., certain manufacturing sectors), they confirm that baseline risk is not significantly associated with WTP, and that there is no evidence of endogeneity between WTP and baseline risks.

Table 9.1 Continuous/interval-data model with endogenous baseline risk.

```
bivariate model
Data Set:  data1

return code =    0
normal convergence

Log-likelihood      -1046.11
Number of cases     344

Covariance matrix of the parameters computed by the following method:
Inverse of computed Hessian
```

Parameters	Estimates	Std. err.	t stat.	P Value	Gradient
RISK EQUATION					
R_CONST	0.8000**	0.1719	4.653	0.0000	0.0000
R_ORISK	0.8772**	0.2420	3.625	0.0003	0.0000
R_UNION	0.1379*	0.0585	2.359	0.0183	0.0000
R_MFG	-0.0642	0.0598	-1.073	0.2834	0.0000
R_BLUE	0.2358**	0.0621	3.794	0.0001	0.0000
R_EDUC	-0.0109	0.0106	-1.031	0.3024	0.0000
WTP EQUATION					
W_CONST	2.9664	1.9644	1.510	0.1310	-0.0001
W_INC	0.0247^	0.0148	1.675	0.0940	0.0000
W_BLACK	2.4058*	1.0232	2.351	0.0187	0.0000
W_HISP	0.7774	1.2830	0.606	0.5446	0.0000
W_EDUC	0.0493	0.0857	0.576	0.5647	0.0000
W_MALE	-0.2827	0.5934	-0.476	0.6338	0.0001
W_AGELES	-0.8910	0.7611	-1.171	0.2417	0.0000
W_AGE404	-1.9771*	0.8557	-2.311	0.0209	0.0000
W_AGE505	-0.8781	0.8364	-1.050	0.2938	0.0000
W_SUBURB	0.5866	0.5901	0.994	0.3202	0.0001
W_RURAL	0.9024	0.6761	1.335	0.1820	0.0000
SIGMA1	0.4708**	0.0180	26.228	0.0000	0.0000
SIGMA2	3.3999**	0.2076	16.373	0.0000	0.0000
RHO	0.0886	0.1728	0.513	0.6082	0.0000
DELTA	-0.3158	1.2020	-0.263	0.7927	0.0001

^ = significant at the 10% level; * = significant at the 5% level; ** = significant at the 1% level.

B. The Persson et al. Survey.

The purpose of examining the Persson et al. data in this chapter is two-fold. First, we wish to explore the importance of allowing for subjective risk reductions to be endogenously determined with the willingness to pay for them. Second, we wish to examine whether respondents were valuing the absolute risk reduction, or simply responding to the proportional risk reduction stated to them in the survey, when answering the WTP questions. Regarding the latter goal, it is of interest to see if the results depend on whether risks are treated as exogenous or endogenous with WTP.

Persson et al. (2001) conducted a mail survey about WTP for mortality risk reduction among adult Swedes in March 1998. [35] There were two versions of the questionnaire. The first version of the questionnaire concerns risks of dying in road traffic accidents, while the other focuses on risks of injuries in the same context. In the *Journal of Risk and Uncertainty* article, and in this report, attention is restricted to the former subsample.

The questionnaire can be roughly divided into six sections. The first section queried them about the use of private safety devices such as seatbelts for backseat passengers and helmets, and subsequently elicited extensive information about the distance traveled in a car, by public transit, bicycle, motorcycle, moped and as a pedestrian. At the end of this section, the respondents were asked whether they had ever been injured in an accident, if this had happened the year before, and if anyone else in their household had ever been injured in an accident.

In the second section of the questionnaire, respondents were asked to rate their health on a scale from 0 to 100, where 0 represents the "worst imaginable condition" and 100 represents the "best imaginable condition," on a thermometer with readings ranging from 0 to 100.

The third section of the questionnaire introduced individual mortality risks. Respondents were told that a fifty-year old[36] had a risk of dying of 300 in 100,000 in a year, and were shown on this risk on a grid of squares, which also showed how the risk of dying for certain causes compares to the total risk of dying. Specifically, the questionnaire stated

[35] The population targeted by the study was a random sample of Swedes of ages 18-74. The first mailing of the questionnaire was in March 1998, and was followed by two follow-up remainders in hopes of raising the return rate. The overall return rate is 51%. In addition, to check for possible selection into the sample, the authors sent out 2645 "drop out" questionnaires, 659 of which were eventually filled out and returned. The authors conclude that the final sample is wealthier, drives more miles, and has a higher educational attainment than the average Swede and the typical dropout respondent, but does not differ from the Swedish population and dropout respondents in terms of gender and access to a car.

[36] This person's gender was not specified in the questionnaire.

that the risk of dying of heart disease is 54 in 100,000, the risk of dying of cancer in the stomach or esophagus is 6 in 100,000, and the risk of dying in a traffic accident is 5 in 100,000.

After this example, respondents were asked to report their subjective risk of dying in one year, considering their age and current state of health, per 100,000. They were then asked to state how much they would be willing to pay for a reduction in this risk of dying.[37] The reduction was expressed in proportional terms, with respondents randomly assigned one of three possible percentages: 10%, 30%, and 50%.

People were asked to refer to safety equipment and preventative health care, and were instructed to think only of the risk of dying (without considering the risk of being injured or permanently disabled). They were also reminded that the risk reduction would apply for one year only, and that they should keep their budget in mind when answering this question.

Following the WTP question, respondents were asked which other category of expenditure (or savings) they would reduce in order to pay for the risk reduction.

In the fourth section of the questionnaire, people were to focus on their risk of dying in a road traffic accident. They were first asked to indicate their own subjective risk of dying in a road traffic accident, after being reminded that they should consider how much they travel, their age and gender, and their own driving behavior when answering this question. Next, they were to consider a safety device that could be worn without any inconvenience or pain, that would reduce risks. The safety device would serve the only purpose of reducing risk, and would have to be paid for every year, offering protection only for that year. Given that the safety device would reduce the risk by a certain percentage, how much would the respondent be willing to pay for it?[38]

In the fifth section of the survey, the subjects were asked how they would spend an increase of 1000 SEK monthly in their take-home pay. They were shown several categories of expenditure, including food, additional savings, safety equipment and preventative health care. Finally, the respondents were asked the usual socio-

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[37] The payment question was open-ended, and the WTP responses are on continuous scale. This allows the use of linear regression models and of OLS and 2SLS estimation techniques, but it should be kept in mind that open-ended questions is not regarded as an incentive compatible technique for eliciting information about WTP. If this survey had used dichotomous choice payment questions, it may well have resulted in different WTP estimates.

[38] The percentage risk reduction was the same as that in the question about the risk of dying for any cause. However, those respondents who were previously asked about a 30% reduction in the risk of dying for any cause were randomly assigned to one of two possible risk reductions in their risk of dying in a road-traffic accident, 30% and 99%, respectively.

demographic questions and some additional questions about the vehicles in their households, including their safety features.

C. The Persson et al. data

Person et al (2001) focus on the reductions in the risk of dying in road traffic accidents. The dataset that I received from these authors contains a total of 2884 observations. A total of 1384 people answered the question about their own risk of dying for any cause, and 776 of these people also reported a WTP amount for reducing their risks of dying of any cause. Of these 776 people, 32 reported a positive WTP even though their baseline risk, and hence the absolute risk reduction, was zero. (In addition, forty-three people reported WTP amounts despite not answering the question about their baseline risks.)

A total of 1358 people in the dataset answered the question about their own risk of dying in a road transportation accident, and 960 of these people reported a WTP amount for reducing this risk. Twelve respondents estimated their chance of dying in car accident to be zero, implying that no reduction in such a risk was possible, and yet reported a positive WTP amount.

Tables 9.2 and 9.3 report information about the distributions of the subjectively reported risks of dying for all causes and in road-traffic accidents, respectively.

Table 9.2. Variable AEGRISK: subjective risk of dying for any cause.

N	Mean	Std. Dev.	Minimum	Maximum
1384	551.55	5589.39	0	99999

Seventy-three respondents (or 5.27% of the usable data) believed that their own risk of dying is zero. At the other end of the spectrum, two people thought that their risk of dying of any cause in the coming year is 0.50, one person thought his risk was 0.80, another thought it was 0.95, and, finally, two people wrote that their own chance of dying in the coming year is 99.99%. (The latter two respondents rated their health as 95 and 98 on a scale from 0 to 100, suggesting that they are *not* terminally ill.) The median risk was 10 in 100,000.

Regarding the risk of dying in a road accident, only 2.28 percent of the respondents wrote that their risk was zero. The median risk is 3 in 100,000, but the average is 88, which is about an order of magnitude larger than the mean risk in the Swedish population (6 in 100,000). This large average reflects a small number of observations with very high self-assessed risks. For example, two respondents estimate their own risk of dying in a road-traffic accident in the coming year as 50,000 in 100,000.

Table 9.3. Variable DEGRISK: subjective risk of dying in a road-traffic accident.

N	Mean	Std. Dev.	Minimum	Maximum
1384	88.35	1919.09	0	50000

Descriptive statistics about the distribution of WTP for reductions in the risk from all causes and road-traffic fatalities are reported in table 9.4 and 9.5. These tables refer to usable samples that exclude those respondents who estimated their own risks—and hence risk reductions—at zero, and yet reported positive willingness to pay. Observations with missing baseline risks are also excluded.

Table 9.4. WTP for a reduction in the risk of dying for all causes (SEK).

N	Mean	Std. Dev.	Minimum	Maximum
735	2913.46	10822.92	0	150000

Table 9.5. WTP for a reduction in the risk of dying in a road-traffic accident (SEK).

N	Mean	Std. Dev.	Minimum	Maximum
940	2192.95	7255.47	0	100000

Table 9.6.
Frequencies of zero and positive WTP responses.
Usable samples with positive baseline risks and non-missing WTP.

	WTP for any cause		WTP for road traffic accidents	
Percentage risk reduction	Positive WTP	Zero WTP	Positive WTP	Zero WTP
10 percent	92	18 (16.36%)	90	20 (18.18%)
30 percent	443	92 (17.19%)	561	91 (13.95%)
50 percent	70	20 (22.22%)	153	25 (14.04%)
All	**605**	**130 (17.69%)**	**804**	**136 (14.47%)**

In table 9.6, we report the frequencies of zero and positive WTP. The sample used to compute these frequencies exclude observations with zero baseline risks. This table shows that the percentages of observations with zero WTP ranges between about 14% and 22%, and is on average 17.7% for the question about reducing all risks,[39] and 14.5% for the question about traffic risks.

[39] It is interesting to note that, at least for the risks of dying for all causes, the percentage of respondents who report zero WTP values does not decrease with the proportional risk reduction, as one would expect.

D. Determinants of WTP and Hypotheses

In this section, we present a model that relates WTP to the absolute risk reduction, so that a VSL can be calculated, while allowing the risk reduction to be endogenous with WTP. For simplicity and to facilitate comparisons with the original Persson et al work, we follow the Persson et al. protocols for constructing the dependent variable and the sample.

We begin by restricting the analysis to WTP in the traffic accident context. It makes sense to report a WTP amount only if the subjective baseline risk, and hence the absolute risk reduction, is positive. Accordingly, following Persson et al., we only include observations for which the subjective risks were greater than 1 (in 100,000). In addition, as in Persson et al., we exclude from the sample those respondents whose WTP for one year is greater than 5% of household income.

Regarding the treatment of zero WTP, Persson et al. estimate three models. The model of table 1 in their article simply drops observations with zero WTP, considers only observations with WTP greater than 1, and takes their logarithmic transformation. The model of table 2 follows the same criteria for constructing the sample and uses log WTP, but replaces the zeros with WTP=2. The model of Table 3 of the Persson et al paper uses the same sample as table 2, but does not take the logarithmic transformation.

In addition, Person et al exclude from the sample those respondents whose subjective baseline risk was so large that the absolute risk reduction (DEGRISK*RISKMD) is greater than 10 in 100,000.[40] We report regression results based on two alternative treatments of observations with large baseline risks and risk reductions. In tables 9.7, 9.8, and 9.9, we exclude observations with baseline risks less than one, but include all others, whereas in table 9.10 we follow Persson et al and exclude observations with absolute risk reduction greater than 10 in 100,000. Comparison between 9.7-9.9 and 9.10 provides evidence about the effect of trimming observations with large subjective risks from the sample.

In formulating our model of WTP, we reason that WTP should be increasing in baseline risk. However, Hammitt and Graham (1999) argue that for small risks the effect of baseline risk on WTP is negligible, and the analysis of the Gerking et al. data confirms that WTP does not depend systematically on baseline risks. We therefore focus on the relationship between WTP and absolute risk, which allows one to calculate VSL.

We assume the WTP equation is:

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[40] They reasoned that greater absolute risk reductions would be not comparable with the risk in the Swedish population, which is 6 in 100,000. After the sample is purged of observations with absolute risk reduction greater than 10 in 100,000, the sample average of subjective baseline risk is 11 in 100,000, which is almost twice as large as the actual risk. Median subjective risk in this cleaned sample is 3 in 100,000.

(9.7) $$WTP = \exp(\mathbf{x}_i \beta_1) \cdot ABSRISK_i^{\beta_2} \cdot \exp(\varepsilon_i),$$

where **x** is a 1×k vector of factors thought to influence risks, ABSRISK is the absolute risk change, and ε is an error term. On taking logs, we obtain:

(9.8) $$\log WTP = \mathbf{x}_i \beta_1 + \beta_2 \log ABSRISK_i + \varepsilon_i .$$

Since ABSRISK, the absolute risk, is the subjective baseline multiplied by the proportional risk reduction stated to the respondent in the survey, equation (9.8) can be re-written as:

(9.9) $$\log WTP = \mathbf{x}_i \beta_1 + \beta_2 \log DEGRISK_i + \beta_3 \log RISKMD_i + \varepsilon_i ,$$

where DEGRISK is the subjective risk of dying in a road-traffic accident, and RISKMD is the percentage risk reduction assigned to the respondent in the survey. While the latter is exogenously given to the respondent, log DEGRISK and log WTP may potentially share common, unobservable characteristics of the respondents. If so, then log DEGRISK and log WTP are potentially endogenous. If so, running OLS on equation (9.9) results in inconsistent estimates of the coefficients. This problem may be addressed by using instrumental variable estimation techniques, such two-stage least squares (2SLS).

In writing equation (9.9), we have allowed the coefficients of log DEGRISK and that of log RISKMD to be potentially different. We wish to test the null hypothesis that $\beta_2 = \beta_3$, which corresponds to the notion that subjects are thinking of their absolute risk reduction when announcing their WTP estimates. Should this null be rejected, we wish to test whether $\beta_2 = 0$.

If we do not reject the null that $\beta_2 = 0$, but β_3 is found to be different from zero, we would surmise that people in the Persson et al survey were responding to the proportional risk reduction, rather than valuing the absolute risk reduction. This finding would be problematic, in the sense that it is not immediately clear how the VSL can be computed from this study, where everyone is valuing a risk reduction of a different size.

If β_2 and β_3 were different from one another, but each of them was individually different from zero, we would regard this as evidence that individuals give at least some consideration to baseline risks when they announce their WTP amounts.

E. Endogeneity of risks and WTP

To examine if log DEGRISK is endogenous with log WTP, we specify an additional equation, and instruments, for log DEGRISK. Specifically, we posit that:

(9.10) $$\log DEGRISK = \mathbf{z}_i \gamma_1 + \mathbf{w}_i \gamma_2 + \eta_i ,$$

where **z** is a vector of instruments, some of which may overlap with **x** in equation (9.9), **w** is a vector of instruments that are excluded from the right-hand side of equation (9.9) to ensure identification, γ_1 and γ_2 are vectors of coefficients, and η_i is an error term. We allow the covariance between ε and η to potentially different from zero, in which case log DEGRISK and log WTP are econometrically endogenous.

To test for endogeneity, we estimate (9.10) by OLS, form the OLS residuals, and add the latter to the right-hand side of (9.9). The test of endogeneity is the t statistics on the coefficient of the residuals in this augmented regression (Rivers and Vuong, 1988).[41]

If this procedure rejects the null that the coefficient of the residuals is zero, suggesting that there is endogeneity between log WTP and log DEGRISK, we re-estimate the system (9.9)-(9.10) using 2SLS.

F. Results

Table 9.7 reports regression results based on a sample that applies the same exclusion criteria as Persson et al., except that observations with absolute risk greater than 10 in 100,000 are retained. The dependent variable, as in table 1 of Persson et al., is log WTP. Observations with WTP equal to zero are, therefore, dropped.

Column (A) reports the results of OLS regressions that uses the same specification as in table 1 of the Persson et al. paper. (In practice, this can be interpreted as a reduced form regression, where log WTP depends on determinants of subjective risk, such as miles driven, the relative risk reduction, plus income and past experience with accidents, which can influence the rate at which people are prepared to trade off income for risk reductions. However, Persson et al do not necessarily interpret it as a reduced form equation: They simply state that they arrive at this specification through stepwise selection, without explaining whether the selection relies on the judgement of the researcher or on automatic procedures.)

In column (B), we include log ABSRISK, which is the same as including log DEGRISK and log RISKMD, and restricting the coefficients of these two variables to be equal to one another. The estimated coefficient of log ABSRISK is 0.08, implying that WTP grows less than proportionately to the size of the risk reduction. The associated t statistic is only 1.84, implying significance at the 10% level, and suggesting only weak evidence of a scope effect.

In column (C), we report the results of 2SLS estimation. Imposing that log DEGRISK and log RISKMD have the same coefficient, but treating log DEGRISK as endogenous with WTP, results in a coefficient that is almost three times as large. The t statistic, 1.96, indicates marginal significance at the 5% level, suggesting that when subjective risk was

[41] Implicit in this procedure is the assumption that ε and η are jointly normally distributed.

treated as exogenous, its effect on WTP was understated considerably. WTP continues to increase in a less than proportional fashion with risk, however.

We report the results of a regression of log DEGRISK on several instruments in table 9.8 because they are of independent interest and because this is the first stage of the 2SLS procedure. Table 9.8 shows that individuals were able to relate their own subjective risks to exposure (the number of kilometers traveled in a car per year), and to age, but the coefficients of all of the other instruments included in this equation proved insignificant. For example, traveling by moped, motorcycle, or bicycle, which may be presumed to imply higher risks in the event of an accident, was not significantly associated with subjective risks. (In regressions not reported, we tried accounting for the distance traveled by these modes of transportation, but to no avail.) Likewise, using safety equipment was not significantly associated with subjective risks, nor were gender and education level.

Table 9.7. Dependent variable: ldwp (=log WTP). T statistics in parentheses.

	A OLS n=587	B OLS N=587	C 2SLS n=505	D OLS N=587	E 2SLS n=505
Intercept	1.6280 (1.44)	0.3125 (0.23)	-0.1579 (-0.11)	0.7403 (0.54)	-1.7928 (-0.75)
Log income per household member	0.2221** (2.51)	0.2405** (2.43)	0.2994** (2.72)	0.2231* (2.25)	0.3449* (2.08)
Log km traveled in car	0.3855** (4.44)	0.3041** (3.16)	0.2359** (2.42)	0.3318** (3.64)	-0.0957 (-0.39)
Log DEGRISK		0.0847^ (1.84)	0.2077* (1.96)	0.0337 (0.64)	1.5369^ (1.86)
Log RISKMD	0.2569** (2.79)	0.0847^ (1.84)	0.2077* (1.96)	0.2489** (2.61)	0.1390 (0.92)
Injured in accident (Dummy)	0.1563 (1.16)	0.1484 (1.08)		0.1434 (1.05)	0.2519 (1.16)
Log age		0.2009 (1.15)	0.2266 (1.18)	0.1663 (0.95)	0.5887^ (1.64)
Log age squared		0.0202 (0.67)	0.0173 (0.52)	0.0140 (0.46)	-0.0281 (-0.49)
High school diploma		0.1930 (1.21)	0.2136 (1.21)	0.2177 (1.37)	0.1440 (0.54)
College degree		0.2499 (1.54)	0.2293 (1.28)	0.2590 (1.60)	0.2926 (1.09)
Household members ages 0-3		0.0025 (0.02)	-0.0208 (-0.15)	-0.0128 (-0.10)	0.0282 (0.14)
Household members ages 4-10		0.1283 (1.49)	0.1740 (1.93)	0.1298 (1.51)	0.2097 (1.55)
Household members ages 11-17		-0.0887 (-0.89)	-0.0346 (-0.32)	-0.0763 (-0.77)	-0.0256 (-0.16)
Household members ages 18+		0.1637* (2.21)	0.1975** (2.48)	0.1583* (2.14)	0.2015^ (1.71)
Test that b2=b3		F=3.87 Pval = 0.0496			F=2.75 P val=0.0981

Observations with missing baseline risk and missing WTP, observations with baseline risk smaller than 1, observations with WTP less than 1 and with WTP greater than 5% of annual income are excluded.

^ = significant a the 10% level; * = significant at the 5% level; ** = significant at the 1% level.

Table 9.8. First-stage regression. Dependent variable: log DEGRISK. N=518.

	Coefficient	T statistic
Intercept	1.1095^	1.85
Age	-0.0647**	-3.44
Age squared	0.00068**	3.16
Male (CHECK)	-0.0613	-0.69
Log km traveled in a car	0.2816**	3.83
Travels by moped or motorcycle (dummy)	0.0077	0.06
Travels by bicycle (dummy)	0.0735	0.42
Wears helmet when bicycling (dummy)	0.1875	1.36
Uses seatbelt when in back seat of car (dummy)	-0.1374	-1.28
High school diploma (dummy)	0.0489	0.39
College degree (dummy)	-0.0578	-0.46

^ = significant at the 10% level; ** = significant at the 1% level.

In columns (D) and (E) of table 9.7, I re-estimate equation (9.9) by OLS and 2SLS after relaxing the restriction that log DEGRISK and log RISKMD have identical coefficient, and perform tests of the null that $\beta_2 = \beta_3$. The results are rather surprising. When log DEGRISK is treated as exogenous, we marginally reject the null that these two coefficients are equal at the 5% level, but conclude that log DEGRISK is not a significant determinant of log WTP. People, it would seem, responded to the proportional risk reduction, but not to the absolute risk reduction, which does not permit one to compute the VSL.

By contrast, when we use an instrumental variable technique, 2SLS, to allow for log DEGRISK to be simultaneously determined with log WTP, we cannot reject the null that $\beta_2 = \beta_3$ at the 5% level.[42] When we allow these two coefficients to be different, the subjective baseline is significantly associated with WTP, but the proportional risk reduction is not, which would prompt us to the opposite conclusion about what drives willingness to pay. The coefficients of specification (E), however, are rather different

[42] It should be kept in mind, however, that the coefficient of log DEGRISK is not very precisely estimated, a consequence of using 2SLS.

than their counterparts in equations (D) and (C), suggesting that this equation is rather unstable, and that the equality restriction may play a role in "stabilizing" results.

These runs were repeated using the sample construction procedure of table 2 of the original Persson et al. article, where observations with zero WTP were kept in the usable sample and were replaced with WTP=2. As before, the dependent variable is log WTP. Results are shown in table 9.9.

Table 9.9. Dependent variable: ldwp (=log WTP). T statistics in parentheses. Persson et al. data.

	A OLS (n=676)	B OLS n=676	C OLS N=579	D OLS n=676	E 2SLS N=579
Intercept	0.8445 (0.40)	-1.0221 (-0.45)	-2.4185 (-0.97)	0.0810 (0.04)	-0.5672 (-0.18)
Log income per household member	0.3596 (2.32)	0.4213 (2.47)	0.4777 (2.50)	0.3747 (2.19)	0.4373 (2.19)
Log km traveled in car	0.5681 (3.75)	0.4949 (3.19)	0.4368 (2.60)	0.5502 (3.52)	0.6573 (2.47)
Log DEGRISK	0.0564 (-0.87)	0.1850 (2.32)	0.4092 (2.38)	0.0671 (0.73)	-0.6332 (-0.65)
Log RISKMD	0.5592 (3.46)	0.1850 (2.32)	0.4092 (2.38)	0.5467 (3.35)	0.4420 (2.47)
Injured in accident (Dummy)		0.3779 (1.58)	0.4372 (1.71)	0.3659 (1.53)	0.4262 (1.63)
Log age	-0.6677 (-2.53)	-0.4943 (-1.68)	-0.3308 (-1.03)	-0.5770 (-1.96)	-0.6910 (-1.48)
Log age squared	-0.0395 (-0.87)	-0.0085 (-0.17)	-0.0105 (-0.18)	-0.0211 (-0.42)	0.0350 (0.45)
High school diploma		-0.1275 (-0.47)	0.0829 (0.28)	-0.0635 (-0.23)	0.1567 (0.50)
College degree		0.1298 (0.47)	0.3539 (1.16)	0.1627 (0.59)	0.3249 (1.03)
Household members ages 0-3		0.1754 (0.75)	0.1008 (0.42)	0.1441 (0.62)	0.0552 (0.22)
Household members ages 4-10		0.2401 (1.59)	0.3410 (2.14)	0.2396 (1.60)	0.3088 (1.87)
Household members ages 11-17		-0.0445 (-0.26)	0.0366 (0.20)	-0.1594 (-0.09)	0.0269 (0.14)
Household members ages 18+		0.0543 (0.43)	0.1545 (1.14)	0.0372 (0.30)	0.1494 (1.07)
Test that b2=b3				F=6.44 P val=0.0114	F=1.18 P val=0.2776

Observations with missing baseline risk and missing WTP, observations with baseline risk smaller than 1 in 100,000, observations with WTP less than 1 and with WTP greater than 5% of annual income are excluded. Observations with WTP equal to zero are replaced by WTP=2.

As before, both OLS and 2SLS estimation confirms that WTP increases less than proportionately with the absolute risk change, the coefficient on log ABSRISK being less than 1 (0.18 and 0.40, respectively). However, WTP is more responsive to the change in risk, as shown by both the magnitude and the significance of the coefficients. As before, the instrumental variable procedure results in a stronger coefficient on risk.[43]

Regarding the question whether WTP depends on absolute or the relative risk reduction, specifications (D) and (E) suggest that WTP is associated with the relative risk reduction. When the 2SLS estimation procedure is used, however, we cannot reject the null hypotheses that the coefficients of log DEGRISK and log RISKMD are equal, despite the fact that individual t statistics indicate that the former is insignificant, while the latter is strongly statistically significant.[44]

The regressions of this section were repeated after the sample was allowed to include persons whose WTP exceeds 5% of household income but is less than 12.5% of household income. If zero WTP responses are discarded, the coefficient on log absolute risk is 0.15 when subjective risk is treated as exogenous, and 0.21 with 2SLS. Both coefficients are been significant at the 5% level. If zero WTP is replaced by WTP=2, then the elasticity of WTP with respect to absolute risk reduction is equal to 0.30 when OLS is used, and to 0.51 when 2SLS is used.

As we pointed out in section B of this chapter, respondents in this study were first asked to report their WTP for a reduction in their own risk of dying for any cause. The risk of dying for any cause, AEGRISK, is well predicted by age and by the respondent's self-assessed health on a scale from 0 to 100, but there is little evidence of a relationship between WTP and absolute risk, whether or not we treat AEGRISK as endogenous with WTP. The test of the null hypotheses of exogeneity of AEGRISK does not reject the null at the conventional levels.

G. Robustness of results to excluding large subjective risks

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[43] To test the null of exogeneity of log subjective risk, we enter the residual from the first-stage regression of log DEGRISK on a set of instruments (regression reported in table 9.8) in the right-hand side of the equation for log WTP. When log DEGRISK and log RISKMD are restricted to have equal coefficients, the test of exogeneity, which is the t statistic for the coefficients on these residuals, is -1.85, which rejects the null at the 10% level, but not at the 5% level. When log DEGRISK and log RISKMD are allowed to have potentially different coefficients, the test of the null of exogeneity is -0.85, which does not reject the null at the conventional levels.

[44] We also repeated the regression of table 3 of the Persson et al article, which relies on the sample used for table 8, and on a linear model of WTP. We specified WTP to be a linear function of income per household member, kilometers traveled by car in one year, absolute risk reduction, education dummies and variables measuring the numbers of children and adults in the household. While WTP is well predicted by income and kilometers traveled by car, the OLS estimate of the coefficient on absolute risk reduction is *negative* and very small (-0.077) and statistically insignificant (t statistic -0.76). Two-stages least squares result in an estimate that has the correct sign (71.27) and a somewhat better significance level (t statistic 1.67).

Table 9.10 reports regression results based on the full set of data cleaning criteria devised by Persson et al., including the requirement that respondents with absolute risk reductions greater than 10 in 100,000 be excluded from the sample.

In column (A), we report the OLS regression in table 1 of the Persson et al. article. Column (B) reports the results of a specification that includes absolute risk reductions as well as other variables. As mentioned, one would expect the coefficient of the risk reduction to be positive and significant if WTP is to satisfy the scope effect requirement (i.e., WTP must increase with the size of the absolute risk reduction) and if a VSL is to be computed based on the survey responses. The specification in column (B) also controls for other individual characteristics of the respondent thought to influence WTP.

Despite repeating exactly the same data cleaning as in Persson et al., the sample size I obtained is larger than that in the Persson et al paper, and the regression coefficients somewhat different. Restricting the coefficients of log RISKMD and log DEGRISK to be equal, which implies that log WTP depends on log ABSRISK, and treating DEGRISK as exogenous, results in a regression coefficient on log ABSRISK of about 0.15. This time, the instrumental variable procedure results in an even *smaller* coefficient on log ABSRISK, and in a weaker statistical association with log WTP (the t statistics dropping from 2.15 to 1.65). This suggests that WTP increases only weakly with the size of the risk reduction, and is, again, less than proportional to it. Further inspection of table 9.10 reveals that there is little evidence that the subjective risk is endogenous with WTP, and that people respond differently to baseline risk and relative risk reductions.

When the sample is augmented by including observations where WTP was originally zero, and has been recoded to 2, the results (shown in columns (D)-(E) of table 9.10) suggest instead that individual *do* respond to absolute risk reductions. Evidence that subjective risk is endogenous is still relatively weak (the t test on the residuals from the first-stage residuals from the log DEGRISK equation is only -1.83), but the 2SLS procedure results in a coefficient on log absolute risk that is 50% larger than its OLS counterpart.

What emerges from this section is, therefore, that it is important to test for possible endogeneity of subjective risks with WTP. It is also important to check for implausibly large and small risk values, and to keep in mind that excluding such risk values from the sample tends to affect the results. Regression relationships, however, do not necessarily become more stable, or result in more significant associations with WTP, once the sample is purged of very large subjective risk values. In some cases, this could be due to the loss of variability in one of the key regressors of WTP.

Table 9.10. Dependent variable: ldwp (=log WTP). No observations with absolute risk reduction greater than 10 in 100,000. T stats in parentheses.

	Zero WTP excluded			Zero WTP replaced with WTP=2		
	A Persson et al. (OLS) (N=439)	B Alberini, exogenous risk (OLS) (N=557)	C Alberini, endog. risk (2SLS) (N=481)	D Persson et al. (OLS) (N=662)	E Alberini, exogenous risk (OLS) (N=642)	F Alberini, endog. risk (2SLS) (N=550)
Intercept	3.1714 (3.13)	0.4085 (0.29)	-0.2515 (-0.17)	5.324** (2.45)	-0.4279 (-0.18)	-1.7805 (-0.69)
Log income per household member	0.237* (3.22)	0.2206* (2.15)	0.2808** (2.48)	0.298^ (1.76)	0.3845* (2.16)	0.4237* (2.14)
Log km traveled in car	0.475** (3.28)	0.3265** (3.32)	0.2742** (2.78)	0.607** (2.49)	0.5134** (3.21)	0.4626** (2.71)
Log RISKMD	0.183* (2.06)			0.393** (2.58)		
Log DEGRISK				0.217** (2.67)		
Log ABSRISK		0.1488* (2.15)	0.1659^ (1.64)		0.3057** (2.58)	0.4535** (2.59)
Injured in accident (dummy)	0.286 (1.92)	0.1453 (1.03)	0.2553^ (1.72)		0.3274 (1.33)	0.4301^ (1.66)
Log age		0.2093 (1.13)	0.2576 (1.32)	-0.791** (-2.73)	-0.5661^ (-1.85)	-0.3675 (-1.11)
Log age squared		0.0042 (0.14)	0.0090 (0.27)	0.217** (2.67)	-0.0178 (-0.35)	-0.0093 (-0.17)
High school diploma		0.2362 (1.45)	0.2430 (1.36)		-0.0217 (-0.08)	0.1848 (0.61)
College degree		0.2687 (1.63)	0.2235 (1.23)		0.2017 (0.71)	0.4032 (1.30)
Household members ages 0-3		-0.0010 (-0.01)	-0.0106 (-0.12)		0.1471 (0.62)	0.0782 (0.32)
Household members ages 4-10		0.0957 (1.07)	0.1271 (1.36)		0.1849 (1.18)	0.2617 (1.58)
Household members ages 11-17		-0.1264 (-1.24)	-0.0807 (-0.80)		-0.0799 (-0.46)	-0.0242 (-0.13)
Household members ages 18+		0.1598* (2.09)	0.2040** (2.50)		0.0290 (0.22)	0.1430 (1.03)
Test of the null of exogeneity of subjective risk			-0.90 does not reject null			-1.83 rejects null at the 10% level
Test of the null that b2=b3			F=2.40 Pval=0.12			F=0.09 Pval=0.77

Note: the Alberini specifications restrict the coefficients of log RISKMD and log DEGRISK to be equal, which means that log ABSRISK is entered in the equation.

Conclusions

- When baseline risks and the risk reductions are subjectively reported by the respondents, it is important that regressions be run that relate WTP to the size of the risk reduction.
- These regressions should test for the possible endogeneity of the risks, or risk reductions, with WTP. In one of the two applications examined in this chapter, there was no evidence of endogeneity of baseline risks and WTP. In the other, there was evidence of such endogeneity, and the coefficient on log absolute risk increased when these two variables were explicitly treated as endogenous, providing support to the notion that WTP does increase with the risk reduction (sensitivity to scope).

10. SAMPLE SELECTION ISSUES

Chapter 2 discussed possible ways to control for sample selection issues. In practice, although a number of studies appear to have at least checked for possible self-selection bias (including Gerking et al., 1988, Lanoie et al., 1995, Persson et al., 2001, and Corso et al., 2001), we have not been able to obtain the variables that are necessary to run our own models of participation into the survey.

Gerking et al's study used a mail survey to elicit information about WTP and WTA for changes in occupational risks. The survey questionnaires were mailed to a random sample of 3000 US residents, and to an additional sample of 3000 respondents, randomly selected among the residents of 105 US counties with disproportionately large concentrations of high-risk industries. The ages, income, education levels, and other characteristics of those who elected to fill out and return the questionnaires can therefore be compared with those of the US population, using Census and Current Population Survey data. If the researchers kept track of the addresses of the mail questionnaires who did not return the questionnaire, it would have been possible to check, using multivariate probit regressions, whether participation in the survey is more likely in areas—such as Census tracts, zipcodes or counties—where the residents have certain characteristics. Indeed, some analysis along these lines were reported in Gegax et al. (1987), the article on the companion compensating wage study, but the data are no longer available for our use. Gegax et al. (1985) also plot the response rate against the day of the study, and the baseline risks of the samples from the general population, and from the residents of the counties with high concentration of risky industries.

Similarly, Persson et al. mail questionnaires to a random sample of Swedes of ages 18-74. The first mailing of the questionnaire was in March 1998, and was followed by two follow-up remainders in hopes of raising the return rate. The overall return rate is 51%. In addition, to check for possible selection into the sample, the authors sent out 2645 "drop out" questionnaires, 659 of which were eventually filled out and returned. The authors conclude that the final sample is wealthier, drives more miles, and has a higher educational attainment than the average Swede and the typical dropout respondent, but that it does not differ from the Swedish population and dropout respondents in terms of gender and access to a car.

Corso et al. (2001) check that if samples of respondents that received survey materials with different visual aids have similar individual characteristics. Since they find that the composition of the samples is similar, this allows them to conclude that any differences in WTP must be attributed to the experimental treatment, which is the type of visual aid.

11. QUESTIONNAIRES

We examined four questionnaires from selected CV studies about mortality risks. We present a detailed discussion of these questionnaires in Appendix C, and the questionnaires from selected CV surveys are included in Appendix D. Two of these questionnaires (Johannesson and Johansson, 1996; Johannesson et al., 1997) were administered over the phone. Persson et al. and Gerking et al. conducted mail surveys, while the Corso et al. questionnaire was administered through (i) an initial phone contact, (ii) mailing of survey packets, and (iii) phone re-contact to elicit the answers to the questions. By contrast, Krupnick et al. brought respondents into a centralized facility to take a self-administered computer survey, and in Alberini et al. the same questionnaire was administered by Web-TV.

Inspection of these questionnaires reveal that they are very different in length. For example, the Johannesson and Johansson (1996) and the **Johannesson** et al. (1997) questionnaires are short, contain no warm-up questions, and jump right to the matter at the heart of the study—an extension in expected remaining lifetime at age 75, and a reduction in the risk of dying over the next year, respectively. The fact that the survey was done over the phone precludes the use of visuals to depict risks. Respondents are not asked to practice with the concept of probability, and no debriefing questions are asked to test comprehension of probabilities and acceptance of the scenario. Information about education, income and other basic sociodemographics characteristics of the respondent is gathered, but there are no questions about risk reducing behaviors or attitudes.

Even more important, people are told that about the probability of dying, presumably for all causes, but the risk reduction is described as delivered by a medical intervention, without explaining which illnesses or cause of deaths this medical intervention would address. Finally, it is not clear whether the payment would be out of one's pocket, would be in the form of a copay to the national health care system, or could have been construed to be entirely covered by the national health care system. The questionnaire does not investigate reasons for answering either "yes" or "no" to the payment questions, but does ask whether people were absolutely certain that they would make the payment.

The **Persson** et al. questionnaire is a nice example of a mortality risk questionnaire. The context is road transportation risks, and the questions were asked in such a way that virtually no skip pattern (which is sometimes confusing in a self-administered mail survey) had to be created. For example, people were asked to indicate their own risk, and the risk reduction was a specified percentage. Respondents, however, are not explicitly instructed to write down the absolute risk change corresponding to the indicated percentage (e.g., "30% risk reduction, implying that your risk would be reduced by ____"), which could be one of the reasons why—as shown in section 9—people may have paid more attention to the percentage *per se*, rather than the absolute risk reduction.

One attractive feature of this questionnaire is before assessing their subjective risks the respondents are instructed to think of their own health, miles traveled, and how they drive. Presumably, they were already focused on the latter, since they had answered several questions about them.

Another attractive feature of the Persson et al. questionnaire is that respondents are shown the risks of death for all causes for a reference person (a 50-year old) on graph paper, with areas of different shades of gray representing the risks of dying for various causes, such as heart diseases, cancer, and traffic accident. These were annual risks, expressed as X in 100,000, and shown on graph paper.

The questionnaire begins with simple questions about the use of safety devices (such as seatbelts and bike helmets), moving quickly through detailed questions about commuting and travel patterns for each possible mode of transport, broken down by winter and summer. Three questions are dedicated to finding out whether the respondent has been in a transportation accident, and how recently.

Just before the risk information portion of the survey, people are asked to assess their own health status using the "health thermometer." This device appears simple and appropriate for a mail survey where the main focus is the risk of dying in road traffic accident. (It would be too simple, and not disaggregate enough to be able to establish the relevant health status, in a survey about risks from heart disease, for example.)

The risk reduction is a private commodity and the risk reduction scenario is abstract. This has the advantage of keeping respondents focused on key aspects of the risk reduction itself (e.g., one year only, death risks only, risk reduction for the respondent only, no inconvenience associated with the use of safety devices) and on their budget constraint. This abstract scenario seems well suited for valuing traffic-related risks, where it has the additional advantage of potentially catering to other modes of transportation, and not just to automobiles.

By contrast, the abstract risk reduction scenario posited for the general risk reduction is unconvincing. Blending "safety equipment and preventive health care" seems a rather vague way of promising a reduction in one's overall risk of death. Perhaps this is one reason why WTP for a change in the overall risk of death fails to be significantly related to the size of the risk change.

This questionnaire took great pains to remind respondents to consider their budget constraint when answering the WTP questions, and reinforced this notion after the WTP questions by asking respondents to indicate how they would reallocate the household's expenditures to afford to pay for the risk reduction.

The questionnaire used for the **Corso** et al. study varied the visual aids across respondents in order to find out what works best for communicating risks, measuring performance in terms of internal validity of the WTP responses. The questionnaire itself, however, is comprised of six sections that value risks of completely different nature, including the risk of (i) contracting and dying for a rare by serious foodborne illness, (ii) dying in an auto accident, and (iii) contracting hepatitis and HIV through blood transfusion. The questionnaire also elicits WTP for (iv) an extension in remaining expected life time, or a reduction in the risk of dying at various ages, via a pneumonia vaccine.

It would seem difficult for most people to able to move through so many risk reductions and to place a value on them within a single survey instrument. Moreover, the magnitude of the risks varied from one section of the questionnaire to the next, ranging from X in 10000 to X in a million. The blood safety of the questionnaire described the risk of contracting hepatitis through a transfusion as X in 10000, and the risk of contracting HIV as X in a million. The former was then re-stated as 100X in a million. Keeping a track of all of this would seem rather difficult even for mathematically talented respondents with the survey materials in front of them.

Regarding the visual aids, risk ladders are generally useful in assessing the magnitude of risk changes, but the ones used in this questionnaire are a bit cluttered and the information about the “community risk” equivalents (e.g., 1 in 1000 is one in a village) is distracting. It is, therefore, not surprising that more abstract ways to depict risks seemed to have worked better.

The **Gerking** et al. and the **Lanoie** et al. questionnaire focus on workplace risks and are relatively similar to one another. After eliciting initial information about the occupation and industry of the respondent, Gerking et al. inquire about the respondent’s subjective assessment of various types of occupational risks, ranging from fires and explosions to the risk of being crushed by equipment. They use a risk ladder to elicit the respondent’s subjective assessment of his or her own workplace risk.

The ladder represents risks ranging from 1 to 10 in 4000 per year, with each step of the ladder representing a 1 in 4000 increment in risk, and respondents are asked to indicate on a payment card the amount they would give up (accept) in earnings to reduce (increase) risks by one step. The risk ladder is simple, clear, and easy to interpret. The risk reduction is abstract, in the sense that no specifics are provided to indicate how exactly the risk reduction would be attained. We did not find this to be a problem, and we thought it actually helped keeping the respondent focused on the risk reduction *per se*. Extensive questioning is dedicated to wages, income, other aspects of the workplace and occupation, experience, and education.

The Lanoie et al. adopts the risk ladder approach, but with revised risk figures, and could be considered as a more streamlined variant of the Gerking et al. questionnaire. It differs from the latter in that respondents are educated about risks using an auto safety example, and that hypothetical questions are included that elicit WTP for a reduction in occupational risks by 2 in 10,000. An advantage of this questionnaire is that the respondent is clearly told that the risk reduction would halve the baseline risk. The questions about hypothetical risk reductions ask respondents to imagine being construction workers (or other high risk occupation) facing a specified risk level.

Conclusions

- Visual aids should be as simple and uncluttered as possible. In one instance, additional details that were meant to help understand the magnitude of the risks were, in fact, distracting.

- In some cases, it may be possible to successfully craft abstract or stylized risk reduction scenarios. The advantage of abstract risk reduction scenarios is that they help keep the respondent focused on the magnitude of the risk and risk reductions, without being distracted by other concerns, such as the specifics of the risk reduction delivery mechanism.

- The survey instrument should not attempt to elicit WTP for several types of risk reductions, e.g. risks in the transportation and food safety contexts. These risks are likely to differ in magnitude and to require setting up alternative hypothetical scenarios, resulting in (i) a heavy cognitive burden on the respondent, (ii) the possible loss of credibility of the survey, and (iii) diminished focus and increased fatigue on the part of the respondent.

- Survey instruments that focus on WTP for reductions in the risk of dying for specific causes (e.g., auto accident, or heart disease) should also educate the respondent about the magnitude of these risks when compared to other causes of deaths.

- When stating the risk reduction to the respondent, it is important to also explain what percentage of the baseline risk this reduction represents.

12. CONCLUSIONS

This research project has re-analyzed the data collected through a number of contingent valuation studies eliciting WTP for mortality risk reductions. It has also collected questionnaires and survey materials. Our conclusions are as follows.

1. Given that the estimates of the VSL and the WTP regression coefficients are often sensitive to the specification of the model, it is imperative that the data collected through contingent valuation studies be archived with full documentation for possible re-examination on the part of the Agency.

2. When one-shot dichotomous choice questions are used to elicit information about WTP, the estimates of mean WTP can be large and unreliable, even if the sample size is large, when one works with a skewed distribution of WTP.

This is because of three concurrent factors. First, the estimate of mean WTP depends crucially on the shape of the upper tail of the distribution of WTP. This means that any distributional choices that do not match well the observed frequencies may have a large effect on the estimates of mean WTP. Second, the estimates of mean WTP are sensitive to the more or less complete coverage of the range of WTP by the selected bid amounts. Third, outliers can dramatically alter the estimates of mean and median WTP.

When one works with symmetric distributions of WTP, in some cases (i.e., when many "no" responses are observed for low bid values) the estimated mean is negative.

These difficulties suggest that information about WTP should be refined using follow-up questions, although we recognize that doing so may no longer constitute an incentive-compatible elicitation procedure.

3. Median WTP is more robust to the choice of the distributional assumption, to outliers, and to the possible presence of undesirable response effects, such as completely random responses to the payment question, than mean WTP. This result is not unique to CV surveys about mortality risks. Choosing median WTP is justifiable in that it has a natural majority vote interpretation, from a welfare perspective it provides a more conservative measure of the VSL.

4. Contingent valuation researchers (Carson, 2000) worry about the presence of "yea-sayers," "nay-sayers," and persons who provide completely random responses. In this research, we attempted to account for their presence using (discrete) mixture models that do not rely on or require any additional information coming from the responses to debriefing questions or other questions in the survey. In practice, these models often failed to identify any contaminating component. When they did, they pointed to an implausibly high likelihood that an observation reflects one of the abovementioned undesirable response behaviors.

These findings should be interpreted in the light of related research (Alberini and Carson, 2001) suggesting that identification of mixtures with yea-saying etc. is difficult, and works well only when the mixture is specified correctly, and the contaminating component is not negligible. This suggests that questionnaires incorporate questions that aid in identifying potential yea-sayers, etc. and that the econometric analysis examines alternate usable cleaned samples on the basis of the answers to these questions.

5. Researchers should check for observations such that WTP accounts for a large share of household income. In addition to increasing the estimates of mean WTP, these observations may affect the estimate of income elasticity of WTP.

Large WTP amounts relative to one's income could be caused by respondent failure to understand aspects of the provision of the risk reduction, failure to give proper consideration to the budget constraint, or poor measurement of income. Reminders of the budget constraint before the valuation question and instructions on how to compute total personal or household income may alleviate some of these problems. These reminders will not, however, prevent a small percentage of respondent from deliberately misreporting their income. Auxiliary regressions of income on education and age may help uncover some of these problems, and suggest that aberrant observations be excluded from the sample, or their income replaced with an imputed value.

6. When respondents are asked to estimate their own subjective risks and/or risk reductions, it is important to check whether WTP and subjective risks are endogenous. Endogeneity in this case would be driven by the presence of unobserved individual factors that are common to both WTP and subjective risks. In one of the two examples presented in this report, we found that accounting for endogeneity of risks and WTP improved the sensitivity of WTP to the size of the risk reduction, which is an important internal validity criterion.

7. Researchers have typically asked respondents in a contingent valuation surveys to value risk reductions expressed in one of two possible ways. The first is an absolute risk reduction (e.g., 5 in 10000), and the second is a relative risk reduction (e.g., reduce the baseline risk by 30%). The analysis of the Persson et al. data, where the risk reduction was expressed as a stated percentage of the subjective baseline risks, suggests that WTP was driven by the relative risk reduction, and not by the absolute risk reduction. This is problematic, because it is not clear how VSL would be calculated.

We would recommend that researchers express risk reductions in both absolute *and* relative terms. For example, they may say that the risk reduction is "5 in 10000. This represents a 30% reduction in your risk of dying." Of the questionnaire that we examined, only one, the Lanoie et al. questionnaire, explicitly reminds respondents that 2 in 10,000, the risk reduction they are to value, is a 50% risk reduction, since the baseline is 4 in 10,000.

8. We endorse the practice of showing the respondents one's risk of death for a specific cause (e.g., traffic accidents) in the context of the risk of dying for all causes, and for other specific causes. This was done, for example, in the Persson et al. questionnaire.

9. A number of studies ask people to rate their health, using a scale from 1 to 10 (Johannesson et al., 1997) or 0 to 100 (Persson et al., 2001), or response categories like "excellent," "very good" etc. relative to other people the same age. For the most part, current health has not proven to be a strong predictor of WTP, although it generally correlates well with one's own subjective risks of dying.

If, for policy purposes, it is deemed important to see if WTP for risk reductions depends on health status, future studies might consider oversampling among the chronically ill or persons with specified ailments to maximize the "contrast" with respect to the rest of the population. If possible, this stratification of the sample should be done on the grounds of physician-diagnosed illnesses, rather than on the basis of self-assessed health status.

10. Comparison of the visual aids used in various studies (e.g., Corso et al., Persson et al., Krupnick et al.) suggests that it is best to keep the visual depiction of risk as simple as possible. We appreciate the effort by Corso et al. to help the respondent digest the magnitude of the risks by thinking of the frequency of deaths in a community of the appropriate size, but we found the added icons and language distracting.

11. Mortality risks can be a delicate matter. We prefer to avoid mentioning the risk of dying in the title of the survey instrument or on the cover page of the questionnaire. This is better left to the middle of the survey, after the respondent has been "warmed up" and guided through exercises about probabilities. Focus groups and qualitative research should be devoted to finding out how risks can be presented in a non-offensive, meaningful way to minorities and the elderly.

When the Krupnick et al questionnaire was administered, after being translated into Italian, to a sample of Italians, several of the elderly respondents that had been recruited for the study left in the middle of the survey, feeling offended by the topic of the survey (death).

12. Mortality risk reduction scenarios can frame the risk reduction to be delivered by a public program, or as a private good. When choosing the private good route, it is important to craft the scenario in such a way that the respondent does not question the legitimacy, social and medical acceptability of the proposed risk reduction. To elaborate on this matter, it seems surprising that the Johannesson et al (1997) questionnaire mentions a medical intervention that would reduce the risk of dying, but does not reassure respondents that this is approved by the national health care system, and does not say anything about the payments being out-of-pocket, co-pays, etc.

13. When attempting to answer questions about the relationship between WTP and, say, age, researchers should keep in mind that results can be affected by the choice of the econometric model (e.g., the distribution of WTP) and of the welfare statistic. For

example, the curvature of the inverted-U shape identified by Johannesson et al. (1997) would have been much more pronounced, with the elderly reporting much lower values than people in their 40s, had they used a lognormal distribution of WTP, and had they focused on median WTP, instead of mean WTP.

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Appendix A.

Summaries of Articles.

Summary form 1

Author(s) Persson, Ulf, Anna Norinder, Krister Hjalte and Katarina Gralen

Year of publication 2001

Title "The Value of a Statistical Life in Transport: Findings from a New Contingent Valuation Study in Sweden"

Journal Journal of Risk and Uncertainty, 23(2), 121-134.

Valuation method contingent valuation

Type of risk valued risk of death in transportation accidents (road traffic) and risk of non-fatal injury in road-traffic accidents

Magnitude of the risk baseline risk expressed as X in 100,000 per year

Magnitude of the risk reduction X in 100,000 annual

Population adults in Sweden ages 18-74

Sampling frame not specified

Survey method mail

Response rate 5650 questionnaires mailed out; two reminders; 2884 returned questionnaire, for a return rate of 51%. There was a "drop out" questionnaire (an in-depth follow-up for those respondents who did not return the questionnaire after the first mailing?), which resulted in 659 answered drop out questionnaires.

When first mailing in March 1998

WTP elicitation method open ended

Subjective or objective baseline risks? Both. Respondents were told what the risks for the average person their gender and age are, but then are asked to report their perceived risks, considering how often they are exposed to traffic, distance traveled, mode of transport, and how safely the respondent drives.

Commodity being valued reduction in the risk of death. The paper is somewhat ambiguous about this, but from communications with the authors it would appear that the risk reduction to be value is Z% risk reduction from the subjectively assessed baseline risk. Private risk reduction, to be attained by wearing a special device. The payment is intended for one year only, and the risk reduction delivered by the device is for one year only.

Risk communication Grid of 100,000 squares, with black squares representing deaths

Is the questionnaire reported in the article? No—received questionnaire from authors. Questionnaire is in Swedish and is being translated into English.

Is questionnaire Available to us? yes

Experimental Design Two independent subsamples. First subsample values a reduction in the risk of death in road traffic accident. Second subsample values a reduction in the risk of non-fatal injuries in road traffic accident. Of the 2884 returned questionnaire, 935 were about fatal risks, and the remaining (2884-935) were about risk of non-fatal injuries. The paper only reports the results of the mortality risk component of the study (subsample 1).

Within subsample 1, respondents were randomly assigned to one of four possible groups, each group being given a different percentage risk reduction. The proportional risk reductions being assigned to respondents are 10%, 30%, 50% and 99%.

In addition, subample 1 was further subdivided into other groups (presumably, in a manner that is orthogonal to the proportional risk reduction treatment) to test other effects, but no details are reported in the paper about this additional experimental treatments.

Bid Design not applicable (WTP question is open-ended)

Statistical model(s) to estimate mean or median WTP Since WTP is a non-linear function of the absolute risk reduction, mean WTP and hence VSL is predicted for absolute risk reductions of various sizes using a regression model (see below)

WTP Regressions? Yes. Two types of regressions. The first type is a regression model where the dependent variable is logWTP. Both a semi-log and a log-log specification are reported.

The semi-log model only uses observations with WTP>1 SEK (n-439). In the log-log model, zero WTP observations are replaced by a small positive number, and then the log transformation of all (original and revised) WTP amounts is taken (n=662).

The second type is a LAD regression, where the influence of outliers (zero WTP amounts and large WTP amounts) is reduced. This second regression relates WTP to the absolute risk reduction. The absolute risk reduction is equal to (subjective baseline risk × exogenously given percentage risk reduction). N=675.

Regressors Subjective baseline risk, percentage risk reduction, age, age square, income, miles driven or ridden in a car, dummy for prior accident experience (in various combinations, depending on the specification)

Results Based on the more complete specification for the log-log regression, WTP is significantly related to subjective baseline risk and risk reduction, and the coefficients of these variables are positive, as expected. WTP also grows with household income and miles driven, and has an inverted-U relationship with age. The income elasticity of WTP is 0.3, which is in line with other studies of this kind. The semi-log specification suggests that previous accident experience raises WTP, as does the number of miles driven.

The results of the LAD regression suggest that the slope of the relationship between absolute risk reduction and WTP is very steep for very small risk reductions, implying that for very small risk reductions some people will report zero WTP amounts and others will report positive WTP amounts. The function flattens out at higher absolute risk reductions. The results of the LAD model are used to compute WTP, and hence risk reduction, for various levels of absolute risk reductions.

Implied VSL WTP, and hence VSL, is calculated for various absolute risk reductions. Hence, this study does not result in a unique VSL figure. The authors comment that the figure that is closest to the needs for policy purposes is that for risk reduction of 2.4 in 100,000. They also comment that VSL figures are in line with those for US studies and Viscusi's acceptable range.

	ΔR=1.8 in 100,000	ΔR=2.4 in 100,000	ΔR=5 in 100,000
VSL in million Swedish Kroners (SEK)	30.38	24.01	13.17
VSL in million US dollars*	3.59	2.84	1.56

*The exchange rate used in the paper is 6.6 SEK to 1 US dollar.

Other * The final survey was preceded by a 280-person pilot.

* The authors use the sample that excludes respondents whose WTP is greater than 5% of annual household income.

* Analysis of sample selection bias: the sample has higher income, education, miles driven than the Swedish average and than the people who did not return the survey

questionnaire. Access to a car and gender in the sample are roughly the same as for the average Swede. Women tend to drop out (not return the questionnaire) more than men.

* The average subjective risk was 11 in100,000. This is almost twice as much as the average (objective) risk in Sweden, which is 6 in 100,000. Median subjective risk, however, is 3 in 100,000.

* Using income elasticity of WTP, which is 0.237 based on table 1 (but 0.298 based on table 2) the authors estimate that WTP would decrease by 7% if the sample's income was more in line with the Swedish average. Since the average income in the sample is about 30% greater than the Swedish average, the calculation is $0.3\times0.237=0.0711$.

* The specification searches in the regressions were done using stepwise regressions.

* Robustness checks were done by dropping, in turns, one of the groups with specified percentage risk reductions (e.g., the 99% risk reduction group).

Limitations of the Study

Econometric analysis: (1) The subjective risk is entered as a regressor in the WTP equation, but authors do not check if it is simultaneously determined with WTP (possible endogeneity bias). We will attempt to do this if we get the complete dataset.

(2) Somewhat unsatisfactory treatment of the zero WTP responses, which account for 16.81 percent of the sample.

(3) No attempt to identify determinants of zero WTP responses.

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Summary form 2

Author(s) Johannesson, Magnus and Per-Olov Johansson
Year of publication 1996

Title "To be or Not to Be, That is the Question: An Empirical Study of the WTP for Increased Life Expectancy at an Advanced Age"

Journal Journal of Risk and Uncertainty, 13, 163-174.

Valuation method contingent valuation

Type of risk valued An extension of one year to the expected remaining life at age 75

Magnitude of the risk the probability of surviving to age 75 is expressed in percentage terms (e.g., 75 percent for a woman of age 18-39)

Magnitude of the risk reduction Not a risk reduction. The good to be valued is a one-year extension in remaining lifetime at age 75.

Population Adults in Sweden ages 18-69

Sampling frame "Random sample of individuals in the age 18-69 group."

Survey method Telephone.

Response rate 82% (2455 contacted, 2013 completed surveys).

When June 1995

WTP elicitation method Single-bounded dichotomous choice. In addition to offering the standard response categories (yes and no), people were subsequently asked whether they felt "totally sure whether they would pay or not."

subjective or objective baseline risks? Objective. The probability of surviving to age 75 is given For the average person of the respondent's age and gender.

Commodity being valued A one-year extension of the expected remaining life at age 75, which is 10 (hence the respondent, if he or she reached age 75, would expect to live an additional 11 years). This extension would be obtained through a privately purchased medical treatment.

Is the questionnaire reported in the article? No, but the payment question is reported: "The chance for a man/woman of your age to become at least 75 years old is X percent. On average, a 75-year-old lives for another ten years. Assume that if you survive to the age of 75 year you are given the possibility to undergo a medical treatment. The medical treatment is expected to increase your expected remaining length of life to 11 years. Would you choose to buy this treatment if it costs SEKC and has to be paid for this year? Yes/no"

Do we have questionnaire Available to us? Yes, in Swedish (to be translated)

Risk communication No visuals (the survey was done over the telephone).

Experimental Design The probability of surviving to age 75 is assigned to each person, and varies with age and gender.

Bid Design Six bid values: 100, 500, 1000, 5000, 15000, and 50000, equivalent to about 14, 69, 138, 689, 2069 and 6896 US dollars at the exchange rate of August 1995, which is 7.25 SEK to 1 US dollar). Bids cover only upper tail of the distribution of WTP, since the percentage of "yes" responses to the bid amount varies from 53% to 9%.

Statistical model used To estimate mean or Median WTP The authors first fit a standard logit model of the yes/no responses, the bid value being the only regressor. Then they compute the expected value of WTP, conditional on WTP being positive. The expression for mean WTP based on this approach is $(-1/\beta)\ln[1+\exp(\alpha)]$, where α is the logit intercept and β is the coefficient on bid from the logit model.

In practice, given their interest in the relationship between WTP and age of the respondent, they estimate mean WTP using this approach for each of three age groups, after separating the sample into the three groups and fitting separate logit regressions.

All regressions were done using the yes/no responses, plus conservative recodes (only those people who said that they were sure that they would pay were assigned a "yes").

WTP Regressions? Yes. Standard logit model with bid plus other regressors (see below).

Regressors Bid, age, personal income, education, and gender.

Results The logit regressions indicate that the likelihood of answering "yes" to the payment question is significantly and negatively related to the bid level (as expected), positively and significantly related to age, positively and significantly related to income, and positively and significantly related to an education (dummy equal to one if respondent has at least completed high school). Gender was insignificant.

Implied VSL Using the standard yes/no responses and the parametric approach based on the logit, mean WTP is predicted to be 8113 SEK in the 18-34 age group, 10208 SEK in the 35-51 year-old group, and 11707 SEK in the 52-69 year old group. (The age cut-offs for these groups were selected so as to ensure groups of roughly the same size.)

This is equivalent to US$ 1119 (1995 dollars), 1408 and 1615, respectively, and to VSL ("according to a rough estimate") of about $93,000, $106,000, and 121,000.

Average WTP for all sample is 9787 SEK, or $1350 (1995 US dollars). This is roughly equivalent to a VSL of $101,000.

Estimates based on conservative recodes are 25% to 60% the estimates based on the standard yes/no responses, depending on the age groups, whereas estimates based on a non-parametric approach (see Kristrom, 1990) are 70-80 percent those of the standard approach.

Other: The authors also estimate the implicit discount rate, based on the comparison across WTP of respondents of different ages. Specifically, based on the standard responses and the logit-based approach, discount rates are 1.3 percent in the 18-34 age group, and 0.4% in the 35-52 age group. The discount rates are higher when the conservative recodes of the responses are used (roughly 3% in both groups).

Limitations of the study * The survey was done over the telephone, so no visuals were used to aid in the explanation of the probability of surviving until age 75.

* People were not asked whether they accepted the baseline survival probabilities or though their own probabilities were higher or lower than the average person of the same gender and age. If people were thinking of different probabilities of survival than those stated to them in the survey, an econometric model relating WTP to baseline survival probabilities would suffer from the problem of regressors measured with errors, which typically results in downward biased estimates. It should be noted that the authors did not attempt this model.

* The explanation of the commodity is somewhat unclear, at least in English translation, and one wonders whether people may have thought of a certain extension of their lifetime, rather than a change in life expectancy. Also, the concept of life expectancy was not defined to the respondent.

* The bids are rather high, relatively to Swedish income, and one wonders whether asking people about bids this high may have created undesirable response effects.

* The estimation approach for mean WTP is inappropriate. If first allows negative WTP values, and then sets them to zero, resulting in inflated mean WTP.

* the questionnaire asks quality of life questions, but does not ask questions about the health status of the respondent, which could affect their acceptance of the survival probabilities.

* no attempt to elicit subjective survival probabilities.

Summary form 3

Author(s) Johannesson, Magnus, Per-Olov Johansson, and Karl-Gustav Löfgren

Year of publication 1997

Title "On the Value of Changes in Life Expectancy: Blips Versus Parametric Changes"

Journal Journal of Risk and Uncertainty, 15, 221-239.

Valuation method contingent valuation

Type of risk valued risk of dying over the next year

Magnitude of the risk baseline risk is expressed as X in 10,000 annual, and varies by gender and age

Magnitude of the risk reduction 2 in 10,000 over the next year for everyone in the sample

Population adults in Sweden ages 18-74

Sampling frame "Random sample of Swedes between the ages of 18 and 74 years"

Survey method Telephone

When Sept. to Nov. 1996

elicitation method Single-bounded dichotomous choice

subjective or objective baseline risks? Objective only. Respondents are told what the risks of death over the next year is for the average person of their gender and age.

Commodity being valued A reduction of 2 in 10,000 from the (objective) baseline risk. The risk reduction is <u>private</u> and would be delivered by a treatment. The effect of the treatment is one year only.

This allows one to estimate the WTP for a reduction in the risk of death that lasts only in the year to come ("blip"). The authors also try to use the responses about WTP for the blip to infer the value of a "parametric change," i.e., "a change in the hazard rate that pertains throughout

Is the questionnaire

reported in the article? No, but the WTP question is: “It is estimated that X(Y) men (women) out of 10,000 in the same age as you will die during the next year. Assume that you could participate in a preventive and painless treatment which would reduce the risk that you will die during the next year, but has no effects beyond that year. The treatment reduces the risk of your dying during the next year from X(Y) to X-2 (Y-2) out of 10,000. Would you at present choose to buy this treatment if it costs SEK I? yes/no”

Do we have questionnaire Available to us? Yes, being translated from Swedish.

Risk Communication No visuals, as the survey was done over the telephone.

Experimental Design baseline risk varies systematically with gender and age; absolute risk reduction is the same for everyone.

Bid Design Six bids: 300, 500, 1000, 2000, 5000 and 10000 SEK, equivalent to US $45, 76, 151, 303, 757 and 1515 (at the 1996 exchange rate of 6.6 SEK to 1 US $). After 1000 surveys, the 2000 SEK bid was replaced by 10000 SEK. Bids cover only upper tail of the distribution of WTP, since the percentage of “yes” responses to the bid amount varies from 51% to 7%.

Statistical Model(s) To estimate mean or Median WTP The authors first fit a standard logit model of the yes/no responses, the bid value being the only regressor. Then they compute the expected value of WTP, conditional on WTP being positive. The expression for mean WTP based on this approach is $(-1/\beta)\ln[1+\exp(\alpha)]$, where α is the logit intercept and β is the coefficient on bid from the logit model.

In practice, given their interest in the relationship between WTP and age of the respondent, they estimate mean WTP for various ages using the results of a logit regression (see below).

WTP Regressions? Yes. Logit regressions of the yes/no responses on various covariates.

Regressors Bid, age, age square, quality of life measure, household income, household size, gender, education

Results WTP in an inverted-U function of age, the highest WTP being at age 40.Income, size of the household and quality of life were not found to affect WTP. Education is negatively and significantly related to WTP, and males are willing to pay significantly less for the risk reduction.

Implied VSL WTP for the “blip” risk reduction of 2 in 10,000 is calculated for persons of age 20, 30, 40, 50, 60 and 70, as follows:

Age	Mean WTP in SEK (US $ in parentheses)	Implied WTP in million SEK (million US $ in parentheses)
20	6100 (924)	30.3 (4.59)
30	6900 (1045)	34.6 (5.24)
40	7200 (1091)	36.1 (5.47)
50	6900 (1045)	34.3 (5.19)
60	6000 (909)	29.8 (4.51)
70	4600 (697)	23.3 (3.53)
Average for entire sample	6300 (954)	31.4 (4.75)

The authors conclude that the VSL figures are roughly comparable to the range recommended by Viscusi.

Limitations of the Study

* No mention of debriefing questions to assess comprehension of risks and/or acceptance of the scenario And valuation exercise

* Survey was done over the telephone, precluding the use of visual aids to explain risks and risk reductions

* poor choice of econometric model for computing mean WTP

* failure to test for the relationship between WTP and baseline risks, and possibility that such an econometric model might be affected by an error-in-variable bias

* no mention of robustness checks in the article

Summary form 4

Author(s) Johannesson, Magnus, Per-Olov Johansson, and Richard M. O'Conor

Year of publication 1996

Title "The Value of Provate Safety Versus the Value of Safety "

Journal Journal of Risk and Uncertainty, 13, 263-275.

Valuation method contingent valuation

Type of risk valued risk of dying in a road traffic accident over the next year

Magnitude of the risk baseline risk is the same for everyone in the sample; respondents were told the total number of people that die in Sweden every year in car accidents, but this number was not divided by the Swedish population to produce an individual risk of death figure.

Magnitude of the risk reduction 50% of the baseline risk. People were told that the "risk would be cut in half."

Population Car owners in Sweden

Sampling frame Random sample of Swedes between the ages of 16 and 74 years; only car owners were interviewed. 2000 people contacted, the final sample size (of car owners) was 1067

Survey method Telephone

When Sept.-Oct. 1995

elicitation method Single-bounded (no follow-up), but there are three response categories: 1. no; 2.yes, fairly sure; 3. yes, absolutely sure

subjective or objective baseline risks? Objective only. However, people are asked whether they think their risk is higher, the same as, or lower than the average driver.

Commodity being valued Split sample study. One of the two independent subsample values a public program that reduces risk. Respondents are told that more resources would be devoted to preventing traffic accidents and are read a list of measures such as straightening bends, build safer crossings, etc. This would cut the annual number of fatalities in half.

The other subsample was asked to consider a special safety device, such as airbags, that would have to be installed each year to work, and would cut in half the risk of dying for the respondent and for everyone else traveling in the car. This is a private risk reduction and refers to annual risk.

The authors follow up on earlier research by Jones-Lee et al. (1985) and Viscusi et al. (1988) that focuses on altruism. To explore the issue of altruism, they also ask respondents whether they think they pay more or less than the average car owner. They also ask questions to find out whether the respondent thought the public program would have bring other benefits, in addition to the reduction in traffic fatalities. For example, if people thought that pollution would also be reduced by the program, they might be willing to pay more for the program than if they had not believed there would have been such a spillover.

Is the questionnaire reported in the article? No, but the WTP question is. The text in brackets refers to the version with a public program to reduce risks: "In Sweden, 600 people die annually in traffic. A possible measure to reduce traffic risks is to equip cars with safety equipment, such as airbags. [We can denote more resources to preventing traffic accidents. We can, for instance, straighten out roads, build safer crossings, etc.] Imagine a new type of safety equipment. If this equipment is installed in your car, the risk of dying will be cut in half for you and everyone else traveling in the car [all road users]. This safety equipment must be installed each year to wok. Would you choose to install this safety equipment in yourcar if it will cost you SEK B a year? Yes/no."

Do we have questionnaire Available to us? No.

Risk Communication No visuals, as the survey was done over the telephone.

Experimental Design split sample to test difference in WTP between public program and private risk reduction.

Bid Design Six bids: 200, 1000, 2000, 5000, 10000 and 20000 SEK, equivalent to US $30, 151, 303, 757 and 3030 (at the 1996 exchange rate of 6.6 SEK to 1 US $).

The bids give a good coverage of the range of WTP values. For example, when attention is restricted to the sample valuing the privare risk reduction, at 200 SEK 82% of the respondents is willing to pay the bid amount (66% is willing to pay and is absolutely sure about it), whereas at 20000 SEK only 9% is willing to pay the bid (1% if only absolutely sures are considered). Regarding the public program, 63% is willing to pay SEK 200 (43% if only absolutely sures are examined) and 3% at 20000 SEK (3% if only absolutely sure responses are treated as yes responses). This suggests that WTP for the private risk reduction is actually higher than WTP for the public program.

Statistical Model(s) To estimate mean or Median WTP The authors fit an ordered logit model of the responses, the bid value being the only regressor, because there are three response categories, and these response categories are naturally ordered. The authors recognize that the estimated coefficients of the ordered logit model predict that a certain percentage of the respondents would have a negative WTP amount, but subsequently argue that WTP must be non-negative, at least for the private risk reduction. This leads them to computing E(WTP) after truncating the distribution of WTP and restricting it to the positive semi-axis. The expression for mean WTP based on this approach is $(-1/\beta)\ln[1+\exp(\alpha)]$, where α is the logit intercept and β is the coefficient on bid from the logit model.

WTP Regressions? Yes. Odered logit rgressions of the responses on various covariates, including income, male, the bid amount, household size, age, education, two dummies constructed from the responses to the question about risk being the same as, lower or higher than the average driver, and two dummies based on the answer to the question about effects of the program on the environment. Separate regressions for the public and private risk reduction. Many of the covariates are insignificant. When the coefficients are significant, they seem to be different across the two equations (note that the specification is slightly different for public and private risk, because the questions about the effect of the program on the environment was asked only in the version of the questionnaire that dealt with the public program).

Regressors Bid, age, income, male, household size, education, two dummies constructed from the responses to the question about respondent's risk as compared to the average driver, and two dummies based on the answer to the question about effects of the program on environmental quality.

Results Two sets of estimates of WTP are produced. The first is based on the standard interpretation of the responses (whether fairly or absolutely sure, a yes is a yes), while the other is based on interpreting as yes only the absolutely certain responses. In both cases, mean WTP for the public risk reduction is lower than for the private risk reduction. Only when the conservative coding is used, however, is the difference in mean WTP statistically different.

Coding of the yes responses	(1) Mean WTP for private risk reduction	(2) Mean WTP for public program delivering risk reduction	Are (1) and (2) statistically different from each other?
Conservative	2400 SEK	1300 SEK	Yes
standard	4700 SEK	3900 SEK	No

The authors repeat the calculations for different subsamples:

(a) by including all respondents, even those with missing values for the covariates, which they do by fitting a logit (ordered logit) where the only regressor is the bid. The point estimates of mean WTP are similar to those shown in the table above, but both sets of estimates are statistically different from one another.
(b) By using only the responses where the respondents recognized that there would be no effects on the environment. The results are the same as in (a).

Implied VSL

To compute VSL, the authors first compute the risk reduction implied by the questionnaire: 300 (Half of the 600 annual fatalities)/3,700,000 (the number of households in Sweden, assuming that everyone gets the same risk reduction). This gives 8.1 in 100,000 annual.

On dividing mean WTP by this risk reduction, one gets VSL values ranging from $4.5 million to $8.9 million (private risk reduction) and $2.6 million to $7.4 million (public program).

Interpretation of the results. Altrustic considerations would suggest that WTP for a public program should be higher than that for a private risk reduction of equal magnitude. The authors argue that whether or not this is true depends on the nature of the altruism. Here, they argue that people facing the public program scenario might have underreported their own WTP for fear that, if others were made to pay the former respondent's WTP amounts, their utility would have been lower. In other words, respondents reacting in this way would have pure, rather than paternalistic, altruism.

Limitations of the Study

* Survey was done over the telephone, precluding the use of visual aids to explain risks and risk reductions

* poor choice of econometric model for computing mean WTP

* authors tried to control for the respondent's subjective baseline risk, but it is possible that whether or not the respondent thought he was at higher, lower, or the same risk as the average driver is endogenous with WTP.

* no attempt to identify what kind of respondent believes that the policy has effects on the environment as well as on traffic.

* respondents were told about the total number of fatalities every year in traffic accidents, but were not told about the size of the population in Sweden, so it is unclear that they grasped what the risk was.

*no attempt to see if respondent accepted that a 50% risk reduction is possible.

Summary form 5

Author(s) Lanoie, Paul, Carmen Pedro, and Robert Latour
Year of publication 1995
Title "The Value of A Statistical Life: A Comparison of Two Approaches"
Journal Journal of Risk and Uncertainty, 10, 236-257.

Valuation method contingent valuation and compensating wage study Within the same survey questionnaire

Type of risk valued risk of dying in a workplace accident over one year

Magnitude of the risk subjective baseline risk is identified by the respondent on a ladder with 10 steps, and ranges from 0 to 8.64 in 10,000; objective risk figures are available from the Quebec Compensation Board, vary by occupation, were the number of deaths per 10,000 workers averaged over Jan 1, 1981 to May 31, 1985. The average in this sample is 1.261 in 10,000.

Magnitude of the risk reduction in the CV study, one step down the ladder. Note that the ladder identifies the occupation that is represented at each step of the ladder, but not the actual risk. Only after the WTP and WTA questions, the respondent is told that firefighters (a category shown on step 9 on the ladder, one of the occupations with the highest risks) have a risk of dying of 5.8 per 10,000 per year, and that an office worker has a risk of 0.057 in 10,000, and then he is asked what he thinks his or her risk is.

Population Employees of firms in the Montreal area with 100+ employees. Firms were in the transportation, business and manufacturing industries.

Sampling frame (a) researchers formed the universe of firms in the Montreal area with 100+ employees. (b) they randomly selected 13 of these firms, wishing to interview about 15 employees in each such firm, for a total of about 200 interviews.

Final sample size: n=191

Later in the paper, however, it is reported that there were a total of 16 firms. It is also mentioned that in some firms employees were sent the questionnaire, which suggests there may have been a mix of sampling/administration modes.

The article claims that the sample was representative of the general population of the Montreal area for sex, age, education and marital status.

Survey method in person.
At cooperating firms, notices were posted announcing that the study would be done at specified times. One interviewer was in the workplace lunchroom at the specified times,

randomly contacting employees and asking them to participate in the study. The interviews were conducted in separate, quiet areas.

The questionnaire was available in both English and French. The authors report the response rate to be 69%.[1]

When Not mentioned in the articles. The wage data refer to 1990.

elicitation method in the CV portion of the questionnaire, open-ended.

subjective or objective baseline risks? For workplace risks, in the CV component of the study: Subjective risks. People are asked to pinpoint on a 10-step ladder which step corresponds to their own job risks. Objective risks are available to the researchers and used in the econometric analysis, but they are never shown to the respondents.

For the risk of dying in an auto accident, in the CV component of the study: objective risks. Respondents were asked to report WTP (WTA) for a change in risk from 4 to 2 per 10,000 (2 to 4 per 10,000).

Commodity being valued (a) workplace risks, using a compensating wage study. (b) WTP for reducing risks by one step on the ladder relative to the current risks. (c) WTA compensation to accept in increase by one step on the ladder. (d) WTP for a reduction in the risk of dying in a car accident from 4 to 2 in 10000. (e) WTA for an increase from 2 to 4 per 10,000 in the risk of dying in car accident.

Is the questionnaire reported in the article? No, but the WTP/WTA questions are.

Do we have questionnaire Available to us? Yes.

Risk Communication Step ladder, with risks being represented as follows:

(high risks)	10	dynamiter in a mine
	9	firefighter
	8	metal worker (iron, steel, etc.)
	7	worker in production of chemical products
	6	truckdriver
	5	lumberjack
	4	electrician

[1] It is not entirely clear how the response rate was calculated, due to the possibility of different methods of administration of the questionnaire. Presumably, this should be equal to (Number of completed questionnaires or interviews)/(Number of persons contacted and asked to participate + number of persons who were sent the questionnaire).

	3	driver/salesman
	2	teller and cashier
(low risks)	1	secretary

Experimental Design n/a. No split samples.

Bid Design n/a

Statistical Model(s) The purpose of the paper is to (i) compare VSL estimated in a contingent valuation (both WTP and WTA) with the VSL from a compensating wage study on the same sample.
VSL from the wage-risk study is estimated using a regression model (see below). VSL from the contingent valuation survey is reported directly in table 4 of the paper for the entire sample, and is based presumably on computing the VSL for each individual (his or her WTP or WTA, divided by the risk of the risk change implied by going down or up the risk ladder by one step) and then averaging over the sample (or over certain subsamples).

VSL from the Contingent Valuation questions of the questionnaire, Lanoie et al. study. All figures in million 1986 Can $.

	All sample	Manual workers	Unionized manual workers
WTP car safety	1.570	1.466	1.506
WTA car safety	2.809	2.618	2.073
WTA job safety	26.191	39.222	31.472
WTP job safety	22.968	24.908	27.314
WTP job safety*	24.152	--	--

* based on those respondents who pinpointed the lowest risk on the ladder. In the WTP question, these respondents were asked to imagine that they were a step higher.

The table shows that VSL in the auto accident context is stable across subsamples, and is, as expected, higher when measured using WTA than when measured using WTP. The latter point is true both in the auto and the job accident contexts.

VSL based on WTP for job safety (which is judged more reliable) ranges from Can $ 22 to $27 million, and is thus more variable across groups that VSL in the auto accident context, but less variable than VSL for job safety based on WTA compensation measures.

Dropping three workers whose WTP for going one step down the ladder is 1/3 of their pre-tax income reduces VSL to Can $ 15 million.

Compensating wage regression
Dependent variable log weekly wages before tax

Regressors risk variables: fatal risk rate per 10,000 (DEATH);[2] risk of non-fatal injuries involving at least one day of absence from work (per 10,000) (RISK); SEVERITY (average number of work days lost per compensated accident);[3]

Occupational attibutes: physical exertion, cold, humidity, heat, noise, atmosphere (fumes, odor, dust, etc.). All of these are self-reported and are indices on a scale from 1 to 9.

Individual characteristics: log hours worked, union status (D[4]), age, age squared, experience, supervisor (D), married (D), gainfully employed spouse (D), dependents, gender (D), manual (D), two education dummies (HIGHELM and COLLEGE), three industry dummies (transportation sector, business and other, and manufacturing industry).

Results Compensating wage regressions are attempted for the entire sample (n=162), for manual workers only (n=68), for unionized manual workers (n=63).

In general, the coefficient on the DEATH variable is positive but insignificant for the entire sample, whether DEATH is the objective fatality rate or subjective risk. Focusing on manual workers results in stronger coefficients on the DEATH variable, however measured, but the coefficient is still statistically insignificant. When subjective risks are used and attention is restricted to the unionized manual workers, the coefficient on the DEATH rate thus measured is positive, strongly significant, and robust across different specifications of the regression model. The coefficient ranges from 0.048 to 0.053, and the t statistics range from 2.78 to 3.16.

Implied VSL can only be calculated for the sample of unionized manual workers. The paper reports that VSL ranges from Can $17.3 to $19.2 million, which is high relative to previous Canadian studies.

VSL Comparison across CV and wage-risk study This the focus of the paper. Comparison can only be done for the sample of unionized manual workers. As stated, VSL is Can $ 22 to 27 million from the CV component of the study, and Can $17.3 to $19.2 million from the compensating wage study.

The latter figures fall within the 95% confidence interval around the former, suggesting that the alternative estimates of VSL are not statistically different.

However, the authors also use a bootstrap approach, which shows that VSL is much more unstable. The approach identifies 12 persons that are classified as risk-averse, in the sense that they would be willing to pay more for a risk reduction than is implied by their compensating wage equation. Presumably these people inflate VSL relative to the

[2] In alternative runs, the authors included either the objective or objective workplace risks. The subjective workplace risks are those identified by the respondents on the risk ladder.
[3] The authors point out that Viscusi (1993) emphasizes the importance of including (WC×non-fatal risk rate) in the right-hand side of the econometric model, where WC is worker's compensation. However, in this sample WC is about 15% of the wages for virtually all workers, implying insufficient variation across the sample to use this variable as a regressor. This variable is, therefore, omitted.
[4] D = dummy variable.

remainder of the sample. For example, their weekly wages average Can $778.50 but their WTP for reducing risk by one step is Can $106.00, wheres averages wages and WTP for the sample of 63 unionized manual workers are Can $58.00 and 844.70, respectively.

Positive Aspects and limitations of the Study

- In conducting this study, which is based on the Gegax et al (1988) and Gerking et al. (1987) questionnaire, Lanoie et al. recognize that Gegax et al. and Gerking et al's risk ladder asked respondents to consider workplace risks ranging from 1 in 4000 to 10 in 4000, but the actual US fatality rate of 1 in 10000 was well outside of this range. This resulted in overstating risks, and hence biasing VSL downwards. Their risk ladder is designed very carefully.
- The authors discuss the theoretical reasons why VSL could differ when measured from the compensating wage and the CV contexts.
- Excellent discussions of the reasons why these VSL figures are different from others in Canada and in the literature are provided. The authors are also well aware of the large difference between VSL in the auto and job accident context, and offer reasons for such a difference.
- The authors discuss two key concepts against which they compare the results of their study: reliability and validity. Reliability refers to the closeness/difference of VSL measured using two alternative constructs, such as WTP and WTA. Reliability is examined by looking at the correlation between WTP and WTA for the same phenomenon. Validity refers to regression context but also to the fact that we would expect WTP and WTA for the auto risks to be closer with one another than with either WTP or WTA for job risks.

They find that the cross correlation between WTP and WTA measures for auto and job risks is virtually zero. By contrast, the correlation between WTP and WTA for the auto accident risks is 0.478, and the coefficient of correlation between WTP and WTA for workplace risk reductions is 0.777 (individuals who chose the lowest step on the risk ladder) to 0.827 (all other individuals).

- Despite the interest in correlation coefficient, the paper does not attempt to account for possible correlation within the same individual for WTP from the CV questions and wage rates. No regressions for WTP are reported, and there is no attempt to model the baseline risks as endogenous with either WTP from the CV question or the wage rate.

Summary form 6

Author(s)	James K. Hammitt and John D. Graham
Year of publication	1999
Title	"Willingness to Pay for Health Protection: Inadequate Sensitivity to Probability?"
Journal details	Journal of Risk and Uncertainty, FILL, 33-62.
Valuation method	(i) review of 25 previous contingent valuation studies; (ii) replication of CV study by Johannesson et al. (1997); (iii) two new contingent valuation studies
Type of risk valued	(i) previous studies: various types of risk (ii) replication of Johannesson et al: own risk of dying for any cause in one year (iii) original studies: risk of dying in an auto accident, food safety
Magnitude of the risk	(i) previous studies: ranges from 1 in 20 to 1 in 100,000 (ii) replication of Johannesson et al: in split samples, X in 10,000 and 10X in 100,000 (iii) original surveys: 20 and 25 in 100,000 (auto accident), 1 in 37,000 or 1 in 370,000 (outdoor eating establishment in developing country) v. 1 in 100 million (indoor eating estabslishment in developing country)
Magnitude of the risk reduction	(i) previous studies: varies (ii) replication of Johannesson et al.: 2 in 10,000 or 2 in 100,000 (split samples), with bids divided by 10 to keep them corresponding to the same VSL (iii) original studies: auto accident: 15 in 100,000 or 10 in 100,000, food safety: respondent must choose between the indoor and the outdoor eating establishment, where the cost of the meal is different
Population	(i) previous studies: varies (ii) replication of Johannesson et al.: US residents ages 18-65 (iii) original studies: US residents, ages 18 to 65
Sampling frame	(i) Lit review: varies (ii) replication of Johannesson et al.: Random digit dialing, with quotas to ensure geographical representativeness

	(iii) original studies: same as (ii)
survey method	(i) previous studies: varies (ii) replication of Johannesson et al.:telephone (iii) original CVs: telephone
Elicitation method	(i) previous studies: open ended/payment card, dichotomous choice (ii) replication of JOhannesson et al.: Single bounded dichotomous choice (iii) original studies: survey 1, auto accident, double bounded dichotomous choice; survey 2, auto accident, follow-up question to get closer to the indifferent risk;[5] survey 2, foodborne safety, 1 ½ bound dichotomous choice
Subjective or objective baseline risks?	(i) literature review: mostly objective risks (ii) replication of Johannesson et al.: objective (iii) original studies: objective, although in survey 2 about auto risks, the questionnaire attempts to elicit indifference risks, which, of course, vary over the respondents.
Commodity being valued	(i) literature review: mortality risk reductions in various Contexts, including auto accidents and food safety (ii) replication of Johannesson et al.: reduction in own risk of dying for various causes, where the reduction is delivered by medical intervention (iii) survey 1: air bags of two different types; survey 2, auto accident: airbags, survey 2 food safety, different eating establishments
Is the questionnaire reported in the article?	(i) literature review: no (ii) replication of Johannesson et al.: No, but original questionnaire is available to us; the Hammitt and Graham article spells out the WTP question and the bids (iii) original studies: no, but the valuation questions are usually spelled out
Do we have questionnaire	

[5] If the respondent was willing to pay the proposed bid for the initial risk reduction, he was asked whether he would pay the bid even if the risk was smaller. If the respondent declined to pay the initial bid, he was asked whether he would pay the bid for a larger risk reduction. Respondents who switch response (yes-no and no-yes) imply that the indifference risk is between the first and the second risk reductions stated to them in the survey. Respondent who answered yes-yes are construed to hold indifferent risk values that are smaller than the smaller of the two risk reductions stated to them. Respondents who answered no-no are construed to have indifference risks that are greater than the larger of the two risk reductions stated to them.

Available to us?

No, but we do have the original Johannesson et al. survey Questionnaire

Experimental Design

(i) lit review: varies
(ii) replication of Johannesson et al.: split sample, with one subsample being given risks expressed in X per 10,000, and risk reductions of 2 in 10,000. The other subsample was given risks expressed in 10X in 100,000, and the risk reduction was 2 in 100,000, but the bids were 1/10 of the corresponding bids for the other group, so as to keep the implied VSL the same.
(iii) original surveys. Survey 1. respondents are asked a probability quiz question to see if they understand changes in the denominators the risks are expressed in. Split sample, with one subsample being exposed to analogies, and the other not being exposed to analogies. Analogies were intended to help people grasp the magnitude of risks, and are in terms of distance (inches in a mile), population (cities, empty seats in a full stadium), time (minutes in a year), games of chance (coin flipping). People exposed to the analogies were first asked to rate them in terms of helpfulness understanding risks; questions about risks were re-stated using the analogies, and then people were asked again if they found the analogies helpful.

Survey 2, the only experimental design is different bids.

Bid Design

see above

Statistical model used To estimate mean or Median WTP

(i) lit review: varies
(ii) replication of Johannesson et al: single bounded logit model
(iii) original surveys: double or 1 ½ bounded

WTP Regressions?

(i) lit review: varies
(ii) replication of Johannesson et al study: same logit model as original (regressors: bid, age, age sq., education, income, plus a dummy for the high-bid subsample)
(iii) original surveys: survey 1: separate regressions for persons who answered the probability quiz correctly and incorrectly, analogies exposure and no analogy exposure. Survey 2: separate regressions for persons that are and are not confident in their responses to the payment questions.

Regressors in WTP

Regression see above

Results the goal of the research is to assess (i) sensitivity of WTP to the size of the risk reduction, and (ii) strict proportionality of WTP with respect to risk reduction, using both internal and external tests.

(i) lit review. Scope is more easily satisfied in internal than external tests, but there is a sizeable portion of the sample whose WTP does not change with risk reduction. Proportionality is violated in internal tests. External tests see little responsiveness to the size of the risk reduction and generally fail the proportionality tests.

(ii) replication of Johannesson et al. and original studies. Little responsiveness to risk reduction and no proportionality, despite the use of analogies etc. Things improve slightly for persons that are more confident about their responses to the payment questions.

Implied VSL varies, but in the replication and in the original studies is Generally within the accepted range

Limitations: the authors recognize that part of the problem could be the reliance on telephone surveys. It is possible that analogies and other devices intended to promote understanding of the magnitude of risk and risk reduction will perform differently if paired with visual aids

Other : the study was conducted to investigate scope and proportionality. It nicely discusses three reasons why in the past there has been little responsiveness of WTP to scope, and failure of proportionality. The first reason is that people do not grasp small risks and risk changes; the second that they value perceived risks, and not the objective risks stated to them in the survey. In this case WTP may well vary with perceived risks, but the latter are not explicitly observed by the researchers. Finally, it is possible that people use models other than the expected utility models, but even so WTP should be approx proportional to the risk changes, at least for small risks.

The authors also discuss how WTP should increase with baseline risks, but that this effect should be negligible when the baseline risks are very small, as is the case with most mortality risk reductions studies.

When respondents were asked to state how confident they felt about their responses to the payment questions in original survey 1, 70% stated that they felt very confident, 26% somewhat confident, 3% not too confident, and 1% not at all confident.

Summary form 7

Author(s)	Corso, Phaedra, James K. Hammitt and John D. Graham
Year of publication	2001
Title	"Valuing Mortality Risk Reductions: Using Visual aids to Improve the Validity of Contingent Valuation"
Journal details	Journal of Risk and Uncertainty, 23(2), 165-184.
Valuation method	contingent valuation
Type of risk valued	risk of death in auto accidents
Magnitude of the baseline Risk	2.5 or 2.0 in 10000 in split samples
Magnitude of the risk reduction	1.0 or 0.5 in 10000, directly corresponding to the baseline risks of 2.5 and 2.0, respectively. The final risk, after the risk reduction, is 1.5 in 10000 for both subsamples. Risks and risk reductions are per year.
Population	residents of the US of ages 18 and older
Sampling frame	residents of the US of ages 18 and older, recruited through random digit dialing
Survey method	phone-mail-phone: combination of phone contact, mailing of survey materials, and phone interview. Once participation was secured during the initial phone call, respondents were mailed the survey materials plus $5 compensation. Responses to all questions were collected during the final interview.
Elicitation method	dichotomous choice with dichotomous choice follow-up
Subjective or objective risks?	Respondents were told about objective baseline risks, and asked to value an objective risk reduction. However, they were also asked whether they thought their own risks were higher than the average, the same as the average, or lower than the average (qualitative information only).
Commodity being valued	Reduction in the risk of dying in a car accident. The risk

reduction is delivered by a side-impact airbag, and is thus a private risk reduction.

Payment vehicle increase in the auto insurance annual rate for the next 5 years.

Is the questionnaire reported in the article? No, but two of the three types of visual aids used in the survey (the linear and the logarithmic risk ladder) are reported in the Appendix

Do we have questionnaire Available to us? Yes, but not the survey packet, which contains the visual aids. Two visual aids are reported in the Appendix of the paper, but not the visual aid based on the dots.

Experimental Design two experimental treatments: (i) baseline risk and Accompanying risk reduction (two groups: 2.5 in 10000 → 1.5 in 10000, 2.0 in 10000 → 1.0 in 10000), and (ii) visual aid types (4 levels: linear scale, logarithmic scale, dots, and no visual aids), for a total of 8 independent samples. Within each of these, random assignment of respondent to one of the bid levels described below.

Bid Design Four bid sets total:

Bids in US dollars	Initial bid	If yes	If no
I	50	100	25
II	100	200	50
III	200	400	100
IV*	400	800	200

* This bid set was added during the course of the study, as the first three bid sets appeared to be low.

The bids imply VSL values ranging from $250,000 to $16 million.

Sample size N=1104 total, with 277 in the linear scale group, 288 in the Logarithmic scale group, 264 in the dots group, and 275 in The no aids group.

Statistical model used To estimate mean or Median WTP double-bounded model of WTP, WTP is a lognormal. Median WTP figures are reported in the paper.

WTP Regressions? Yes

Regressors in WTP Regression dummy for the size of the risk change, age, male, income, dummy if airbags are perceived by the respondent as effective, perceived risk dummy, perceived risk dummy × dummy if airbags are thought effective. Regressions are separate for the subsamples with different type of visual used, but pool people with different risks and risk reductions.

Results the purpose of the paper is two-fold. It wishes to
(i) investigate whether failure to understand probabilities is one reason why sometimes WTP for mortality risk reductions sometimes fails to increase with the size of the risk reduction, and/or fails to be proportional to the size of the risk reduction, and
(ii) see if this problem can be mitigated or avoided by using visuals that aid the respondent in grasping the magnitude of the risk and risk changes.

The paper conducts two types of scope tests. The weak scope test is passed if WTP increases significantly with the size of the risk reductions. The strong scope test is passed if WTP is strictly proportional to the size of the risk reduction.

The paper finds that only two of the types of visuals (the Logarithmic risk scale and the use of dots) result in WTP That exhibits both the weak and the strong scope effect.

In the group with the linear risk scale, the coefficient on the dummy for larger risk reduction is positive and significant at the 10% level, implying that WTP barely passes the weak scope test, but not the proportionality test, and in the group with no visual aid, the coefficient on the risk dummy is positive but insignificant.

Relatively few other regressors are found to be significant in the WTP regression. Income is significant only in the regression for the no visual and the dots group. The implied income elasticities of WTP are 0.4704 (no visuals) and 0.4048 (dots).

Another significant regressor is whether side impact Airbags are perceived as effective in reducing risks.

On pooling the data from all groups and using dummies for the type of visual aid used, the authors find that the dots depiction works best, while the others are not different than using no visual aids at all.

The main conclusion is that the dots work well.

Implied VSL

Vary by visual aid group and by the size of the risk and risk reduction. See table below.

VSL in million $

	No aid	Linear scale	Log scale	Dots
1 in 10,000 risk reduction	2.530	3.620	3.370	2.900
0.5 in 10,000 risk reduction	4.700	5.860	4.180	3.180
WTP ratio*	1.08	1.24	1.61[a], [b]	1.82[a], [b]

* based on median WTP. If WTP were truly proportional to the size of the risk reduction, this ratio would be 2.
[a] = WTP increases significantly with the size of the risk reduction (weak scope test)
[b] = WTP is proportional to the size of the risk reduction (= WTP ratio is not statistically different from 2) (strong scope test)

Self-selection into the sample

Comparison with the population of the US indicates that the sample has a slighter lower income than the typical US household (median household income in the sample: $46,000; median household income in the US in 1997: $49,000) and somewhat more highly educated.

Selection into the sample due to interest in the topic is judged as not important, because people were not told what the survey was about before securing their participation.

The four subsamples that were given different visual aids are similar in terms of socio-demographics, belief in airbags, and rating of own risks relative to the stated baseline, suggesting that any difference in WTP are solely due to the visual aid treatment.

Debriefing questions

respondents were asked: (i) if they had frontal impact airbags, (ii) if they thought airbags were effective in preventing fatalities, and (iii) whether they thought their

	own risk of dying in a car accident was higher, the same as, or lower than that stated to them in the questionnaire. After the WTP questions, respondents were asked how confident they felt about their responses to the WTP questions.
Robustness checks	(i) The authors tried Single bounded models, finding that WTP was higher than in the double-bounded models, and that it increased more than proportionally to the size of the risk change. (ii) Weibull WTP models were tried, with similar results as those reported in the paper. (This is unsurprising: the paper reports median WTP, which is generally very similar across weibull and lognormal models. In addition, the typical weibull regression model gives regression coefficients that are virtually identical to those of log normal regressions.) (iii) people were separated into groups based on their degree of confidence when answering WTP questions, but this made no change.
Limitations of the study	(i) Considering that the authors use double-bounded models, the sample sizes are really too small to afford separate samples. This might explain why the group with no visuals and that with the linear scale did not exhibit (or only exhibited very weak) scope effects. (ii) the authors did not attempt to explore what kind of respondents reports that their risks are higher or lower than the risks stated to them, and whether these beliefs are endogenous with WTP. (iii) the dots visuals are not available, so it is not possible to judge their quality. However, based on examining the linear and log scale reported in the Appendix of the paper, which very “busy,” they are probably correct in concluding that the simpler, more abstract dots managed to get respondents more focused on the risks and less distracted by other factors.

Summary form 8

Author(s)	Miller, Ted, and Jagadish Guria
Year of publication	1991
Title	"The Value of a Statistical Life in New Zealand: Market Research on Road Safety"
Journal details	Report to the New Zealand Ministry of Transport
Valuation method	contingent valuation
Type of risk valued	risk of death in auto accident/road safety
Magnitude of the risk	X in 10000[6]
Magnitude of the risk reduction	(a) in the question about WTP for safer toll road, the risk reduction is 3 in 10000 (b) in the question about choice of neighborhood to live, the risk reduction is 200 to 1000 in 10,000
population	New Zealand households
Sampling frame	stratified random sample of New Zealand households.
Sample size	N=655. After persons who failed arithmetic questions are Excluded from the sample, N=629
Survey method	in person. The auto safety survey was mounted on a pre-existing travel mode survey. Interviewers were sent to the homes of persons who had previously participated in the travel mode survey. The travel mode survey surveyed all family members over the age of 5.
Date of the survey	December 1989
elicitation method	open ended
subjective or objective baseline risks? report	presumably objective. In the questions about driving Behavior in bad weather, respondents were asked to the subjective risk reduction incurred by driving more

[6] Not explicitly reported. It appears that in one valuation question—subsequently judged to be poorly phrased, and dropped from the analyses—the baseline risk would have been in order of 1 in 10,000.

	slowly, relative to the risk for the average driver.
commodity being valued	> WTP for five commodities: • Safer toll road (private risk reduction, just for self) • Driving safety course (private risk reduction, self and family) • Car safety features (private risk reduction, self and family) (airbags, side impact) • Living in a safer neighborhood (private risk reduction, self and family) • Roadway and pedestrian safety via taxes (public risk reduction, self, family, and other people) > Tradeoff between severy injuries and death, and between severe head injuries and death > Driving behavior to observe time sacrificed to reduce risks when driving in bad weather
Is the questionnaire reported in the article?	No. They say it is in Appendix 1, but there is no such Appendix in this paper and in the accompanying report on the VSL for family members[7]
Do we have questionnaire Available to us?	No
Experimental Design	No mention where the risk reduction is varied across Respondents or other experimental treatments are Implemented. Risk reduction is varied within the Respondent in different questions.
Bid Design	not applicable (not a dichotomous choice questionnaire). (An initial bid is used in a few of the elicitation questions For some of the risk reductions)
Statistical model used To estimate mean or Median WTP	sample average
WTP Regressions?	Yes, but they are not reported
Regressors in WTP	gender, age, race, income, urban v rural, and family size. The authors produce estimates of WTP by gender, age

[7] Miller, Ted, and Jadish Guria (1991), “Valuing Family Members’ Statistical Lives,” Report to the New Zealand Ministry of Transport.

group (the elderly have a lower VSL), and report that WTP increases with income (1 or 2 percent for every $1000 of household income) and decreases in family size.

The authors also collect information about behavior that Could be related with risk aversion, and report that those Perceive their risks as large are willing to pay less for risk Reductions.

The responses to the speed choice question were affected by driving experience, and by previous accident experience.

Results

VSL based on the responses to the safer toll road, safer car, And safer neighborhood questions Estimated to be 1.9 million NZ dollars, with 95% confidence interval $1.4 to $2.3 million.

VSL based on pooled data $1.9 million, with 95% c.i. $1.7 to $2.2 million.

VSL based on speed choice behavior $1.9 million, with 95% c.i. $1.4 to $1.9 million.

Implied VSL

VSL ranges between NZ $1.4 million and $2.3 million, Depending on the risk reduction being valued. The Composite (pooled data) estimate of VSL is NZ $1.9 Million (all figures in 1989 NZ dollars). These estimated Values are judged to compare well, and be on the low side Of the range of, VSL from other countries and contexts.

Summary of VSL. All figures in 1989 NZ dollars (at the time of the study, 1 US dollar = 1.60 NZ dollar).

How valued	VSL*	Sample size used
Average WTP	1.893	
Safer toll road	2.009	308
Road safety course	1.437	296
Safer car**	1.871	226
Safer neighborhood	1.871	500
Roadway and pedestrian safety taxes	2.297	108
Speed choice question	1.938	

* VSL figures refer to all household members; ** = used respondent-reported estimate of how long they would hold on to the car, and applied various discount rates (0 to 10%).

Data cleaning: the responses to the payment questions were subjected to a Number of data cleaning criteria:

* if person reported zero WTP for safety course, but later stated he was WTP something for the car safety features, the zero response was treated as a protest and discarded.

* if value of safety for the rest of the family, which was derived as difference between total WTP and WTP for own risk reduction, is negative, observation is discarded

* if value for other members of the family is > 4 times value for own risk reduction, observation is discarded

* excessively large bids discarded

	missing	Discarded		Kept	
		Positive WTP	Zero WTP	Positive WTP	Zero WTP
Toll road	5	215	111	291	17
Training course		10	226	97	268
Car safety features	96	230	42	230	23
Safer neighborhood	87	11	31	487	13
Taxes for road safety	225	150	123	102	6

Observations: * the authors recognize that the toll question tended to elicit WTP “by coin” (50 cents or 1 dollar), and they strove to Convert the cost on an annual basis; the question was Judged to have worked well.

* the safety course question did not work well. People did not find the course effective, or would not have taken it for other reasons.

* the car safety feature question was judged to have worked well, with most people capable of providing an estimate of the lifetime of the car. Because benefits are spread over the

lifetime of the car, discount rate was used, with the authors experimenting with 0 to 10% discount rates. The actual discount rate used did not make a difference, but using discount of 0% introduced some inconsistencies between the responses to different risk questions for some people.

* the neighborhood safety question worked well, but required disentangling the tradeoff between injuries and deaths, and head injuries and deaths. On average, 30 severe injuries were judged by the respondents to be equivalent to one death, and 45% of the respondents found that the head injury (with person affected unable to take care of himself, move, speak, etc) was worse than death.

* driving behavior question was found to work well, with people giving responses consistent with the VSL values implied by the responses to the CV questions. People were asked to identify a road that they frequently used, and say whether they would drive more slowly in windy and rainstorm conditions. They were also to estimate the difference in risk between driving at the usual (dry weather) speed in bad weather, and driving more slowly. One death was judged to be equivalent to 253,000 hours (average) (median in 189,539 hours), which can be valued at the average hourly rate of $13.37 to produce VSL.

Other: one of the goals of the study was to elicit how much one Is prepared to pay for one's own risk reduction, and how Much other members of the family are willing to pay for This person's safety.

The study elicited WTP for risk reductions that affected the Entire household, then multiplied WTP by the number of People in the household, then subtracted value of own risk Reduction, and finally divided by the number of people in The family.

To compute a value for the family, they recommend just Using two adults, ignoring children.

Limitations of the study use of open ended questions; no attempt to control for Correlation between questions, and/or sequencing effects. Also, many discarded observations because the consistency Checks were done ex post by the researchers, but were not Imposed on the respondents during the course of the survey.

Summary form 9

Author(s)	Viscusi, W. Kip, Wesley A. Magat, and Joel Huber
Year of publication	1991
Title	"Pricing Environmental Health Risks: Survey Assessments of Risk-risk and Risk-dollar Tradeoffs for Chronic Bronchitis"
Journal details	Journal of Environmental Economics and Management, 21, 32-51
Valuation method	variant of conjoint choice survey. Respondents were asked To indicate which of two locations they would live. The Two locations were described by risk in terms of (a) risk of In an auto accident, (b) risk of chronic bronchitis, and (c) Cost. The attributes were altered across alternatives A and B In an interactive way to get to the indifference points. The Following were estimated: (a) treadeoff rates between the Risk of chronic bronchitis and the risk of dying in an auto Accident; (b) the risk-dollar tradeoff referring to the risk of Dying in an auto accident, and (c) the risk-dollar tradeoff For the risk of chronic bronchitis.
Date of the survey	not reported.
Type of risk valued	risk of death in auto accidents; risk of contracting chronic bronchitis
Magnitude of the risk	X in a population of 100,000
Magnitude of the risk reduction	X in 100,000
Population	persons visiting a shopping mall in Greensboro, North Carolina
Sampling frame	shoppers were intercepted at the shopping mall. No details Are provided about how exactly these persons were Intercepted (by stopping every other fifth person passing by, etc). N= 389.

survey method	self-administered computer survey
elicitation method	point values for the risk-dollar tradeoffs and for the risk risk tradeoffs. These are attained through a sequence of discrete choice questions. After observing which alternative is picked, the next question alters one attribute, until indifference is reached
subjective or objective risks?	objective. However, (i) respondents were told that the risks were specific for them and had been calculated on the basis of their behaviors like smoking, miles driven, etc. even though this was not true and the same number was presented to everyone. Also, (ii) the authors worry that some respondents may have replaced the objective risks of auto accident fatality with their own risks, based on driving skills and miles driven.
commodity being valued	risks are presented as the choice between location A and B
is the questionnaire reported in the article?	No
Do we have questionnaire Available to us?	No
Experimental Design	two versions of the questionnaire, presented to split Samples. Version I obtains (a) tradeoff rates between Chronic Bronchitis and auto death risks and (b) risk-dollar tradeoffs for chronic bronchitis. Version II obtains (a) same as for version I, and (c) risk-dollar tradeoffs for auto risks.
Bid Design	not applicable. However, the risks used in the initial questions are not reported in the paper.
Statistical model used To estimate mean or Median WTP	paper presents mean and median tradeoff rates between risks, and risks v. dollars; percentiles of the distributions of tradeoff rates are also presented
WTP Regressions?	No. Paper reports that regressions were attempted to relate The tradeoff rates to individual characteristics but that no meaningful relationships were found.
Regressors in WTP Equations	not applicable

Results	mean tradeoff rate between the risk of contracting chronic bronchitis and the risk of dying in an auto accident indicate that a change in the risk of the former is viewed as equivalent to a 0.70 as great a change in auto risk. This means that chronic bronchitis is seen as a little less serious than the risk of dying in an auto accident. (The median tradeoff rate is about ½ as much.) The mean risk-dollar tradeoff for chronic bronchitis is 8.83 dollars for a 1 in 100,000 risk reduction, which implies a VSL of \$883,000. Using median tradeoff rate, instead of mean, results in VSL of \$457,000. The mean risk-dollar tradeoff for auto accident is 81.84 dollar for a 1 in 100,000 risk reduction, which implies a VSL of \$8,184,000. Based on the median tradeoff, VSL is 2,286,000.
Implied VSL	see above
Limitations of the study.	Creative approach for estimating the value of risk Reductions. However, it is difficult to extrapolate results to The population, because the sample is presumably not Representative, there is no statistical relationship with Individual characteristics so that predictions can be made For another population, and the sampling scheme is not reported.
Other:	the authors implemented a series of consistency checks, Excluding from the usable sample those respondents who (i) never changed the choice (e.g.,always chose A) (ii) only changed to indifference (iii) reversed response (iv) indicated a boundary result, or (v) were always indifferent. This resulted in dropping almost two thirds of the responses.

Summary form 10

Author(s)	Viscusi, W. Kip and Wesley A. Magat
Year of publication	1991
Title	"Policy Analysis and Benefit Valuation for Environmental Regulation"
Journal details	Draft Report to the US Environmental Protection Agency, Cooperative Agreement CR 814388424, Durham, NC, January
Valuation method	variant of conjoint analysis questions
Type of risk valued	risks of three diseases potentially associated with environmental exposures (peripheral neuropathy [a nerve Disease]; curable lymphoma (chance of dying 10%) and terminal lymphoma (chance of dying 100%). Risks of dying in an auto accident. People are asked to indicate which location they would prefer to live in, A or B, which differ for risks and cost of living. Indifference points are elicited. Risk-risk tradeoffs between risk of a disease and risk of auto accident, and risk-dollar tradeoffs.
Magnitude of the risk	X in 1,000,000 for the diseases
Magnitude of the risk reduction	1 in 1,000,000
Population	shopping mall visitors (blue collar shopping mall in Greensboro, NC)
Sampling frame	respondents were intercepted at the shopping mall, but no details are provided about how persons were intercepted
Survey method	self-administered interactive computer surveys
elicitation method	first questions, nine-point scale to indicate strength of

preference between A and B, then indifference points are elicited, resulting in tradeoffs rates and point values for dollar amounts

subjective or objective baseline risks? objective. In discussing people with outlier tradeoff rates, the authors point out that some people may have substituted their own subjective beliefs about the risk of dying based on skills, miles driven, etc.

commodity being valued changes in the risks of diseases and car accident deaths associated with moving from one place to the next. Respondents were told that at these locations the risks of these diseases were lower than those experienced in the city where they live now (to avoid alarmistic reactions).

is the questionnaire reported in the article? No—sample initial choice question reported in table 2.

Do we have questionnaire Available to us? No

Experimental Design eight independent samples with various sequences of risk—risk and risk-dollar tradeoffs

Bid Design not applicable. However, the report does not contain any information about the (initial) levels of risks or costs used in the study

Statistical model used To estimate mean or Median WTP mean and median tradeoff rates, plus other descriptive statistics of the distribution of the tradeoff rates

WTP Regressions? No, but correlations were attempted between the mean aversion scores and the tradeoff rates. (Respondents were asked to rate every consequence of a disease on a scale from 1 "least important to avoid" to 9 "most important to avoid." People generally judged as highly undesirable symptoms that they are relatively little familiarity with, such as bleeding of the joints and skin.)

A person's mean aversion score for one disease generally correlated well with the tradeoff rate between the risk of that disease and the risk of dying in an auto accident. By contrast, the mean aversion scores did not correlate well at all with the risk-dollar tradeoffs for that disease.

Regressors in WTP Regression N/A

Results curable lymphoma: tradeoff rate with auto accident is such that a 1.6 in 1,000,000 change in risk is judged equivalent to a change in risk of 1 in 1,000,000 in the risk of auto death. The corresponding tradeoff rate is 1/1.6=0.625, implying that the value of a statistical case of curable lymphoma should be VSL for auto accident, times 0.625. Using a VSL of 4 million dollars for auto accidents, this means that the value of curable lymphoma is 2.5 million dollars.

Tradeoff rate between terminal lymphoma and auto death is about 1, implying that the corresponding VSL is equal to that in car accident, or $4 million.

Tradeoff rate between the nerve disease and an auto death is such that a risk change of 2.5 in 1,000,000 in former is judged equivalent to a change of 1 in the risk of dying in a car accident. The corresponding value is thus 1 / 2.5 =0.4, or $1.6 million.

Implied VSL see above

Limitations of the Study

Other: the paper concludes that the risk-risk tradeoff approach worked well, especially with these relatively little known illnesses. By contrast, the risk-dollar tradeoffs did not work quite so well, appeared to be affected by initial values included in the questions, and several people reported implausibly high values.

Summary form 11

Author(s) Jonathan Baron

Year of publication 1997

Title "Confusion of Relative and Absolute Risk in Valuation"

Journal details Journal of Risk and Uncertainty, 14, 301-309

Valuation method contingent valuation

Type of risk valued risk of death for various causes, including diseases and non-diseases causes of death (e.g., firearms, etc.)

Magnitude of the risk experiment 1: lives saved: 900 in 1000, 900 in 10000, 90 in 100, 90 in 1000; experiment 2: 5% reduction in a given cause of death, 2600 American lives saved

Magnitude of the risk reduction see above

population students at the Univ. of Pennsylvania and Philadelphia College of Pharmacy and Science

sampling frame recruiting of the students not reported in the paper. Participants in the study were paid by the hour.

survey method written questionnaire

elicitation method open-ended

subjective or objective baseline risks? objective, and referred to a population (1000 people who Die of this disease each year).

commodity being valued treatment that would save a specified number of lives and have to be paid for with extra health insurance above and beyond basic coverage

Is the questionnaire reported in the article? No (but WTP questions are reported in the article).

Do we have questionnaire Available to us? No

Experimental Design experiment 1: n=95, experiment 2: n=29 experiment 1: lives saved: 900 in 1000, 900 in 10000, 90 in 100, 90 in 1000 (total eight questions for each respondent); experiment 2: 5% reduction in a given cause of death, 2600 American lives saved

Bid Design N/A

Statistical model used To estimate mean or Median WTP geometric means

WTP Regressions? Possible for experiment 1 data, but not explicitly mentioned

Regressors in WTP Regression experiment 1: lives saved, percentage risk reduction, age (presumably, of the persons whose lives are saved)

Results the paper starts from the implicit assumption that WTP should be proportional to the number of lives saved: $WTP=\alpha \times L$, where L is lives saved. $L=N \times R \times \%$ risk reduction, where N is the population and R is baseline risk.

But the first experiment finds that the percentage risk reduction has further explanatory power for WTP, above and beyond that of L. In addition, WTP grows with L, but not in a proportional fashion. There is a ten-fold increase in the number of lives saved from some questions to the others, but only a two-fold increase in WTP.

The second experiment finds that WTP depends on prevalence, but is not proportional to prevalence (prevalence presumably means baseline risk).

Implied VSL not computed

Limitations of the Study Small sample sizes, use of students rather than general population, use of a context (universal coverage health care which in itself may have been controversial and may have distracted respondents).

Summary form 12

Author(s) Johannesson, Magnus, Bengt Jönsson, and Lars Borgquist

Year of publication 1991

Title Willingness to Pay for Antihypertensive Therapy—Results Of a Swedish Pilot Study

Journal details Journal of Health Economics, 10, 461-474

Valuation method contingent valuation

Type of risk valued risk of death for myocardial infarction and stroke

Magnitude of the risk subjective baseline risk without treatment over 5 years: Mean (median) 7.5% (5.0%)

Subjective risk with treatment over 5 years: Mean (median) 3.2% (3.8%)

Subjective risk reduction due to treatment: Mean (median) 4.3% (1.2%)

Magnitude of the risk reduction see above

population 481 patients on the hypertension register at the primary health care center Atvidaberg. The average age of the patients is 64.

sampling frame questionnaire were mailed to the entire patient roster. The average age of the sample is about 64, and of the persons in the sample who were asked the risk questions about 59.

survey method mail

date of the survey 1989

elicitation method open-ended to one of two split samples, single-bounded dichotomous choice to the other sample

subjective or objective risks? subjective baseline if the respondent were to go without treatment, subjective risk reduction due to the treatment. Risks expressed over 5 years. All respondents were currently receiving treatment.

commodity being valued antihypertensive treatment (private risk reduction). Respondents were also asked whether they would undertake changes in diet, exercise, etc. if user fees increased, in an effort to find out about substitution patterns with non

is the questionnaire reported in the article? Only the wording of the WTP question (in the Appendix)

Do we have questionnaire Available to us? No

Experimental Design two split sample. Sample 1 received the WTP question in an open-ended format. People were to report max WTP for current treatment of hypertension (high blood pressure, or HBP). Sample 2 received the WTP question in a dichotomous choice format. People were asked whether they would pay a higher fee of X SEK for the current treatment.

The 175 people on the patient register of age 70+ were not asked the risk question.

Bid Design (in the sample that was asked the payment question in the dichotomous choice format) bids range from 100 SEK to 10,000 SEK. Specific bids not reported in the article. Percentage yes/no for each bid not reported in the article.

Return rate 322 returned questionnaire out of 481 (return rate about 67%). Return rate was slightly higher among those persons who received the dichotomous choice questionnaire version (68% return rate v. 65% among those with open-ended questionnaire).

Return rate is 67% among those who were asked the risk question, and 67% among those who were asked the question about substitution, so it does not seem to be influenced by the inclusion of these questions in the questionnaire.

Selection into the sample The researchers had access to the register of patients with hypertension at the hospital, so they knew some basic information about these persons, and were able to compare the individual characteristics of those persons who returned the questionnaires with those of the persons who received the mailings. The likelihood of returning the questionnaire does not depend on age, but does depend on gender. Specifically, the percentage of males is higher among the respondents (49%) than it is among the non-respondents (31%).

Item nonresponse The item nonresponse rate for the WTP question is much higher in the questionnaire version with the open-ended WTP question (59%) than for the discrete choice version (18%). This and other considerations (see “Results” below) led to the conclusion that the open-ended question did not work well.

Statistical model used
To estimate mean or Median WTP logit model of the dichotomous choice responses, with log bid. WTP is the area under the 1-cdf curve. Two limits of integration were used, 10000 SEK and 15000 SEK.

Results are compared with non-parametric procedure by Kristrom, which computes the area under the (1-cdf), based on plotting out the cdf.

WTP Regressions? Yes, logit model for the dichotomous choice responses.

Regressors in WTP Regression log bid, risk reduction, perceived substitution (dummy) with non-medical prevention, age, sex, taxable income. The signs of the respective coefficients are negative, negative, negative, negative, positive. This is the most complete model, but it could only be run with n=61 (older people were not asked the risk question, and there were lots of missing values for other covariates). An alternative specification is reported that drops risk and keeps only log bid and the non-medical substitute dummy (n=135).

Results 1. the open-ended format for the WTP question did not work well. Those respondents who were given this version of the questionnaire had a higher item nonresponse rate, more protest responses, and about one-third of those respondents with usable WTP answers censored their WTP at 350 SEK, the annual cost of antihypertensive treatment, which they were told about in the questionnaire.

2. WTP is as follows (1989 SEK)

	Median WTP	Mean WTP	
		Limit of integration 10000 SEK	Limit of integration 15000 SEK
Logit model	2900	4500	5500
Non-parametric (Kristrom, 1990)	2500	4200	5100

It should be noted that the parametric and non-parametric approach give similar results. The authors also point out that while median WTP is the most desirable welfare measure from the statistical point of view, the appropriate measure to use for cost-benefit analysis purposes is mean WTP.

3. based on the regression results,

$\log WTP=7.49-0.028*Substitute+0.375 \log \text{risk reduction} + 0.444 \log \text{income} -0.7 \log \text{age} -0.18 \text{ sex}$.

This implies that WTP is not strictly proportional to the size of the risk reduction, but increases with it. Also note that the income elasticity of WTP is 0.444. The effect of the substitute is small in this equation, but it is much larger in the more parsimonious equation.

Implied VSL 2500-5000 SEK a year, which (at the mean risk reduction) implies a VSL of 280,000-560,000 SEK.

Limitations of the Study This appears to be a nicely designed and interesting study that EPA might have an interest in, due to its focus to persons at higher risk of certain adverse health effects of air pollution. Possible limitations:

* the study does not seek to describe the socio-economics of hypertension patients at the clinic with those of the general population. (It does, however, explore possible self-selection into the sample on the part of those who returned the questionnaires).

* in the econometric model of the WTP responses, the risk reduction is treated as exogenous with WTP, but it is likely to be endogenous with it. Perhaps this is one reason why the p-value of the coefficient on log risk reduction is only 0.16 (another reason might be that the sample size for this regression run is very small). Similar considerations apply to the dummy measuring non-medical prevention substitute activities.

* a logit model with log bid implies a log logistic distribution of WTP. Depending on the value of the parameters, a log logistic distribution may have infinite mean, so it would be best to avoid using this distributional assumption.

Summary form 13

Author(s) Johannesson, Magnus, Per-Olov Johansson, Bengt Kristrom and Ulf-G. Gerdtham

Year of publication 1993

Title "Willingness to Pay for Antihypertensive Therapy—Further Results

Journal details Journal of Health Economics, 12, 95-108

Valuation method contingent valuation

Type of risk valued health improvements due to hypertension therapy. This study is not on reductions in the risk of dying.

Magnitude of the risk the health change is based on a visual analog scale (VAS), where 0 cm is the worst possible health and 15 cm is the best possible health state.

Magnitude of the risk reduction subjective change in health with the treatment

population 535 patients on the hypertension register at a primary health care center near Linkoping.

sampling frame questionnaire were mailed to the entire patient roster. The average age of the sample is about 60.

survey method mail

return rate 335 questionnaires returned, response rate 64%

item nonresponse very low for the WTP question (only 5%)

date of the survey 1991

elicitation method polychotomous choice (instead of yes/no, the response categories are def. yes, probably yes, probably not, def. not, don't know)

subjective or objective risks? subjective

commodity being valued antihypertensive treatment, and the related change in health status (a private commodity).

Is the questionnaire reported in the article? Only the wording of the WTP question (in the Appendix)

Do we have questionnaire Available to us? No

Experimental Design people were assigned to one of 15 possible bids, but there is no other meaningful experimental design.

Bid Design 15 bids ranging from 100 to 1500 SEK per month. Specific bids not reported in the article. Percentage yes/no for each bid not reported in the article.

Selection into not discussed in the article

Statistical model used To estimate mean or Median WTP logit model of the dichotomous choice responses. bid. WTP is the area under the 1-cdf curve. Three limits of integration were used, 1500, 2000, and 2500 SEK.

Results are compared with non-parametric procedure by Kristrom, which computes the area under the (1-cdf), based on plotting out the cdf.

WTP Regressions? Yes, logit model for the dichotomous choice responses. WTP is treated as a logistic. They experimented with the log logistic distribution as well. Initial round of regression treat any yes (def or probably) as yes, and subsequent rounds focus only on respondents with certain yes and certain nos.

Regressors in WTP Regression bid, Change in VAS, age, sex, education, taxable income.

Results

Mean WTP	Limit of integration 1500 SEK	Limit of integration 2000 SEK	Limit of integration 2500 SEK
Logit model	735	795	855

Annual WTP is 9000 SEK.

Implied VSL N/A

Limitations of the Study Relative to the earlier study on hypertension, the focus of the study was changed to morbidity. This means that VSL cannot be computed.

Polychotomous choice response categories were used, but their use is not warranted and leaves the researcher with the problem of having to interpret the meaning of the responses and the econometric model.

Summary form 14

Author(s) Gerking, Shelby, Menno de Haan, and William Schulze

Year of publication 1988

Title The Marginal Value of Job Safety: A Contingent Valuation Study

Journal details *Journal of Risk and Uncertainty*, 1, 185-199

Valuation method contingent valuation (the survey questionnaire obtains information that can be used to do estimate a compensating wage hedonic model, as was done in a companion paper, Gegax et al. (1987)

Type of risk valued workplace risks

Magnitude of the risk X in 4000, with X ranging from 1 to 10; this is annual risk

Magnitude of the risk reduction 1 in 1000

Population US households

Sampling frame (a) simple random sample of US households (3000), plus (b) simple random sample from counties with disproportionately high workplace risks (3000).

Of these 6000 mailings, 749 (12.5%) were returned as undeliverable and 2103 completed.

Survey method mail survey

Elicitation method payment card. Responses to the payment card are interpreted as continuous observations on WTP, except if the respondent circles the highest amount on the payment card ($6001+).

subjective or objective baseline risks? Baseline risk is subjective. The risk reduction is 1 in 1000 and it objective.

Commodity being valued WTP for one step down the risk ladder, WTA to accept for one step up on the risk ladder (in split samples). No specific risk reduction delivery method.

Is the questionnaire reported in the article? No

Do we have the questionnaire Available to us? Yes

Were visual aids used? Yes. Risk ladder, with low and high risk extremes marked to the respondent, and the occupation(s) corresponding to the various risk levels listed.

Experimental Design In split samples, respondents are asked about WTP or WTA for a step down (or up) on the risk ladder. Across respondent variation in baseline risk, but all respondents are given the same risk reduction (increase in the case of WTA) (=1 in 1000 in a year).

Bid Design not applicable

Statistical model used To estimate mean or Median WTP Would appear to be just the sample average (but double. model is used to run regressions)

WTP Regressions? Yes

Regressors in WTP Regression subjective baseline risk, annual labor earnings, race, gender, age, union membership, schooling

Results mean WTP = \$665, mean WTA = \$1705

Implied VSL VSL = mean WTP or WTA, $\times$ 4000= \$2.66 million (WTP)
= \$6.82 million (WTA)

Limitations of the Study large discrepancy (one order of magnitude) between risks shown to the respondents valued in the study and actual workplace risks.

Other:

-- large number of zero bids (47.4% in WTP responses, 23.2% in WTA responses)

-- the authors recognize that it may be important to identify or eliminate outliers and protest zeros, but are unwilling to do so for fear of introducing arbitrary criteria for defining an outlier or a protester.

--attempt to compare the sample of returned questionnaire with the recipients of the mailings

Summary form 15

Author(s) Timothy McDaniels

Year of publication 1992

Title "Reference Points, Loss Aversion, and Contingent Values For Auto Safety"

Journal details Journal of Risk and Uncertainty, 5, 187-200

Valuation method contingent valuation

Type of risk valued risk of death in auto accidents (road traffic) (private risk)

Magnitude of the risk 10 in 100,000 (WTP) or 5 in 100,000 (WTA)

Magnitude of the risk reduction 5 in 100,000 risk reduction or 5 in 100,000 increase in risk. WTP for the former, WTA for latter. The questionnaire emphasizes that ging from 10 to 5 is halving, and 5 to 10 s doubling.

population mixed. Questionnaires were handed out to students as well as groups of non-student adults (parents, professionals, staff in an economics consulting firm)

sampling frame sample is admittedly not representative of the population at large. N=55 and n=194 (auto dealership).

survey method self-administered in person. Experiment 1 was conducted Apri-May 1986 at Pennsylvania State University. Experiment 2 was conducted in March 1990 in Washington State.

elicitation method experiment 1: open ended, experiment 2: dichotomous choice

subjective or objective baseline risks? Objective

commodity being valued car safety (through safety features). Private risk reduction.

is the questionnaire reported in the article? No, but the phrasing of the payment question is.

Do we have questionnaire Available to us? No

Experimental Design split samples with two experiments. Experiment 1 elicits WTP and WTA for auto safety features that change risk (both WTA and WTP within the subject).

Experiment 2 entails a 2×2 design, where the treatments are (i) WTA v. WTP, and (ii) mentioning or not mentioning the safety of other auto makers (this is the reference). The author checked that the four group were uniform by individual characteristics, and they were.

Bid Design only one bid in experiment 2

Statistical model used To estimate mean or Median WTP experiment 1: mean WTP (mean WTA). Experiment 2: percentage of "yes" responses to the payment question.

WTP Regressions? No

Regressors in WTP Regression N/A

Results experiment 1: avoiding losses in safety is valuing more highly than gaining safety. Experiment 2: the percentage of "yes" responses to the payment questions varies across the gains/losses context and information about other auto makers' safety.

Implied VSL experiment 1: $6.15 million (1986 dollars)

Limitations of the Study

Other: Experiment 1 elicits WTA, WTP for car safety features that change risk (both WTP and WTA eliciting within a subject). 12 protesters with WTA question (these observations were discarded). Seven subjects gave WTP=WTA, 31 WTA/WTP less than 2, 33 WTA/WTP greater than 2). The author also reports that Jones-Lee et al. (1985) finds outliers with WTP or WTA orders of magnitude greater than the other responses.

Appendix B

Selected questionnaires

Corso et al. questionnaire

FINAL: November 10, 1998

Survey II

Hello, my name is _________, and I am calling on behalf of the Harvard School of Public Health. In the last few weeks, someone in your household agreed to participate in a national telephone survey about health and safety issues. May I speak to this same individual [Ms/Mrs/Mr ___________]?

Thank you again for agreeing to participate in this survey. Do you have a few moments now to answer the survey questions?

YES
NO* *Can I set up a more convenient time for the interview?

To assist you in answering these survey questions, you should have already received a packet of materials in the mail. Have you received your packet?

Yes
No **[Confirm address and reschedule time for call back]**

Do you have the packet in front of you?

Yes
No* *I'd be glad to wait while you locate your packet

Can you please tell me the code # on the cover page of your packet. It is located in the bottom right-hand corner of the page?

[Record code#]

If A1: Version A, Linear scale
If A2: Version A, Log scale
If A3: Version A, Dots
If A4: Version A, Control group
If B1: Version B, Linear scale
If B2: Version B, Log scale
If B3: Version B, Dots
If B4: Version B, Control group

Thank you. Now if you are ready, I'd like to begin. This survey will take approximately 25 minutes to complete.

The first few questions that I'd like to ask you relate to how you perceive your quality of life.

Q1. First, think about your current physical and emotional well being, and your involvement in family and community activities. On a scale from 1 to 10, where 10 is the best you can imagine and 1 is the worst you can imagine, how would you rate your current quality of life?

1 2 3 4 5 6 7 8 9 10

Q2. How would you compare your current quality of life with others of your same age and gender **(READ LIST)**? Is yours…

1) Much better
2) A little better
3) About the same
4) A little worse, or
5) Much worse
6) (Don't know)

Q3. [SKIP IF AGE > 65]** Now think about what you expect your life to be like as a senior citizen, beginning at age 66. Think about your involvement in family and community activities, and your physical and emotional well being. How would you rate your expected quality of life as a senior on a scale from 1 to 10, where 10 is the best you can imagine and 1 is the worst you can imagine?

1 2 3 4 5 6 7 8 9 10

Section 1: Longevity Lotteries

For the next section of this survey, I want to ask you about the choices that you would make if you developed a serious disease or illness. Please turn to page 1 in your packet. This page should be light YELLOW.

Q4. Imagine that your physician has told you that you have a rare disease. While you experience no pain or impairment from this disease, it is always fatal. Without treatment, it is expected that you will live for only about 8 more years from your current age. Fortunately, there is a painless surgical procedure available to treat your disease. While the surgery can be provided to you free of charge, it is not always successful. 50% of the time, the surgery is successful and you would live about 12 more years. However, 50% of the time, the surgery is unsuccessful and you would live about 4 more years. Would you choose to have the surgery?

YES
NO
Both options are equally preferred
Refused
Don't Know*

*Does this mean that you have no preference between the two options? **[Don't Read]**

YES
No, Choose No Surgery
No, Choose Surgery

No, Don't Know

Q5. After additional testing, your doctor discovers that your disease is not as bad as first suspected. Now, he informs you that you are expected to live for 16 more years without treatment. The chance that surgery will be successful is still uncertain. 50% of the time the surgery is successful and you would live about 24 more years. And 50% of the time the surgery is unsuccessful and you would live about 8 more years. In light of this new information, would you choose to have the surgery?

YES
NO
Both options are equally preferred
Refused
Don't Know*

*Does this mean that you have no preference between the two options? **[Don't Read]**
YES
No, Choose No Surgery
No, Choose Surgery
No, Don't Know

Now turn to page 2 in your packet. This page should also be light YELLOW.

Q6. Now consider an entirely different situation. This time you go to your doctor for a routine physical exam and he informs you that you have developed a rare condition that can lead to a blood clot forming in the brain. Your doctor has told you that a clot like this can form at any time in the next 3 months and is always fatal. Fortunately, there are two pills that can be used to treat your condition. With Pill A, you have an equal chance of living 6 or 10 more years. With Pill B, you have an equal chance of living 4 or 12 more years. If these pills are provided at no expense to you, which pill would you choose to take?

Pill A
Pill B
Both options are equally preferred
Refused
Don't Know*

*Does this mean that you have no preference between the two options? **[Don't Read]**
YES
No, Choose Pill A
No, Choose Pill B
No, Don't Know

Q7. What if the chance of success for Pill B has changed? Now with Pill B, you have a 90% chance of living 6 more years and a 10% chance of living 26 more years **[Version**

B: …, you have a 70% chance of living 11 more years and a 30% chance of living only 1 more year]. If you now had to choose between Pill A and Pill B, which pill would you choose?

Pill A
Pill B/revised
Both Options are equally preferred
Refused
Don't Know*

*Does this mean that you have no preference between the two options? **[Don't Read]**
YES
No, Choose Pill A
No, Choose Pill B/Revised
No, Don't Know

Now turn to page 3 in your packet. This page should also be light YELLOW.

Q8. Now imagine that a pharmaceutical company has developed a new type of treatment for your condition. With Pill C, you have an equal chance of living 14 or 18 more years. With Pill D, you have an equal chance of living 12 or 20 more years. If this new medication is offered to you free of charge, which pill would you choose?

Pill C
Pill D
Both options are equally preferred
Refused
Don't Know*

*Does this mean that you have no preference between the two options? **[Don't Read]**
YES
No, Choose Pill C
No, Choose Pill D
No, Don't Know

Q9. Now thinking about the choices that you have made between having surgery and not having surgery or choosing between the pill options, how confident are you in your previous responses?
Are you…?

Very confident
Somewhat confident
Not too confident
Not at all confident
DK

Section2: Attitudes and Beliefs about Prevention and Treatment

For the next section, I'd like to ask your opinion about SOCIETAL programs that the federal government might adopt to help control health problems in the US. You do not need the packet of materials for this section.

An important question in the health care sector is how to allocate scarce resources between preventive measures that save lives by preventing disease and promoting health and treatment efforts that save lives among persons already suffering from disease.

Prevention and treatment differ in many ways.

1. Prevention interventions are generally applied to a group of individuals or a population, although typically only a fraction of those individuals would have ultimately gotten the disease without the intervention. An example of a prevention intervention is diet or medicine to lower cholesterol to PREVENT heart attacks.
2. Treatment interventions are generally applied to individuals who already have a disease. An example of a treatment intervention is medicine or surgery to TREAT heart failure after a heart attack.

[Version A: Ask Q10 first; Version B: Ask Q11 first]

Q10. On a scale from 1 to 10, where 1 means not at all effective and 10 means very effective, how effective do you think prevention programs are in reducing health problems in the US?

1 2 3 4 5 6 7 8 9 10

Q11. On a scale from 1 to 10, where 1 means not at all effective and 10 means very effective, how effective do you think treatment programs are in reducing the health problems in the US?

1 2 3 4 5 6 7 8 9 10

Q12. If a prevention program and treatment program both saved the same number of lives from the same health problem, which program do you think would be more costly for society?

Prevention Program
Treatment Program
Same cost
Don't Know/Refused

Section 3: WTP for Prevention or Treatment

For the next section of this survey, I'd like to ask you a few questions about food safety. Please turn to page 4 in your packet. This page should be light GREEN.

[Version A: SKIP Q14; Version B: SKIP Q13]

Q13a. Imagine that you are planning a trip to a foreign country where for every 100,000 people visiting per year, 400 people contract a virus from eating contaminated food. If you get the virus, the only symptom is a slight yellowing of the skin for 2 or 3 days. The virus causes no other discomfort and does not interfere with any of your activities.

However, studies have shown that for every 100 people who get the virus, 1 will die. Further, there is no treatment at this time. Fortunately, there is a US medication available that will protect you from getting the virus in the first place, no matter what foods you eat while traveling. This medication has NO side effects. Tests have shown that this preventive medicine will decrease your risk of getting the virus by 50%. Thus, your overall chances of dying from this illness can be reduced from 4 in 100,000 to 2 in 100,000 if you take the preventive medicine.

Would you consider taking the medication before traveling?

Yes (skip to Q13c)
No
Don't Know

Q13b. What is the main reason you would not be willing to take the medication before traveling? **[Prompt if needed]**

Benefits too small (skip to Q15)
Uncertain about benefits (skip to Q15)
Don't like receiving medication (skip to Q15)
Concerned about safety of medication (skip to Q15)
Other (specify) (skip to Q15)
DK/Refused (skip to Q15)

[Link dollar amounts]
Q13c. Now assume that you would have to pay some money to get this medication -- insurance would not cover it. Considering your current income and other household expenses, would you pay **($50, $100, $200)** for this medication before you leave on your trip?

Yes (skip to Q13f)
No
DK/Refused

Q13d. Would you buy this medication if the out-of-pocket cost was **($20, $50, $100)**?

Yes (skip to Q13g)
No
DK/Refused

Q13e. What is the main reason you would not pay for the medication before your trip? **[PROMPT IF NEEDED]**

Too expensive/costs too much (skip to Q15)

Somebody else should pay (insurance etc.) (skip to Q15)
Benefits too small (skip to Q15)
Uncertain about benefits (skip to Q15)
Benefits not worth the cost (skip to Q15)
Don't like receiving medication (skip to Q15)
Concerned about safety of medication (skip to Q15)
Other (specify) (skip to Q15)
DK/Refused (skip to Q15)

Q13f. Would you buy this medication if the out-of-pocket cost was **($100, $200, $400)**?
Yes
No
DK/Refused

Q13g. How confident are you about the amount you would be willing to pay for this medication? Are you...
Very confident
Somewhat confident
Not too confident
Not at all confident
DK

Q14a. Imagine that while traveling in a foreign country you notice that your skin has been slightly yellow for 2 or 3 days. While you experience no other discomfort and have been able to conduct your normal activities, you decide to visit a local clinic run by US doctors.

The doctors tell you that you have contracted a virus, probably from eating contaminated food, where for every 100,000 people who have the virus, 4 will die. Fortunately there is a US medication available at the clinic. This medication has NO side effects. Tests have shown that this medication will reduce your chance of dying by 50%. Thus your overall chance of dying from this virus can be reduced from 4 in 100,000 to 2 in 100,000.

Would you consider taking the medication?
Yes (skip to Q14c)
No
Don't Know

Q14b. What is the main reason you would not be willing to take the medication?
[Prompt if needed]
Benefits too small (skip to Q15)
Uncertain about benefits (skip to Q15)
Don't like receiving medication (skip to Q15)
Concerned about safety of medication (skip to Q15)
Other (specify) (skip to Q15)
DK/Refused (skip to Q15)

[Link dollar amounts]

Q14c. Now assume that you would have to pay some money to get this medication -- insurance would not cover it. Considering your current income and other household expenses, would you pay **($50, $100, $200)** for this medication?

Yes (skip to Q14f)
No
DK/Refused

Q14d. Would you buy this medication if the out-of-pocket cost was **($20, $50, $100)**?

Yes (skip to Q14g)
No
DK/Refused

Q14e. What is the main reason you would not pay for this medication to treat your foodborne illness? **[PROMPT IF NEEDED]**

Too expensive/costs too much (skip to Q15)
Somebody else should pay (i.e., insurance) (skip to Q15)
Benefits too small (skip to Q15)
Uncertain about benefits (skip to Q15)
Benefits not worth the cost (skip to Q15)
Don't like receiving medication (skip to Q15)
Concerned about safety of medication (skip to Q15)
Other (specify) (skip to Q15)
DK/Refused (skip to Q15)

Q14f. Would you buy this medication if the out-of-pocket cost was **($100, $200, $400)**?

Yes
No
DK/Refused

Q14g. How confident are you about the amount you would be willing to pay for this medication? Are you...

Very confident
Somewhat confident
Not too confident
Not at all confident
DK

Q15. How serious do you think the risk of dying is from contracting a foodborne virus while traveling in a foreign country: if 1 means not at all serious and 10 means very serious?

1 2 3 4 5 6 7 8 9 10

Q16. Assume that the government has to make a choice between 2 equally costly programs, A and B, both of which could save the lives of US residents traveling in foreign countries who are at risk of contracting a virus from eating contaminated food.

Which program would you choose if…

- Program A saves the lives of 100 American travelers per year by providing preventive medicine to all persons who are planning to travel to a foreign country.

AND

- Program B saves the lives of 100 American travelers per year by treating those persons who have already contracted the virus during their travels in a foreign country?

PROGRAM A
PROGRAM B
Both are Equally Preferred (skip to Q18b)
Don't Know/Refused (skip to next section)

Q17. How much better would it be to invest in the project that you chose?
Extremely better
Much better
Somewhat better
Only a little better
No better **[Don't read aloud: Mark if they answer 'Both are Equally Preferred' on Q16]**

Q18a. What is the main reason you preferred Program___ over Program ___? **[OPEN-ENDED]**
(skip Q18b)

Q18b. What is the main reason you have no preference between Program A and Program B? **[OPEN-ENDED]**

Section 4: Automobile Safety

[Read if Versions A1, A2, A3, B1, B2, or B3]: For the next two sections, I'd like you to use the visual aid located on the last page of your packet. On Page 5, we provide a brief description of the **[Version A3 and B3: dots; Version A1, A2, B1, B2: risk scale]** visual aid. Would you like a few moments to read through this description? **[Allow time as needed].**

[Read if Version A4 or B4]: For the remainder of the survey, you do not need to look at your packet of materials.

Now I'd like to ask you several questions related to automobile safety and the use of airbags to prevent injuries and deaths in the event of an automobile accident.

Q19. Do you have an airbag in any of the vehicles in your household?
- Yes
- No
- DK/Refused

Q20. How effective do you think airbags are in preventing death and injury in automobile accidents?
- Very effective
- Somewhat effective
- Not too effective
- Not at all effective
- DK/ No opinion/Refused

Q21.

<u>Version A1, A2, B1, B2</u>

A1 and A2: Based on government statistics, the average driver in the US has about 2 **(B1 and B2: 2.5)** chances in 10,000 of being killed in a crash in any given year. On your risk ladder, please note that the highest star symbol in blue represents your baseline annual risk of dying in an automobile accident. In community terms it means that you could expect to find about 2 people per year killed by an automobile accident in every small town in the US. **(B1 and B2: about 2.5 people killed by an automobile accident in every small town in the US).** On a scale from 1 to 10, with 10 being very concerned and 1 being not at all concerned, how concerned are you about the risk of dying in an automobile accident?

1 2 3 4 5 6 7 8 9 10

<u>Version A3 and B3:</u>

A3: Based on government statistics, the average driver in the US has about 2 **(B3: 2.5)** chances in 10,000 of being killed in a crash in any given year. On your visual aid, a 2 in 10,000 risk is equal to 5 dots on the page **(B3: equal to about 6 dots).** On a scale from 1 to 10, with 10 being very concerned and 1 being not at all concerned, how concerned are you about the risk of dying in an automobile accident?

1 2 3 4 5 6 7 8 9 10

<u>Version A4 and B4:</u>

A4: Based on government statistics, the average driver in the US has about 2 **(B4: 2.5)** chances in 10,000 of being killed in a crash in any given year. On a scale from 1 to 10, with 10 being very concerned and 1 being not at all concerned, how concerned are you about the risk of dying in an automobile accident?

1 2 3 4 5 6 7 8 9 10

Q22. Thinking about your personal situation, how would you judge your chances of being killed in a traffic accident...

Much larger than average
Somewhat larger than average
About average
Somewhat smaller than average
Much smaller than average
DK/Refused

Version A1, A2, B1, B2
A1 and A2: Now I would like to ask you a question about your willingness to pay money for a new safety device that can be installed in cars to protect drivers. It works like an airbag but protects drivers in a side impact rather than in a head-on crash. This device is well tested, safe and reliable. For the typical driver, this new device will reduce the yearly chance of dying in a crash from 2 in 10,000 **(B1 and B2: 2.5 in 10,000)** to 1.5 in 10,000. Please note on your risk ladder that the lowest star symbol in blue represents your annual risk of dying in an automobile crash when a side-impact airbag is added to your vehicle. In community terms it means that during one year you could expect to find on average 1.5 people killed in an automobile accident in every small town in the US.

Version A3 and B3
A3: Now I would like to ask you a question about your willingness to pay money for a new safety device that can be installed in cars to protect drivers. It works like an airbag but protects drivers in a side impact rather than in a head-on crash. This device is well tested, safe and reliable. For the typical driver, this new device will reduce the yearly chance of dying in a crash from 2 in 10,000 **(B3: 2.5 in 10,000)** to 1.5 in 10,000. On your visual aid, a 1.5 in 10,000 risk is equal to about 4 dots on the page. Thus, by adding a side-impact airbag, your risk is reduced from 2 in 10,000, or 5 dots on the page -- to 1.5 in 10,000, or about 4 dots on the page**. (B3: ... from 2.5 in 10,000, or about 6 dots on the page -- to ...).**

Version A4 and B4
A4: Now I would like to ask you a question about your willingness to pay money for a new safety device that can be installed in cars to protect drivers. It works like an airbag but protects drivers in a side impact rather than in a head-on crash. This device is well tested, safe and reliable. For the typical driver, this new device will reduce the yearly chance of dying in a crash from 2 in 10,000 **(B4: 2.5 in 10,000)** to 1.5 in 10,000. Thus, by adding a side-impact airbag, your risk is reduced from 2 in 10,000 to 1.5 in 10,000**. (B4: ... from 2.5 in 10,000 to ...).**

Q23. If this device were offered as an option on the next car you buy, would you be willing to pay $100 more per year in car payments for five years to have this device in your car?

YES (skip to Q25)
NO

Don’t Know/Refused

Q24. Would you be willing to pay $50 more per year in car payments for five years to have this device in your car?

Yes (go to Q26)
No (go to Q26)
DK/Refused (go to Q26)

Q25. Would you be willing to pay to pay $200 more per year in car payments for five years to have this device installed in your car?

Yes
No
DK/Refused

Q26. Now thinking about your household income and other expenses, how confident are you in your previous answers about what you would be willing to pay for a side-impact airbag?

Very confident
Somewhat confident
Not too confident
Not at all confident
DK/Refused

Section 5: Blood Safety

Now, I would like to ask you some questions about the safety of the blood supply used by hospitals.

[Read if Version A1, A2, A3, B1, B2, B3]: Again, please refer to the visual aid on the last page of your packet.

When surgery is performed, a patient will often require a blood transfusion. The needed blood is usually supplied by someone else. If the donated blood is infected with viruses, the patient receiving the blood can develop a serious illness such as Hepatitis or HIV, the Human Immunodeficiency Virus. Hospitals screen blood donors to prevent this problem but no tests are perfect and there is a chance that patients will contract Hepatitis or HIV from donated blood.

Q27. Before I go on, have you ever had surgery in the hospital?

Yes
No
DK/Refused

Q28. Have you ever donated blood?

Yes
No

DK/Refused

Suppose that in the future you decide to undergo elective surgery to reduce chronic chest pain that, while painful, does not threaten your life. Your doctor advises you that a blood transfusion will be required during surgery. Although blood donors are well screened, there is a chance that you will contract either Hepatitis or HIV from the transfusion.

Hepatitis is like a very bad flu. It typically causes nausea and weight loss, and often lasts a few weeks. In rare cases, it can cause death.

HIV, which leads to AIDS or Acquired Immunodeficiency Syndrome, is almost always fatal. However, people infected with the AIDS virus can often lead normal lives at least 10 years or more before their symptoms become severe and eventually fatal.

Version A1, A2, B1, B2
The chance of contracting Hepatitis from a blood transfusion is about 3 in 10,000. On your risk ladder, please note that the highest triangle symbol in green represents your risk of contracting Hepatitis from a blood transfusion. In community terms this means that if everyone in a small town received a blood transfusion in one year, 3 people would contract Hepatitis from the transfusion.

The chance of contracting HIV from a blood transfusion is about 4 in one million. On your risk ladder, please note that the highest circle symbol in red represents your risk of contracting HIV from a blood transfusion. In community terms this means that if everyone in a city received a blood transfusion in one year, 1 person would contract HIV from the transfusion.

Version A3 and B3
The chance of contracting Hepatitis from a blood transfusion is about 3 in 10,000. On your visual aid, a 3 in 10,000 risk is equal to 7 1/2 dots on the page. The chance of contracting HIV from a blood transfusion is about 4 in 1 million. On your visual aid, a 4 in 1 million risk is equal to 10% of 1 dot on the page.

Version A4 and B4
The chance of contracting Hepatitis from a blood transfusion is about 3 in 10,000. The chance of contracting HIV from a blood transfusion is about 4 in 1 million.

[Randomly ask Q29a, Q29b, OR Q29c]

Q29a. Your doctor can order that the blood be tested with a special viral-screening test to reduce the risks associated with blood transfusion, but you must pay an out-of-pocket charge that is not covered by insurance. The special test reduces the chance of developing HIV from 4 in 1 million to 1 in 100 million. The test does not detect the Hepatitis virus, so the risk of Hepatitis is not reduced by the test.

<u>Version A1, A2, B1, B2</u>
On your risk ladder, please note that the lowest circle symbol in red represents your risk of contracting HIV from a blood transfusion, after screening of the blood. In community terms this means that if everyone in a large country received a blood transfusion in one year, one person would be infected with HIV from the transfusion.

<u>Version A3 and B3</u>
On your visual aid, a 1 in 100 million probability is equal to one ten-thousandth of one dot on the page.

Q29b. Your doctor can order that the blood be tested with a special viral-screening test to reduce the risks associated with blood transfusion, but you must pay an out-of-pocket charge that is not covered by insurance. The special test reduces the chance of contracting **Hepatitis** from 300 in 1 million to 1 in 100 million. The test does not detect HIV, so the risk of HIV is not reduced by the test.

<u>Version A1, A2, B1, B2</u>
On your risk ladder, please note that the lowest triangle symbol in green represents your risk of contracting Hepatitis from a transfusion, after screening of the blood. In community terms this means that if everyone in a large country received a blood transfusion in one year, one person would be infected with Hepatitis from the transfusion.

<u>Version A3 and B3</u>
On your visual aid, a 1 in 100 million probability is equal to one ten-thousandth of one dot on the page.

Q29c. Your doctor can order that the blood be tested with a special viral-screening test to reduce the risks associated with blood transfusion, but you must pay an out-of-pocket charge that is not covered by insurance. The special test reduces the chance of developing **either Hepatitis or HIV** to 1 in 100 million. That is, the risk of contracting Hepatitis is reduced from 300 in 1 million to 1 in 100 million and the risk of contracting HIV is reduced from 4 in 1 million to 1 in 100 million.

<u>Version A1, A2, B1, B2</u>
On your risk ladder, please note that the lowest circle in red represents your risk of contracting HIV from a transfusion, after screening of the blood. The lowest triangle in green represents your risk of contracting Hepatitis from a transfusion, after screening of the blood. In community terms this means that if everyone in a large country received a blood transfusion in one year, one person would be infected with HIV and one person would be infected with Hepatitis from the transfusion.

<u>Version A3 and B3</u>
On your visual aid, a 1 in 100 million probability is equal to 1 ten-thousandth of one dot on the page.

[for Q30-Q32, link dollar amounts]

Q30. Would you elect the special blood-screening test if the out-of-pocket charge is **[$100, $250, $500, $700, $1500]?**

Yes
No (skip to Q32)
DK/Refused (skip to Q32)

Q31. Would you elect the special blood screening test if the out-of-pocket charge is **[$200, $500, $1000, $1200, $3000]?**

Yes (skip to Q33)
No (skip to Q33)
DK/Refused (skip to Q33)

Q32. Would you elect the special blood-screening test if the out-of-pocket charge is **[$50, $100, $200, $500, $700]?**

Yes
No
DK/Refused

Q33. Now thinking about your household income and other expenses, how confident are you in your previous answers about how much you would be willing to pay for the special blood test?

Very confident
Somewhat confident
Not too confident
Not at all confident
DK/Refused

****Section 6: WTP for Pneumonia**

[Version A: END if age 60+; Version B: END if age 70+]

For the final set of questions, I would like to ask you about measures you might want to take to prevent Pneumonia and how much they would be worth to you. For this final section of this survey, you do NOT need to look at your packet.

Pneumonia is a serious disease. It is characterized by severe flu-like symptoms and a build-up of fluids in the lungs, and often causes death in older people. For a person of your age and gender, the average chance of living to age 60 **(Version B: ...to age 70)** and older is **[percentage from chart].**

	Version A:		**Version B:**	
	Male	Female	Male	Female
18-29	84%	91%	67%	80%

30-39	86%	92%	68%	81%
40-49	89%	93%	70%	82%
50-59	94%	97%	75%	85%
60-70	NA	NA	87%	92%

Q34. How would you rate your chance of living to age 60 **(Version B: ...to age 70),** compared with others of your age and gender? Is your chance...

Much higher
A little higher
About the same
A little lower
Much lower
DK

[Randomize: ask either Q35a OR Q35b]

Q35a. On average, a person aged 60 has a life expectancy of 21 years. That is, the average 60-year-old will live to age 81. **(Version B: On average, a person aged 70 has a LE of 14 years. That is, the average 70-year-old will live to age 84).**

Suppose that a Pneumonia vaccine will be available to you at age 60 **(B: age 70).** The vaccine is perfectly safe and if you get vaccinated on your 60th **(B: 70th)** birthday, your life expectancy will increase from 21 years to 21 years and 11 months **(B: from 14 years to 14 years and 5 months).** Would you consider getting the vaccine at age 60 **(B: ...age 70)?**

Yes (skip to Q37)
No
DK

Q35b. On average, a person aged 60 has a 4.8% probability of dying each year from all causes. That is, the average chance of living at least one more year is 95.2%. **(Version B: On average, a person aged 70 has a 7% probability of dying each year from all causes. That is, the average chance of living at least one more year is 93%).**

Suppose that a Pneumonia vaccine will be available to you at age 60 **(B: 70).** The vaccine is perfectly safe and if you get vaccinated on your 60th **(B: 70th)** birthday, your annual probability of dying each year would decrease from 4.8% to 4.6% **(B: ...from 7% to 6.8%).** That is, your annual probability of surviving each year will increase from 95.2% to 95.4% **(B: from 93% to 93.2%).** Would you consider getting the vaccine at age 60 **(B: age 70)?**

Yes (skip to Q37)
No
DK

Q36. What is the main reason you would not be willing to get vaccinated against Pneumonia **[Prompt if needed]?**

Benefits too small (skip to End)
Uncertain about benefits (skip to End)
Don't like receiving injections/vaccines (skip to End)
Concerned about safety of vaccine/injection (skip to End)
Other (specify) (skip to End)
DK/Refused (skip to End)

Q37. Now assume that you would have to pay some money this year to have the vaccine available to you when you reach age 60 **(B: age 70).** Assume that you would need to pay this cost out of your own pocket; it would not be covered by insurance. Also assume that no better vaccine would become available before you reach age 60 **(B: age 70).** Considering your current income and expenses, would you pay **(randomize: $220, $400, $750)** this year to have the vaccine available for you on your 60th **(B: 70th)** birthday?

Yes (skip to Q40)
No
DK/Refused

Q38. If it cost only **($40, $80, $130),** would you pay this year to have the vaccine available to you on your 60th **(B: 70th)** birthday?

Yes (skip to Q41)
No
DK/Refused

Q39. What is the main reason you would not pay to have yourself vaccinated against pneumonia **[Prompt if needed]**?

Too expensive/costs too much (skip to Q41)
Benefits too small (skip to Q41)
Uncertain about benefits (skip to Q41)
Benefits not worth the cost (skip to Q41)
Don't like receiving medication (skip to Q41)
Concerned about safety of medication (skip to Q41)
Somebody else should pay (skip to Q41)
Other (specify) (skip to Q41)
DK/Refused (skip to Q41)

Q40. Would you pay **($700, $1,500, $3,500)** this year to have the vaccine available to you on your 60th **(B: 70th)** birthday?

Yes
No
DK/Refused

Q41. How confident are you about whether you would pay to get the Pneumonia vaccine?

Very confident
Somewhat confident
Not too confident

Not at all confident
DK

END: This completes the survey. On behalf of the Harvard School of Public Health, we'd like to thank you very much for your participation in this study.

Johannesson and Johannesson (1996) questionnaire

June 95 **Business School**

July 95

1 **Serial number**

Cost of operation in questions 2 and 3

6 A 100 kronor
B 500 kronor
C 1,000 kronor
D 5,000 kronor
E 15,000 kronor
F 50,000 kronor

FOR WOMEN

1920 - 1934:

A woman of your age has an 85% chance of reaching the age of at least 75. A 75-year-old lives an average of 10 more years.

1935 - 1954:

A woman of your age has an 80% chance of reaching the age of at least 75. A 75-year-old lives an average of 10 more years.

1955 - 1978:

A woman of your age has a 75% chance of reaching the age of at least 75. A 75-year-old lives an average of 10 more years.

FOR MEN

1920 - 1934:

A man of your age has a 75% chance of reaching the age of at least 75. A 75-year-old lives an average of 10 more years.

1935 - 1954:

A man of your age has a 65% chance of reaching the age of at least 75. A 75-year-old lives an average of 10 more years.

1955 - 1976:

A man of your age has a 60% chance of reaching the age of at least 75. A 75-year-old lives an average of 10 more years.

2. Consider the following possibility. If you survive to the age of 75, you will have an opportunity to receive a new medical treatment. It is estimated that this will increase your remaining years of life by one year, i.e. to about 11 years.

Would you buy the treatment if it cost xx kronor and had to be paid for this year?

7 1 No
2 Yes

IF YES:

3. Are you fairly certain or totally certain that you would buy the treatment if it cost xx kronor?

8 1 Totally certain
2 Fairly certain

3. In what year were you born?

9 Year 19........

4. How many people, including yourself, live in the same household as you?

11. Number of people: ....

5. How much education do you have?

12 1 9 years or less, e.g. nine-year compulsory school, elementary school, junior secondary school
2 10 - 12 years, e.g. upper secondary college-preparatory school, trade school, girls' school
3 13 years or more, e.g. university studies

6. Approximately how large is your own monthly income before taxes? Include pensions, but not study allowances.

13 Thousands of kronor, before taxes: ....

IF MARRIED OR LIVING WITH SOMEONE:

7. Approximately how large is your own and your spouse's/cohabitant's combined monthly income before taxes? Include pensions, but not study allowances.

15 Thousands of kronor, before taxes: ....

9. Gender

17	1	Female
	2	Male
		10. County code
18		2-digits
		11. Regional breakdown
20	1	Norrland (counties X, Y, Z, AC and BD)
	2	Stockholm county (county A)
	3	Rest of Svealand (counties C, D, S, T, U and W)
	4	Eastern Götaland (counties E, F, G, H, I and K)
	5	Southern Götaland (counties K, L and M)
	6	Western Götaland (counties O, P and R)
		12. Type of area where you live
21	0	Non-urban
	1	Metropolitan Stockholm, Gothenburg or Malmö

Johannesson et al. (1997) questionnaire

<u>Sept. 1996</u>

Serial no.	☐☐	☐☐☐☐☐	Date	☐☐

Interviewer ☐☐☐☐ Time..... To.......

1 **Serial number**

5 **Blank**

6 **Secondary choices**

A 300 kronor in questions 2 and 3
F 500 kronor in questions 2 and 3
H 1,000 kronor in questions 2 and 3
O 2,000 kronor in questions 2 and 3
R 5,000 kronor in questions 2 and 3
X 10,000 kronor in questions 2 and 3

7 **Blank**

1. In what year were you born?

Year 19 ☐☐

10 **Blank**

<u>IF BORN IN 1957 OR BEFORE:</u>

<u>FOR MEN:</u> It is estimated that roughly 10 men out of 10,000 of your age will die in the next year.

Imagine that you could receive a preventive and pain-free treatment that would reduce your risk of dying during the next year, but have no effects after that year. The treatment would lower your risk of dying next year from 10 to 8 in 10,000.

<u>FOR WOMEN:</u> It is estimated that roughly 6 women out of 10,000 of your age will die in the next year.

Imagine that you could receive a preventive and pain-free treatment that would reduce your risk of dying during the next year, but have no effects after that year. The treatment would lower your risk of dying next year from 30 to 28 in 10,000.

IF BORN 1947-1956:

FOR MEN: It is estimated that roughly 30 men out of 10,000 of your age will die in the next year.

Imagine that you could receive a preventive and pain-free treatment that would reduce your risk of dying during the next year, but have no effects after that year. The treatment would lower your risk of dying next year from 30 to 28 in 10,000.

FOR WOMEN: It is estimated that roughly 20 women out of 10,000 of your age will die in the next year.

Imagine that you could receive a preventive and pain-free treatment that would reduce your risk of dying during the next year, but have no effects after that year. The treatment would lower your risk of dying next year from 20 to 18 in 10,000.

IF BORN 1937-1946:

FOR MEN: It is estimated that roughly 70 men out of 10,000 of your age will die in the next year.

Imagine that you could receive a preventive and pain-free treatment that would reduce your risk of dying during the next year, but have no effects after that year. The treatment would lower your risk of dying next year from 70 to 68 in 10,000.

FOR WOMEN: It is estimated that roughly 40 women out of 10,000 of your age will die in the next year.

Imagine that you could receive a preventive and pain-free treatment that would reduce your risk of dying during the next year, but have no effects after that year. The treatment would lower your risk of dying next year from 40 to 38 in 10,000.

IF BORN 1936 OR BEFORE:

FOR MEN: It is estimated that roughly 200 men out of 10,000 of your age will die in the next year.

Imagine that you could receive a preventive and pain-free treatment that would reduce your risk of dying during the next year, but have no effects after that year. The treatment would lower your risk of dying next year from 200 to 198 in 10,000.

FOR WOMEN: It is estimated that roughly 100 women out of 10,000 of your age will die in the next year.
Imagine that you could receive a preventive and pain-free treatment that would reduce your risk of dying during the next year, but have no effects after that year. The treatment would lower your risk of dying next year from 100 to 98 in 10,000.

2. Would you buy this treatment at present if it cost you XXX kronor?

☐ 1 Yes
☐ 2 No
11

12 **Blank**

IF YES: 3. Are you fairly certain or totally certain that you would buy this treatment if it cost you XXX kronor?

☐ 1 Totally certain
☐ 2 Fairly certain
13

14 **Blank**

The next question concerns the quality of life you imagine having during the next year. Assume that the highest possible quality of life is rated at 10, and that the lowest possible quality of life is rated at 1.

4. Rate the quality of life you believe you will have during the coming year on a scale of 1 to 10.

15 ☐☐ (between 1 and 10)

17 **Blank**

5. How many people, including yourself, live in the same household as you?

18 Number of people ☐

19 **Blank**

6. Are you married or living with someone?

☐ 1 No
☐ 2 Yes, married
☐ 3 Yes, living with someone
20

21 **Blank**

7. How much education have you had?

☐ 1 9 years or less, e.g. nine-year compulsory school, elementary school, junior secondary school
☐ 2 10 - 12 years, e.g. upper secondary college-preparatory school, trade school, girls' school
☐ 3 13 years or more, e.g. university studies
22

23 **Blank**

8. Approximately how large is your own monthly income before taxes? Include pensions, but not study allowances.

24 Thousands of kronor before taxes:

26 **Blank**

IF MARRIED OR LIVING WITH SOMEONE:

9. Approximately how large is your own and your spouse's/cohabitant's combined monthly income before taxes? Include pensions, but not study allowances.

27 Thousands of kronor before taxes:

28 **Blank**

10. Do you live in
READ THE CHOICES ALOUD!

☐ 1 A large city with more than 200,000 inhabitants
☐ 2 A town with 81,000 - 200,000 inhabitants
☐ 3 A town with 21,000 - 80,000 inhabitants
☐ 4 A place with 20,000 or fewer inhabitants
30

31 **Blank**

11. Gender

☐ 1 Female
☐ 2 Male
32

33 **Blank**

12. Region
34 1 Norrland (counties X, Y, Z, AC and BD)
2 Stockholm county (county A)
3 Rest of Svealand (counties C, D, S, T, U and W)
4 Eastern Götaland (counties E, F, G, H, I and K)
5 Southern Götaland (counties L, M and N)
6 Western Götaland (O, P and R counties)

35 **Blank**

13. Type of area where you live
36 0 Non-urban
1 Urban (metropolitan Stockholm, Gothenburg or Malmö)

37 **Blank**

14. Weighting with respect to gender, age and region
38 10 positions

Persson et al. (2001) questionnaire

[University emblem] TRAFFIC ENGINEERING DIVISION

LUND INSTITUTE OF TECHNOLOGY

LUND UNIVERSITY

IHE

The Swedish Institute for

Health Economics

[illegible]

How do you perceive the traffic risks when you walk, bicycle, drive a car, moped or motorcycle, or when you use public transportation?

BACKGROUND QUESTIONS

We would like to begin by asking you a few questions about yourself and your travel habits. Note that the question about how far you drive and/or travel by car refers to travel per ***year***, while this question regarding other means of transportation refers to travel per ***week***. For all means of transportation, except for car, that question is also subdivided into the winter (October – March) and summer (April – September) months.

---

1. Are you? ☐ Male
   ☐ Female

---

2. How old are you?

   ______ years

---

3. How many people live in your household, and how old are they?
   Do not forget to include yourself.

   ______ 0-3 yrs
   ______ 4-10 yrs
   ______ 11-17 yrs
   ______18 and above

---

4. Does your household have access to a car?

   ☐ Yes
   ☐ No

---

5. If the back seat is equipped with a seat belt, do you wear it when seated there?

   ☐ Yes
   ☐ No
   ☐ Sometimes

---

6. Do you use a bicycle helmet when you bicycle?

   ☐ I never bicycle
   ☐ Yes
   ☐ No
   ☐ Sometimes

---

7. How many Swedish miles [1 Swedish mile = 10 km] do you drive and/or travel by car per ***year***?

☐ I never drive or travel by car
☐ 1 – 999 Swedish miles
☐ 1000 – 1499 Swedish miles
☐ 1500 – 1999 Swedish miles
☐ 2000 – 2499 Swedish miles
☐ 2500 Swedish miles or more

---

8. How many km do you travel by any of the following means of public transportation during a ***typical week***?
Bus, street car, commuter train, train or subway

a) During the winter months (October – March)?

☐ I do not normally use public transportation during the winter months
☐ Less than 100 km
☐ 100 – 249 km
☐ 250 – 399 km
☐ 400 km or more

b) During the summer months (April – September)?

☐ I do not normally use public transportation during the summer months
☐ Less than 100 km
☐ 100 – 249 km
☐ 250 – 399 km
☐ 400 km or more

---

9. If you normally do not use public transportation during the summer and winter months, what is your reason for not using public transportation?

☐ There are no connections to and from my normal destination
☐ Departure and arrival times do not fit my schedule
☐ I have to have a car for my job
☐ It seems too much of an effort/too impractical to use public transportation
☐ Other, please specify ________________________

---

10. How many km do you walk by foot in trafficked areas during a ***typical week***?

a) During the winter months (October – March)?

☐ I do not normally walk by foot in trafficked areas during the winter months
☐ Less than 10 km
☐ 10 – 19 km
☐ 20 – 39 km
☐ 40 km or more

b) During the summer months (April – September)?

☐ I do not normally walk by foot in trafficked areas during the summer months
☐ Less than 10 km
☐ 10 – 19 km
☐ 20 – 39 km
☐ 40 km or more

---

11. How many km do you bicycle during a ***typical week***?

a) During the winter months (October – March)?

☐ I do not normally bicycle during the winter months
☐ Less than 25 km
☐ 25 – 49 km
☐ 50 – 99 km
☐ 100 km or more

b) During the summer months (April – September)?

☐ I do not normally bicycle during the summer months
☐ Less than 25 km
☐ 25 – 49 km
☐ 50 – 99 km
☐ 100 km or more

---

12. How many km do you drive a moped during a ***typical week***?

a) During the winter months (October – March)?

☐ I do not normally drive a moped during the winter months
☐ Less than 50 km
☐ 50 – 99 km
☐ 100 – 199 km

☐ 200 km or more
b) During the summer months (April – September)?

☐ I do not normally drive a moped during the summer months
☐ Less than 50 km
☐ 50 – 99 km
☐ 100 – 199 km
☐ 200 km or more

---

13. How many km do you drive and/or ride with someone on a motorcycle during a ***typical week***:

a) During the winter months (October – March)?

☐ I do not normally drive and/or ride with someone on a motorcycle during the winter months
☐ Less than 100 km
☐ 100 – 249 km
☐ 250 – 399 km
☐ 400 km or more

b) During the summer months (April – September)?

☐ I do not normally drive and/or ride with someone on a motorcycle during the summer months
☐ Less than 100 km
☐ 100 – 249 km
☐ 250 – 399 km
☐ 400 km or more

---

14. a) Have you ever been injured in a traffic accident? "Injured" means here seriously enough to require medical care.

☐ Yes
☐ No

b) Have you been injured in a traffic accident during the past year?

☐ Yes
☐ No

c) Has anyone else in your household been injured in a traffic accident during the past year?

☐ Yes

☐ No

Best imaginable condition

100

15. As a way to assess how good or poor a person's health condition is, we have provided the thermometer-like scale to the right. On it, the best imaginable health condition is marked 100 and the worst condition imaginable is marked 0.

We would like you to use the scale to mark how good or poor your health is, in your own opinion. Do this by drawing a line from the box below to the point on the scale that best corresponds to how good or poor your current health condition is.

[scale]

Your current health condition

0

Worst imaginable condition

RISK OF DYING

Most activities that we as human beings perform involve exposing ourselves to different kinds of risks. In the grid on the page to the right, a few different risks of dying are illustrated. There are 100,000 squares in the grid. Each square represents an individual 50 years of age.

The hatched area corresponds to the number of individuals in their fifties per 100,000 people who die on average during a year in Sweden. The risk of dying for individuals in their fifties is 300 in 100,000. That means that in an area where 100,000 inhabitants are in their fifties, 300 of them will die during one year.

The squares marked in black correspond to the number of individuals in their fifties in 100,000 who, during an average year in Sweden, die of the following causes:

The risk of dying of heart disease is 54 in 100,000
The risk of dying of cancer in the stomach or esophagus is 6 in 100,000
The risk of dying in a traffic accident is 5 in 100,000

Below are questions about your perception of the risk of ***dying***

In an average year, the risk of dying is 300 in 100,000 for individuals in their fifties.

16. How great do you think ***your own risk of dying*** is during the coming year? Your own risk may be higher or lower than the average. Consider your age and your current state of health.

    I think the risk is __________ in 100,000

[grid]

The risk of dying is 300 in 100,000

The risk of dying from heart disease is 54 in 100,000

[hatched area and black squares]

The risk of dying in a traffic accident is 5 in 100,000

The risk of dying of cancer in the stomach or esophagus is 6 in 1000,000

In the following question, we would like you to answer how much you personally would be willing to pay for safety equipment and preventive health care that would reduce your own risk of ***dying*** by 10%. Before you decide how the maximum that you- would be willing to pay, we ask you to consider the following conditions:

- The risk reduction only applies to the risk of dying. The risk of being injured or sustaining permanent impairment is not included.
- The safety equipment and preventive health care do not entail any sacrifices or inconveniences etc. You are the only benefiting from these measures. Nobody else's risk factor is involved.
- The safety equipment and preventive health care would be effective during a single year only. You would subsequently have to renew payment if you wanted to continue to benefit from risk reduction.
- If you die, your family's finances will not suffer, since it is assumed that the insurance system will fully cover your family's loss of income and any hospital and medication costs.
- The amount you pay to reduce risk means that you will have less money left over to consume other products and services.

---

17. a) How much would you pay at the most per year to reduce your risk of ***dying*** by 10%?

    Answer: ___________SEK per year

    b) From where would you take the money to pay for a reduction of the risk of dying? In other words, what things would you spend less on?
    You may select more than one alternative.

    ☐ food
    ☐ entertainment, leisure activities, culture and TV
    ☐ alcohol and tobacco
    ☐ saving
    ☐ clothes and shoes
    ☐ residence, house appliances, furniture and decorating
    ☐ computer, cellular phone, etc.
    ☐ daily travel and vacation trips
    ☐ other, please specify _________________

The following questions are about the risk of ***dying*** in a ***traffic accident***

In an average year, the risk of ***dying*** in a ***traffic accident*** is 5 in 100,000 for individuals in their fifties.

---

18. How great do you think ***your own risk*** of ***dying*** in a ***traffic accident*** is in an average year? Your own risk may be higher or lower that the average. Consider how often you are or how much time you spend in traffic, which means of transportation you use, and how you behave in traffic, e.g. how safe a driver you are.

    I think the risk is ___________ in 100,000.

Now, we would like you to disregard completely what you would be willing to pay for increased safety, as covered in question 17, but concentrate only on the following:
In the following question, please answer how much you personally would be willing to pay for safety equipment that would reduce your risk of ***dying*** in a ***traffic accident*** by a 10%. Before you decide the maximum you would be willing to pay, we ask you to consider the following conditions:

- The risk reduction only applies to the risk of dying in a traffic accident. The risk of being injured in a traffic accident is not affected.

- The safety equipment is not inconvenient, unattractive or uncomfortable to use. It is not noticeable at all. You are the only person who can use the equipment. Nobody else's risk factor is involved.

- The safety equipment will only function for one year. You would subsequently have to renew payment if you wanted to continue to benefit from risk reduction.

- An accident will not affect your family's finances, since it is assumed that the insurance system will fully cover your family's loss of income and any hospital and medication costs.

- The amount you pay to reduce the risk means that you will have less money left over to consume other products and services.

---

19. How much would you pay at the most per year to reduce your risk of ***dying*** in a ***traffic accident*** by a 10%?

Answer: ___________SEK per year

The following question is about how you would use an increase in income

---

20. Imagine that you get a monthly increase in income of SEK 1000 after taxes next year. How would you distribute this increase in income? Distribute the SEK 1000 on the alternatives below. You do not need to use all the alternatives. When you add the amounts that you spend on the different alternatives, it should equal SEK 1000.

______ SEK on food

______ SEK on entertainment, leisure activities, culture and TV

______ SEK on alcohol and tobacco

______ SEK on saving

______ SEK on clothes and shoes

______ SEK on home ownership/rent, household appliances, furniture and interior decoration

______ SEK on safety equipment, e.g. life jacket, helmet, fire extinguisher, winter tires

______ SEK on preventive health care, exercise, dental care, etc.

______ SEK on computer, cellular phone, etc.

______ SEK on daily travel and vacation trips

______ SEK on a new car

______ SEK on other things, please specify ________________________

CONCLUDING QUESTIONS

---

21. What kind of car/cars does your household own?

	Car # 1	Car # 2	Car # 3
Make and model:			
Year of manufacture:			
Equipment in the car:	☐ ABS brakes ☐ Airbag ☐ Neither	☐ ABS brakes ☐ Airbag ☐ Neither	☐ ABS brakes ☐ Airbag ☐ Neither

---

22. What is your level of education?

☐ Grades 1 – 8 (Elementary and Middle School) or the equivalent
☐ High School (9-12) or the equivalent
☐ College, University or the equivalent
☐ Other, please specify ______________________

---

23. What is your combined yearly ***household*** income (i.e. income from employment, pension and/or own business?) before taxes?

☐ SEK 0 – 79, 900
☐ SEK 80,000 – 159,999
☐ SEK 160,000 – 239,999
☐ SEK 240, 000 – 319,999
☐ SEK 320, 000 – 399, 999
☐ SEK 400,000 – 599,999
☐ SEK 600,000 or above

---

24. May we contact you by phone you if we need additional information about any of the answers in your questionnaire? If yes, please provide your telephone number (including area code) below.

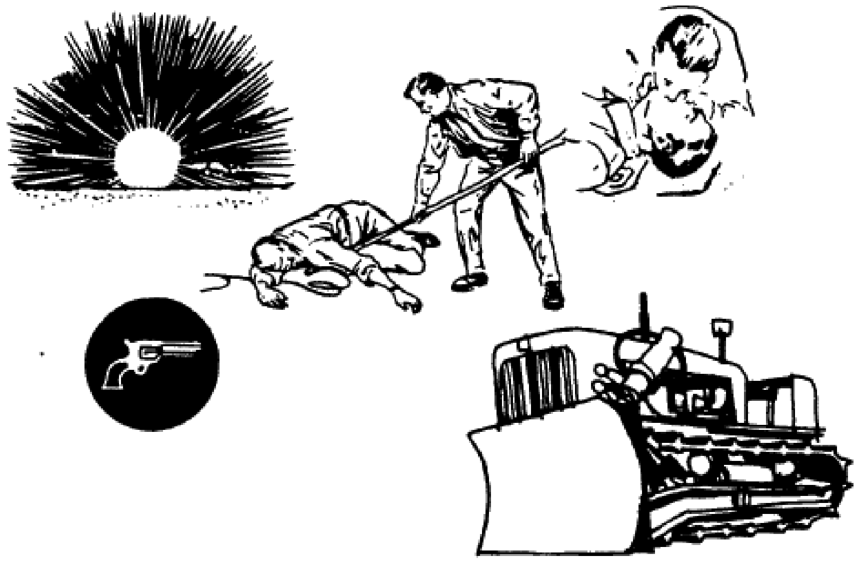
My phone number: __________________________

THANK YOU FOR YOUR HELP!

Gerking et al. questionnaire

[redacted] [redacted]

Job Safety In The United States
How Much Is Needed?



A Nationwide Survey on an Important Issue Facing Congress and the American People.

This questionnaire should be completed by the principle wage-earner in your household.

INSTITUTE FOR POLICY RESEARCH
University of Wyoming
Laramie, Wyoming 82070

1

ABOUT YOUR JOB

Q-1 First, we would like to ask a few questions about the work you do. In 1983 were you: (Please circle the number of your answer)

1 EMPLOYED PART-TIME
2 EMPLOYED FULL-TIME
3 RETIRED
4 UNEMPLOYED

> **Inasmuch as the questions we need to ask only concern people's 1983 job, it won't be necessary for you to complete the rest of the questions. However, we would greatly appreciate your checking this box /_/ and returning the questionnaire so we can take your name off of the mailing list. Many thanks for your cooperation. We greatly appreciate it.**

Q-2 Please describe your main job or position in 1983 (if you had more than one job in 1983 we only need to know about your main job).

TITLE OF JOB OR POSITION: ______________________

NATURE OF THE WORK YOU DO: ______________________

IN WHAT KIND OF BUSINESS OR INDUSTRY IS YOUR WORKPLACE: ______________________

Q-3 Which one of the following occupational categories most closely reflects the type of work you do in your job? A few examples are given to help you decide. (Please circle the number of your answer)

1	SERVICE WORKER	(Food service workers, Cleaning service workers, Dental assistants, Policemen)
2	LABORER	(Longshoremen, Construction workers, Loggers, Garbage collectors)
3	TRANSPORTATION OPERATOR	(Bus drivers, Taxicab drivers, Truck drivers, Railroad switch operators)
4	EQUIPMENT OPERATOR . .	(Textile workers, Drillers, Photographic processors, Smelters)
5	CRAFT WORKER	(Carpenters, Machinists, Bakers, Tailors, Repairmen, Mechanics)
6	CLERICAL WORKER	(Cashiers, Tellers, Secretaries, Receptionists, Telephone operators, Dispatchers)
7	SALES WORKER	(Advertising agents, Real estate agents, Sales clerks, Sales representatives, Vendors)
8	MANAGER OR ADMINISTRATOR	(Bank officers, Purchasing agents, Restaurant managers, School administrators)
9	PROFESSIONAL OR TECHNICAL	(Accountants, Engineers, Physicians, Teachers, Entertainers)
10	FARMWORKER	(Farmers, Farm laborers, Farm supervisors)

HOW SAFE IS YOUR JOB

Q-4 Some people face a high risk of injury and death from accidents on the job and others face a very low risk. Compared to most other jobs, do you feel your main job in 1983 was: (Please circle the number of your answer)

1 MUCH SAFER
2 SOMEWHAT SAFER
3 ABOUT AVERAGE
4 SOMEWHAT RISKIER
5 MUCH RISKIER

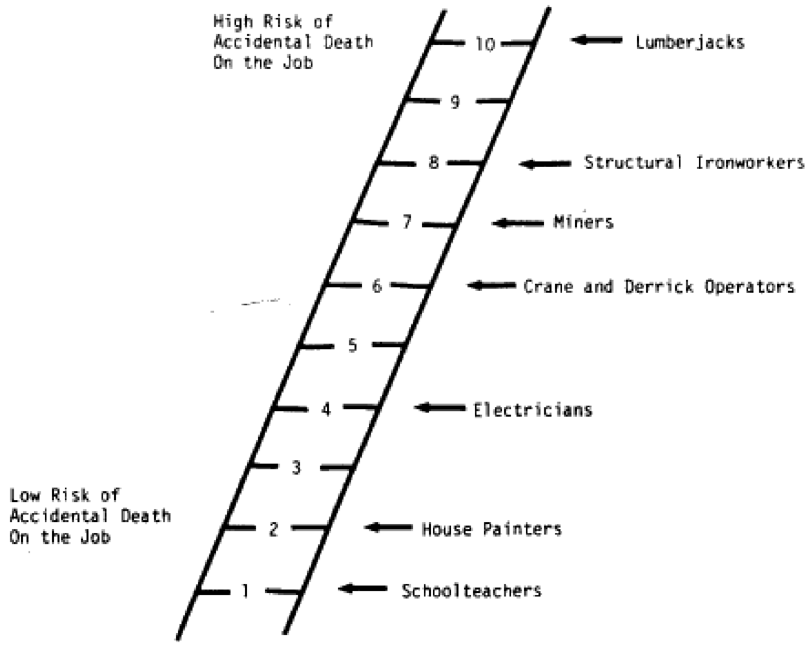
Q-5 Below are listed the major causes of how people die on the job. Depending on your particular job, some causes are not very likely to happen to you while others are more likely to happen. On a scale from 1 (could never happen) to 5 (most likely to happen), please circle the number which best indicates your feelings towards the chances of dying on a job like yours, as compared to other jobs, from each of the causes.

Please circle one number for each cause

MAJOR CAUSES OF ACCIDENTAL DEATH AT WORK	COULD NEVER HAPPEN				MOST LIKELY TO HAPPEN
A. On the road motor vehicle accident	1	2	3	4	5
B. A fall	1	2	3	4	5
C. Heart attack	1	2	3	4	5
D. Getting hit by industrial vehicle or equipment	1	2	3	4	5
E. Getting hit by object other than vehicle or equipment	1	2	3	4	5
F. Caught in, under or between objects other than vehicle or equipment	1	2	3	4	5
G. Electrocution	1	2	3	4	5
H. Gun shot	1	2	3	4	5
I. Airplane crash	1	2	3	4	5
J. Fire	1	2	3	4	5
K. Plant machinery operation	1	2	3	4	5
L. Explosion	1	2	3	4	5
M. Gas inhalation	1	2	3	4	5

JOB RELATED RISK

The ladder below shows levels of job-related accidental risk of death. Each step shows the number of deaths per year for every 4,000 people in an occupation. The higher on the ladder, the more accidental "on the job" deaths there are each year for that occupation. A few example occupations are given and they are placed on the ladder according to their actual levels of risk. Note that schoolteachers have about one death per 4,000 workers and lumberjacks have about 10 deaths per 4,000 workers each year. Of course, your 1983 job does have a level of risk somewhere on the ladder even if it has not been listed as one of the examples. Questions 6 and 7 refer to this ladder.



Q-6 Now, please think about your main job in 1983 for a minute. In your opinion, which step on the ladder comes closest to describing the risk of accidental death in your job. (Please circle the step number of your answer)

STEP 10
STEP 9
STEP 8
STEP 7
STEP 6
STEP 5
STEP 4
STEP 3
STEP 2
STEP 1

(IF you picked STEP 10)

Just suppose the actual risk of accidental death in your job was Step 9 (meaning 9 out of every 4,000 workers in this job die each year). What is the smallest increase in annual gross (i.e., before deductions and taxes) income from your job that you would have to be paid in order to accept an increase in the risk of accidental death from Step 9 up to Step 10 (i.e., one more death per year for every 4,000 workers)? (Please circle the number)

1	($ 0)	20	($ 380)
2	($ 20)	21	($ 400)
3	($ 40)	22	($ 420)
4	($ 60)	23	($ 440)
5	($ 80)	24	($ 460)
6	($100)	25	($ 480)
7	($120)	26	($ 500)
8	($140)	27	($ 600)
9	($160)	28	($ 700)
10	($180)	29	($ 800)
11	($200)	30	($ 900)
12	($220)	31	($1000)
13	($240)	32	($2000)
14	($260)	33	($3000)
15	($280)	34	($4000)
16	($300)	35	($5000)
17	($320)	36	($6000)
18	($340)	37	(More than $6000)
19	($360)		

(IF you picked a step from 1 to 9)

Consider a situation in which you were asked to face more risk on your job. What is the smallest increase in annual gross (i.e., before deductions and taxes) income from your job that you would have to be paid in order to accept an increase in the risk of accidental death by one step (i.e., one more death per year for every 4,000 workers)? (Please circle the number)

1	($ 0)	20	($ 380)
2	($ 20)	21	($ 400)
3	($ 40)	22	($ 420)
4	($ 60)	23	($ 440)
5	($ 80)	24	($ 460)
6	($100)	25	($ 480)
7	($120)	26	($ 500)
8	($140)	27	($ 600)
9	($160)	28	($ 700)
10	($180)	29	($ 800)
11	($200)	30	($ 900)
12	($220)	31	($1000)
13	($240)	32	($2000)
14	($260)	33	($3000)
15	($280)	34	($4000)
16	($300)	35	($5000)
17	($320)	36	($6000)
18	($340)	37	(More than $6000)
19	($360)		

Q-7 In this question several different jobs are listed (A through O). Each of the jobs are identical to your 1983 job except that their risk and salary levels are different than your 1983 job. The risk level for each job is one of the steps on the ladder (see page 3). The salary for each job is your 1983 salary, plus or minus some percentage of that salary. On a scale from 1 (much worse job) to 10 (much better job), please circle the number which best indicates your opinion of how each job would compare to your 1983 job. Thus, a job with risk level 1 and twice your 1983 salary might get a high number. A job with risk level 10 and half your 1983 salary might get a low number. Also, a job that you feel would be just as good as your 1983 job would get a 5. Please circle one number for each job.

JOB	RISK LEVEL	SALARY COMPARED TO 1983	Circle one for each job: MUCH WORSE JOB				YOUR 1983 JOB				MUCH BETTER JOB
A.	Step 2	10% more . . .	1	2	3	4	5	6	7	8	9
B.	Step 7	5% less . . .	1	2	3	4	5	6	7	8	9
C.	Step 3	the same . . .	1	2	3	4	5	6	7	8	9
D.	Step 9	10% less . . .	1	2	3	4	5	6	7	8	9
E.	Step 1	10% less . . .	1	2	3	4	5	6	7	8	9
F.	Step 4	5% more . . .	1	2	3	4	5	6	7	8	9
G.	Step 5	10% less . . .	1	2	3	4	5	6	7	8	9
H.	Step 8	the same . . .	1	2	3	4	5	6	7	8	9
I.	Step 6	10% more . . .	1	2	3	4	5	6	7	8	9
J.	Step 4	5% less . . .	1	2	3	4	5	6	7	8	9
K.	Step 10	the same . . .	1	2	3	4	5	6	7	8	9
L.	Step 5	the same . . .	1	2	3	4	5	6	7	8	9
M.	Step 7	5% more . . .	1	2	3	4	5	6	7	8	9
N.	Step 9	10% more . . .	1	2	3	4	5	6	7	8	9
O.	Step 2	10% less . . .	1	2	3	4	5	6	7	8	9

MORE ABOUT YOUR JOB

Q-8 How much formal education is required to get a job like your 1983 job? (Please circle the number)

1 0- 8 GRADES
2 6- 9 GRADES; FINISH GRADE SCHOOL
3 9-11 GRADES; SOME HIGH SCHOOL
4 12 GRADES; FINISH HIGH SCHOOL
5 SOME COLLEGE, NO DEGREE NECESSARY
6 COLLEGE DEGREE; BA OR BS
7 SOME GRADUATE WORK
8 ADVANCED COLLEGE DEGREE OR PROFESSIONAL DEGREE

Q-9 Do you have to have some work experience or special training to get a job like your 1983 job? (Please circle the number)

1 YES → If YES, what kind of experience or special training is that? (Please circle the number)

1 APPRENTICESHIP
2 VOCATIONAL TRADE SCHOOL
3 ON-THE-JOB TRAINING
4 WORK EXPERIENCE FROM ANOTHER JOB
5 OTHER (Please specify) ______________________

____

2 NO

Q-10 On a job like your 1983 job, how long would it take the average new person to become fully trained and qualified?

____________YEARS OR ____________MONTHS (IF LESS THAN A YEAR)

Q-11 How long have you worked for your present employer?

____________YEARS OR ____________MONTHS (IF LESS THAN A YEAR)

Q-12 How long have you done the type of work you do?

____________YEARS OR ____________MONTHS (IF LESS THAN A YEAR)

Q-13 Do you have any physical or nervous condition that limits the type of work or the amount of work you can do in your job? (Please circle the number)

1 YES
2 NO

Q-14 Do you have any physical or nervous condition that would limit the type of work or the amount of work you could do in another job you would like? (Please circle the number)

1 YES
2 NO

Q-15 In 1983, did you work for yourself? (Please circle the number)

1 YES
2 NO → If NO, then did you work for the Federal, state or local government? (Please circle the number)

1 YES
2 NO

Q-16 Did you supervise the work of other employees, or tell them what to do? (Please circle the number)

1 YES → If YES, then did you have any say about their pay or promotion? (Please circle the number)
2 NO

1 YES, ALL OF THEM
2 YES, SOME OF THEM
3 NO, NONE OF THEM

About how many people did you supervise?

_____________PEOPLE

Q-17 Approximately how many people are employed where you work?

_____________NUMBER OF PEOPLE

Q-18 Is your job covered by a union contract? (Please circle the number)

1 YES → If YES, do you belong to that union? (Please circle the number)
2 NO

1 YES
2 NO

Q-19 How many weeks did you actually work on your job in 1983?

_____________WEEKS

Q-20 On the average, how many hours a week did you work on your job in 1983?

_____________HOURS

Q-21 Did you have any overtime which is not included in that? (Please circle the number)

1 YES → If YES, then how many hours did that overtime amount to in 1983?
2 NO

_____________HOURS

ABOUT YOU

Q-22 What is your age?

_____________ YEARS

Q-23 What is your sex? (Please circle the number)

1 MALE
2 FEMALE

Q-24 What is your race? (Please circle the number)

1 BLACK
2 ORIENTAL
3 HISPANIC OR PERSON OF MEXICAN DESCENT
4 WHITE
5 OTHER (Please specify) ______________________________

Q-25 How much formal education have you completed? (Please circle the number)

1 0- 5 GRADES
2 6- 8 GRADES; FINISHED GRADE SCHOOL
3 9-11 GRADES; SOME HIGH SCHOOL
4 12 GRADES; FINISHED HIGH SCHOOL
5 TRADE SCHOOL
6 SOME COLLEGE
7 COLLEGE DEGREE; BA OR BS
8 SOME GRADUATE WORK
9 ADVANCED COLLEGE DEGREE OR PROFESSIONAL DEGREE

Q-26 In what type of area do you live? (Please circle the number)

1 RURAL
2 SUBURBAN
3 CENTRAL CITY

Q-27 Have you moved in the last three years? (Please circle the number)

1 YES
2 NO

Q-28 About how many miles is your job from where you live?

_____________ MILES

Q-29 On the average, how long does it take to travel from your home to your job?

_____________ HOUR <u>OR</u> _____________ MINUTES (IF LESS THAN AN HOUR)

Q-30 In what type of area is your job located? (Please circle the number)

1 RURAL
2 SUBURBAN
3 CENTRAL CITY

Q-31 How many years have you been employed since you were 18?

______________YEARS

Q-32 How many of these years were you employed full time for most of the year?

______________YEARS

Q-33 In general, how satisfied were you with your main job in 1983? (Please circle the number)

1 VERY SATISFIED
2 SATISFIED
3 NEUTRAL
4 DISSATISFIED
5 VERY DISSATISFIED

Q-34 Are you a veteran? (Please circle the number)

1 YES
2 NO

Q-35 Of the total fringe benefit package <u>paid by your employer</u> of your job in 1983 (e.g., workman's compensation, pension plan payments, health insurance payments, etc.), approximately what percentage of your gross annual earnings was this package worth? (Please circle the number)

1 0%-10%
2 11%-20%
3 21%-30%
4 31%-40%
5 41%-50%
6 DON'T KNOW

Q-36 Approximately what percentage of your <u>total income</u> received in 1983 was made up of government assistance payments (e.g., welfare, social security, veterans benefits, unemployment compensation, etc.)? (Please circle the number)

1 0%
2 1%-10%
3 11%-20%
4 21%-30%
5 31%-40%
6 41%-50%
7 51%-60%
8 61%-70%
9 71%-80%
10 81%-90%
11 91%-100%

Q-37 How were you paid in your 1983 job? (Please circle one number)

1 SALARY
2 HOURLY WAGE
3 OTHER

(IF Other) How were you paid on your job in 1983? (Please circle the number)

1 PIECE WORK
2 COMMISSION ONLY
3 COMMISSION AND SALARY
4 TIPS ONLY
5 TIPS AND SALARY
6 OTHER (Please specify) ______________________

How much was the annual gross (i.e., before deductions and taxes) income you received from your main job in 1983?

$____________

If you worked more hours than average during some week, did you get paid for those extra hours? (Please circle the number)

1 YES → (IF Yes) About how much would you make, per hour, for that overtime?
2 NO

$____________PER HOUR

(IF Hourly) What was your hourly wage rate on your job for your regular or "straight" work time in 1983?

$____________PER HOUR

What was your hourly wage rate on your job for your overtime in 1983?

$____________PER HOUR

(IF Salary) How much was your annual gross (i.e., before deductions and taxes) income you received from your job in 1983?

$____________

If you worked more hours than average during some week, did you get paid extra for those extra hours? (Please circle the number)

1 YES → (IF Yes) About how much did you make, per hour, for that overtime?
2 NO

$____________PER HOUR

Is there anything we may have overlooked? Please use this space for any additional comments you would like to make about the need for on-the-job safety in the United States.

Your contribution to this effort is very greatly appreciated. If you would like a summary of results, please print your name and address on the back of the return envelope (NOT on this questionnaire). We will see that you receive it.

Lanoie et al. questionnaire

Respondent No.			Industry No.			

Hello, my name is Carmen Pedro, a graduate student in Economics at the Hautes Études Commerciales. Today, I will be asking you some questions on the value you give to safety. The answers that you will provide will be used to complete my thesis. Your cooperation would be greatly appreciated and please rest assured that **all answers are** strictly **confidential**. Please read the questionnaire carefully.

MAIN QUESTIONNAIRE

Brief Explanation **(This is a fictitious example)**

The probability of a bus passenger being killed in a road accident is 1 in 10 000 per year. This means that out of 10 000 passengers, 1 will die in a road accident this year.

The probability of a motorcyclist being killed in a road accident is 8 in 10 000 per year. This means that out of 10 000 motorcyclists, 8 will die in a road accident this year.

Therefore, a motorcyclist has a greater probability of dying in a road accident than a bus passenger.

1A. Imagine that you have to buy a new car. You have chosen a model which you find satisfactory. The salesperson informs you that for an additional amount you can have air bags installed which inflate automatically in case of a collision reducing your risk of injury. These inflatable air bags reduce your personal risk of death in car accident from 4 in 10 000 to 2 in 10 000. Your risk level is therefore reduced in half.

What amount would you be willing to pay to have inflatable air bags installed in your car. The cost of inflatable air bags is the same regardless of the type of car (compact, sportive, luxurious, etc.). Please bear in mind how much you can afford.

____________ ENTER IN AMOUNT, YOU WOULD BE WILLING TO PAY TO HAVE YOUR PERSONAL RISK REDUCED FROM 4 IN 10 000 TO 2 IN 10 000

IF YOU HAVE ANY COMMENTS PLEASE INSCRIBE THEM BELOW.

1B. Imagine that the only model of the car that you desire is equipped with inflatable air bags. The salesperson informs you that the price of the car can be reduced if the air bags are removed. What reduction in price must you be offered before you accept to have the inflatable air bags removed?

____________ ENTER IN AMOUNT

IF YOU HAVE ANY COMMENTS PLEASE INSCRIBE THEM BELOW.

2. Below you will find a list of 10 major causes of injury at work. For each cause, I would like you to rate the likelihood of each occuring to you at work on a scale of 1 to 5.

"1" Could never happen
"2" Could rarely happen
"3" Could possibly happen
"4" Likely to happen
"5" Most likely to happen

In each case, please **CIRCLE** the number which best applies.

		Could never	Could rarely	Could possibly	Likely to	Most likely to
1.	**Fall from elevation**	1	2	3	4	5
2.	**Caught in, under or between object**	1	2	3	4	5
3.	**Motor vehicle accident**	1	2	3	4	5
4.	**Fall (not from elevation)**	1	2	3	4	5
5.	**Physical overexertion**	1	2	3	4	5
6.	**Electrocution**	1	2	3	4	5
7.	**Bodily reactions (allergies, headaches... due to work substances or improper ventilation)**	1	2	3	4	5
8.	**Exposure to extreme temperatures**	1	2	3	4	5
9.	**Struck by/against an object**	1	2	3	4	5
10.	**Exposure to radiation, toxic and noxious substances**	1	2	3	4	5

3. Below you will find a ladder defining the risk level of certain occupations, I would like you to circle the step number that most closely describes **your** risk of a job-related accidental death.

High Risk of Accidental Death on the Job

- 10- ← Dynamiter in a mine
- 9 - ← Firefighter
- 8 - ← Metal worker (iron, steel, etc.)
- 7 - ← Worker in production of chemical products
- 6 - ← Truck driver
- 5 - ← Lumberjack
- 4 - ← Electrician
- 3 - ← Driver-salesman
- 2 - ← Teller & Cashier
- 1 - ← Secretary

Low Risk of Accidental Death on the Job

4A. IF CIRCLED "10" AT QUESTION 3, PLEASE GO TO QUESTION 4B.

How much more per week, before taxes, would you require to voluntarily work at the same job if the risk of accidental death was instead **one step higher on the ladder**?

____________ ENTER IN ADDITIONAL AMOUNT PER WEEK, BEFORE TAXES

IF YOU HAVE ANY COMMENTS PLEASE INSCRIBE THEM BELOW.

IF YOU ANSWERED QUESTION 4A, PLEASE GO TO QUESTION 5A.

4B. Suppose initially you were on the 9th step, how much per week, before taxes, would you require to voluntarily work at the **same** job if the risk of accidental death was instead **one step higher on the ladder**; i.e., how much would you require to move from the 9th step to the 10th step?

____________ ENTER IN ADDITIONAL AMOUNT PER WEEK, BEFORE TAXES

IF YOU HAVE ANY COMMENTS PLEASE INSCRIBE THEM BELOW.

5A. IF CIRCLED "1" AT QUESTION 3, PLEASE GO TO QUESTION 5B.

How much of a decrease per week, before taxes, would you willingly forego to work at the **same** job if the risk of accidental death was instead **one step lower on the ladder**?

____________ ENTER IN AMOUNT PER WEEK, BEFORE TAXES, THAT YOU WOULD WILLINGLY FOREGO

IF YOU HAVE ANY COMMENTS PLEASE INSCRIBE THEM BELOW.

IF YOU ANSWERED QUESTION 5A, PLEASE GO TO QUESTION 6.

5B. Suppose you were initially on the 2nd step, how much of a decrease per week, before taxes, would you willingly forego to work at the same job if the risk of accidental death was instead one step lower on the ladder; i.e., how much would you willingly forego to move from the 2nd step to the 1st step?

____________ ENTER IN AMOUNT PER WEEK, BEFORE TAXES, THAT YOU WOULD WILLINGLY FOREGO

IF YOU HAVE ANY COMMENTS PLEASE INSCRIBE THEM BELOW.

____

____

6. The probability of being killed on the job as a firefighter, a relatively dangerous job, is approximately 5.8 in 10 000, per year.

The probability of being killed on the job as an office worker, a relatively safe job, is approximately .057 in 10 000, per year, which is 102 times lower than the probability of being killed on the job as a firefighter.

With this in mind, what do you think is your risk of being killed on the job per year? Please take into consideration that if your job requires that you travel, excluding transportation to and from work, this may constitute an element of risk.

_____._____ ENTER IN YOUR RISK OF BEING KILLED ON THE JOB PER YEAR

7A. Suppose now, that you are a construction worker for the city of Montreal. Your risk of being killed on the job in a given year is 2 in 10 000. This means that out of 10 000 construction workers for the City, 2 will die on the job this year. You have been offered a job with a private construction company. Your work hours, fringe benefits and everything else will be equal, except that you will be exposed to more dangerous working conditions, for example working on higher buildings. Your risk of being killed on the job is now 4 in 10 000 in a given year. This is twice the risk of the job with the city of Montreal. How much more will you have to be paid, before taxes, per week, before you accept this job?

__________ ENTER IN ADDITIONAL AMOUNT PER WEEK, BEFORE TAXES

IF YOU HAVE ANY COMMENTS PLEASE INSCRIBE THEM BELOW.

____

____

7B. Suppose now, you are a production worker in a chemical company in Montreal. Your risk of being exposed to radiation leading to an incurable disease in a given year is 2 in 10 000. This means that out of 10 000 chemical workers, 2 will contract an incurable disease this year. You have been offered a job with another company. Your work hours, fringe benefits and everything else will be equal, except that you will be working in more dangerous conditions; for example, with older equipement. Your risk of being exposed to radiation leading to an incurable disease in a given year is now 4 in 10 000. This is twice the risk of the previous job. How much more will you have to be paid, before taxes, per week, before you accept this job?

__________ ENTER IN ADDITIONAL AMOUNT PER WEEK, BEFORE TAXES

IF YOU HAVE ANY COMMENTS PLEASE INSCRIBE THEM BELOW.

____

____

8. How many work days last year, if any, have you missed due to illness or injury related to your job?

__________ ENTER IN NUMBER OF DAYS

Please pardon the delicate nature of this next question.

9. Have you or any family member (close friend) ever been severely injured, sick or killed due to a job-related accident? «Severely injured or sick» in this case means a permanent disability, hospitalization, a loss of a limb, etc.

YES..........1
NO............2

10. Is you car equipped with inflatable air bags?

YES..........1
NO............2

Finally, so that I may classify the information you have given me, I would like to ask you some questions about yourself. Please let me remind you, again, that all answers are strictly confidential.

11. In your present employment do you work?

FULL-TIME..........1
OR PART-TIME..........2

12. How many hours per week do you work on average?

____________ ENTER NUMBER OF HOURS

13. What is your total weekly wage before taxes?

____________ ENTER IN YOUR WEEKLY WAGE BEFORE TAXES

14. What is your occupation? NOT where you work, just the type of job that you perform.

____

____

____

15. How long have you been employed at this occupation? Please also include in your answer, the number of years employed in other firms working in this occupation.

_____________ ENTER IN NUMBER OF YEARS

16. Are you unionized?

YES..........1
NO............2

17. How long have you been employed with this establishment?

_____________ ENTER IN NUMBER OF YEARS

18. In your present job, are you a supervisor?

YES..........1 → GO TO QUESTION 19
NO............2 → GO TO QUESTION 20

19. How many people do you supervise?

_____________ ENTER IN NUMBER OF PEOPLE

20. Are you married or living with someone, as a married couple?

YES..........1 → GO TO QUESTION 21
NO............2 → GO TO QUESTION 22

21. Does your spouse work outside the home?

YES..........1
NO............2

22. How many dependents do you have?

_____________ ENTER IN NUMBER OF DEPENDENTS

23. What level of formal education did you reach?

	REACHED	GRADUATED
Elementary	1	2
Secondary or high school	3	4
Community College, CEGEP, or Vocational School	5	6
University: Undergraduate school	7	8
University: Postgraduate school	9	10
No formal schooling	11	

24. Could you please tell me how old you are?

____________ ENTER IN AGE

25. Please circle your sex:

MALE............1
FEMALE........2

26. Is there anything else that you would like to add that we have not covered? For instance, did you have any difficulties answering any of the questions. If yes which questions and why.

____

____

Optional:

Name ________________________
Telephone ________________________

Thank you very much for your cooperation.

Appendix C.

Comments on selected questionnaires.

Corso et al questionnaire

This questionnaire was administered over the telephone, but the respondent had been previously sent written survey materials (i.e., this was a phone-mail-phone survey).
The respondent is first asked quality of life questions, then the first of 6 modules begins. The six modules (=sections) are on unrelated causes of deaths or of contracting certain diseases, and risk reductions are considered separately in each of these modules. Only the responses to the WTP questions collected as part of section four are analyzed in Corso et al. (2001), and we are not aware of other peer-reviewed articles based on this questionnaire at this time.

The **first section** is dedicated to longevity lottery questions. The **second questions** introduces the concept of prevention and treatment, and the **third section** applies it to the hypothetical situation where the respondent is traveling to a foreign country and exposing himself to the risk of contracting a foodborne illness. The concept of compound probability is used, because the respondent is first told about the probability of contracting the disease, and then of dying, assuming that the disease is contracted in the first place.

Double-bounded dichotomous choice questions are asked about (i) a preventive treatment that would reduce the risk of contracting and hence dying of the foodborne illness from 4 in 100,000 to 2 in 100,000 (a 50% reduction, which is pointed out to the respondent), and a (ii) treatment that would reduce the same risk by the same amount, assuming that the illness is contracted. The respondent is told that he would be seen by an American doctor and treated in an American clinic with American medication. If the respondent declines to pay, follow-up questions inquire about the reasons why.

The **fourth section** is about auto safety. The responses collected through this section are those analyzed in Corso et al. (2001). This section starts with questions about use and beliefs about the effectiveness of airbags. The respondent is told that the average baseline risk of dying in a car accident is X in 10,000 every year. (Slightly different wording is used in describing this risk, depending on which visual aid treatment the respondent is assigned to.) This is followed by questions asking the respondent how concerned he is about this risk, and whether he thinks that his own risk is higher, lower, or the same as the average.

The risk reduction would be delivered by a side-impact airbag, which would reduce the risk of dying from 2 in 10,000 (2.5 in 10,000 in split samples) to 1.5 in 10,000. Clearly, the risk reduction is varied to the respondent, and can take one of two possible values (1 or 0.5 in 10,000.) The risk reduction is also translated into “community comparison” (i.e., how many people out of the residents of a small town).

The payment vehicle for the risk reduction is an increase in the car payments per year over 5 years. The elicitation format is double-bounded dichotomous choice.

In **section five**, respondents are asked to consider blood safety in the hypothetical situation that they need a transfusion during elective surgery to reduce chest pains (a painful but not a life threatening situation). They are told that the risk of blood infected with the hepatitis virus is 3 in 10,000 while the risk of blood infected with the HIV virus is 4 in a million. This section focuses on the risk of contracting the disease, but not on the risk of dying (respondents are told that hepatitis is rarely fatal, whereas HIV is, although people live normal lives for 10 years or more before experiencing severe symptoms).

Respondents are then asked questions about their WTP for a screening exam that would reduce the risk of hepatitis alone, HIV alone, or both (split samples). It should be noted that the risks of exposure to the different type of virus is shown in the same visual aid, so that the 3 in 10000 is converted into 300 in a million, and later the respondent is explicitly told about this risk being 300 in a million.

Section six of the questionnaire focuses on (a) a risk reduction or (b) extension in life expectancy to be experienced at age 60 (70—split sample for this age as well as for (a) and (b)), and begins with providing the respondent the chance for a person of his age and gender to survive to age 60 (70).

It describes a pneumonia vaccine that would (a) extend remaining life expectancy at age 60 (70) from 21 (14) years to 21 years and 11 months (14 years and 5 months), or (b) reduce the risk of dying at age 60 from 4.8% in that year to 4.6% in that year. One of the two versions of this questions, therefore, is similar to that of Johannesson and Johansson (1996), although the latter focuses on a life expectancy extension at age 75 and is not specific about the type of the medical intervention that would raise the remaining life expectancy.

Clearly, this is a rather long questionnaire with many risk reductions, baseline risks and risk reductions of different magnitude. One wonders about possible confusion on the part of the respondent and the ability to stay focused through this phone interview. It should be pointed out that the risk reduction and WTP responses analyzed in Corso et al. refer to section 4 of the questionnaire.

- **Mode of administration of the survey.** Phone-mail-phone.

- **Does the questionnaire use visuals to explain risks and risk reductions?** Yes, in some of the split samples, and the visual aids vary across respondents (one of the purposes of the study was to test visual aids and their effects on scope).
- **What is the type of graphical representation (pie chart, grid, risk ladder, etc.) and is the quality of the graphical representation acceptable?** No aid, linear scale, log scale, dots, with the scale always providing a community comparison or a comparable risk. I find the risk scales distracting—busy and with information overload that might detract attention from the magnitude of the risks.

- **Were respondents ask to report subjective baseline risks?** Only in a qualitative fashion. For example, after being told that the risk of dying in a car accident is X in 10,000, they are asked whether they think that their own risk is higher or lower.
- **Subjective or objective risk reduction?** objective.
- **If the baseline risk was assigned to the respondent by the questionnaire, was the risk gender-specific? Age-specific? Location-specific? Occupation-specific?** No, only random variation as per randomized treatment.
- **What context does the risk refer to? Transportation risk? Workplace risks? Risks associated with a particular health condition (e.g., diabetes or hypertension)? Generic/abstract risks?** Foodborne illness, auto accident, blood transfusion.
- **What was the order of magnitude of the risks?** Varies—ranging from X in a million to X in 10,000. There is also X in 100,000.
- **Was risk referred to over a number of years?** No, risks are generally expressed on a per year basis.
- **What was the smallest and largest baseline risk, and what was the percentage risk reduction?** The smallest is 4 in a million, largest is 3 in 10,000. Percentage risk reductions can be as high as 50%.
- **In the WTP question, was the risk reduction delivered by a private or public mechanism?** Private (air bags, medical treatment, blood screening test, pneumonia vaccine).
- **Was the payment for the risk reduction supposed to take place over a number of years, or was it a one-time payment?** Varies. In the auto safety question, for example, people were asked to consider increased car payment per year over 5 years, whereas paying for medical tests or treatment is on a one-time basis.
- **What was the elicitation method?** Dichotomous choice with dichotomous choice follow-up (resulting in double-bounded analysis).
- **WTA or WTP?** WTP.
- **Are respondents allowed to change/revise their responses to the WTP questions?** No
- **Are respondents asked questions about how certain they feel about their responses to the WTP questions?** Yes
- **Are respondents tested for probability comprehension?** No
- **Are respondents tested for whether they accept the baseline risks (if the baseline risks are objectively assigned to the respondent by the researcher)?** Yes, by asking qualitative questions at least for the auto risk question (do you think your own risk is higher, the same, or lower than…).
- **Are respondents asked about risk-reducing or risk-loving behaviors that might explain their acceptance of baseline risks and/or their responses to the WTP questions?** In the case of the auto safety questions, they are asked if they have air bags in their own car, and if they think them to be effective.
- **Are respondents asked debriefing questions to understand their acceptance of the risk reducing mechanism and scenario?** Some, such as whether they

thought that the medication would be effective in treating or preventing foodborne illnesses.

- **What kind of information about family status, other sociodemographics, etc are the respondents asked**? Not shown in the partial questionnaire I have

Does the questionnaire ask about respondent health? How? General quality of life question.

Johannesson and Johansson (1996) questionnaire.

This is a telephone survey.

The questionnaire has virtually the same structure and questions as the instrument used by Johannesson et al., except for the commodity to be valued, and for the fact that the quality of life question is dropped. The questionnaire opens with telling people that for a person of their gender and age, the “chance of reaching the age of at least 75 is X percent.” The very next sentence states that a 75-year-old lives an average of 10 more years.

The commodity to be valued is explained immediately thereafter, and it is not a risk reduction, but a life expectancy increase: “Consider the following possibility. If you survive to the age of 75, you will have an opportunity to receive a new medical treatment. It is estimated that this will increase your remaining years of life by one year, i.e., to about 11 years. Would you buy the treatment if it costs X kronor and had to be paid for this year?”

As before, people are not offered any verbal analogies to help them digest the concept of chance and think of remaining lifetimes. They are also not asked about their current health, so there is no way of telling whether the chance of making to age 75 was accepted by the respondent. Moreover, it is surprising that people were told about a new medical treatment, but were given no details about approval by the Ministry of Health, about payments being out of pocket or co-pays under the national health care system, etc.

- **Mode of administration of the survey**: telephone.
- **Does the questionnaire use visuals to explain risks and risk reductions**? No
- **Type of graphical representation (pie chart, grid, risk ladder, etc.):** N/A
- **Is the quality of the graphical representation acceptable?** N/A
- **Does the questionnaire use verbal analogies to explain risks and risk reductions**? No
- **Were respondents asked to report subjective baseline risks?** No
- **Subjective or objective risk reduction?** No
- **If the baseline risk was assigned to the respondent by the questionnaire, was the risk gender-specific? Age-specific? Location-specific? Occupation-specific?** The chance of surviving to age 75 was age- and gender-specific, but the extension in remaning life expectancy at age 75 was the same for everyone (one year).
- **What context does the risk refer to? Transportation risk? Workplace risks? Risks associated with a particular health condition (e.g., diabetes or hypertension)? Generic/abstract risks?** All causes of death, although these are not mentioned, but the risk reduction would be from a medical intervention. No specific context is provided.

- **Were people given the comparison with the risks of dying from other causes?** No
- **What was the order of magnitude of the risks?** The chance of surviving to age 75 was expressed as, e.g., "75 percent."
- **Was risk over a number of years?** N/A
- **What was the smallest and largest baseline risk, and what was the percentage risk reduction?** N/A
- **Is the risk reduction delivered by a private or public mechanism?** life expectancy extension is a private good
- **Was the payment for the risk reduction supposed to take place over a number of years, or was it a one-time payment**? One-time payment (now)
- **Elicitation method?** Dichotomous choice, single-bounded
- **WTA or WTP?** WTP
- **Are respondents allowed to revise their responses to the WTP questions?** No
- **Are respondents asked questions about how certain they feel about their responses to the WTP questions?** Yes, but only if they answer "yes" to the payment question, and the English translation suggests a vague question ("are you fairly or totally certain that you would buy" the treatment at the stated price?)
- **Are respondents tested for probability comprehension?** No
- **Are respondents tested for whether they accept the baseline risks (if the baseline risks are objectively assigned to the respondent by the researcher)?** No
- **Are respondents asked about risk-reducing or risk-loving behaviors that might explain their acceptance of baseline risks and/or their responses to the WTP questions?** No
- **Are respondents asked debriefing questions to understand their acceptance of the risk reducing mechanism and scenario?** No
- **What kind of information about family status, other sociodemographics, etc are the respondents asked?** Household size, marital status, education, income (personal and combined with their spouse), size of the city they live in, region of Sweden, urban v. non-urban. The year of birth is asked among the sociodemographics questions, but people must have been asked what age group they belong to in the screener part of the survey.
- **Does the questionnaire ask about respondent health? How?** No.

Johannesson et al. (1997) questionnaire

This is a telephone survey.

The interview starts with asking the respondent's age, and moves immediately to the heart of the matter. Respondents are given the risk reduction scenario and then asked a DC choice question about their WTP for it. Respondents are told that "It is estimated that roughly X men out of 10,000 your age will die in the next year. Imagine that you could receive a preventive and pain-free treatment that would reduce your risk of dying during the next year, but have no effects after that year. The treatment would lower your risk of dying in the next year from X to (X-2) in 10,000. Would you buy this treatment at present if it cost you XXX kronor?" (yes/no) Risks vary with age and gender.

If the respondent answered "yes," he was then queried whether he was "fairly certain" or "totally certain" that he would buy this treatment for the stated price.

Next, the respondent is asked to rate his quality of life on a scale from 1 (=worst imaginable condition) to 10 (=best imaginable condition).

This closes the portion of the survey dedicated to risks and/or health. The next question are about socio-demographics and include household size, marital status, education, income (monthly and pre-tax; both personal and combined with spouse), size of the city where the respondent lives, region, and urban v. non-urban area.

Clearly, this survey must have taken a very short time to complete. People are taken straight to the point, but are offered no analogies for them to digest the magnitude of the risks, etc. There is no debriefing about acceptance of the medical intervention that would deliver the risk reduction, nor about any other aspect of the survey. Also, respondents are not told what the main causes of death for people their age and gender are. There is no mention of auto fatalities, cardiovascular illnesses, etc.

The absolute risk reduction is the same for everyone (2 in 10,000), and the questionnaire only presents the absolute risk reduction, but the corresponding relative risk reductions range from 1 percent to 33 percent of the baseline risks.

Respondents are asked how they rate their quality on a scale from 1 to 10, but there are no questions about chronic illnesses or other ailments. Lacking such questions, it is impossible to say whether people accepted or may have questioned their risks of dying over the next year.

- **Mode of administration of the survey**: telephone.
- **Does the questionnaire use visuals to explain risks and risk reductions**? No
- **Type of graphical representation (pie chart, grid, risk ladder, etc.):** N/A
- **Is the quality of the graphical representation acceptable?** N/A

- **Does the questionnaire use verbal analogies to explain risks and risk reductions**? No
- **Were respondents asked to report subjective baseline risks?** No
- **Subjective or objective risk reduction?** Objective
- **If the baseline risk was assigned to the respondent by the questionnaire, was the risk gender-specific? Age-specific? Location-specific? Occupation-specific?** Age- and gender-specific.
- **What context does the risk refer to? Transportation risk? Workplace risks? Risks associated with a particular health condition (e.g., diabetes or hypertension)? Generic/abstract risks?** All causes of death, although these are not mentioned, but the risk reduction would be from a medical intervention. No specific context is provided.
- **Were people given the comparison with the risks of dying from other causes?** No
- **What was the order of magnitude of the risks?** X in 10,000 over the next year
- **Was risk over a number of years?** Annual risk
- **What was the smallest and largest baseline risk, and what was the percentage risk reduction?** 6 in 10,000 to a max of 200 in 100,000, and the percentage risk reduction ranges from 1 percent to 33 percent.
- **Is the risk reduction delivered by a private or public mechanism?** Private
- **Was the payment for the risk reduction supposed to take place over a number of years, or was it a one-time payment**? One-time payment, and the risk reduction is only over the next year.
- **Elicitation method?** Dichotomous choice, single-bounded
- **WTA or WTP?** WTP
- **Are respondents allowed to revise their responses to the WTP questions?** No
- **Are respondents asked questions about how certain they feel about their responses to the WTP questions?** Yes, but only if they answer "yes" to the payment question, and the English translation suggests a vague question ("are you fairly or totally certain that you would buy" the treatment at the stated price?)
- **Are respondents tested for probability comprehension?** No
- **Are respondents tested for whether they accept the baseline risks (if the baseline risks are objectively assigned to the respondent by the researcher)?** No
- **Are respondents asked about risk-reducing or risk-loving behaviors that might explain their acceptance of baseline risks and/or their responses to the WTP questions?** No
- **Are respondents asked debriefing questions to understand their acceptance of the risk reducing mechanism and scenario?** No
- **What kind of information about family status, other sociodemographics, etc are the respondents asked?** Household size, marital status, education, income (personal and combined with their spouse), size of the city they live in, region of Sweden, urban v. non-urban. Age is asked at the very beginning of the survey.
- **Does the questionnaire ask about respondent health? How?** No, but people are asked to rate their quality of life on a scale from 1 to 10.

Lanoie et al. (1995) questionnaire

This is an in-person survey, although the paper suggests that some questionnaires may have been mailed to respondents.

In the paper, Lanoie et al. state that they wish to conduct a study similar to that of Gerking et al. (1988), but their risk ladder shows risks that are within the range of those facing the respondents.

The questionnaire opens with a simple probability tutorial based on the road transport context. The risks are of the order of X per 10,000 in a year.

In the second question, respondents are asked to imagine that they have to buy a car, and that airbags could be installed to reduce the risk of injury in an accident. They are told that the airbags would reduce the risks of dying from 4 to 2 in 10,000 (but they are not reminded that this is an annual figure). The question reminds them that this is 50% risk reduction, queries them about how much they would pay for the airbags.

It seems to me that providing the relative risk equivalent of the absolute risk reduction is a very good idea. The authors demonstrate care in writing out the survey instrument by also proving a reminder of the budget constraint.

The third question re-phrases the previous one in terms of WTA to have the airbags removed.

After this exercise and valuation questions, which refer to the road transportation context, the survey instrument moves to workplace risks. It guides the respondent to considering different types of workplace accidents (ranging from “fall from elevation” to “bodily reactions” and “exposure to radiation, toxic and noxious substances”) asking them to rate them as events that “could never happen,” “could rarely happen” etc. up to “most likely to happen.”

Next, the respondents are shown a risk ladder, with steps from 1 to 10, that gives examples of the type of occupation corresponding to the each of the steps. The lowest possible risk is that of secretaries, while the highest possible risk is that of a dynamiter in a mine.

Respondents are asked both a WTA question to go up the risk ladder by one step, and a WTP question to go down the risk ladder by one step. For people that were already at the top and at the bottom of the ladder, respectively, Lanoie et al use Gerking et al’s approach of asking them to imagine being one step lower and higher, respectively.

After the WTA and WTP questions, respondents are told that a firefighter faces a risk of dying of 5.8 in 10,000 a year, while an office worker faces a risk of 0.057 in 10,000. This is in contrast with the risk ladder, where no quantitative information about the risk was provided.). People are then asked to estimate their own risk of dying. Even in this question, the authors take care of explaining that the risk of office workers is 102 times lower than that of a firefighter, and of reminding the respondent that travel and transportation to and from work may be an element of risk.

In the next page, page 6, people are asked once again a question about WTA for a change of risk of 2 in 10000, just like in the auto and airbag question, but this time the context has changed, as the respondents are to imagine being a construction worker in the city of Montreal. This is question 7A. In question 7B, people are asked to report their WTA for an increase in risk of 2 in 10,000. The context has been changed again, since the respondent is asked to imagine being a production worker in a chemical plant in Montreal. Also, the risk comes from exposure to radiation, which results in an "incurable disease." (This may mean cancer, or leukemia, and the associated morbidity and suffering, plus dread, may influence WTP and make it different from that for a reduction in risk in the worker's own occupation.)

Questions 8 and 9 ask about the number of days that the respondent has had to take off from work for sickness of injuries, and about any workplace sicknesses or injuries experienced by other family members (or close friends).

Since questions 8 and 9, and question 11 and the following, are about work and workplace risks, question 10, which asks whether the respondent's car is equipped with airbags, seems out of place.

Questions 11-19 are about wages, hours worked, experience with the company and nature of work. They appear a bit more streamlined than in the Gerking et al. questionnaire. Questions 20-25 are about marital status, whether they spouse works, number of dependents, education, age and gender.

- **Mode of administration of the survey**: in person (some could have been sent to the respondents).
- **Does the questionnaire use visuals to explain risks and risk reductions**? Not to explain risks, but to elicit perceived riskiness of the job. This is the same risk ladder as in Gerking et al. (1988), but with revised occupations.
- **Type of graphical representation (pie chart, grid, risk ladder, etc.):** risk ladder
- **Is the quality of the graphical representation acceptable?** Very simple
- **Does the questionnaire use verbal analogies to explain risks and risk reductions**? It uses a probability tutorial, but not analogies that might help the respondent process the size of the risk. The tutorial refers to the transportation safety context. Respondents are told that the probability of being killed in a road accident is, for example, 8 in 10000 per year for a motorcyclist. "This means that

out of 10,000 motorcyclists, 8 will die in a road accident this year." Repsondents are not told how to imagine 10,000 (e.g., a small town), or 8 out of 10000.

- **Were respondents asked to report subjective baseline risks?** Yes, in two places. First, they are asked to pinpoint their own job on a risk ladder. Second, after the valuation questions for their own occupations, they are asked to estimate subjective risks, after being told the risk figures for firefighters and office workers.
- **Subjective or objective risk reduction?** The main WTA/WTP refer to one step up/down the ladder. Analysis treats this as an objective risk reduction. In addition, there are four more valuation questions: WTA and WTP for risk change in the transportation safety question (2), WTA for 2 in 10000 risk change if the respondent was a construction worker, and WTA for 2 in 10000 risk change if the respondent was a production worker at a chemical plant. WTP and WTA for the airbags could be treated as a warm-up question or analyzed on its own. WTA as a construction and plant worker should be treated differently, since the latter refers to radiation exposure that results in an incurable illness, and not in a workplace accident.
- **If the baseline risk was assigned to the respondent by the questionnaire, was the risk gender-specific? Age-specific? Location-specific? Occupation-specific?** N/A
- **What context does the risk refer to? Transportation risk? Workplace risks? Risks associated with a particular health condition (e.g., diabetes or hypertension)? Generic/abstract risks?** Two questions on transportation risks, two questions on risk at one's job, two questions on risks in different occupations.
- **Were people given the comparison with the risks of dying from other causes?** No
- **What was the order of magnitude of the risks?** When shown to the respondent, they are X in 10,000.
- **Was risk over a number of years?** Annual risks
- **What was the smallest and largest baseline risk, and what was the percentage risk reduction?** when stating a risk reduction to the respondent, the instrument uses a baseline of 4 in 10000, and a risk reduction of 2 in 10000, emphasizing that this is 50% reduction.
- **Is the risk reduction delivered by a private or public mechanism?** private
- **Was the payment for the risk reduction supposed to take place over a number of years, or was it a one-time payment**? Sacrifice or increase in income per week. This is a bit of a discrepancy, since risks are expressed on an annual basis. But if the workers are paid weekly, this may be a more natural way to think. No testing to make sure that the respondents converted WTP or WTA to annual basis was done.
- **Elicitation method?** Open-ended
- **WTA or WTP?** WTA and WTP
- **Are respondents allowed to revise their responses to the WTP questions?** No
- **Are respondents asked questions about how certain they feel about their responses to the WTP questions?** No.
- **Are respondents tested for probability comprehension?** No

- **Are respondents tested for whether they accept the baseline risks (if the baseline risks are objectively assigned to the respondent by the researcher)?** No, but they are asked what they think their current baseline risk is, and in four of the valuation questions they are asked to imagine that their risks would be X.
- **Are respondents asked about risk-reducing or risk-loving behaviors that might explain their acceptance of baseline risks and/or their responses to the WTP questions?** Yes, if they have airbags in their cars.
- **Are respondents asked debriefing questions to understand their acceptance of the risk reducing mechanism and scenario?** No
- **What kind of information about family status, other sociodemographics, etc are the respondents asked?** Number of dependents, marital status, education, wage rate, whether spouse works, age, gender, other occupation characteristics, including union status.
- **Does the questionnaire ask about respondent health? How?** No, but it asks how many days the respondent has taken off from work for sickness or injury.

Gerking et al. questionnaire

* This questionnaire was administered by mail.

* It opens with questions about the respondent occupation status, job title, type of occupation and industry.

* The second page of the questionnaire tackles the issue of perceived safety at work. The respondent is first asked to rate the safety of his or her job on a scale from 1 to 5, where 1 means "much safer" than the average and 5 means "much riskier" than the average.

The respondent is then shown a matrix with a list of possible accidental causes of death at work, including motor vehicle accidents, explosion, gas inhalation, electrocution, gun shot, being hit by industrial machinery or caught between machines (no mention of exposure to radiation or substances that would cause long term health damage), and is asked to rate them on a scale from 1 to 5, where 1 means "could never happen" and 5 means "most likely to happen."

* The following page shows a risk ladder. Respondents are told that for the profession at the bottom of the ladder (schoolteacher) the annual risk of dying on the job is 1 in 4000, whereas for the profession at the top of the ladder (lumberjack) the risk of dying in a year is 10 in 4000. Each step is, therefore, implied to mean a 1 in 4000 change in the risk of dying.

In the subsequent page, the respondent is asked to indicate which step of the ladder he thinks his risk is, and to poinpoint on a payment card his WTP (WTA) for a step down (up) on the ladder. Those respondents who picked the bottom (top) of the ladder are instructed to consider a movement from the next step downward (upward).

* Additional information about the tradeoffs between income and risks are elicited on the next page, where the respondent is shown a matrix with various combination of risk ladder step levels and percentage changes relative to current wages. The respondent is to indicate on a non-point Likert scale whether the stated situation is worse, about the same as, or better than his current situation. In the Likert scale, 1 indicates "much worse" than the current situation, and 9 means "much better" than the current situation. (It should be noted that the responses to these questions are analyzed in a separate paper, and not in Gerking et al. (1988)).

* page 6 of the questionnaire elicits information about the respondent's current job, education and experience. One interesting question is whether the respondent suffers from physical or nervous conditions that limit the type of work that they can do.

* the subsequent pages asks about individual characteristics of the respondent, including age, race, gender, urban/suburban/rural environmental where he lives; plus

past employment, job satisfaction, fringe benefits, transfer payments received; and how the respondent is paid at his current job (salary, by the hour, by the piece, on commission, etc.), and total income.

- **Mode of administration of the survey**: mail survey

- **Does the questionnaire use visuals to explain risks and risk reductions**? Risk ladder
- **What is the type of graphical representation (pie chart, grid, risk ladder, etc.) and is the quality of the graphical representation acceptable**? Risk ladder, with added annotation about lowest risks and highest risks. Quantitative information about the change in risk implied by each step of the ladder is in writing above the ladder.
- **Were respondents ask to report subjective baseline risks**? Yes, pinpoint a step on the ladder
- **Subjective or objective risk reduction**? objective—one in 4000 (move one step on the ladder)
- **If the baseline risk was assigned to the respondent by the questionnaire, was the risk gender-specific? Age-specific? Location-specific? Occupation-specific**? n/a
- **What context does the risk refer to? Transportation risk? Workplace risks? Risks associated with a particular health condition (e.g., diabetes or hypertension)? Generic/abstract risks?** Workplace risks.
- **What was the order of magnitude of the risks?** Baseline risks: from 1 to 10 in 4000 per year. Risk reduction is by 1 in 4000 per year.
- **Was risk referred to over a number of years?** Over one year.
- **What was the smallest and largest baseline risk, and what was the percentage risk reduction?** baseline ranges from 1 to 10 in 4000, and the change is by 1 in 4000, implying relative changes of 10% to 100%.
- **In the WTP question, was the risk reduction delivered by a private or public mechanism?** no mention was made of the risk reduction mechanism, but it would be presumably private.
- **Was the payment for the risk reduction supposed to take place over a number of years, or was it a one-time payment?** Income sacrifice or increase per year.
- **What was the elicitation method? Open-ended? Dichotomous Choice? If dichotomous choice, was it single-bounded or double-bounded?** Payment card used, so technically speaking it would be on a interval-data basis. However, the paper treats it as open-ended except for the extreme amounts on the payment card.
- **WTA or WTP?** Either WTP or WTA, in split samples.
- **Are respondents allowed to change/revise their responses to the WTP questions?** No.

- **Are respondents asked questions about how certain they feel about their responses to the WTP questions?** No.
- **Are respondents tested for probability comprehension?** No.
- **Are respondents tested for whether they accept the baseline risks (if the baseline risks are objectively assigned to the respondent by the researcher)?** No, but the baseline risk is subjective.
- **Are respondents asked about risk-reducing or risk-loving behaviors that might explain their acceptance of baseline risks and/or their responses to the WTP questions?** No.
- **Are respondents asked debriefing questions to understand their acceptance of the risk reducing mechanism and scenario?** No.
- **What kind of information about family status, other sociodemographics, etc are the respondents asked?** Age, gender, race, income, if moved recently, education.
- **Does the questionnaire ask about respondent health? How?** The questionnaire asks whether the respondent has a physical or nervous condition that affects the type of work they can do.

Appendix D.

The following reports are available for download using the links provided:

Gegax, D., S. Gerking and W. Schulze et al. (1985). "Valuing Safety: Two Approaches," in *Experimental Methods for Assessing Environmental Benefits, Volume IV*. Report prepared for the U.S. EPA, Office of Policy Analysis under Assistance Agreement #CR811077-01.

Link:
http://yosemite.epa.gov/EE/epa/eerm.nsf/vwRepNumLookup/EE-0280E?OpenDocument

Viscusi, W.K., W.A. Magat and J. Huber (1991). *Issues in Valuing Health Risks: Applications of Contingent Valuation and Conjoint Measurement to Nerve Diseases and Lymphoma.* Draft report to EPA, Office of Policy, Planning and Evaluation under Assistance Agreement CR# 815455-01-1 and 814388-02.

Link:
http://yosemite.epa.gov/EE/epa/eerm.nsf/vwRepNumLookup/EE-0223?OpenDocument

Full text reports for the following references can be obtained by contacting Anna Alberini (aalberini@arec.umd.edu) or Nathalie Simon (simon.nathalie@epa.gov):

Miller, Ted, and Jagadish Guria, "The Value of Statistical Life in New Zealand: Market Research on Road Safety, January 30, 1991.

Miller Ted, and Jagadish Guria, "Valuing Family Members' Statistical Lives,"

APPENDIX E

Self-Protection and Averting Behavior, Values of Statistical Lives, and Benefit Cost Analysis of Environmental Policy[1]

by Glenn C. Blomquist

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Self-Protection and Averting Behavior, Values of Statistical Lives, and Benefit Cost Analysis of Environmental Policy

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Abstract. Situations in which risk is at least partly a matter of choice provide opportunities to analyze behavior and estimate the willingness to pay for small changes in mortality risks. Individuals engage in household production of health and safety as long as the value of the gain in risk reduction is worth the money, time, and any disutility necessary to produce the reduction in risk. This paper reviews values of statistical life inferred from choices about highway speeds, traveler use of protective equipment, crash-worthiness of motor vehicles, and housing location near Superfund sites. The best estimates, close to $4 million in year 2000 dollars, are valuable complements to estimates from labor and constructed markets. Interestingly some evidence suggests that values for children and seniors are not less than middle-aged adults. Issues of risk perception and other challenges related to estimation are discussed.

Keywords: value of statistical life, mortality risks, benefit cost analysis, self-protection

JEL Classification: D13, D61, I12, J17

1. Introduction

Individuals can be observed in a variety of activities that affect their health and safety. Protective behavior is evident in motorist choice of automobile type, safety equipment such as seat belts, and speed of travel. Choices concerning safety helmets,

*An earlier version was presented at "Economic Valuation of Mortality Risk Reduction: Assessing the State of the Art for Policy Applications," an Environmental Policy and Economics Workshop held by the U.S. Environmental Protection Agency in Silver Spring, MD, on November 6–7, 2001 under EPA Cooperative Agreement R-82957201-0. Comments from Shoshana-Grossbard-Shechtman, Paul Hansen, Bryan Hubbell, Magnus Johannesson, Ted Miller, Laura Taylor, John Whitehead, workshop participants at EPA, Lund University, Stockholm School of Economics, and University of Kentucky, and several anonymous reviewers are appreciated. Helpful comments were also received from participants in a Southern Committee on Resource and Environmental Economics session at the Southern Economic Association meetings held in New Orleans, LA on November 24–26, 2002. Nicole Owens deserves special thanks for her encouragement and support in her role as project officer. I alone am responsible for the views in this paper. I thank too, the Centre for Health Economics, Department of Economics, Stockholm School of Economics where I was visiting on sabbatical when the first draft of this paper was written.

cigarette smoking and installation of fire alarms change risks of death that individuals experience. Choice of residence when housing markets encompass Superfund sites influences the amount of risk that individuals face. Visits to health clinics for preventive care can reduce risks to health. The purpose of this paper is to review studies which estimate values of mortality risks based on the tradeoffs which individual consumers make. The common feature is that the estimates of values of small changes in mortality risks are implied by observable consumer behavior as individuals protect themselves against, or avert, risk. These values of mortality risks, for convenience, are sometimes referred to as "values of life" or "values of statistical life" (VSL). Interest in estimates of these values exists, in part, because the U.S. Environmental Protection Agency (EPA), the Department of Transportation, and other agencies evaluate policies and regulations that are expected to have impacts on individuals' health and safety and their mortality risks. Benefit cost analysis (BCA) of such policy requires VSL estimates.

This review is made with a constructively critical eye. It is potentially too costly to go with a destructively critical review because it could be counterproductive by fostering the mistaken notion that the whole methodology and entire body of evidence on VSL are unreliable. Such a review can make it easier for critics of BCA to disregard accomplishments, mistakenly abandon valuation, and promote an absolutist position that the concept of valuing mortality risks is immoral.[1] A case can be made that economists take for granted that there is substantial agreement that individual willingness to pay for changes in risk is the best way to think about valuing the policy benefits and that sound, theoretically based methods exist for estimating VSL.[2] A great deal has been learned about valuing mortality risks since estimation of willingness to pay for risk changes began nearly 30 years ago.

2. Frameworks for estimating values of mortality risks based on averting behavior

The thought of inferring individuals' values of reductions in mortality risks from their behavior intended to influence risk is appealing. Situations in which risk is at least partly a matter of choice provide opportunities to analyze behavior and estimate the willingness to pay (WTP) for risk reductions or willingness to accept (WTA) compensation for risk increments. These situations can involve choices among various types of work in the labor market, or the situations can involve choices in consumption activity. Self-protection or averting behavior in consumption, i.e., household production, is the focus of this paper. The theory for understanding these activities is built on the foundation laid by Jacob Mincer (1962, 1963) in his household framework, Gary S. Becker (1965) in his theory of allocation of time, and Isaac Ehrlich and Gary S. Becker (1972) in their theory of self-insurance and self-protection. The New Home Economics has been a useful framework for analyzing behavior with respect to physical risk. There are two closely related models that guide thinking about valuing changes in mortality risks.[3]

2.1. The basic model

A basic model with the present period and one future period captures the essence of estimating risk tradeoffs in consumption.[4] Let the individual maximize expected utility, $E(U)$, that consists of utility in the first period, $U(C_1,S)$, and expected utility in the second period, $PU(C_2)$, where U is a well-behaved single period utility function, C_i is composite consumption in period i, $i = 1,2$, P is the probability of survival to period 2, and S is protective health or safety activity in which the individual can engage. The production function for changing P is left general as $P = P(S)$. P', the marginal product of averting behavior, is the reduction in the mortality risk. P' is assumed to be positive and diminishing. Averting activity can affect utility directly with U_S negative if S generates disutility or U_S positive if averting activity generates utility. Expected utility is described by the following equation:

$$E(U) = U(C_1, S) + PU(C_2) \tag{1}$$

Differentiating equation (1), holding $E(U)$ constant, and solving for dC_1/dP yields

$$dC_1/dP = -U(C_2)/\lambda P \tag{2}$$

where λ is the marginal utility of consumption (or income). Equation (2) shows the marginal rate of substitution between consumption and the probability of survival, or the marginal willingness to tradeoff current consumption for reductions in mortality risk (increases in the probability of survival). Although changes in P are exogenous at this point, i.e., no averting behavior yet, several implications follow: (1) Willingness to pay for reductions in mortality risk is not a single value but depends on several factors that vary among individuals and circumstances. Willingness to pay will differ depending on $U(\cdot)$, C_2, P, and λ. (2) The tradeoff depends on the base level of risk and will be smaller as the probability of survival (P) increases. (3) The tradeoff depends on future consumption (C_2), but it increases with increases in the *utility* of future consumption (or income or earnings) and not directly with future earnings. Changes in the marginal utility of income (λ) may act to partly offset this effect.

Maximization of $E(U)$ is subject to the budget constraint, that the present value of expenditures on consumption and averting behavior, cannot exceed the present value of income,

$$C_1 + qS + dC_2 = wT + dwT + A \tag{3}$$

where q is the cost of averting behavior, d is the factor that discounts the amount in period 2 back to the present, w is the wage rate, T is time available for work in each period, and A is the present value of nonlabor income. The cost of averting behavior, q, is composed of a money cost "m" and a time cost, awt, where "a" is a factor which relates the value of time in averting activity to the wage rate, and "t" is the time input into averting activity.

The first order condition of interest is

$$P'U(C_2)/\lambda = q - (U_S/\lambda) \tag{4}$$

The left-hand side of equation (4) is the marginal benefit of averting activity and the right-hand side is the marginal cost. Individuals engage in household production of health and safety as long as the value of the gain in risk reduction is worth the money, time, and any disutility necessary to produce the reduction in risk. Because risk is partly endogenous, it is possible to infer a VSL from averting behavior.

The value of a gain in the probability of survival (or reduction in mortality risk), is shown by the term, $U(C_2)/\lambda$, which is the monetary value of the utility of future consumption. Let this value be V so that

$$V \equiv U(C_2)/\lambda \tag{5}$$

Notice that if equation (4) is solved for V, the value can be expressed as

$$V = [q - (U_S/\lambda)]/P' \tag{6}$$

If for convenience of comparability V is evaluated for a unit (0–1) change in P, then V is an estimate of VSL, value of statistical life. So, the value of a change in mortality risk for a unit change in P is

$$VSL = [m + awt - (U_S/\lambda)]/P' \tag{7}$$

If, for example, it is known that the sum of the components of cost shown in brackets is \$400 per period and the change in the mortality risk per period is 0.0002, then the VSL implied is \$2 million.

Each component of the equation presents a challenge in estimating VSL. The marginal monetary cost, m, is sometimes negligible for averting activity. It is sometimes estimated by an annual average cost. Marginal inputs of time, t, are sometimes small and sometimes substantial. The value of time spent in producing changes in mortality risks can equal the market wage rate, w, for the individual, or be some proportion of it, aw, as in most motor vehicle travel. The monetary worth of the marginal utility of the averting activity, U_S/λ, may be trivial, or may be a major cost, such as has been the case with manual (nonpassive) seat belts in cars. Estimating P' may be simple if expert estimates are available and individuals engaging in averting behavior perceive the changes in risks to be the same as the experts. Any misperception of risk makes estimating the perceived P' more challenging. Several of these components that are typically necessary for estimating VSL based on averting behavior will be discussed below.

2.2. *The life cycle model*

While a model with one future period is useful for understanding the basic tradeoff between mortality risk and consumption, a multi-period model with uncertain lifetime allows derivation of individual WTP for changes in mortality risks that would occur at different stages of the life cycle. Life-cycle models can define, for example, individual WTP now for a change in the conditional probability of survival in 10 years. These models can be useful for considering environmental policy that is expected to reduce future mortality risks. From life-cycle models have followed several implications that have shaped expectations about VSL estimates. Some testable implications are:[5] (1) generally WTP declines with age, (2) under plausible conditions WTP exceeds discounted present value of future earnings, (3) WTP declines with latency, the length of time in the future when risk will be reduced, and (4) WTP now for a risk reduction in year t is equal to WTP in year t for that risk reduction discounted back to the present.

Current research continues to probe. For example, Per-Olov Johansson (2002) uses a life-cycle model to demonstrate that, in contrast to the first implication listed above, there is no obvious age pattern for WTP for mortality risk reductions over the life cycle. Brian W. Bresnahan and Mark Dickie (1995) discuss the implications of endogenous risk and other issues in using values based on averting behavior in policy evaluation. In this paper estimates of VSL based on self-protection and averting behavior are reviewed, and issues in using the basic model are discussed. Some of the empirical results reviewed are surprising given the implications of life-cycle models, at least as we currently understand them.

3. Estimates of values of mortality risks based on self-protection and averting behavior in consumption

3.1. Previous reviews

Interest in estimates of values of mortality risk reductions has produced several relatively recent reviews.[6] W. Kip Viscusi's (1993) survey of the literature covers all types of studies and includes a summary of studies based on tradeoffs in consumption, or what he calls "outside of the labor market." There are seven early studies on highway speeds, seat belt use/nonuse, smoke detectors, housing prices and air pollution, and auto purchases. The average of the VSL estimates shown in Viscusi's Table 5 summary of these studies is \$1.6 million in year 2000 dollars.[7]

Ted R. Miller (1990) also reviewed VSL estimates from all types of studies and using 47 (unadjusted) VSL estimates that he considered sound, he found an average VSL of \$3.9 million in 2000 dollars for the U.S. Later, using the previously reviewed studies and 21 additional estimates, Ted. R. Miller (2000) does a meta-analysis of international studies. The average of the 12 countries for which he has estimates plus the U.S. is also \$3.9 million in 2000 dollars.[8] Differences in income, base risk levels, opportunities for reducing risk, availability of health care, culture, and other factors can influence values of mortality risks. The fact that Miller finds the average VSL to be similar is interesting, but should not be interpreted as strong evidence for VSL being equal in all countries. On the contrary, Miller reports an estimate for Taiwan, for example, that is less than 30% of that for the U.S.

Rune Elvik (1995) performs a meta-analysis of 169 estimates of values of mortality risk changes. Table 1 is based on his summary of 11 averting behavior studies, see Elvik (1995, Appendix C, p. 19). The average of the VSL estimates he reports is $3.0 million 2000 dollars if all values except for children and motorcyclists are included. One feature of Elvik's review is that he notes whether or not each study has a test of rationality, or risk perception, and whether the analysis uses individual or aggregate data. Elvik considers these two characteristics to be indicators of high relative validity.

The most recent review, by Arianne de Blaeij et al. (2002), is a meta-analysis of all types of studies that estimate VSL based on a tradeoff related to traffic safety.

Table 1. Elvik's summary of studies of averting behavior by road users.

Author and publication year	Type of unit[a]	Sample size	Road user[b]	Test of rationality[c]	Base risk[d]	Risk change[d]	VSL[e] (millions, US $)
Melinek (1974)	Aggr	1	Ped	No	0.035	0.035	1.1
Debapriya Ghosh, Dennis Lees, and William Seal (1975)	Aggr	1	Car	No	–	–	1.1
Jones-Lee (1977)	Aggr	1	Car	No	–	–	5.2
Blomquist (1979)	Indiv	5517	Car	Yes	30.3	15.1	1.0[f]
Jondrow, Bowes, and Levy (1983)	Aggr	1	Car	No	–	–	2.9
Winston and Mannering (1984)	Indiv	220	Car	No	12	12	2.2
Atkinson and Halvorsen (1990)	Aggr	112	Car	No	19	19	5.2
Blomquist and Miller (1992)	Indiv	5378	Car	Yes	7.4	3.3	2.8
(published as Blomquist, Miller, and Levy (1996))	Indiv	934	Car-child[g]	Yes	3.6	2.6	6.5
	Indiv	178	Motor cyclist[h]	Yes	77	22	1.7
Dreyfus and Viscusi (1995)	Indiv	1775	Car	No	19.6	–	5.5

Source: Based on Elvik (1995), Appendix, Part D.

[a] The data unit is either aggregated or individual. An example of an aggregated data unit is average speed on highways.

[b] The type of roadway user can be pedestrian, car driver, or motorcycle rider. The Blomquist and Miller estimate of $6.5 million is inferred from drivers' use of child safety seats and belts for passengers who are less than 5 years of age.

[c] Tests examine understanding of probability concepts or conformity to normative axioms of rational choice.

[d] Deaths per 100,000 motor vehicles.

[e] All money values are reported in year 2000 US dollars. Values are multiplied by 1.227 to convert from the 1992 dollars reported by Elvik to 2000 dollars using the Consumer Price Index for all urban consumers.

[f] This value is reported in 2000 dollars. It converts the estimate in 1978 dollars reported in Blomquist (1979) to 2000 dollars. The value of 1.2 reported by Elvik appears to be based on the assumption that the Blomquist estimate was reported in 1972 dollars.

[g] This value is inferred for children under 5 years based on driver use/nonuse of child safety seats and belts.

[h] This value is inferred for motorcyclists based on their use/nonuse of helmets.

Included are four studies and 10 estimates that are inferred from public agency programmatic decisions rather than individual behavior. While this type of study reveals something about public decision making, the values are different in nature from the values estimated from individual self-protection, averting behavior. The public tradeoffs do not directly inform about individual WTP. de Blaeij et al. report that the average of the VSL estimates in their Table 1 is \$4.7 million. A weighted average of estimates with and without the public tradeoffs implies that the preferred average VSL excluding the public tradeoffs is \$4.2 million in year 2000 dollars.

These reviews find that VSL estimates based on averting behavior in consumption tend to be less than estimates from averting behavior in the labor market based on risk compensating wage differentials or estimates from stated preferences in hypothetical or constructed markets. This relationship will be discussed further after considering the most recent studies of averting behavior in consumption.

3.2. A review of recent studies of averting behavior in consumption

Table 2 provides a summary of eight relatively recent studies that estimate VSL based on averting behavior in consumption. The first four analyze choices about highway speeds and roadway user use of protective equipment and infer VSL from tradeoffs between risk reductions and combinations of money, time, and disutility. The second four analyze choices in motor vehicle and housing markets using hedonic analysis of prices and infer VSL from tradeoffs between product prices and either vehicles with different designs that affect safety or houses with various cancer risks due to nearby Superfund sites. Through hedonic analysis the marginal implicit prices for mortality risk are estimated. The range of values for adults is something less than \$1.7 million to \$7.2 million in year 2000 dollars. The average value for adults is approximately \$4.5 million if \$1.5 million is used for the speed/fatality study and averages are used for the two studies with a range reported. Four very recent studies are worth more detail.

Speeds on interstate highways: One recent study was presented by Orley Ashenfelter and Michael Greenstone (2002) at a session in honor of Sherwin Rosen at the 2003 AERE/ASSA meetings. They estimate the VSL from changes in speeds on interstate highways. In 1987 federal law was changed to allow states to raise the speed limit on rural interstates from 55 to 65 mph. Their estimates are based on laws being changed so that motorists can make tradeoffs for time savings at the expense of bearing greater mortality risks. Motorists are assumed to base their behavior on the actual tradeoff of risks realized. This assumption is the same as is typically used in the labor market in which workers are assumed to make the tradeoff between higher wages and the mortality risks that actually occur.[9]

Ashenfelter and Greenstone analyze speeds and road fatalities for 28 states for which they can get data for the period 1982–1993. Based on models which include state-by-road-type and year-by-road-type fixed effects, they estimate that speeds

Table 2. U.S. studies of self-protection and averting behavior in consumption that estimate VSL, 1990–2002.

Author (year)	Behavior and tradeoff (year)	Best Estimate of VSL (range), 2000 US dollars, millions
Ashenfelter and Greenstone (2002)	Speeds and fatalities on interstate highways with higher speed limits, 1982–1993	\$1.7 as upper bound Typical vehicle occupant
Jenkins, Owens, and Wiggins (2001)	Bicycle helmet use with fatality risk reductions and costs, 1997	\$4.3 adult \$2.9 child 5–9 \$2.8 child 10–14 Users of helmets
Blomquist, Miller, and Levy (1996)	Car seat belt use with fatality risk reductions and time and disutility costs, 1983	\$2.8–4.6 adult[a] \$3.7–6.0 child under 5[a] \$1.7–2.8 motorcyclist[a] Typical driver or rider
Carlin and Sandy (1991)	Child safety seat use with fatality risk reductions with time and money costs, 1985	\$0.8 Child under 5
Mount et al. (2001, workshop paper)	Hedonic analysis of motor vehicle prices with fatality risks, 1995	\$7.2 adult[a] \$7.3 child[a] \$5.2 elderly[a] Typical vehicle occupant
Dreyfus and Viscusi (1995)	Hedonic analysis of car prices with fatality risk, 1988	\$3.8–5.4 Typical car occupant
Atkinson and Halvorsen (1990)	Hedonic analysis of car prices with fatality risk, 1978	\$5.3 (4.6–5.4) Typical car occupant
Gayer, Hamilton, and Viscusi (2000)	Hedonic analysis of housing prices with fatality risk near Superfund sites, 1988–1993[b]	\$4.7 (4.3–5.0) Typical resident

No adjustment is made for differences in base level risk.
Source: Author.
[a] Higher value reflects adjusted for risk perception bias by multiplying by 1.634.
[b] Values after release of the Remedial Investigation of the Superfund sites. Values are for a statistical cancer case.

increased by approximately 3.5% (2 mph) and fatalities increased by approximately 35% in states which adopted the higher speed limit. They calculate the time savings associated with the increase in speeds, approximately 125,000 hours saved per life lost. This tradeoff between time gained and life lost implies an upper bound on VSL of approximately \$1.7 million in year 2000 dollars if time is valued at the wage rate.

Ashenfelter and Greenstone argue that their empirical analysis should be interpreted as reflecting the preferences of the median driver/voter. The estimate is an upper bound because the tradeoff is observed only for states and drivers in which the value of the time savings exceeds the cost of the fatalities. Motorists would not trade off the mortality risks if the risks were worth more than the savings in time. State legislators would not permit the tradeoff if they thought the mortality risks were worth more than the savings in time. In the second part of their paper they attempt to recover the structural estimates of the VSL based on analysis of the tradeoff in each of the states. The estimates of this "average" tend to be slightly lower than the upper bound estimate, but they are imprecisely estimated.

Ashenfelter and Greenstone's upper bound estimate of \$1.7 million appears to be reasonably robust statistically, but their estimate depends on values used for valuing motorists' time, number of passengers per vehicle, and perceptions of risks and time savings as well. These parameters matter. To adjust for the average number of occupants per vehicle from the assumed value of 1 to the average number of 1.7 would increase their VSL estimate by 70%. To adjust for the value of time from the assumed ratio of 1 for (value of time)/wage to 0.6 as used in Glenn Blomquist, Ted R. Miller, and David T. Levy (1996) would decrease their VSL estimate to only 60% of what it is. While these two adjustments just about offset one another, if an adjustment is made for misperception of risk by motorists and the value of 1.634 is used as in Blomquist, Miller, and Levy (1996), then their upper bound estimate of VSL would be \$2.8 million in 2000 dollars.[10]

Bicycle helmets, seat belts, child safety seats, and motorcycle helmets: Robin R. Jenkins, Nicole Owens, and Lanelle Benbench Wiggins (2001) calculate the VSL implied by use of bicycle helmets and find it to be approximately \$4.3 million in 2000 dollars for adults who purchase and wear the helmets. They consider their estimate to be a lower bound because buyers (and presumably users) find it worth at least as much as the cost to gain the added protection. Including time and disutility costs would increase the implied value and reinforce the claim that the estimate is a lower bound if only money costs are relevant to the use decision. However, their estimated VSL is an *upper* bound for bicyclists who are not buyers (and users) under the same assumption of time and disutility costs being equal to zero. If potential time and disutility costs are important for all bicyclists and those costs are different for users and nonusers, then their estimate is not necessarily an upper bound for nonusers. Because the calculations are based on aggregate data, it is not clear what the VSL is for the average bicyclist. This aspect aside, their study is noteworthy in that it is one of only a few that estimate VSL for children and the only published study that infers a value from bicycle helmet use by individuals.

In contrast to the study of bicycle helmet use, Blomquist, Miller, and Levy (1996) and Paul S. Carlin and Robert Sandy (1991) estimate VSL based on statistical analysis of individual micro data that provides evidence of rationality of users of protective equipment. Blomquist, Miller, and Levy incorporate disutility costs associated with use of equipment and estimate VSLs for the typical users rather than upper or lower bounds for users. Elvik notes that these characteristics increase the quality of the studies and estimates.

Motor vehicle models and crashworthiness: An ambitious hedonic study of prices of motor vehicles and associated fatality rates by Timothy Mount, et al. (2001) seeks to estimate VSL for household members of different ages. They build upon earlier related analysis by Scott E. Atkinson and Robert Halvorsen (1990) and Mark Dreyfus and W. Kip Viscusi (1995) and devote more attention to household use of the vehicles and distribution within the household. A noteworthy characteristic of the Mount et al. study is the set of detailed estimates of mortality risks that account for differences in vehicle use by various members of households. Another advantage of their study is the inclusion of a wider range of motor vehicles than only passenger cars and a rich set of driver characteristics. Their preliminary estimates of VSL are among the highest of the recent studies. Their point estimate of VSL for adults is \$7.2 million in 2000 dollars.

Residential location and Superfund sites: Ted Gayer, James T. Hamilton, and W. Kip Viscusi (2000, 2002) analyze the housing market surrounding Superfund sites in Grand Rapids, Michigan. They use a specially constructed, expert measure of cancer risk as well as distance measures and other proxies for physical risk. They find that the proxies for risk can explain about half the variation in expert risk and that housing with less (either proxy or expert statistical) risk sells for higher prices. After the release of the EPA Remedial Investigation, premiums for safer locations imply values of statistical *cancer* of approximately \$4.3–5.0 million in 2000 dollars. Estimates are much higher if prerelease risk perceptions are used. This estimate is especially relevant to BCA of environmental policy because it is inferred from valuing reductions in environmentally-related cancer risks rather than VSL from other averting behavior.

3.3. Best estimate from recent studies

As noted at the beginning of this section reviewing recent averting behavior in consumption studies, the range of values for adults is something less than \$1.7 million to \$7.2 million in year 2000 dollars as reported in Table 2. The simple average value for adults is approximately \$4.5 million if \$1.5 million is used for the speed/fatality study and averages are used for the two studies with a range reported. The range for the best estimates is not much different. Even though Ashenfelter and Greenstone claim that their estimate is an upper bound, some of their own estimates of the average VSL are not much different from their own upper bound. In addition, their estimate is from behavior in traffic and not directly related to behavior related to environmental risks. If their estimate is adjusted for risk misperception, it would be about \$2.8 million in 2000 dollars. \$2 million could be considered to be at the lower end of the range. The VSL estimate adjusted for risk from Blomquist, Miller, and Levy of \$4.6 million is closer to the average. The VSL estimate adjusted for risk from Mount et al. is \$7.2 million and could be considered to be at the upper end of the range. The Gayer, Hamilton, and Viscusi estimate of \$4–5 million for cancer is the estimate most directly related to environmental risks. Presumably the VSL is greater than the value of a statistical case of cancer because not all cancer results in death. The best estimate of VSL from averting behavior in consumption is probably close to $4 million in year 2000 dollars.

One aspect of the recent estimates worth noting is that the best estimates are greater than values estimated in earlier studies of averting behavior in consumption. One of the reasons for the increase in average values is the greater use of hedonic approaches compared to estimation based on risk related behavior combined with calculations that use other related parameters such as values of time, number of vehicle occupants, and disutility costs. Both types of analysis are valuable. Another reason for the increase is the upward adjustment for underperception of risks.

4. Risk perception and values implied by averting behavior in household consumption

A crucial element in estimating VSLs from self-protection and averting behavior is the amount that risk changes when the individual engages in the activity. Atkinson and Halvorsen (1990, fn. 2), for example, explicitly acknowledge that they assume that the automobile purchaser's perception of risk is consistent with actual risk in making their VSL estimates. Their estimates, as do others' estimates shown in Table 2, depend directly on this assumption. It is no secret that individuals can have difficulty understanding risk and making decisions involving risk. However, this imperfection is not fatal for estimating VSL based on observable behavior in product markets and using this information in BCA.

First, an impressive amount of evidence exists that reveals that individuals respond to risk in expected ways. They respond in the expected direction and they respond more, the greater is the risk. Analysis of motorist use of protective equipment such as safety belts and child safety seats, for example, typically shows that motorists protect more when expected benefits are greater such as when traveling at higher speeds and protect less when it costs more such as using child safety seats on older children who should be fitted with larger seats and can protest confinement more effectively, see Glenn C. Blomquist (1991). When individuals have something like their own health and safety at stake, they tend to act as if they perceive risks in ways that indicate their perceptions are positively correlated with expert estimates of the risks.

However well individuals perceive increases and decreases in risk and rank them correctly, their ability to perceive risk in a cardinally correct way is questioned. For example, Sarah Lichtenstein et al. (1978) found that when individuals' perceptions of risks are compared to expert estimates of risks, low risks tend to be overestimated and higher risks tend to be underestimated. Other differences between individual perceptions and expert estimates exist and the relationships have been estimated. Thus, a second reason for thinking that averting behavior is useful despite imperfect perceptions of risk is that, as part of the sensitivity analysis, the estimates of VSL can be adjusted using the relationships between individual perceptions and expert estimates. For example, if individual risk estimates are known to be 20% lower than the expert risk estimates, then the VSL can be recalculated with the lower risk. The rationale is that the lower risk is the level on which the individual is basing behavior and making tradeoffs. Ideally, the individual's perceived risk is the risk appropriate for estimating the VSL.[11]

If the policy maker believes that the adjusted risk is preferred, then the VSL can be estimated based on it. Relying on the Lichtenstein et al. relationship, however, is not wholly satisfactory. Daniel K. Benjamin and William R. Dougan (1997) suggest caution in adjusting risks in this way. They reanalyze the Lichtenstein data and show that differences between individual perceptions and expert estimates disappear if the risks are limited to risks in the person's age group. They find there is no perception "bias." Jahn K. Hakes and W. Kip Viscusi (1997) also reanalyze augmented Lichtenstein et al. data using a Bayesian learning approach. They find that the differences between the individual perceived risks and expert risks are explained by the actual population mean death risk, the discounted lost life expectancy associated with the cause of death, and the age-specific hazard rate. The more specific is the expert, statistical risk estimate is to the individual, the smaller the "bias." If there is no bias, then no adjustment is needed. If something is known about the relationship between individual perceived risk and expert estimates of risk, that relationship can be used in making estimates of the VSL based on averting behavior in consumption. Before the Benjamin and Dougan's reexamination of risk perception bias, Miller (1990) used the Lichtenstein study as the basis for adjusting VSL estimates for perception bias in his critique of wage-risk estimates. Blomquist, Miller and Levy (1996) presented VSL estimates for adults, children, and motorcyclists unadjusted and adjusted for perception bias. After the reexamination of Lichtenstein et al., Miller (2000), in his review and analysis of VSL across countries, uses VSL estimates which are not adjusted for perception bias, but he allows for misperception through various regression specifications. Mount et al. (2001) estimate VSLs for children, adults, and senior adults based on a hedonic analysis of motor vehicle prices and their own extremely detailed estimates of risks of fatal and nonfatal accidents. They report their VSL estimates based on expert statistical risks and on risks corrected for perception bias. They consider their best estimates to be ones based on adjusted risks.

Economists have paid a great deal of attention to perception of environmental risks. V. Kerry Smith and F. Reed Johnson (1988) evaluated how Maine residents form perceptions about radon risks. They found support for a modified form of a Bayesian learning model and further that individuals who took mitigating action reported lower perceived risks. David S. Brookshire et al. (1985) estimated the impact of a risk notification program on perceptions of earthquake risks in the California housing market and found that the implicit values of risk after notification were comparable to the contingent values. Mark Dickie and Shelby Gerking (1996) found that the formation of risk beliefs about skin cancer depends on complexion and sunlight exposure, and link the risk beliefs to estimates of willingness to pay for avoiding skin cancer. W. Kip Viscusi and William N. Evans (1998) studied nonfatal health risks associated with a toilet bowl cleaner and an insecticide. They estimated the relationship between the stated (expert) risk and perceived risk and reported a relationship in a way similar to Lichtenstein et al. except that it is for the risks associated with the products being studied. They report the willingness to pay values implied by both the stated risk and stated risk adjusted for perception bias.

Averting behavior through job choice in the labor market provides another example of attention to risk perception. Douglas Gegax, Shelby Gerking, and William Schulze (1991) survey workers to get data on individuals' perceived mortality risks of specific jobs and wages rather than use observed frequencies to estimate occupation or industry average fatality rates.[12] This study and the other examples illustrate that studies of risk belief about averting behavior and valuation of risks can be combined to obtain better VSL estimates.[13]

The final reason why potential problems with risk misperception are not fatal to estimating VSL based on averting behavior in consumption is that the standard is not one of perfection. Alternative estimates implicit in the labor market and estimates elicited in hypothetical markets can contribute to our understanding of the VSL, but they are not perfect. Another alternative, the democratic process, has much to commend it, but preference revelation through the political process is not perfect either.

Concern about risk perception bias must be thought through carefully. It is straightforward that if perceived risks and expert risks match well for averting behavior studies, then these studies can reveal the values that individuals place on changes in their own mortality risks, and the estimated values can be used in BCA to evaluate environmental programs which reduce similar risks. If risk perceptions are biased and the bias is known, then the values implied by the biased perceptions are the VSL estimates that are appropriate for BCA because they reflect the tradeoff that individuals thought they were making. This adjustment is appropriate if the "correction" can be made in a convincing manner. Agreement with this adjustment probably depends on assessments of how convincing the corrections are. When evidence exists that individuals are willing to pay for perceived risks even though expert estimates are much lower, it poses a policy problem discussed by Gary H. McClelland, William D. Schulze, and Brian Hurd (1990) and Paul R. Portney (1992). The problem is that, from an expert perspective, resources might be wasted. Regardless of this policy problem if the proximate objective is to estimate individual WTP to reduce mortality risk, then VSLs implied by tradeoffs of perceived risk are appropriate.

5. Values of reductions in mortality risks for children and senior adults

5.1. Children

Children and senior adults are currently of special interest for environmental policy. The review of recent studies shown in Table 2 includes four that estimate VSL for special groups. Carlin and Sandy (1991) analyze mothers' use and nonuse of child safety seats for their children. Based on their analysis they find that their estimates of time and money use costs and external estimates of the reduction in mortality risks for the children imply a VSL for children of approximately $0.8 million in 2000 dollars. They report that their estimate of mothers' VSL for their children who are under the age of 5 years is approximately 87% of Glenn C. Blomquist's (1979) estimate of VSL for adult drivers based on use and nonuse of seat belts. While their study is thoughtfully executed, they do not include an estimate for mothers' disutility costs of using child safety seats.

Three studies estimate VSL for both adults and children. Blomquist, Miller, and Levy (1996) analyze motorists' use and nonuse of safety equipment. Their best estimate of VSL for children less than 5 years of age based on use and nonuse of child safety seats and belts stands at $3.7 million in 2000 dollars. This value is approximately 32% greater than the best estimate of VSL for adults of $2.8 million based on driver use and nonuse of seat belts. If the imprecisely estimated point estimate for child safety seat use only (not combined with harness use), then the VSL for children is roughly twice the VSL for adults. Jenkins, Owens, and Wiggins (2001) estimate parents' VSL for their bicycling children as approximately $2.9 million, a value that is less than the VSL of $4.3 million for bicycling adults who buy and use bicycle helmets, but their estimate is based on aggregate data and ignores utility/disutility of wearing helmets. Mount et al. (2000) estimate VSL based on a hedonic analysis of motor vehicle prices. They use detailed vehicle, driver, and vehicle use data along with an intertemporal adjustment based on Michael J. Moore and W. Kip Viscusi's (1988) article to estimate VSLs for adults and children. Their estimate for children of $7.3 million is slightly greater than the estimate for adults of $7.2 million. Because the difference for the adults and children is due to the intertemporal adjustment, it is not as convincing as the estimate of that for adults that is based on the detailed vehicle, driver, and use data. After considering the limited number of estimates we have for children, a hunch is that the VSL for children exceeds that for adults by at least one-third.

Given the limited number of estimates of VSL for children, it is worth trying to glean something from the estimates of values of children's health. Mark D. Agee and Thomas D. Crocker (2001) analyze data from the 1991 National Maternal and Infant Health Survey to estimate smokers' substitution rates between own consumption and own health, between own consumption and their children's exposure to tobacco smoke, and between own health and their children's health. They estimate that parents value their children's health twice as much as their own health. The measure of health is parents' rating of child health and not mortality risk, but surely the parents, mostly mothers, perceive that mortality risk increases with poorer health. The risk would be of fatal acute episodes associated with respiratory attacks and of fatal chronic diseases which develop later in children's lives.

Jin-Tan Liu et al. (2000) design and implement a stated preference study in Taiwan to estimate a mother's WTP for medicine that would prevent her from getting another case of the cold she typically gets and her WTP for medicine that would prevent her child from getting another case of the cold the child typically gets. They find that for comparable colds a mother's WTP to prevent her child's cold is approximately twice her WTP to prevent her own cold. A stated preference study of acute bronchitis by Mark Dickie and Victoria L. Ulery (2001) also finds parental altruism toward their children and that WTP for avoiding episodes is less for parents than for their children. The value for their children is about twice the value for themselves. These three morbidity studies are consistent with the mortality risk studies of child safety seat/belt use that find that VSLs are greater, or at least not less, for children compared to adults. In light of the decision to have children, accommodate them in labor market choices, and the costly investments in other forms of human capital such as education, perhaps it is not surprising that VSL for children is not less than that for adult parents.

5.2. *Senior adults*

Few estimates of VSL exist for senior adults. The only study that estimates VSL based on self-protection, or averting behavior, in consumption is Mount et al. (2000). Based on a hedonic analysis of motor vehicle prices using detailed vehicle, driver, and vehicle use data and an adjustment using the Moore and Viscusi intertemporal model they estimate that VSL for senior adults is approximately $5.2 million. This preliminary estimate is less than the estimate for all adults. However, the difference for seniors is partly due to the intertemporal adjustment.

The only study that estimates a VSL for older adults based on risk compensating wage differentials is by V. Kerry Smith, Mary F. Evans, Hyun Kim, and Donald H. Taylor (forthcoming). Their analysis of data from the Health and Retirement Survey and the Bureau of Labor Statistics yields estimates of VSL for all workers in the sample of approximately $6 million in year 2000 dollars. This estimate is within the range of estimates reviewed in the Janusz R. Mrozek and Laura R. Taylor (2002) meta-analysis and other labor market studies such as those in Viscusi's (1993) and W. Kip Viscusi and Joseph E. Aldy's (2003) reviews. The Smith et al. VSL estimates for workers who are 51–65 years of age are *greater* than for all workers and as much as twice the size of VSL for all workers. Their study does not include older seniors such as individuals over 75. More empirical and theoretical research is warranted because these estimates come from one study and are inconsistent with the implications of the life cycle model of VSL as normally interpreted. Implications from the life cycle model are being reconsidered. Recent papers by Johansson (2001, 2002), for example, demonstrate that the assertions that there are strong theoretical grounds for the view that VSL falls with age have been overstated. He shows that the implication that VSL declines with age is sensitive to assumptions about consumption over the life cycle. If consumption is not constant, then the VSL can decrease, stay constant, or increase with age.

The stated preference study of Canadian adults by Alan Krupnick et al. (2002) finds lower values for adults than Smith et al. (forthcoming), but a similarity is that they too find that the VSL does not change much with age during the 50s and 60s.[14] They find it is about 30% lower for individuals aged 70 and over compared to younger adults. In another stated preference study, Magnus Johannesson and

Per-Olov Johansson (1997) elicit a premium Swedish adults are willing to pay for a program that would extend life expectancy by one year conditional upon reaching age 75. They too do not find much change in value with age, but in contrast to Krupnick et al., they find that WTP increases slightly with age.

5.3. *If values differ, should different values be used in policy?*

In addition to future research on VSL for children and senior adults, a formal study of the ethics and practicality of using different VSLs for different groups in BCA would be worthwhile. The theory of using the values of the individuals who receive the benefits and bear the costs of policy is clear when the goal is maximizing efficiency. It is the basis for using individual WTP. However, what is the ethical basis for using population average values in some cases and values of specific subpopulation groups in other cases? *If* the primary beneficiaries of a policy that improves air quality are smokers and smokers have lower VSL than nonsmokers, is the policy to be evaluated with those lower values? *If* the primary beneficiaries of remediation of a Superfund site are nonminority poor and the VSL for them is lower than for individuals with higher income, is the policy to be evaluated with those lower values? Liu et al. (2000) find that in Taiwan mothers' WTP for preventing illness is 20% higher for their sons than for their daughters. *If* the same relationship is found in the U.S., is EPA going to use higher values for boys? *If* marital status influences VSL, should different values be used for single, married, and divorced individuals?[15] Notice how the previous concern for the representativeness of the estimates from a set of individuals is replaced by concern for estimates for individuals with a specified characteristic. It is unclear, however, that policy makers have a well developed conceptual framework for applying the different VSL estimates to policy. A hunch is that a consistent conceptual approach may be one that recognizes that benefit-cost analysis takes place within a particular society and the legal framework of that society, see Richard O. Zerbe (1991). Within such a context different VSL would be used for different types of individuals when evidence shows that the estimates of VSL differ and when the legal and regulatory framework indicates that different values are to be applied.

6. Comparing estimates from averting behavior in consumption to estimates from the labor market and stated preferences

The focus of this review is on estimates of VSL based on self-protection or averting behavior in consumption. One aspect of the recent estimates reported in Table 2 that is worth noting is that the simple average of the VSL estimates of $4.5 million for adults and what is probably the best estimate of about $4 million fall in the range of estimates based on averting behavior in the labor market. $4 to $5 million in 2000 dollars is within the range of studies reviewed by Mrozek and Taylor (2002) and Viscusi (1993). It is a bit higher than the estimates in the range of $1.5–2.5 million based on best practice in Mrozek and Taylor, but they place a greater weight on studies that control for more occupation and industry characteristics and those studies tend to yield lower estimates of VSL. The VSL estimate of about $4 million from averting behavior in consumption is also close to Miller's (2000) average of $3.9 million in 2002 dollars based on his review of studies of all three types (consumption, labor, and stated preferences).

Although wage-risk studies have tended to produce estimates of VSL greater than estimates from averting behavior in consumption, there is some reason to believe that the estimates from the labor market are too high. Jason Shogren and Thomas D. Crocker (1991, 1999) emphasize the importance of endogenous environmental risk and its implications for self-protection as a lower bound on the value of risk reductions. Jason Shogren and Tommy Stamland (2002) offer a reason for *upward* bias in risk compensating wage studies. They demonstrate that if workers differ in their individual, private ability to reduce risk and the ability is unobservable by employers, then a market wage must be offered to attract the marginal worker who faces the most risk of those employed. If the average risk of all workers is used to estimate a VSL, then risk is lower than that faced by the marginal worker and the VSL estimate is biased upward. If unbiased estimates from the labor market are even lower than the meta-analysis of Mrozek and Taylor indicates, then more thought about the difference between them and the higher estimates from the recent averting behavior studies is warranted.

The de Blaeij et al. (2000) meta-analysis of estimates of VSL finds that stated preference, or contingent valuation, studies yield higher estimates of VSL than estimates of VSL implied in studies of self-protection or averting behavior. Miller (2000) reports coefficients from his regression meta-analysis that imply that VSL estimates based on wage-risk tradeoffs are significantly and substantially higher than the VSL estimates based on averting behavior in consumption. He finds that the VSL estimates based on stated preferences are higher yet. Stated preference studies in their rawest form are subject to "yea saying" hypothetical bias. For example, for the simple, hypothetical purchase of the private good, sunglasses, Karen Blumenschein et al. (1998) find strong evidence of hypothetical bias relative to actual purchases. Significant numbers of individuals say they will purchase at the stated price, and then, in fact, do not purchase when given the opportunity. One explanation for the stated preference VSLs being greater than the implied VSLs is that early stated preference studies did not include specific countermeasures to "yea saying" that have been developed recently. If recent research including such countermeasures is indicative, then future stated preference studies need not yield estimates of the VSL which are greater than values implied by averting behavior due to hypothetical bias. Presumably if a countermeasure to hypothetical bias such as cheap talk and calibration by stated certainty continue to be effective and others are developed, then a future meta-analysis of VSL would show less difference between stated preference and averting behavior estimates. Other factors may cause estimates from the approaches to differ, but the contribution of hypothetical bias will shrink if countermeasures are effective.[16]

7. The research portfolio and behavior in the household

Valuing life may appear to be impossible, but for practical purposes it is straightforward. People, as individuals and as societies, make choices all the time in which they implicitly make tradeoffs between changes in their mortality risks and valuable time and money. Estimates of these values of changes in mortality risks, or alternatively, values of statistical lives, come from analyses of jobs with different wages and risks, household consumption decisions involving changes in risk and time and money, and from direct questioning involving risk-money tradeoffs in constructed or experimental markets. The estimates come from a large number and a wide variety of studies. This broad nature of the evidence is a strength.

To ignore the prospect of new information from observable consumption behavior and to rely on only one approach would indicate a lack of appreciation for how current understanding was achieved. To invest in research on only one type, say stated preference, would make the research investment portfolio a risky one. Estimates of the VSL based on willingness to pay are considerably more reliable than, say, 20 years ago. When the whole of the literature on VSL is viewed, its strength lies in the quantity and variety of estimates (see Glenn C. Blomquist, 2001). Future research should include a variety of approaches including averting behavior in consumption. Despite the considerable effort that has gone into estimating some components in many studies, more research should be done on some of the components such as disutility costs and risk perception for the household activity from which VSLs will be inferred. Tension exists between scholars probing the edges of our understanding and practitioners who must make decisions and defend them in the face of demand for perfect estimates. A tendency is to favor one method as the best and defend it. Because we do not know exactly what future research will bring, the temptation to pursue a strategy of investing in only the "best" method is risky. Prudent investors who are at all risk averse diversify. Research on self-protection and averting behavior in household consumption decisions should be a vital part of current and future research on valuing mortality risks.

Notes

1. The ethical foundation for benefit cost analysis can be found in teleology. One form of teleology is utilitarianism in which goodness can be judged on the basis of choosing alternatives that maximize the good for all. A deontologist, in contrast, might object that any tradeoff of risk for money or time is morally objectionable and the concept of VSL for use in BCA is wrong; see Sherry I. Brandt-Rauf and Paul W. Brandt-Rauf (1980).
2. If we fail to emphasize what we know and what we agree on while striving to improve the practice of economics, we risk having the whole approach dismissed as we are viewed as just squabbling, see *The Economist* (1997).
3. V. Kerry Smith's (1991) "Household Production Functions and Environmental Benefit Estimation" and A. Myrick Freeman's (2003, Chapter 4) "Revealed Preference Models of Valuation: Basic Theory" provide broad reviews of the theory and use of household production approaches to valuing

environmental changes. These chapters reflect, in part, the fundamental influence of the individual, household approach in benefit estimation in environmental economics. Freeman's (2003, Chapter 10) "Valuing Longevity and Health" provides a careful review of approaches to valuing changes in health risks, and in particular, to estimating values of changes in mortality risks, VSLs. The literature is well developed and will not be reviewed again in this paper.

4. The model is only sketched here. For a more complete presentation see Blomquist (1979). For a more complete discussion of this approach including refinements, see Freeman (2003, Chapter 10).
5. See Freeman (2003, Chapter 10) for a more complete presentation of life cycle models. The list of implications given above is based on his summary on page 311. See Rosen (1988) also.
6. For early reviews see Glenn C. Blomquist (1981, 1982), Michael W. Jones-Lee (1985) and Ann Fisher, Dan Violette, and Lauraine Chestnut (1989).
7. Throughout this paper estimates are reported in 2000 U.S. dollars. The annual average Consumer Price Index for all urban consumers for all items is used to convert values from studies with VSL reported in dollars for another year.
8. The countries are Australia, Austria, Canada, Denmark, France, Japan, New Zealand, South Korea, Sweden, Switzerland, Taiwan, and the United Kingdom.
9. While one can question the assumption and attempt to obtain the subjective estimates of risk or adjust for perception bias, the tradeoff is no more *ex post* than the typical estimate from self protection and averting behavior in the labor market.
10. Ghosh, Lees, and Seal (1975) used observed speeds on British motorways to estimate VSL 26 years ago and their estimate is $1.1 million in 2002 dollars. Although it is not as sophisticated as Ashenfelter and Greenstone's, both averting behavior studies contribute to what is now considered known, that VSL is greater than discounted foregone earnings.
11. In Blomquist (1982) the Lichtenstein et al. (1978) estimated relationship is used to adjust estimates of the VSL and offer policy makers an alternative to VSL estimates based on expert risk estimates.
12. Mrozek and Taylor (2002) find in their meta-analysis that the estimated effect of using worker's self-assessed risk on the VSL estimates is not robust across specifications. However, in their sample only the Gegax, Gerking, and Schulze (1991) and Moore and Viscusi (1988) studies used perceived risk. They define their best estimates based on studies using expert risk because of their concern that too few wage-risk studies use perceived risk.
13. Misperception of the risk level need not be equal to misperception of the change in risk. However, correct perception of the level of risk is more important in that mortality risks vary by orders of magnitude and gross misperception of the level will dominate misperception of the change even if it is by as much as a half. Another reason for interest in perception of risk levels is that correct perceptions of the levels of risk before and after a change will lead to correct perception of the change in risk.
14. If the emphasis is on quality of life, an alternative to using different VSL for individuals who differ in age would be estimate QALYs directly, see Magnus Johannesson (1996).
15. Shoshana Grossbard-Shecthman (1993) addresses the influence of marital status and the value of homemaker's time.
16. A "cheap talk" script about how individuals tend to say yes appears to have mitigated the tendency to say yes in experiments about contributions for environmental goods, see Ronald G. Cummings and Laura O. Taylor (1999). For calibration of responses by self ratings of certainty of purchase, see Patricia A. Champ et al. (1997) and Blumenschein et al. (1998, 2001).

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APPENDIX F

An Empirical Bayes Approach to Combining Estimates of the Value of a Statistical Life for Environmental Policy Analysis

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May 12, 2003

An Empirical Bayes Approach to Combining and Comparing Estimates of the Value of a Statistical Life for Environmental Policy Analysis*

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Abstract

An empirical Bayes pooling method is used to combine and compare estimates of the Value of a Statistical Life (VSL). The data come from 40 selected studies published between 1974 and 2000, containing 196 VSL estimates. The estimated composite distribution of empirical Bayes adjusted VSL has a mean of $5.4 million and a standard deviation of $2.4 million. The empirical Bayes method greatly reduces the variability around the pooled VSL estimate. The pooled VSL estimate is sensitive to the choice of valuation method and study location, but not to the source of data on occupational risk.

Key words: Value of a Statistical Life (VSL), empirical Bayes estimate, environmental policy, health policy, contingent valuation method, hedonic wage method

JEL subject category number: J17, C11, Q28

The value of a statistical life is one of the most controversial and important components of any analysis of the benefits of reducing environmental health risks. Health benefits of air pollution regulations are dominated by the value of premature mortality benefits. In recent analyses of air pollution regulations (United States Environmental Protection Agency (USEPA), 1999), benefits of reduced mortality risks accounted for well over 90 percent of total monetized benefits. The absolute size of mortality benefits is driven by two factors, the relatively strong concentration-response function, which leads to a large number of premature deaths predicted to be avoided per microgram of ambient air pollution reduced, and the value of a statistical life (VSL), estimated to be about \$6.3 million[1]. In addition to the contribution of VSL to the magnitude of benefits, the uncertainty surrounding the mean VSL estimate accounts for much of the measured uncertainty around total benefits. Thus, it is important to obtain reliable estimates of both the mean and variance of VSL.

The VSL is the measurement of the sum of society's willingness to pay (WTP) for one unit of fatal risk reduction (i.e. one statistical life). Rather than the value for any particular individual's life, the VSL represents what a whole group is willing to pay for reducing each member's risk by a small amount (Fisher et al. 1989). For example, if each of 100,000 persons is willing to pay \$10 for the reduction in risk from 2 deaths per 100,000 people to 1 death per 100,000 people, the VSL is \$1 million (\$10 × 100,000). Since fatal risk is not directly traded in markets, non-market valuation methods are applied to determine WTP for fatal risk reduction. The two most common methods for obtaining estimates of VSL are the revealed preference approach including hedonic wage and hedonic price analyses, and the stated preference approach including contingent valuation, contingent ranking, and conjoint methods. EPA does not conduct original studies but relies on existing VSL studies to determine the appropriate VSL to use in its cost-benefit analyses. The primary source for VSL estimates used by EPA in recent analyses has been a study by Viscusi (1992). Based on the VSL estimates recommended in this study, EPA fit a Weibull distribution to the estimates to derive a mean VSL of $6.3 million, with a standard deviation of $4.2 million (U.S. EPA, 1999).

We extend Viscusi's study by surveying recent literature to account for new VSL studies published between 1992 and 2001. This is potentially important because the more recent studies show a much wider variation in VSL than the studies recommended by Viscusi (1992). The estimates of VSL reported by Viscusi range from 0.8 to 17.7 million. More recent estimates of VSL reported in the literature range from as low as $0.1 million per life saved (Dillingham, 1985), to as high as $87.6 million (Arabsheibani and Marin, 2000). Careful assessment is needed to determine the plausible range of VSL, taking into account these new findings.

There are several potential methods that can be used to obtain estimates of the mean and distribution of VSL. In a study prepared under section 812 of the Clean Air Act Amendments of 1990 (henceforth called the EPA 812 report), it was assumed that each study should receive equal weight, although the reported mean VSL in each study differs in its precision. For example, Leigh and Folson (1984) estimate a VSL of $10.4 million with standard error of $5.2 million, while Miller (1997) reports almost the same VSL ($10.5 million) but with a much smaller standard error ($1.5 million)[2]. As Marin and Psacharopoulos (1982) suggested, more weight should be given to VSL estimates that have smaller standard errors.

Our focus is to develop a more statistically robust estimate of the mean and distribution of VSL using the empirical Bayes estimation method in a two-stage pooling model. The first stage groups individual VSL estimates into homogeneous subsets to provide representative sample VSL estimates. The second stage uses an empirical Bayes model to incorporate heterogeneity among sample VSL estimates. This approach allows the overall mean and variance of VSL to reflect the underlying variability of the individual VSL estimates, as well as the observed variability between VSL estimates from different studies. Our overall findings suggest the empirical Bayes method provides a pooled estimate of the mean VSL with greatly reduced variability. In addition, we conduct sensitivity analyses to examine how the pooled VSL is affected by valuation method, study location, source of occupational risk data and the addition of estimates with missing information on standard errors. This sensitivity analysis allows us to systematically compare VSL estimates to determine how they are influenced by study design characteristics.

1. Methodology

1.1 Study selection

We obtained published and unpublished VSL studies by examining previously published meta-analysis or review articles, citations from VSL studies and by using web searches and personal contacts.

The data were prepared as follows. First, we selected qualified studies based on a set of selection criteria applied in Viscusi (1992). Second, we computed and recorded all possible VSL estimates and associated standard errors in each study. Third, we made subsets of homogeneous VSL estimates and calculated the representative VSL for each subset by averaging VSLs and their standard errors[3]. Each step is discussed in detail below.

Since the empirical Bayes estimation method (pooled estimate model) does not control for the overall quality of the underlying studies, careful examination of the studies is required for selection purposes. In order to facilitate comparisons with the EPA 812 report, we applied the same selection criteria that were applied in that report, based largely on the criteria proposed in Viscusi (1992).

Viscusi (1992) examined 37 hedonic wage (HW), hedonic price (HP) and contingent valuation (CV) studies of the value of a statistical life, and listed four criteria for determining the value of life for policy applications. The first criterion is the choice of risk valuation method. Viscusi (1992) found that all the HP studies evaluated failed to provide an unbiased estimate of the dollar side of the risk-dollar tradeoff, and tend to underestimate VSL. Therefore only HW studies and CV studies are included in this study.

The second criterion is the choice of the risk data source for HW studies. Viscusi argues that actuarial data reflect risks other than those on the job, which would not be compensated through the wage mechanism, and tend to bias VSL downward. Therefore some of the initial HW studies that used actuarial data are removed from this analysis. The third criterion is the model specification in HW studies. Most studies apply a simple regression of the natural log of wage rates on risk levels. However, a few of the studies estimate the tradeoff for discounted expected life years lost rather than simply risk of death. This estimation procedure is quite complicated, and the VSL estimates tend to be less robust than in a simple regression estimation approach. Only studies using the simple regression approach are used in this analysis.

The fourth criterion is the sample size for CV studies. Viscusi argues that the two studies he considered whose sample sizes were 30 and 36 respectively were less reliable and should not be used. In this study, a threshold of 100 observations was used as a minimum sample size[4].

There are several other selection criteria that are implicit in the 1992 Viscusi analysis[5]. The first is based on sample characteristics. In the case of HW studies, he only considered studies that examined the wage-risk tradeoff among general or blue-collar workers. Some recent studies only consider samples from extremely dangerous jobs, such as police officer. Workers in these jobs may have different risk preferences and face risks much higher than those evaluated in typical environmental policy contexts. As such, we exclude those studies to prevent likely downward bias in VSL relative to the general population. In the case of CV studies, Viscusi only considered studies that used a general population sample. Therefore we also exclude CV studies that use a specific subpopulation or convenience sample, such as college students.

The second implicit criterion is based on the location of the study. Viscusi (1992) considered only studies conducted in high income countries such as U.S., U.K. and Japan. Although there are increasing numbers of CV or HW studies in developing countries such as Taiwan, Korea and India, we exclude these from our analysis due to differences between these countries and the U.S. Miller (2000) found that income level has a significant impact on VSL, and because we are seeking a VSL applicable to U.S. policy analysis, inclusion of VSL estimates from low-income countries may bias VSL downward. In addition, there are potentially significant differences in labor markets, health care systems, life expectancy, and preferences for risk reductions between developed and developing countries. Thus, our analysis only includes studies in high-income OECD member countries[6]. Finally, our analysis only uses studies that estimate people's WTP for immediate risk reduction□due to concerns about comparisons between risks with long latency periods with inherent discounting or uncertainty about future baseline health status.

1.2 Data preparation

In VSL studies, authors usually report the results of a hedonic wage regression analysis, or WTP estimates derived from a CV survey. In the studies we reviewed, a few authors reported all of the VSL that could be estimated based on their analysis, but most authors reported only selected VSL estimates and provided recommended VSL estimates based on their professional judgment. This judgment subjectively takes into account the quality of analysis, such as the statistical significance of the result, the target policy to be evaluated, or judgments based on comparative findings. Changes in statistical methods and best practices for study design during the period covered by our analysis may invalidate the subjective judgments used by authors to recommend a specific VSL. To minimize potential judgment biases, as well as make use of all available information, we re-estimate all possible VSLs based on the information provided in each study and included them in our analysis as long as they met the basic criteria laid out by Viscusi (1992)[7]. For certain specifications some authors found a negative VSL. However, in every case the authors rejected the plausibility of the negative estimates. We agree that negative VSL are highly implausible and exclude them from our primary data set. However, we do present sensitivity analysis showing the effects of excluding the negative estimates.

Estimation of VSL from HW studies

Most of the selected HW studies use the following equation to estimate the wage-risk premium:

$$LnY_i = a_1 p_i + a_2 q_i + a_3 p_i^2 + X_i\beta + \varepsilon_i \qquad (1)$$

where Y_i is equal to earnings of individual i, p_i and q_i are job related fatal and non-fatal risk faced by i (q_i often omitted), X_i is a vector of other relevant individual and job characteristics (plus a constant) and ε_i is an error term. In many cases, the wage equation will also include fatal risk squared and interactions between risk and variables such as union status. Based on equation (1), the VSL is estimated as follows.

$$VSL = (d\ln Y/ dp_i) \times \text{mean annual wage}^{8} \times \text{unit of fatal risk}^{9} \qquad (2)$$

Note that $d\ln Y/dp_i$ may include terms other than a_1 if there are squared or interaction terms.

VSL is usually evaluated at the mean annual wage of the sample population. The unit of fatal risk is the denominator of the risk statistic, i.e. 1000 if the reported worker's fatal risk is 0.02 per 1000 workers. If there is an interaction term between fatal risk and human capital variables such as "Fatal Risk" × "Union Status", the VSL is evaluated at the mean values of the union status variable. If there is a squared risk term, the VSL is evaluated at the mean value of fatal risk.

Estimation of standard error of VSL from HW studies

The standard error of the VSL (SE(VSL)) from a HW study is $Var(VSL) = \left(\text{unit of risk}\right)^2 Var\left(\partial \ln Y/\partial p \times \overline{Y}\right)$, where $\overline{Y}$ is the average wage for the sample. For example, if the wage equation is specified as $LnY = a_1 p_i + a_2 q_i + a_3 p_i^2 + a_4 p_i UNION + \varepsilon_i$, then

$$Var(VSL) = \left(\text{unit of risk}\right)^2 \left[Var\left(a_1\overline{Y}\right) + 4Var\left(a_2\overline{p}\overline{Y}\right) + Var\left(a_3\overline{Y}\,\overline{UNION}\right) + 2Cov\left(a_1\overline{Y}, a_2\overline{p}\overline{Y}, a_3\overline{Y}\,\overline{UNION}\right)\right]$$

To calculate the full variance, allowing for the observed variability in wages and fatal risk, one needs to calculate the variance of the product of the regression coefficients and the wage, risk, and interaction terms. We use the formula for the exact variance of products provided by Goodman (1960). For the first variance term above, this formula would be

$$Var\left(a_1\overline{Y}\right) = \overline{Y}^2 \frac{s^2\left(a_1\right)}{n} + a_1^2 \frac{s^2\left(\overline{Y}\right)}{n} - \frac{s^2\left(a_1\right)s^2\left(\overline{Y}\right)}{n^2}$$

Most of the studies included in our analysis do not report the variance of annual wage or the covariance matrix (either for the parameter estimates or the variables), so we calculated the standard error of VSL based on the available information, usually consisting of the standard errors of the estimated parameters of the wage equation. In this case the variance formula reduces to

$$Var(VSL) = \left(unit\ of\ risk\right)^2\left[\overline{Y}^2 Var\left(a_1\right) + 4\overline{p}^2\overline{Y}^2 Var\left(a_2\right) + \overline{Y^2}\,\overline{UNION^2} Var\left(a_3\right)\right]$$

To assess the impact of treating mean annual wage as a constant, we estimate the standard error with and without the wage variance for the 45 VSL estimates for which information on the variance of wage was available. We find that the differences between the two estimates of standard error are fairly small, within $0.2 million for most estimates. In no case does the standard error differ by more than 10 percent. We also assess the impact of omitting the covariance term by comparing the reported standard error of Scotton and Taylor (2000) providing a "full" variance estimate for the estimated VSL with our estimated standard error, which does not include the covariance term. We find that the difference in standard error is quite small. Note that the published standard error from this study treats mean annual wage as fixed, so the comparison shows only the effect of excluding the covariance term. These results suggest the impact of omitting the covariance terms and treating mean annual wage as fixed in our calculation of standard errors should not have a significant effect on our results.

Estimation of VSL and standard error from CV studies

For most of the CV surveys, we could not estimate the VSL and its standard error unless the author provided mean or median WTP and a standard error for a certain amount of risk reduction. When this information is available, the VSL and its standard error are simply calculated as WTP divided by the amount of risk reduction, and SE(WTP) divided by the amount of risk reduction, respectively.

Estimation of representative VSL for each study

Most studies reported multiple VSL estimates. For the empirical Bayes approach, which we use in our analysis, each estimate is assumed to be an independent sample, taken from a random distribution of the conceivable population of studies. This assumption is difficult to support given the fact that there are often multiple observations from a single study. To solve this problem, we constructed a set of homogeneous (and more likely independent) VSL estimates by employing the following approach.

We arrayed individual VSL estimates by study author (to account for the fact that some authors published multiple articles using the same underlying data). We then examined homogeneity among sub-samples of VSL estimates for each author by using Cochran's Q-statistics. The test statistic Q is the sum of squares of the effect about the mean where the i_{th} square is weighted by the reciprocal of the estimated variance. Under the null hypothesis of homogeneity, Q is approximately a χ^2 statistic with n -1 degrees of freedom (DerSimonian and Laird, 1986). If the null hypothesis was not rejected, we take the average of the VSL for the subset and the standard error to estimate the representative mean VSL for that author.

If the hypothesis of homogeneity was rejected, we further divided the samples into subsets according to their different characteristics such as source of risk data and type of population (i.e. white collar or blue collar), and tested for homogeneity again. We repeated this process until all subsets were determined to be homogeneous.

1.3 The empirical Bayes estimation model

In general, the empirical Bayes estimation technique is a method that adjusts the estimates of study-specific coefficients (β's) and their standard errors by combining the information from a given study with information from all the other studies to improve each of the study-specific estimates. Under the assumption that the true β's in the various studies are all drawn from the same distribution of β's, an estimator of β for a given study that uses information from all study estimates is generally better (has smaller mean squared error) than an estimator that uses information from only the given study (Post et al. 2001).

The empirical Bayes model assumes that

$$\beta_i = \mu_i + e_i \qquad (6)$$

where β_i is the reported VSL estimate from study *i*, μ_i is the true VSL, e_i is the sampling error and $N(0, s_i^2)$ for all i = 1,…, n. The model also assumes that

$$\mu_i = \mu + \delta_i \qquad (7)$$

where μ is the mean population VSL estimate, δ_i captures the between study variability, and $N(0, \tau^2)$, τ^2 represents both the degree to which effects vary across the study and the degree to which individual studies give biased assessments of the effects (Levy et al., 2000; DerSimonian and Laird, 1986).

The weighted average of the reported β_i is described as μ_w. The weight is a function of both the sampling error (s_i^2) and the estimate of the variance of the underlying distribution of β's (τ^2). These are expressed as follows;

$$\mu_w = \frac{\sum w_i^* \beta_i}{\sum w_i^*} \qquad (8)$$

$$s.e.\ (\mu_w) = \left(\sum w_i^*\right)^{-1/2} \qquad (9)$$

where $w_i^* = \dfrac{1}{w_i^{-1} + \tau^2}$ and $w_i = \dfrac{1}{s_i^2}$

τ^2 can be estimated as

$$\tau^2 = max \left(0, \left(\frac{(Q-(n-1))}{\sum w_i - \frac{\sum w_i^2}{\sum w_i}} \right) \right) \tag{10}$$

where $Q = \sum w_i (\beta_i - \beta^*)^2$ (Cochran's Q-statistic) and $\beta^* = \dfrac{\sum w_i \beta_i}{\sum w_i}$

The adjusted estimate of the β_i is estimated as

$$Adjusted\ \beta_i = \frac{\frac{\beta_i}{e_i} + \frac{\mu_w}{\tau^2}}{\frac{1}{e_i} + \frac{1}{\tau^2}} \tag{11}$$

This adjustment, as illustrated in Figure 1, pulls the reported estimates of β_i towards the pooled estimate. The more within-study variability, the less weight the β_i receives relative to the pooled estimate, and the more it gets adjusted towards the pooled estimate. The adjustment also reduces the variance surrounding the β_i by incorporating information from all β's into the estimate of β_i. (Post et al. 2001). In our analysis, β_i corresponds to the VSL of the ith study.

In order to visually compare the distributions, we used kernel density estimation to develop smooth distributions based on the empirical Bayes estimate. The kernel estimation provides a smoother distribution than the histogram approach. The kernel estimator is defined by $f(x) = \dfrac{1}{nh}\sum_{i=1}^{n} K\left(\dfrac{x - X_i}{h}\right)$. The kernel function, $\int_{-\infty}^{\infty} K(x)dx = 1$, is usually a symmetric probability density function, e.g. the normal density, and h is window width. The kernel function K determines the shape of the bumps, while h determines their width. The kernel estimator is a sum of 'bumps' placed at observations and the estimate *f* is constructed by adding up the bumps (Silverman 1986). We assumed a normal distribution for K and a window width *h* equal to 0.7, which was wide enough to give a reasonably smooth composite distribution while still preserving the features of the distribution (e.g. bumps). The choice of window width is arbitrary, but has no impact on the statistical comparison, which is described below.

To compare the different distributions of VSL, we applied the bootstrap method, which is a nonparametric method for estimating the distribution of statistics. Bootstrapping is equivalent to random sampling with replacement. The infinite population that consists of the n observed sample values, each with probability 1/n, is used to model the unknown real population (Manly 1997). We first conducted re-sampling 1000 times, and compared the distributions in terms of mean, median and interquartile range.

2. Results and sensitivity analyses

In total, we collected 47 HW studies and 29 CV studies. A data summary for each stage of analysis is shown in Table 1. After applying the selection criteria outlined in section 2.1, there were 31 HW studies and 14 CV studies left for the analysis. In our final list, there are 22 new studies published between 1990 and 2000. We re-estimated all possible VSL for the selected studies, and obtained 232 VSL estimates.[10 11] There were 23 VSL estimates from five studies for which standard errors were not available, and thus they are excluded from our primary analysis, although we examine the impact of excluding those studies in a sensitivity analysis. After testing for homogeneity among sub-samples, we obtained 60 VSL subsets, and estimated a representative VSL and standard error for each subset. Finally, we applied the empirical Bayes method and obtained an adjusted VSL value for each subset.

It is worthwhile to note how the empirical Bayes approach reduces the unexplained variability among VSL estimates. Our 196 VSL estimates show an extremely wide range from \$0.1 million to \$95.5 million with a coefficient of variation of 1.3 (in 2000 constant dollars). The VSL estimates from the 60 subsets range from \$0.3 million to \$43.1 million with a coefficient of variation of 1.2, and the adjusted VSL estimates range from \$0.7 million to \$13.9 million with a coefficient of variation of 0.4.

2.1 The distribution of VSL

Figure 2 shows the kernel density estimates of the composite distribution of the empirical Bayes adjusted VSL (using the 60 representative VSL estimates) and the Weibull distribution for the 26 VSL estimates as reported in the EPA 812 report. The summary results are shown in Table 2. The composite distribution of adjusted VSL has a mean of \$5.4 million with a standard error of $2.4 million. This mean value is smaller than that based on the EPA 812 Weibull distribution and has less variance (EPA 812's coefficient of variation is 0.7) even though our VSL sample has a range more than five times as wide as the EPA 812 sample.

2.2 Sensitivity analyses

2.2.1 Sensitivity to choice of valuation method

Many researchers argue that the VSL is sensitive to underlying study characteristics (Viscusi 1992, Carson, et al. 2000, Mrozek and Taylor 2002). One of the most interesting differences is in the choice of valuation method. To determine if there is a significant difference between the empirical Bayes adjusted distributions of VSL using HW and CV estimates, we used bootstrapping to test the hypothesis that HW and CV estimates of VSL are from the same underlying distribution.

We divided the set of VSL studies into HW and CV and applied the homogeneity subsetting process and empirical Bayes adjustment method to each group. The kernel density estimates of the distributions for HW and CV sample are shown in Figure 3. The HW distribution has a mean value of $9.4 million with a standard error of $4.7 million while the CV distribution has much smaller mean value of $2.8 million with a standard error of $1.3 million (see Table 2). Bootstrap tests of significance show the VSL based on HW is significantly larger than that of CV ($p<0.001$), comparing means, medians and interquartile ranges between the distributions.

2.2.2 Sensitivity to study location

Because of differences in labor markets, health care systems, and societal attitudes towards risk, VSL estimates from HW studies may potentially be sensitive to the country in which the study was conducted (this may also be true for CV studies, however there were too few CV estimates to conduct similar comparisons). Empirical Bayes estimation was applied to HW samples from the U.S. and U.K separately. (Comparisons with Canada and Australia were not conducted because of small sample sizes for those countries.) The distribution for the U.S. sample has a mean value of $8.5 million with a standard error of $4.9 million, while the distribution for the U.K. sample has a mean value of $22.6 million with a standard error of $4.9 million. Bootstrap tests of significance show that the U.S. estimates are significantly different from UK estimates based on comparing means and medians between distributions.

2.2.3 Sensitivity to source of occupational risk data

Moore and Viscusi (1988) found that VSL was sensitive to choice of source of occupational risk data. According to their results, the VSL estimated based on Bureau of Labor Statistics (BLS) death-risk data is significantly smaller than that estimated based on National Institute of Occupational Safety and Health (NIOSH) death risk data. We estimated the empirical Byes adjusted VSL distribution for each risk data source, and we did not find a significant difference between the two distributions. However, the reliability of our result is limited due to the small number of studies based on the BLS risk data.

2.2.4 Sensitivity to excluded VSL estimates

We also examined the sensitivity of our results to excluded estimates. To do this, we added to the sample the VSL estimates that were excluded from the primary analysis due to the lack of a standard error. We assumed for this test that all reported VSL estimates should have passed at least a 95 percent significance test, and estimate the corresponding standard error at this significance level for each VSL. This added nine averaged VSL estimates to the set of 60 representative estimates, including four estimates from HW studies and five from CV studies.

The distribution of the enhanced sample has a mean value of \$4.7 million with a standard error of \$2.2 million. Compared with the result of our main analysis, the mean value is reduced by \$0.7 million. This is because we have added more estimates from CV, which tends to produce relatively lower VSL. Bootstrap tests of significance show the VSL from HW studies is still significantly different from that from CV studies (p<0.0001), comparing means, medians and interquartile ranges.

We also report a 5% trimmed mean that increases the combined mean from both valuation methods from \$5.4 million to \$5.8 million with no effect on the coefficient of variation. Finally, we consider the impact of including negative estimates. Since these estimates were all associated with HW studies, the HW mean drops from \$9.4 million to \$6.6 million. This also has a noticeable effect on the combined mean dropping it from \$5.4 million to \$4.1 million. The difference between the CV and HW estimates remains significant based on bootstrap tests of the means and medians.

3. Conclusions

The meta analysis we have used results in a composite distribution of empirical Bayes adjusted VSL with a mean of $5.4 million and a standard deviation of $2.4 million. This is a somewhat lower mean than previous pooled estimates, and because of the Bayesian adjustment process, there is greatly reduced variability as evidenced by the coefficient of variation even though our dataset has a much wider range than previous studies.

Starting from a baseline of the literature used in Viscusi (1992), our approach has generated a set of hypotheses that may challenge some previously held assumptions. It is clear that VSL analysts need to look closely at study location; our estimates show significant differences in VSL even between developed countries with relatively similar income levels. It is also important to look at valuation method as we found quite different VSL estimates in the hedonic wage versus contingent valuation datasets. Our finding that the hedonic method generates significantly larger estimates than the CV approach is consistent with a comparison of CV and revealed preference approaches to valuing quasi-public goods reported by Carson (1996).

Theoretically, the two valuation methods should not necessarily provide the same results because the HW approach is estimating a local trade-off, while the CV approach approximates a movement along a constant expected utility locus (Viscusi and Evans 1990, Lanoie, Pedro and Latour 1995). However, the impact and direction of this difference had not been systematically investigated prior to this analysis

Our sensitivity analysis found no significant difference on average in the VSL estimates between studies using BLS or NIOSH data. Additional research into appropriate measures of risk is needed. Recent work by Black (2001) suggests that measurement errors in estimates of fatal risk can lead to large downward biases in estimates of VSL.

Aggregate level comparisons as we have done in this paper are useful in comparing the overall distribution of VSL estimates from each method, however the resulting comparisons might be significantly affected by differences in the design of each study, as the large variance in the HW distribution suggests. This problem could be addressed by applying meta-regression analysis, which can determine the impact of specific study factors by taking into consideration study characteristics such as sample population, study location, or sources of risk data (Levy et al., 2000; Mrozek and Taylor, 2002; Viscusi and Aldy, 2002).

Study location does seem to matter, but additional investigation is necessary to identify why there are differences. Simply lumping countries together as developed or developing may not be the best way to account for potential differences in VSL. Differences in health care system may be a potential factor, as there are a number of differences in insurance coverage and access to health care across developed countries (Anderson and Hussey, 2000). There may be numerous other socio-cultural factors that can cause VSL estimates to diverge.

As the excluded studies sensitivity analysis indicates, our results are sensitive to the addition of small magnitude VSL estimates with low variances. For example, Krupnick et al. (2000) estimated the VSL as $1.1 million with a standard error of $0.05 million. If we remove this estimate from our main analysis, the overall mean VSL is increased to $5.9 million, implying that one study reduces the overall mean by $0.5 million. It is thus especially important to determine the reliability of CV studies very carefully by assessing any potential questionnaire and scope effects (Hammitt and Graham, 1999). Also, it may be important to investigate why the VSL estimates from CV studies are so similar despite the differences in type of risk, study location and survey method.

In addition to the application of the empirical Bayes method, our analysis demonstrates the importance of adopting a two-stage procedure for combining evidence from the literature when multiple estimates are available from a single source of data. The first stage sorting process using the Cochran's Q test for homogeneity seems a reasonable approach to control for over-representation of any one dataset. From the original set of 40 studies, we obtained 196 VSL estimates and then classified these into 60 homogeneous subsets. This suggests that there was a high probability of assigning too much weight to some estimates if a single stage process were used, treating each of the 196 estimates as independent. Also, the two-stage approach does not discard information from each study. Instead it uses all the available information in an appropriate manner.

As in the field of epidemiology, the economics profession should consider developing protocols for combining estimates from different studies for policy purposes. Consistent reporting of both point estimates of VSL and standard errors, or variance-covariance matrices would enhance the ability of future researchers to make use of all information in constructing estimates of VSL for policy analysis. Additional research is needed to understand how VSL varies systematically with underlying study attributes, such as estimation method or location of studies. The empirical Bayes approach outlined here provides a useful starting point in developing the variables needed for such studies.

The widely cited pooled estimate of $6.3 million from the EPA 812 study based on Viscusi's assessment of the VSL literature was derived from a simple histogram method. This early approach ignored within and between study variability. Mrozek and Taylor presented an alternative method for deriving a mean VSL estimate for policy purposes based on a best fit regression model using only the hedonic wage studies. . We examine both CV and HW studies and present a different methodology using all available information to adjust individual VSL estimates based on the within and between study variability. By generating distributions of VSL, the method allows us to test individual hypotheses regarding study attributes. These comparisons have generated a number of hypotheses that should form the foundation for future meta-analyses of VSL combining the CV and HW approaches.

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Table 1. VSL Data Summary

	HW	CV	Total
Number of collected studies	47	29	76
Number of selected studies	31	14	45
Number of estimated VSL	181	51	232
Number of positive VSL with imputed SE	161	35	196
Mean (million $)	12.3	3.8	10.8
(Coefficient of variation)	(1.2)	(1.5)	(1.3)
Number of VSL subsets at 1st stage	43	17	60
Mean (million $)	12.4	3.8	9.8
(Coefficient of variation)	(1.1)	(0.8)	(1.2)
Number of VSL subsets at 2nd stage	43	17	60
Mean (million $)	9.4	2.8	5.4
(Coefficient of variation)	(0.5)	(0.5)	(0.4)

Table 2. Results of Empirical Bayes Estimates and Bootstrap Tests for Distribution Comparisons (2000 dollars)

	Mean (million $)	SD (million $)	Coefficient of variation	Bootstrap Test		
				Mean	Median	Interquartile
Distribution Comparison by Evaluation Method						
Total (60)	5.4	2.4	0.4	P-value (Ho: HW = CV)		
CV (18)	2.8	1.3	0.5	<0.001	<0.001	<0.008
HW (42)	9.4	4.7	0.5			
Distribution Comparison by Study Location (HW only)						
USA (30)	8.5	4.9	0.6	P-value (Ho: US =UK)		
UK (7)	22.6	4.9	0.2	<0.001	<0.001	<0.403
Distribution Comparison by Occupational Risk Data Source (HW only)						
BLS (3)	10.3	4.3	0.4	P-value (Ho: BLS = NIOSH)		
NIOSH (21)	7.2	3.9	0.5	<0.694	<0.798	<0.734
Distribution Comparison by Evaluation Method After Adding Excluded Estimates						
Total	4.7	2.2	0.5	P-value (Ho: HW = CV)		
CV	2.6	1.3	0.5	<0.001	<0.001	<0.009
HW	8.7	4.6	0.5			
5% trimmed estimate						
Total	5.8	2.5	0.4			
Including negative estimates						
Total (67)	4.1	1.7	0.4	P-value (Ho: HW = CV)		
CV (18)	2.8	1.3	0.5			
HW (49)	6.6	3.6	0.5	<.001	<.004	<.108

Figure1. Illustration of Empirical Bayes Pooling

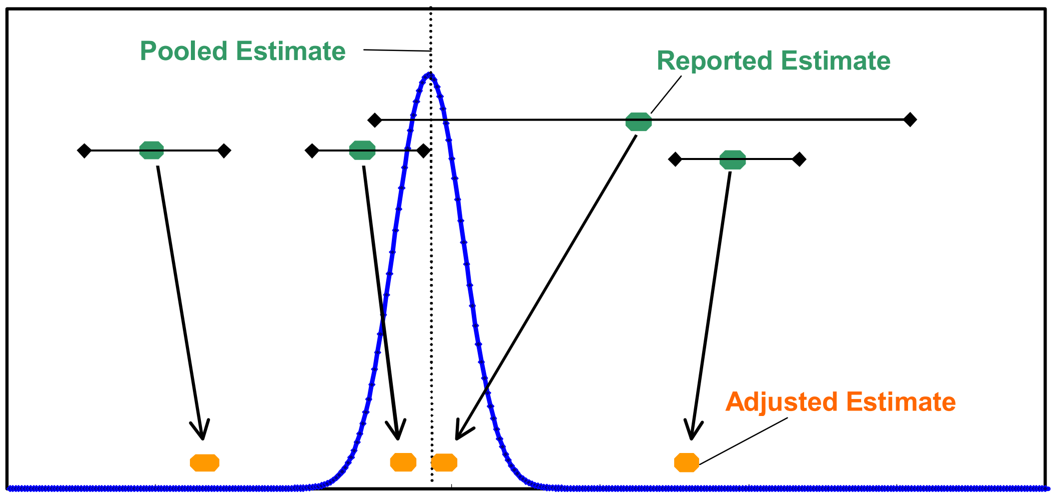


Figure 2. Comparison of Kernel Distribution of Empirical Bayes Adjusted VSL with Distribution of VSL Based on EPA Section 812 Report Estimates

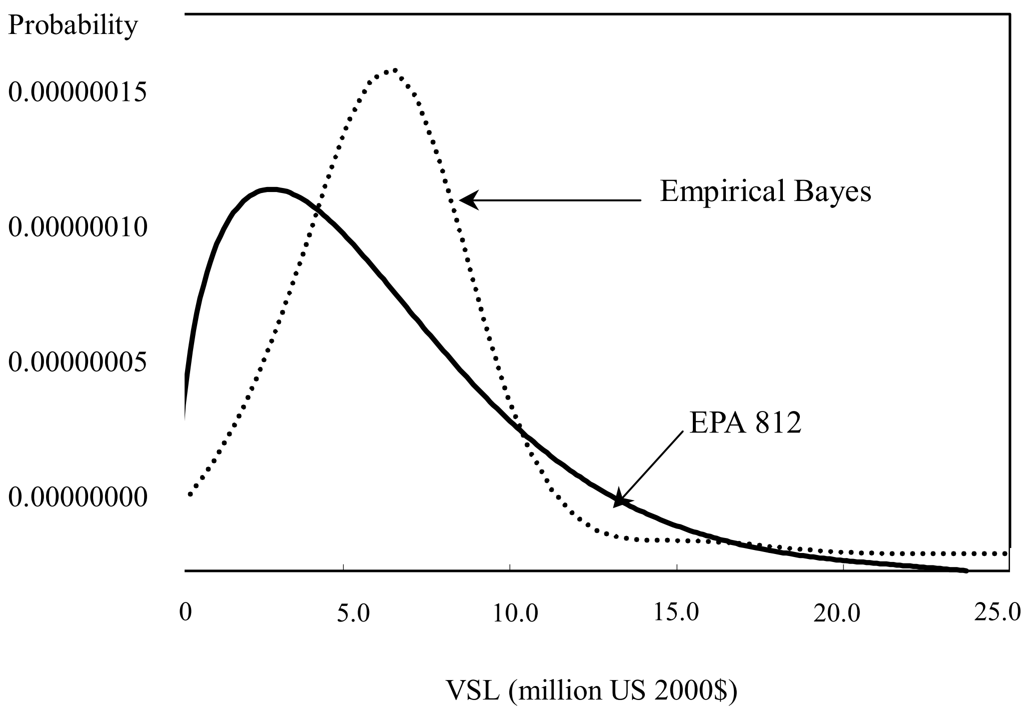
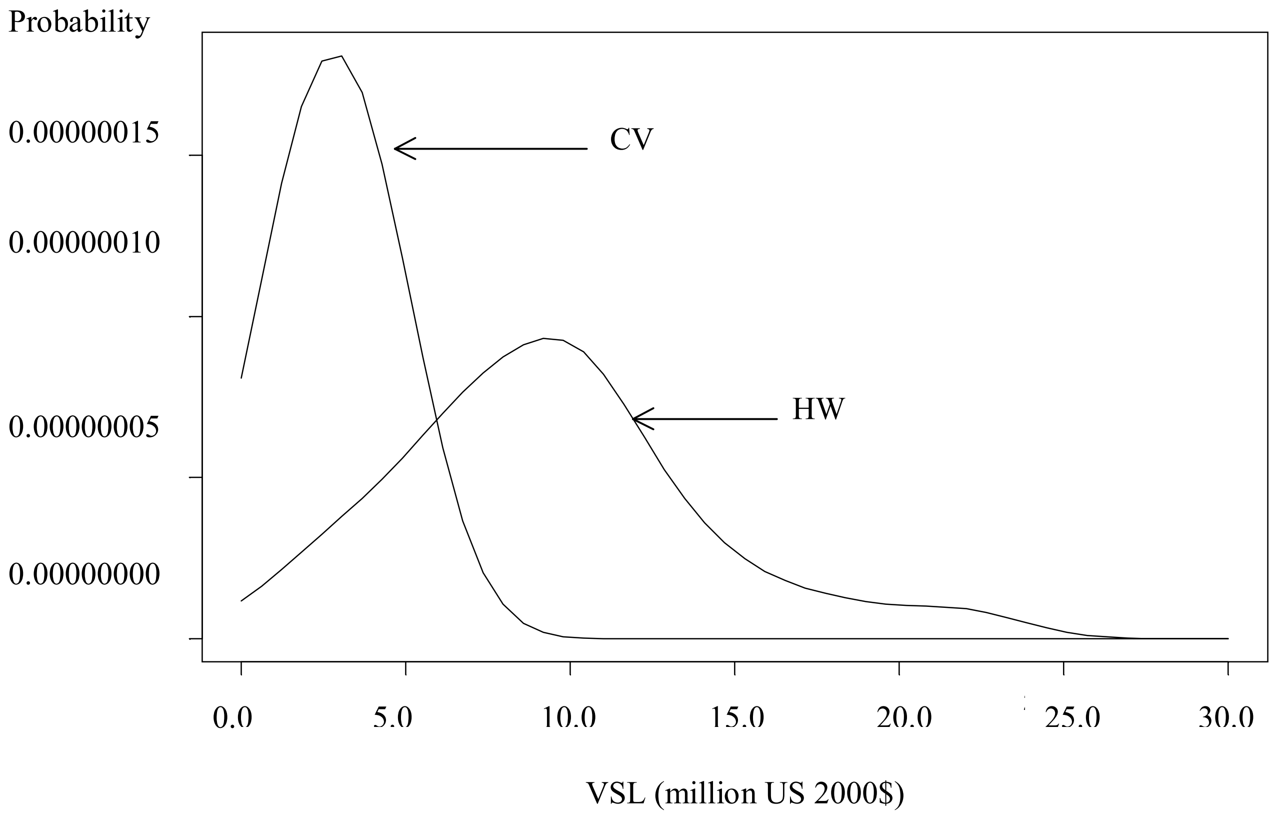


Figure 3. Comparison of Kernel Distribution of Empirical Bayes Adjusted VSL Based on HW and CV Estimates



Notes:

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[1]All estimates reported in this paper have been converted to constant 2000 dollars using the Bureau of Labor Statistics Consumer Price Index (CPI). The CPI inflation calculator uses the average Consumer Price Index for a given calendar year. These data represent changes in prices of all goods and services purchased for consumption by urban households. For estimates reported in foreign currency, we first converted to U.S. dollars using data on Purchasing Power Parity from the Organization for Economic Cooperation and Development, and then converted to 2000 U.S. dollars using the CPI.

[2] Most authors do not report standard errors of VSL estimates. We have estimated the standard errors for these and other studies using an approach discussed later in the paper.

[3] We also employed fixed approaches for pooling, but found this resulted in an artifact of providing greater weight to studies whose authors reported multiple estimates.

[4] This is admittedly an arbitrary cutoff. However, we determined that a sample size of 100 did not result in many studies being excluded and smaller samples did not seem to be reasonable.

[5] We exclude one additional study, by Eom (1994), due to concerns about the payment context for the willingness to pay question. In that study, individuals were asked to choose between produce with different levels of price and pesticide risk. The range of potential WTP was limited by the base price of produce. In order to realize an implied VSL within the range considered by Viscusi, individuals would need to have a WTP of around $400 per year. Because WTP in the study was tied to increases in produce prices, which ranged $0.39 to $1.49, it would be very unlikely that individuals would be willing to pay over a 100 times their normal price for produce to obtain the specified risk reduction. Tying WTP to observed prices thus limits the usefulness of this study for benefits transfer.

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[6] From http://worldbank.org/data/databytopic/class.htm. High-income OECD member have annual income greater than $9,266 per capita.

[7] One reviewer suggested that some published VSL estimates should be excluded from our analysis because the authors judged these estimates to be invalid. Our review of each study did not reveal authors' arguments excluding VSL estimates except a few instances in which authors questioned the reliability of the BLS and NIOSH occupational risk data. Because it is accepted to use these risk data in hedonic wage studies, we did not view this as a valid reason for dropping those VSL estimates. The summary of each author's review of their VSL estimate is in an appendix available upon request from the authors.

[8] Most studies use the hourly wage or weekly wage. In those cases authors multiply by 2000 (some use 2080) for mean hourly wage, and 50 (some use 52) for mean weekly wage to obtain mean annual wage. We follow each study's estimation approach and if that is not available, we use a multiplier of 2000 for hourly wage and 50 for weekly wage.

[9] The coefficient $d\, lnY/ d\, p_i$ does not depend on the units in which Y is measured. The requirement for a comparison across is that results are converted in the same units, e.g. per thousand per year.

[10] To assure the quality of re-estimation of VSL, we matched our results with estimates done by the original authors when available. Although the VSL estimates from Kneisner and Leeth (1991), Smith and Gilbert (1984) and V.K. Smith (1976) are included in EPA 812 report, the original manuscripts do not provide VSL estimates, and we could not replicate the estimates reported in EPA 812. Therefore we exclude those studies from our analysis.

[11] A full listing of studies and their associated VSL are available from the authors upon request.

APPENDIX G

What Determines The Value Of Life? A Meta-Analysis

By Janusz R. Mrozek and Laura O. Taylor

WHAT DETERMINES THE VALUE OF LIFE? A META-ANALYSIS

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WHAT DETERMINES THE VALUE OF LIFE? A META-ANALYSIS

ABSTRACT

A large literature has developed which uses labor market contracts to estimate the value of a statistical life (VSL). Reported estimates of the VSL vary substantially; from under $100,000 to over $25 million. This research uses meta-analysis to provide a quantitative assessment of the VSL literature. Results from existing studies are pooled to identify the systematic relationships between VSL estimates and each study's particular features such as the sample composition and research methods. Our meta-analysis suggests a VSL range of approximately $1.5 to $2.5 million (in 1998 dollars) is what can be reasonably inferred from past labor-market studies when "best practice" assumptions are invoked. This range is considerably below many previous qualitative reviews of this literature.

KEYWORDS: meta-analysis, value of statistical life, benefit-cost analysis

WHAT DETERMINES THE VALUE OF LIFE? A META-ANALYSIS

INTRODUCTION

Many important public policy initiatives have mortality reduction as their primary goal. Evaluation of these policies commonly includes benefit-cost analyses, and at the federal level such analyses are compelled by Executive Orders 12291 and 12866 [Federal Register, 1981 and 1993, respectively]. Proper evaluation requires an estimate of the value society places on a life saved as a result of the policy. The concern is not with the value of an "identified" life, but the value society places on reducing the statistical probability that one among them dies, the so called "value of a statistical life" [Viscusi 1992, 1993]. To date, there have been over 40 studies which rely on labor market contracts to estimate the value of a statistical life (VSL). In these studies, the implicit tradeoffs workers make between incremental increases in the risk of death on the job and the additional wages required to accept these riskier jobs are estimated and converted into corresponding estimates of the VSL.

Controversy regarding the validity of using VSL estimates from labor market studies in benefit-cost analysis of mortality reduction policies is focused on three primary issues. First, even though the majority of published studies report a statistically significant relationship between the risk of death on the job and workers' wages, at least sixteen studies report some results indicating no statistically significant relationship, including Viscusi [1978], Viscusi [1980], Dillingham [1985], Moore and Viscusi [1988a], Leigh [1991 and 1995], and Dorman and Hagstrom [1998]. Indeed, Leigh [1995] argues that the significant wage/risk relationships found in this literature are spurious and due to poor risk measurement and mis-specification of the wage equations [see also Dorman and Hagstrom, 1998 and Miller, 2000].

A second issue is the wide variation in the VSL estimates reported in this literature. For instance, Moore and Viscusi [1990] and Olson [1981] report VSL estimates ranging between $15 and $25 million per life saved. On the other hand, Dillingham [1979], Marin and Psacharopolous [1982], and Kniesner and Leeth [1991] report VSL estimates that are less than $100,000 per life saved (all estimates in 1998 dollars). These extreme estimates, while not necessarily arising from each author's preferred specification, indicate the substantial variation in VSL estimates. Such variation results in considerable uncertainty regarding the choice of which, if any, of these estimates are appropriate for inclusion in benefit-cost analyses.[1]

In response to the need to determine a "best" value of statistical life estimate, several authors have qualitatively reviewed the literature and use their knowledge to make judgements about which VSL estimates are more reasonable or "more correct." For example, Fisher, et al., [1989] review studies using several different methods to estimate the VSL and suggest $2.5 to $12.5 million as being the most defensible VSL range based on the then extant literature (all estimates reported herein are converted to 1998 dollars). Viscusi [1992, 1993] summarizes 24 labor market studies, and suggests that the appropriate range is between $4 and $9 million, as this is the range where "most estimates lie" in the studies he includes. Neuman and Unsworth [1993] for the United States Environmental Protection Agency [1997] calculate a "best estimate" of the VSL from 26 studies (21 of which were labor market studies) which they deem are most reliable: those which include nonfatal risks in the compensating wage equations, and whose

[1]A third, important concern is whether or not a VSL estimate pertaining to fatal, work-place injuries is an appropriate value for policies reducing latent risks (i.e., reducing risks of cancers later in life due to prolonged current exposures). This criticism is certainly worth exploration, but is outside the scope of this paper. It is important to note that despite this criticism, estimates of the VSL for many federal agencies conducting benefit cost analyses are based on information primarily gathered from labor market studies [see for example, U.S. Environmental Protection Agency, 1997 and 1999].

baseline risks were most similar to the mortality risks arising from air pollution. A Weibull distribution was fit to the 26 estimates and a mean of $6 million was computed. Miller [1990] combines a quantitative and qualitative approach, assembling one or two VSL estimates from each of 27 studies and adjusting each estimate to reflect his judgement regarding the deficiencies of each study (and in doing so, he discarded 30 percent of the studies). With these preferred estimates, he computes a mean value of a statistical life of $3 million, with a range of 2.1 to 3.9 million.

The research presented here uses a quantitative meta-analysis framework to evaluate value of statistical life estimates. Meta-analysis involves pooling the results from the existing literature to identify systematic relationships between the outcome of interest (the VSL in this case) and underlying factors influencing that outcome. The key advantage of this approach over existing literature reviews is that the meta-analysis provides a quantitative, systematic analysis of the existing literature to inform the researcher's judgements. Over 40 labor market studies estimating the value of a statistical life are reviewed. With information from these studies, we identify the relationships between their VSL estimates and underlying factors influencing those estimates such as: the baseline level of risks faced by the sample population, demographic characteristics of the sample, the source of the risk data, and researcher judgements such as equation specifications and sample selection. Results of the meta-analysis are then used to develop "best-practice" VSL estimates that are based on the current "weight of the evidence" from this literature. Our results indicate that previous assessments of this literature, and previously applied VSL estimates in benefit/cost analyses of regulatory actions, may overstate the value that can be reasonably drawn from this literature by 50 percent or more.

METHODOLOGY

Wage-Risk Tradeoffs

We focus on research that has estimated the value of a statistical life *via* compensating wage eqeuations. The general form of the compensating wage equation estimated in these studies is:

$$wage_k = \alpha + \beta_r risk_k + \sum_{n=1}^{N} \lambda_n X_{kn} + \sum_{m=1}^{M} \gamma_m D_{km} + \epsilon_k , \qquad \textbf{(1)}$$

in which the wage of the kth worker is estimated to be a function of: the risk of death on the job ($risk_k$); n variables describing human capital and demographic characteristics of the worker (X_{kn}) such as age and education; and m job characteristics (D_{km}) other than the risk of death such as whether or not supervisory activities are associated with the job. It should be noted that information on job characteristics is typically sparse, and so most compensating wage equation applications can include only dummy variables controlling for the occupation and/or industry classification of the worker's job.

In a linear specification such as (1), the coefficient on the risk variable is the additional wages a worker would require to assume an additional increment of risk of death on the job. By normalizing over risk, the compensating wage differential is converted to the value of a statistical life. For instance, suppose risk is measured in units of deaths per 10,000 workers; wages are hourly earnings; and a simple linear compensating wage equation is estimated as illustrated in (1). To compute the value of a statistical life, the estimated coefficient for Mw/Mr = $_r is multiplied by 2000 hours/year and then by 10,000. In this example, an estimate of $_r equal to 0.35 would imply a value of statistical life estimate of $7 million.

Meta-analysis

Meta-analysis is commonly applied in the health and medical sciences literatures [see Mann, 1994] and involves pooling raw data from a variety of clinical studies to evaluate the relationships between a health outcome of interest and key variables assumed to affect that outcome. A primary benefit of these applications is the increased evidentiary weight of the larger data set, which incorporates a larger design space than any one study could provide. Our use of meta-analysis differs somewhat from the traditional use in health sciences in that the data collected and analyzed are estimates of a variable of interest (the VSL) that have been calculated in a number of studies, rather than the original raw data.[2]

In this analysis, existing estimates of the value of a statistical life from compensating wage equation studies are pooled to identify the systematic relationships between these estimates and the particular features of each study in which they are reported, such as the sample composition and research methods. While it would be possible to conduct a meta-analysis of the VSL that pools estimates from studies using methods that are very different from the compensating wage equation approach such as contingent valuation, the idiosyncracies of each of these other studies would make it difficult to assess systematic effects of research judgements on the VSL estimates. A database is constructed of 203 VSL estimates obtained from 33 studies. Multiple observations are drawn from each study if authors reported variations in model specifications or samples from which VSL estimates could be obtained. While we reviewed over 40 studies, some could not be incorporated because of missing information. An appendix is available from the authors which describes these studies and why they could not be included (see

[2]Similar examples in the economics literature include Smith and Huang [1995] and Smith and Osborne [1996] who use meta-analysis to evaluate willingness to pay estimates for improvements in air-quality.

www.gsu.edu/~ecolot/research.html).

The dependent variable in our analysis is an estimate of the value of statistical life generated by a wage equation reported in a particular study. The independent variables used to determine the source of variation in the VSL estimates are those which describe the factors assumed to influence the VSL estimate. For example, overall, one would expect that the VSL will increase with baseline risk and with baseline income [Jones-Lee, 1974; Hammitt, 2000], which therefore are independent variables in our meta-analysis regressions. Factors describing the sample composition of the workers used to estimate the VSL (e.g., blue collar versus white collar) are also included as independent variables. Lastly, factors describing the researcher's methods of estimation are included to determine what effects, if any, some basic researcher judgements might have on VSL estimates.

In our analysis, we are also able to incorporate an important and controversial aspect of the labor market literature. Leigh [1995] suggests that because the risk data most commonly used in these studies differentiates risks only by industry of the worker, and not by his or her occupation, the risk measures are correlated with inter-industry wage differentials. The essential element of Leigh's argument is that the industry wage differentials that have been long noted in the labor market literature are correlated with industry-level risk differentials, but are not due to risk differentials. Thus, to avoid mis-specification and properly assess the impact of risk on wages, compensating wage equations should at least include dummy variables indicating a worker's industry at the broadest classification level to capture the effect of inter-industry wage differentials separately from the effect of risk. Leigh finds that when he includes such dummy variables, risk measures are not significant predictors of wages [see also Dorman and Hagstrom, 1998]. He argues this indicates that risk is indeed a proxy for inter-industry wage differentials and thus the coefficient on risk does not measure compensation for variations in risk. His critique is directly incorporated in our analysis as described in the next section.

In a similar meta-analytic approach, Desvousges et al. [1995] compile 29 VSL estimates that were deemed to be the "most reliable" and regress these 29 estimates on the mean risk of the sample; no other study-specific information was included except a dummy variable indicating the source of the risk data. Their estimated model suggests a preferred VSL of $4 million (1998 dollars). Similarly, Miller, 2000 compiles sixty-eight "best estimates" from labor market, contingent valuation, and consumer behavior studies conducted in the US and abroad and estimates them to be a function of the GDP per capita of the country in which the study was conducted; a dummy variable indicating the methodology used (contingent valuation versus hedonic wage approach); and four dummy variables describing the type of risk used in the original study (e.g., perceived risks versus actual risks). While it is questionable whether a simple fixed-effect for broad methodology adequately captures the relationship between VSL estimates and the methods used to estimate them, Miller nonetheless estimates a VSL similar to Desvousges et al. of $4 million for the U.S. (1998 dollars).[3] Our analysis, by contrast, considers the entire "weight-of-the-evidence" from the literature, incorporating over 200 estimates of the value of a statistical life estimated with samples of workers from the United States and abroad. In addition, a large number of independent variables are included to determine which are the important factors leading to variations in VSL estimates reported within the same study and across studies.

[3] The validity of pooling contingent valuation and wage-risk studies is also called into question by Hammitt [2000] and Hammitt and Graham [1999]who conclude that contingent valuation surveys are not likely to be reliable sources of VSL estimates due to respondents' difficulties in understanding risk changes as they have been presented in CV surveys in the past.

DATA AND META ANALYSIS RESULTS

The Data

Forty-seven studies using compensating wage equations to estimate the value of a statistical life were reviewed. Of these, 33 provided enough information to be included in our data analysis. Summary statistics and the number of observations drawn from each study for our analysis are reported in Table 1. All VSL estimates and worker's earnings are expressed in 1998 dollars, updated using the consumer price index as reported in *The Economic Report of the President*, 1999. There is substantial variation in the data across studies. The maximum VSL estimate in our data is $30,700,000 [Olson, 1981] while the minimum is $15,863 [Marin and Psacharopoulos, 1982]. The mean VSL across all observations in our data is approximately $6 million, with approximately 50% of our observations lying between $1.5 and $8 million.

The variables we use to determine the source of variation in the VSL estimates are of three types; those which models of individual rationality would suggest influence wage/risk tradeoffs, and thus influence VSL estimates; those describing the data sources; and those describing methodological choices of the original researchers. Jones-Lee [1974] describes, in a simple expected utility framework, how rational individuals are willing to trade wealth for increases/decreases in the risk of death [see also Hammitt, 2000]. This type of model implies that for any one individual, the compensation a worker requires to take on a additional unit of risk is increasing in baseline risk and baseline income. As such, we include the mean hourly earnings of the sample used to compute the value of statistical life as well as the sample's mean risk of death as two of our regressors. Summary statistics of these variables are reported in Table 1 and 2.

The mean hourly earnings in each study varied substantially from $2.87 [Liu et al., 1997 in a study of Taiwanese workers] to $27.67 [Meng, 1989 in a study of Canadian workers]. Mean hourly earnings of studies conducted on U.S. workers varied from $10.24 [Dorsey and Walzer, 1983] to $26.17, which was the highest mean earnings for one of the subsamples reported by Herzog and Schlottman [1990]. The mean annual risk of death varied by nearly a factor of 40, ranging from 0.29 deaths per 10,000 [Liu et al., 1997] to 10.98 deaths per 10,000 [Thaler, 1976]. However, 85 percent of the mean risks reported in the studies included in our analysis were less than 2 deaths per 10,000 workers. Related to earnings, we also include in the analysis the national unemployment rate for the year in which the wage data used by a study was collected. This variable is included as one might expect that in years of high unemployment, wage premiums may be smaller.

In addition to the average risk faced by workers and their earnings, other descriptive variables included in the meta-analysis control for differences in VSL estimates across studies that arise from differences in the samples or methods used to estimate the value of a statistical life. Broadly, variables are included that control for: samples with very high risks, characteristics of the sample of workers used in a study, wage data sources, risk data sources, differences in the specification of the wage equation models across studies, and differences in how studies controlled for a job's industry or occupational category.

Four studies reported mean risks that were at least two-fold larger than the mean risk of the majority of our observation, with a range between 5 and over 10 deaths per 10,000 workers.[4] These high risks are primarily due to the specialized sample used in the studies (i.e., Low and McPheters, 1983 used police officers and Liu and Hammitt, 1999 used petro-chemical workers) or the reliance on Society of Actuaries risk data [Thaler and Rosen, 1976 and Gegax, et al., 1991]. The Society of Actuaries (SOA)

[4]Eighty-eight percent of our observations had a mean risk of 2.5 or less.

data has been criticized as overstating the risk of death since it computes the risk of premature death from all causes, not just those that occur on the job, and it is limited to more risky job classifications [Viscusi, 1992]. Each of these highest-risk studies also reported value of statistical life estimates that are at, or below, the mean for all the studies. Thus, the analysis here includes a dummy variable (HIGHRISK), indicating whether or not a VSL estimate was based on a sample of workers whose mean risk is greater than 5 deaths per 10,000 workers.

Variables are constructed to control for variation in sample characteristics that arise through either the data sources or the choice of what types of workers are included in the analysis (Table 2, "Sample Variables"). Three broad categorical variables are created to describe the source of the data on worker's wages and job-characteristics, and four dummy variables are created to control for the source of the risk data. In addition, a dummy variable is created indicating whether or not the measures of job risk used in a study included a worker's self-assessment of his/her job risk. For instance, Gegax, et al. [1991] presented survey respondents with a risk ladder (whose risk levels were derived from SOA data) and ask workers to identify the risk they face on their job. Similarly, Moore and Viscusi [1988b] interact BLS risk measures with a dummy variable indicating whether or not a survey respondent considered his/her job to be risky, thus assigning a zero risk of death to those who did not consider their jobs to be risky.

To control for the specialized samples, variables are created that indicate if a sample of workers was 100 percent unionized, 100 percent white collar, or 100 percent blue collar. Other variables which we attempted to include, but could not due to missing observations, were the mean age of the sample of workers and the racial and gender composition of the sample. Omitting age, race, and gender could bias our coefficient estimates if there are important VSL variations across studies resulting from differences in the sample compositions with respect to these variables. Fortunately, the variation in these measures across studies seemed to be somewhat limited. Based on the information from studies which did report this information, 95 percent of the observations used samples of workers that were at least 75 percent white (or the majority race of the country); 80 percent of the observations had 75 percent or more male workers; and the mean age of the sample of workers varied from 32 to 44 across studies. This limited variation, combined with the controls for these factors within the original studies, may suggest the remaining bias is limited. However, we cannot measure or test for the bias.

It is possible to have several estimates of the value of a statistical life from one study even if the mean risk, mean earnings, and sample used to compute the VSL do not vary in the study. In these cases, VSL variation arises from different estimating equations used by the authors. To control for these effects in the meta-analysis, we include eight variables reflecting the specification of the compensating wage equations underlying each VSL estimate. These are described in Table 2 under "Specification Variables." For instance, for the same sample of workers, some authors may have reported both a linear and a semi-log specification for their compensating wage equations. Although identical in other respects, these two equations would result in different VSL estimates. We therefore include a variable in our analysis describing the original wage model specification to control for this effect within each study.

Lastly, we include in our analysis three variables designed to address the relationship between industry wage differentials, risk measures, and the estimated value of statistical life (Table 2, "Industry/Occupation Variables"). In compensating wage equations, it is important to control for the broad classification of a worker's occupation and industry in explaining variation in wages across workers [see, for example, Ehrenberg and Schumann, 1982]. The degree to which studies in our analysis included variables that control for broad industry and occupation classifications varied. Nine studies did not control for either effect in their wage equations (accounting for 23 percent of our observations), while fourteen studies controlled for both effects (accounting for 44 percent of our observations) and eleven studies controlled for occupational characteristics, but did not control for the industry in which the person worked (26 percent of our observations). We include in our analysis two variables which indicate whether or not the original authors controlled for at least one occupation or job characteristic in their wage equations (OCCDUM and CHARDUM, respectively). We also include variables describing whether or not the original authors included industry-specific dummy variables in their wage equations. We create two variables to control for this important effect; one is a continuous count for the number of industries controlled for by the original authors (INDUSTRIES), and one is a summary dummy variable indicating whether or not the original authors controlled for at least four industries in their wage equations (INDDUM).[5] The robustness of our results is tested with respect to which of these two variables we include in our anslysis.

Results

Table 3 reports four models used to estimate the sources of variation in the value of statistical life estimates. For each model, the natural log of the VSL is the dependent variable. With this specification, we are assuming that additional increments in our explanatory variables affect the VSL proportionally. Since studies vary in the number of estimates they report, we apply weighted least squares rather than

[5]There is a "natural break" in our data in the number of industries authors controlled for in their wage equations. Generally, authors included either zero, one, or two controls for industry, or were very detailed and controlled for 7, 8, or more industry classifications.

OLS, using a weight on each observation equal to the inverse of the number of estimates from that study which we are able to include in our analysis. Thus, in our regression, each study, rather than each observation, has equal weight in determining the regression coefficients. An appendix, available from the authors (www.gsu.edu/~ecolot/research.html), explores variation in the models, estimation methods, and other assumptions used in the meta analysis. The results of these robustness tests support the findings reported here.

An important issue that had to be taken into consideration is the relationship between studies using the same database of workers. Different authors conducting studies on U.S. national samples (91 of the 203 observations) used a few key data sources repeatedly. For instance, the Panel Study of Income Dynamics (PSID), which follows individuals over time, was used by four authors in seven studies (data years were 1974, 1976, 1981, and 1982). While identical samples did not appear to be used across studies due to the criteria each author used for inclusion or exclusion of workers from their data base, some individuals would have appeared in the samples of multiple studies. Thus, even though authors use different sub-samples, different risk measures, and different model specifications for their wage equations,[6] there may be some residual correlation between these studies resulting in inefficient parameter estimates.

To address this issue, we compute robust standard errors which allow for correlation (clustering) among observations across studies arising from data sources that were of the same year (or arising from

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[6]For instance, Leigh and Folsom [1984] and Moore and Viscusi [1988b] both use the Quality of Employment Survey from 1976. However, Moore and Viscusi use only blue collar workers (resulting in a mean wage of $15.73) and Leigh and Folsom use a mix of blue and white collar workers (mean wage $20.79). Leigh and Folsom included the age of worker, marital status, and two-digit occupation dummies, while Moore and Viscusi included race, and the expected life years lost, expected annuity, and estimated discount rates of workers. Moore and Viscusi weighted their regression (dependent variable ln(wage)), while Leigh and Folsom did not, but reported both linear and semi-log wage models.

the same panel of workers if a group was followed over time, as is the case with the PSID).[7] In all other cases, we allowed observations to be correlated within a study, but assumed observations were independent across studies. Models were also estimated which included fixed effects for authors using a common sample of workers. The results of these models for the variables of interest were unchanged and are reported in the appendix available from the authors.

The four models vary by sample composition and the variables used to control for the original treatment of industry variables by the authors. Models 1, 2, and 3 only vary by sample composition. Model 1 is is the most inclusive, including the full sample, while the second model restricts the data set by excluding VSL observations based on samples with risks greater than 5 deaths per 10,000 workers (i.e., samples for which HIGHRISK=1), or based on SOA risk data, which has been severely criticized for not reflecting actual job-risks. Model 3 further restricts the data to just VSL estimates that were computed using US national data sources for worker characteristics. Variables are dropped in models 2 and 3 if the sample restriction eliminates their variation. In addition, UNION and URBAN are dropped because the sample restriction results in a high degree of collinearity between UNION, URBAN and other specification variables such as REGDUM. In addition, UNION becomes a dummy variable specific to Herzog and Schlottmann [1990] which is the only study using a U.S. national sample of workers which did not control for unionization of the worker, and is not significant when included in the model. Model 4 is the same as model 3, but includes the continuous variable representing how industry-categories were controlled for in

[7]If the data used in several studies were from the same source (such as BLS), but collected in different years, we assume independence of the error terms across these studies. We expect that any effects of the sampling methods used by the various agencies in collecting their wage data across years would be captured directly by our inclusion of dummy variables reflecting the data source.

the original regressions (INDUSTRIES), instead of the summary dummy variable (INDDUM).

All models indicate a positive and significant relationship between the mean risk faced by a sample of workers and the value of a statistical life. This relationship is concave, however. Model 1 indicates that the value of statistical life estimates begin to decline when the mean risk of a sample of workers becomes greater than approximately 1.2 deaths per 10,000.[8] Fifty-percent of the full sample has mean risks that are less than 1.2 deaths per 10,000 workers. These results may indicate that selection effects among workers with heterogenous risk-preferences may dominate over some range of risks. In other words, those with lower risk aversion may be self-selecting into higher risk jobs and require less compensation, all else equal. If this selection effect is dominant in the market, we would expect to see the risk premia begin to decline at higher levels of risk when making comparisons across samples of workers with different baseline risks (i.e., across studies).[9] Models 2, 3 and 4 also indicate a similar relationship. However, the relationship between VSL and risk is positive over a larger range of the data when excluding the observations arising from high-risk and SOA samples. For model 2, the value of statistical life estimates begin to decline when the mean risk of a sample of workers becomes greater than 1.67 deaths per 10,000. Approximately 72 percent of the sample used in model 2 have mean risks less than 1.67×10^{-4}. For models 3 and 4, the VSL estimates begin to decline at 1.46×10^{-4} and 1.53×10^{-4}, respectively, and approximately 95 percent of the

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[8]The partial derivative $\partial VSL/\partial MEANRISK$ is evaluated with UNION100 and HIGHRISK set equal to zero (i.e., the partial derivative is computed for non-specialized samples of workers).

[9]This hypothesis is also supported by the surprisingly large, negative coefficients for HIGHRISK in Model 1. This variable reflects six observations in the data set that are from Thaler and Rosen [1976], six from Gegax, et al. [1991], and two each from Marin and Psacharopoulos [1982] (data on U.K. workers) and Lui and Hammitt [2000] (data on Taiwanese workers). Each of these studies report the highest mean risks in our data, yet report relatively small VSL estimates. Twelve of these 16 VSL observations rely on risk data from the Society of Actuaries that is suggested to be biased upwards as it reports the risk of death from all sources, not just on the job risks. An upwardly biased risk measure in a compensating wage equation will result in a downwardly biased VSL estimate.

U.S. national sample used in these two models have mean risks that are less than $1.46x10^{-4}$.

Consistent with our expectations, the coefficient for earnings is positive; however, this variable is only a significant predictor of ln(VSL) in the models that include both U.S. and non-U.S. samples of workers. The elasticity of the VSL estimates with respect to mean earnings of the workers is 0.49 and 0.46 for models 1 and 2, respectively, when evaluated at the approximate mean wages for the two samples of $13.25. The elasticity estimates using models 3 and 4 are 0.37 and 0.46 when evaluated at the sample mean wages of $15.94, although these are based on imprecise coefficient estimates. These measures are about half the magnitude of those reported by Miller, 2000 which varied between 0.85 and 1.0. We might expect Miller's estimates to be different than ours if the important determinants of the VSL that he omitted in his study, such as the mean risks faced by the workers in each study, are correlated with income. Also, Miller uses each country's per capita GDP as a measure of income and we use hourly earnings, a crude proxy for income as it does not incorporate information on the number of hours worked per year or non-wage sources of income. More importantly, approximately 78% of our observations arose from models using a semi-log specification for their wage equation. In the case of semi-log wage models, an artificial relationship between wages and the VSL arises as the VSL is computed by: $VSL = bw_{:}X$, where b is the estimated impact of risk on wages, $w_{:}$ is the mean wage of the sample of workers, and X is an adjustment factor as described earlier. For these reasons, our elasticity measure should be interpreted with caution.

Not surprisingly, the data sources used by the original authors significantly impact their VSL estimates. Estimates arising from studies using U.S. national samples of workers and those using non-US samples resulted in higher VSL estimates than those arising from specialized US samples such as those used by Butler [1983], Gegax, et al. [1991], Low and McPheters [1983], Dillingham [1979], and Brown

[1980]. Results also indicate that use of National Institute for Occupational Safety and Health (NIOSH) risk data results in significantly larger estimates of the VSL as compared to BLS risk data (the category left out of the model).[10] Dillingham constructed a unique data set on risks and when used, it resulted in lower estimates of the VSL. The use of SOA did not significantly affect VSL estimates, once controlling for the fact that this data is associated with very high mean risks (i.e., including HIGHRISK as well). Lastly, our first two models seem to indicate that risk data which incorporates a worker's self-assessed risk of death did not have a significant affect on the VSL estimates. While this variable is significant in models 3 and 4, it should be interpreted with caution as SELF REPORT is equivalent to a dummy variable for the Moore and Viscusi [1988b] study in these models because of the sample restriction.

Restricting the sample of workers to 100 percent unionized workers resulted in larger VSL estimates, however, this result is only significant in models based on U.S. workers only. Value of statistical life estimates arising from samples of white collar workers were significantly higher than estimates arising from samples of mixed-samples of workers. Regressions were also estimated in which BLUECOL was the dummy variable left out of the regression, and WHITECOL was also significant and positive in these cases. Interestingly, VSL estimates arising from samples of all blue collar workers were also significantly larger than those arising from a mix of blue and white collar workers.

We also included eight dummy variables in our model indicating various specification choices of the original researchers. The sign and significance of these variables depended on the sample composition.

[10]Moore and Viscusi [1988a] discuss two differences between the BLS and NIOSH data sources: measurement error and scale-factor bias, that lead to opposite expectations as to which data set will result in higher VSL estimates. Moore and Viscusi suggest that their empirical results demonstrate that the former factor dominates, leading to substantially higher VSL estimates when using the NIOSH risk data.

In general, these variables were not significant predictors of the value of a statistical life in the models based on only U.S. workers. The effects of controlling for occupations in a wage regression were not significant in any model. Wage equations that included at least one job characteristic dummy variable (such as whether a job is supervisory or not) did result in a significantly larger estimate of the VSL in Models 2 and 3.

Other than mean risk, the inclusion of industry dummy variables in the wage equations is the effect of greatest interest as it relates directly to Leigh's [1995] hypothesis that risk/wage tradeoffs found in this literature are spurious relationships. Our results indicate that studies which control for five or more industry classifications in their wage regressions did result in significantly lower estimates of the VSL, although this effect is not significant in the model containing VSL observations arising from high-risk samples or SOA risk data. This effect is robust to the treatment of the variable we use to describe this effect. In model 4, the coefficient estimate for INDUSTRIES indicates that adding an additional industry dummy variable in a wage equation reduces the estimated VSL by 12 percent.[11] The magnitude of this effect on the estimated value of a statistical life is substantial, and is discussed in the next section.

REVISED ESTIMATES OF THE VALUE OF A STATISTICAL LIFE

"Best-Practice" Estimates of the VSL

The models estimated in the previous section may be used to compute estimates of the value of a

[11]Models were estimated that used the same samples as model 1 and 2, but which included INDUSTRIES instead of INDDUM. They are not reported here as their results support models 1 and 2. The variable INDUSTRIES was not significant in the model based on the full sample (like model 1), but was significant and negative in the model dropping VSL estimates arising from high-risk and SOA data sources (like model 2).

statistical life in several ways. One could simply compute the mean, $\Sigma_i \mathrm{VSLHAT}_i/N$, where i represents an observation, N is the number of observations in our data set, and $\mathrm{VSLHAT}_i = \exp(\Sigma_j b_j \cdot X_{ji})$, where X_{ji} is the value of the j^{th} covariate for the i^{th} observation, and b_j is the estimated coefficient for the j^{th} covariate in our model.[12] Such an approach, however, would create VSL estimates whose values vary because of differences in specifications, data, and importantly, whether or not "best-practice" methods were employed. This approach implies a lack of comparability across estimates and continues to make it difficult to infer the appropriate range for the value of a statistical life from this literature.

To avoid these problems, we apply a more structured approach. Rather than simply computing a VSLHAT_i using the values for each observation as contained in the "raw data," we adjust the covariate matrix for all observations to reflect "best-practice" assumptions. For instance, model 2 and 3 indicate that inclusion of variables describing job characteristics in the compensating wage equation yields higher estimates of the value of a statistical life. On theoretical grounds one can assert that such a term should be included in the specification as a preferred practice since job characteristics influence wages. However, a number of studies did not control for job characteristics in their compensating wage equations. We impose a "best-practice" specification on these observations by predicting the VSL as if the studies had included at least one dummy variable describing job characteristics. Specifically, for the j^{th} covariate representing whether or not the wage equations included a job characteristic dummy variable, we set X_{ji} = 1 for all i observations and calculate VSLHAT_i as above. Thus, the fitted VSL calculated for each observation incorporates an adjustment to the VSL estimate if a study did not consider the influence non-

[12]The method of predicting lnVSL and exponentiating the result will underestimate the expected value of VSL. For the model ln(y) = xb + u, it can be shown that $E(y|x) = \alpha^* \exp(xb^*)$, where xb* is the prediction of ln(y), and α^* is a consistent estimate of E(exp(u)|x). See Manning and Mullahy [2001] and Wooldridge [2000] for a discussion.

risk job characteristics may have on wages.

Adjustments to the X_{ji} matrix reflecting best-practice methods were to set RISKSQ, MORBIDITY, UNION, WORKCOMP, URBAN, REGDUM, OCCDUM, and CHARDUM equal to 1 for all observations. This adjusts the predicted VSL from those studies in our database that did not include these effects by an amount suggested by our empirical models. In addition, we make other adjustments to the X_{ji} matrix to impose uniformity across studies in cases where we have no *a-priori* reason to prefer one specification over another. These adjustments include restricting AFTERTAX, US NATIONAL DATA, and LOGDEP equal to 1 for all observations; and to restrict SELFREPORT, NON-US NATIONAL, and DILLINGHAM equal to zero for all observations. We also restrict UNION100, WHITECOL, and BLUECOL to be equal to 0 for all observations in the data set so the results may be as consistent as possible with the general population.

After making these adjustments to the X_{ji} matrix, we compute an adjusted, predicted VSL for each observation using Models 3 and 4 in Table 3. Note that some adjustments discussed in the previous paragraph do not apply as the variables are not contained in model 3 or 4. Each predicted value is also adjusted to account for the bias introduced by Jensen's inequality (see footnote 12). These models are chosen since they contain studies that are most comparable in terms of their sample compositions and data sources. The mean of these predicted values are reported in Table 4. Because the models in Table 3 indicate a non-linear relationship between risk and the VSL, predicted VSL estimates are reported based on five different baseline risks, ranging from 0.25 to 2 deaths per 10,000 workers (98 percent of the observations in models 3 and 4 had a mean risk of less than 2 deaths per 10,000). Also, since BLS and NIOSH are two very commonly used data sources, and the choice of which data source to use leads to significantly different estimates of the VSL, we report two sets of results; one assuming all studies used BLS risk data, and one assuming that all studies used NIOSH risk data. Although we report estimates arising from both BLS and NIOSH data, it should be noted that the risk data from NIOSH are aggregated to the 1-digit industry SIC code (although they vary by state) and as such, are viewed with some skepticism [see also Fisher et al., 1989 and Dorman and Hagstrom, 1988].[13]

Lastly, to incorporate the Leigh [1995] critique of this literature, the results are distinguished according to the degree the original studies controlled for inter-industry differences in the compensating wage equations. One set of results are reported that assumes either INDDUM = 0 or INDDUM = 1 for all observations. For comparison purposes, we also report another set of results based on model 4 in which either INDUSTRIES = 0 or INDUSTRIES = 7 is assumed for all observations. There are eleven 1-digit SIC-code industries (the broadest category of industry classification). We thus consider controlling for 7 broad industry classifications as a reasonable approach in any attempt to capture inter-industry wage differentials (see Leigh [1995] for a more detailed discussion of this issue).

Overall, the results in Table 4 indicate the nonlinear nature of the relationship between baseline risks and the estimated value of a statistical life. The estimated value of statistical life is approximately 75 percent to 110 percent higher at mean risks of $1.5\text{x}10^{-4}$ as compared to mean risks of $0.25\text{x}10^{-4}$. For risks greater than approximately $1.5\text{x}10^{-4}$, our models indicate that VSL estimates begin to decline, with an initial

[13] At the time when NIOSH was actively collecting this data, it was thought to be a more complete census of occupational fatalities than the BLS had been collecting. However, this turns out to not be the case. The NIOSH data collection was based on a review of death certificates, while BLS was based on a survey of employers. Because there were no standard reporting mechanisms for death certificates, it was not clear that all deaths that were job-related were recorded as such. However, the main criticism of the NIOSH data is that recording risk rates at the 1-digit industry SIC code is not likely to accurately reflect the risk rates of all the industries under each SIC code. For example, Bakery Products (SIC code 205) and Petroleum Refining (SIC code 291) are both in the major group “manufacturing industries,” although it is likely the risk rates of general laborers are very different in these two industries.

decrease of approximately 10 percent between risks of $2.0x10^{-4}$ and $1.5x10^{-4}$. The results also indicate that VSL estimates arising from NIOSH data are approximately 75 percent larger than those arising from BLS data; similar to the "doubling" referred to by Moore and Viscusi [1988a]. In comparing our VSL estimates with previous assessments of this literature, we will focus on our values for risks of $1x10^{-4}$ which is both typical of the risks considered in studies based on U.S. samples and is the average risk in the workplace [Viscusi, 1993].[14]

If we assume BLS = 1 and industry wage differentials are <u>not</u> controlled for adequately (i.e., INDDUM=0 or INDUSTRIES=0), our predicted values for the VSL are approximately $3 to $6.5 million. At a risk level of $1x10^{-4}$, our VSL estimate of approximately $6 million is in the middle of the range suggested by Viscusi [1993] and Fisher, et al. [1989], and is the mean value suggested by Neumann and Unsworth [1993], and applied by the U.S. EPA in their retrospective and prospective analysis of the Clean Air Act (U.S. EPA, 1995 and 1997). The mean values suggested by the two quantitative studies most similar to ours, Desvousges et al. [1995] and Miller [2000], are also in this range. However, if we instead assume NIOSH risk data is appropriate, the predicted VSL increases to $6 to $11 million and is commensurate with the upper range of Fisher, et al.'s [1989] assessment of this literature, although Fisher, et al. "place more confidence in the lower end of the range" than they do in their upper range (p. 98).

If instead, we assume BLS = 1 and moderate industry controls are used in the compensating wage equations (INDDUM=1 or INDUSTRIES=7), the VSL estimates are decreased by 60 to 65 percent to a range of $1.3 to $2.5 million. This range is below previous qualitative assessments of this literature

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[14]The median and mean for MEANRISK in our data for U.S. national studies are $1.08x10^{-4}$ and $1.1x10^{-4}$, respectively, while the 25th and 75th percentiles are $0.79x10^{-4}$ and $1.31x10^{-4}$, respectively.

[Fisher et al., 1989, Viscusi, 1993], and are half the size of what Desvousges, et al. [1998] and Miller [2000] suggest as appropriate values ($4 million). This range is also about one third the mean estimate used by the U.S. EPA [1997 and 1999], in their analysis of the Clean Air Act, and 30 to 50 percent lower than what are used by several federal agencies in their benefit/cost regulatory analyses, e.g., the Food and Drug Administration and the U.S. Department of Transportation.[15] If we assume NIOSH=1 with industry controls in place, the value of statistical life estimates again increase approximately 75 percent, and generally fall in the $2 to $4 million range. This is commensurate with the suggested range by Miller [1990], and is in the lowest range, or below, other previous assessments.[16]

CONCLUDING COMMENTS

The studies estimating the value of a statistical life with labor-market data are an example of a literature that has evolved using relatively homogeneous methods, yet has generated results that in some instances vary greatly across studies. As a result, it can be difficult to ascertain the best VSL value, or range of values, for any particular policy application. We build on previous reviews of this literature by using a quantitative, meta-analysis approach. Plausible VSL estimates are developed which use the weight

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[15]The Consumer Product Safety Commission uses $5 million (unindexed for inflation), and the Food and Drug Administration uses $5 million in considering mammography policy [Office and Management and Budget (OMB), 1998]. The Federal Railroad Administration uses a value of $2.9 million in considering roadway worker protection policies [OMB, 1998]; and the U.S. Department of Transportation requires $2.9 million for use in preparing economic evaluations of their regulations [U.S. Department of Transportation General Counsel, 1995], all in 1998 dollars. Also, in preparing regulatory analyses for proposed actions imposing requirements on licensees, the Nuclear Regulatory Commission based its valuation of radiation exposure avoidance based a value of a statistical life estimate of $3 million [U.S. Nuclear Regulatory Commission, 1995a and 1995b, presumedly in 1995 dollars].

[16]An anonymous reviewer notes that our computations, which are based on individual decisions over risk, should be adjusted by approximately $200,000 to reflect "societal" WTP measures, which should include the present value of income taxes foregone and worker's compensation payments per occupational fatality (see Arthur, 1981 and Miller et al., 1989 for a discussion of this issue).

of the evidence from the entire literature, not just a few preferred studies or preferred estimates. This approach also has an advantage over qualitative reviews of the literature since VSL estimates may be adjusted in a systematic manner to reflect "best practice" ideals and to impose uniformity. An adjustment which we find to be important involves consideration of how authors controlled for inter-industry wage differentials in their original compensating wage equations.

Our research suggests the value of a statistical life that can reasonably be inferred from past labor market studies, for populations facing risks approximately equal to the current average risk of accidental death in the workplace of $0.5x10^{-4}$ [Marsh and Layne, 2001], is approximately $2 million in 1998 dollars, a 50 percent or greater decrease over previous assessments of this literature which have tended toward values of $4 million or more [Viscusi, 1993, Neuman and Unsworth, 1993, Desvousges, et al., 1995, and Miller, 2000].[17] Indeed, our analysis suggests that value of statistical life estimates over $2 to $3 million are likely to reflect the lack of attention this literature has given to the control of unobserved determinants of wages at the industry level [Leigh, 1995]. These results are particularly important for the conduct of benefit/cost analyses involving policies that have mortality reduction as a primary goal.

[17]We further refine this VSL estimate by computing a value of statistical life for an 'average' U.S. worker based on workplace fatality data obtained from NIOSH [Marsh and Layne, 2001] for the years 1983 to 1995. Workplace fatalities for 45 occupations were used to compute 10 occupational-risk deciles. According to the NIOSH data, occupations in the lowest risk decile had an average fatality risk of $0.04x10^{-4}$ and occupations in the highest risk decile had an average fatality risk of $2x10^{-4}$. A VSL estimate is computed for each risk decile in the same manner as used to compute the estimates reported in Table 4. We did not have the mean wage corresponding with the workers in each risk decile, and so all deciles are evaluated at the mean wage of our sample. Using model 3 in Table 3 (with NIOSH and INDDUM set equal to one), we estimate the value of statistical life for the average worker to be $2,579,000, which is commensurate with the value reported in Table 4 for risks of $0.5x10^{-4}$. If instead our predictions are adjusted to reflect the upward bias in VSL estimates arising from NIOSH risk data (i.e., set BLS=1 and NIOSH=0), the estimated value of a statistical life for an average U.S. worker is $1,493,000.

Table 1. Meta-analysis Studies and Summary Characteristics[a]

Study	N[b]	Mean VSL (1998 $ in millions)	VSL Range (1998 $ in millions)	Mean Annual Risk (1x 10^{-4})	Mean Hourly Earnings
Berger and Gabriel, 1991	4	8.79	6.6 - 10.2	1.09	21.89
Brown, 1980	2	2.03	2.0 - 2.1	2.25	23.20
Butler, 1983	3	1.12	0.87 - 1.3	0.47	10.75
Cousineau et al., 1992	3	6.81	6.1 - 7.2	0.76	14.04
Dillingham, 1979	10	0.92	0.05 - 1.9	1.53	17.90
Dillingham, 1985	10	3.43	0.14 - 8.6	1.33	13.52
Dillingham and Smith, 1983	11	4.32	0.48 - 7.9	0.82	14.06
Dorsey and Walzer, 1983	2	11.59	11.5 - 11.7	2.25	10.24
Garen, 1988	2	11.15	6.8 - 15.5	1.08	15.08
Gegax et al., 1991	8	2.07	0.46 - 4.1	6.87	19.59
Herzog and Schlottmann, 1990	4	9.07	7.8 - 10.2	0.97	22.89
Kneiser and Leeth, 1991	2	0.24	0.05 - 0.44	4.36	16.02
Leigh, 1991	2	10.20	6.8 - 13.6	1.34	15.60
Leigh, 1995	7	7.18	1.0 - 15.8	1.14	15.50
Leigh and Folsom, 1984	8	10.44	7.7 - 12.7	1.42	20.79
Liu and Hammitt, 1999	2	1.00	0.67 - 1.33	5.13	8.75
Liu, Hammitt, and Liu, 1997	10	0.54	0.17 - 0.85	0.29	2.87
Low and McPheters, 1983	1	1.31	—	3.27	20.93
Marin and Psacharopoulos, 1982	21	6.97	0.02 - 21.5	0.93	5.03
Martinello and Meng, 1992	8	4.06	2.1 - 6.4	2.50	3.42
Meng, 1989	5	4.05	3.7 - 4.4	1.90	27.67
Meng and Smith, 1990	5	6.88	1.1 - 9.7	1.20	18.70
Moore and Viscusi, 1988a	8	5.92	2.9 - 9.8	0.66	12.57
Moore and Viscusi, 1988b	4	2.56[c]	2.3 - 3.2	0.50	15.73
Moore and Viscusi, 1990	1	16.51	—	0.78	19.09

Study	N[b]	Mean VSL (1998 $ in millions)	VSL Range (1998 $ in millions)	Mean Annual Risk (1x 10^{-4})	Mean Hourly Earnings
Olson, 1981	10	16.55	5.2 - 30.7	1.01	17.11
Smith, 1974	2	13.11	8.9 - 17.3	1.25	13.99
Smith, 1976	2	5.23	5.1 - 5.4	1.12	14.51
Thaler and Rosen, 1976	6	0.76	0.34 - 1.3	10.98	18.50
Viscusi, 1978	6	6.69	5.3 - 7.9	1.18	15.12
Viscusi, 1980	4	3.15	0.62 - 5.5	1.18	15.12
Viscusi, 1981	2	9.20	8.0 - 10.4	1.04	15.24
Vodden, et al., 1993	28	4.78	1.9 - 11.8	1.76	9.96

[a] All dollar amounts are in 1998 dollars. The mean VSL we report is the mean over the multiple observations recorded in our database for any particular study. The same is true for the mean risk and mean earnings. As a result, the variable means we report here differ from those reported in past reviews [e.g., Viscusi, 1993] because we are taking a mean over different numbers of observations as compared to these past reviews and/or past reviewers reported means associated with a specific sub-sample of workers in the original study.
[b] Number of observations drawn from each study for use in the meta analysis.
[c] The reported VSL estimates in this study were adjusted as suggested by Miller [1990] because the authors originally report a value based on an undiscounted number of life years remaining.

Table 2. Summary Statistics for the Meta-analysis Dataset

Variable Name	Definition	Summary Statistic[a]
VSL, Risk, and Earnings		
VSL ($ million)	Value of a statistical life (1998 dollars).	5.59 (0.016-30.7)
MEANRISK	Mean annual average risk of death (in deaths per 10,000 workers).	1.81 (0.04-10.98)
HIGHRISK	=1 if MEANRISK is greater than 5 x 10^{-4}.	16/203
EARNINGS	Mean hourly earnings (1998 dollars).[b]	13.44 (2.57-27.67)
UNEMP	National unemployment rate in the year in which the wage data were collected.[c]	6.1 (2.1-11.9)
Sample variables		
US NATIONAL DATA	=1 if wage data is for a national sample of U.S. workers collected by either the University of Michigan or the Census Bureau.[d]	97/203
US SPECIALIZED	=1 if wage data is for U.S. workers, but is not a national sample of workers (category not included in the models).[e]	24/203
NON-US	=1 if wage data is for a non-U.S. sample of workers. This variable also indicates risk data from a foreign source.	82/203
DILLINGHAM	=1 if Dillingham's (1979) constructed risk data for workers in New York is source of risk data.	12/203
BLS	=1 if Bureau of Labor Statistics is source of risk data (category not included in the models).	68/203
NIOSH	=1 if National Institute for Occupational Safety and Health is source of risk data.	9/203
SOA	=1 if Society of Actuaries is source of risk data.	8/203
SELF REPORT	=1 if risk variable included a worker's self-assessment of his/her job risk.	12/203
UNION100	= 1 if the sample was comprised of only unionized workers.	16/203
WHITECOL	=1 if sample is 100 percent white collar workers.	6/203
BLUECOL	=1 if sample is 100 percent blue collar workers.	102/203
MIX	=1 if sample is a mix of white and blue collar workers (category not included in models).	95/203
Specification variables		

Variable Name	Definition	Summary Statistic[a]
RISKSQ	=1 if a risk-squared term is included in the wage equation.	46/203
MORBIDITY	=1 if controlled for other risks (such as risk of injury) in the wage equation.	80/203
LOGDEP	=1 if semi-log functional form (log of the dependent variable) is used for the wage equation, = 0 if linear.	159/203
UNION	=1 if controlled for union status of a worker in the wage equation.	176/203
REGDUM	=1 if controlled for region of worker in the wage equation.	144/203
URBAN	=1 if controlled for urban versus rural in the wage equation.	77/203
WORKCOMP	=1 if controlled for workman's compensation in the wage equation.	26/203
AFTERTAX	=1 if after-tax income is used in the wage equation.	38/203
<u>Industry/Occupation Variables</u>		
INDUSTRIES	= the number of industry categories controlled for in the regression with dummy variables or through sample selection.[f]	3.5 (0 - 30)
INDDUM	=1 if more than four industry dummy variables were included in the wage equation.	71/203
OCCDUM	=1 if at least one occupational dummy variable was included in the wage equation.	142/203
CHARDUM	=1 if at least one dummy variable describing a characteristic of the job was included in the wage equation.	118/203

[a]For VSL, MEANRISK, EARNINGS, UNEMP, and INDUSTRIES the summary statistic reported is the mean (with the range in parentheses). For all other variables, the summary statistic is the number of observations for which the variable is equal to 1 (divided by the number of observations in the data set).
[b] If mean earnings were reported as annual wages, they were divided by 2,000 if the sample was comprised of full-time workers only, by 1900 if the sample was comprised of those working 35 or more hours a week, by 1800 if comprised of those working 30 or more hours a week, and by 1500 if comprised of both full-time and part-time workers. If mean earnings were reported as weekly wages, the weekly wage was divided by 40, 38, 35, or 30 depending on whether the sample included full-time workers only, or those working more than 35, 30, or 20 hours per week, respectively. Currencies were converted from Canadian dollars or U.K. pounds to U.S. dollars using exchange rates for the year in which the original study reported its results. Canadian and U.K exchange rates are from the U.S. Federal Reserve Board: www.stls.frb.org/fred/data/exchange/excaus.html or /exusuk.html, respectively. The U.S. dollar equivalent results were then inflated to 1998 dollars in the same manner as other estimates. Studies based on samples from other countries reported results in U.S. dollars.
[c] Unemployment rates for the US were obtained from the <u>Economic Report of the President</u>, 1998. Unemployment rates for the U.K. were obtained from the Organization for Economic Co-operation and Development, <u>OECD Observer</u> no82, July/August, 1976; rates for Canada were obtained from <u>Labour Force Historical Review</u>, Statistics Canada, CD ROM version, February, 1997; and rates for for Taiwan

were obtained from the Directorate-General of Budget, Accounting, and Statistics, The Republic of China (www.stat.gov.tw), at http://140.129.146.192/census~n/four/english/e44361.htm.

[d] The University of Michigan collects or has collected the "Survey of Working Conditions," "Quality of Employment Survey," and the "Panel Study of Income Dynamics." Earlier analyses were conducted controlling for the US national data sources separately (i.e., controlling for Michigan vs. Census). Results indicated no significant difference between these data sources, and so an aggregate variable was created for US national data.

[e] These sources of data were the South Carolina Department of Labor and the International City Management Association.

[f] Some authors restricted their samples to manufacturing only (a broad SIC classification). In these instances, these studies were coded as having included 7 industry categories, the most common number used to control for broad (1-digit SIC) industry classifications in labor market studies. In two instances [Lui and Hammitt, 2000 and Low and McPheters, 1983], the authors restricted their samples to very specific occupations in a particular industry (petrochemical workers and police officers). In this instance, these authors are coded as having included 15 industry classifications.

Table 3. Model Results (dependent variable is the log of the VSL reported in a study).

Variables	Model 1 all data Coef.	std. error	Model 2 less highrisk & SOA Coef.	std. error	Model 3 US national; less highrisk & SOA Coef.	std. error	Model 4 US national; less highrisk & SOA Coef.	std. error
MEANRISK	0.681**	0.295	1.628***	0.296	1.150**	0.448	1.405***	0.352
$(MEANRISK)^2$	-0.290***	0.068	-0.488***	0.063	-0.394***	0.091	-0.460***	0.077
HIGHRISK*MEANRISK	3.701***	0.776						
HIGHRISK	-16.047***	3.095						
UNION100*MEANRISK	0.041	0.070	-0.259	0.308	-0.344	0.444	-0.202	0.463
EARNINGS	0.037**	0.018	0.035**	0.015	0.023	0.030	0.029	0.033
UNEMP	-0.009	0.053	-0.068	0.044	-0.023	0.053	0.027	0.064
US NATIONAL DATA	1.843***	0.281	1.296***	0.310				
NON-US	1.363***	0.283	0.800*	0.408				
DILLINGHAM	-1.148**	0.441	-1.472***	0.428	-1.072***	0.313	-1.406***	0.385
NIOSH	0.461**	0.199	0.753***	0.160	0.546***	0.157	0.560***	0.156
SOA	0.912	0.602						
SELF REPORT	0.091	0.576	-0.136	0.390	-0.450**	0.157	-0.434**	0.149
UNION100	0.184	0.468	0.734	0.532	1.249*	0.577	1.127*	0.509
WHITECOL	1.852***	0.485	1.754**	0.776				
BLUECOL	0.613***	0.210	0.445*	0.218	-0.210	0.143	-0.035	0.178
RISKSQ	0.721***	0.202	0.718***	0.229	0.059	0.186	-0.087	0.233
MORBIDITY	0.082	0.189	-0.116	0.138	-0.104	0.240	0.017	0.214
LOGDEP	0.184**	0.079	0.325**	0.130	0.129	0.080	0.149	0.087
UNION	1.070***	0.322	1.413***	0.386				
REGDUM	-0.509*	0.253	-0.953***	0.329	0.040	0.208	-0.085	0.270
URBAN	0.394	0.259	0.385	0.281				
WORKCOMP	-0.512*	0.260	-0.231	0.169	-0.224	0.166	-0.224	0.170
AFTERTAX	0.139	0.233	0.129	0.239	-0.463*	0.260	-0.460	0.266
INDDUM	-0.181	0.179	-0.493***	0.159	-1.042***	0.198		
INDUSTRIES							-0.123***	0.026
OCCDUM	-0.204	0.304	-0.292	0.350	0.041	0.353	0.045	0.465
CHARDUM	0.005	0.211	0.340*	0.186	0.329**	0.143	0.196	0.132
CONSTANT	-1.942***	0.383	-1.718***	0.244	0.956	0.840	0.474	0.829
Number of Observations	**203**		**185**		**91**		**91**	
R^2	**0.7104**		**0.7280**		**0.7747**		**0.7711**	
sample mean-VSL [range]	5.59 [0.016 - 30.7]		6.02 [0.05 - 30.7]		7.70 [0.05 - 30.7]			
sample mean-risk [range]	1.9 [0.04 - 11]		1.27 [0.04 - 4.36]		1.11 [0.5 - 4.36]			

[a] A *, **, or *** indicate significance at the 10, 5 and 1 percent-level, respectively.

Table 4. Estimates of the Value of Statistical Life: Mean Adjusted Fitted Values [a]

Risk	Based on Model (3), Table 3		Based on Model (4), Table 3	
(x 10^{-4})	< 5 Industries	$ 5 Industries	0 Industries	7 Industries
BLS Risk Data				
P = 0.25	$3.82m (1.39)	$1.35m (0.47)	$2.99m (1.12)	$1.27m (0.40)
P = 0.5	$4.73m (1.64)	$1.67m (0.53)	$3.90m (1.44)	$1.65m (0.51)
P = 1.0	$6.25m (2.36)	$2.20m (0.73)	$5.57m (2.22)	$2.36m (0.80)
P = 1.5	$6.78m (3.02)	$2.39m (0.92)	$6.33m (2.83)	$2.68m (1.03)
P = 2.0	$6.05m (3.09)	$2.13m (0.92)	$5.72m (2.83)	$2.42m (1.03)
NIOSH Risk Data				
P = 0.25	$6.59m (2.62)	$2.32m (1.00)	$5.24m (2.08)	$2.22m (0.84)
P = 0.5	$8.16m (3.17)	$2.88m (1.20)	$6.82m (2.72)	$2.89m (1.10)
P = 1.0	$10.8m (4.57)	$3.80m (1.65)	$9.76m (4.18)	$4.13m (1.68)
P = 1.5	$11.7m (5.65)	$4.13m (1.95)	$11.1m (5.21)	$4.69m (2.07)
P = 2.0	$10.4m (5.57)	$3.68m (1.85)	$10.0m (5.06)	$4.24m (1.97)

[a] Values are expressed in millions (1998 dollars). Standard errors are in parentheses.

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APPENDIX H

The Value of a Statistical Life: A Critical Review of Market Estimates Throughout the World

by W. Kip Viscusi and Joseph E. Aldy[1]

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The Value of a Statistical Life: A Critical Review of Market Estimates Throughout the World

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Abstract

A substantial literature over the past thirty years has evaluated tradeoffs between money and fatality risks. These values in turn serve as estimates of the value of a statistical life. This article reviews more than 60 studies of mortality risk premiums from ten countries and approximately 40 studies that present estimates of injury risk premiums. This critical review examines a variety of econometric issues, the role of unionization in risk premiums, and the effects of age on the value of a statistical life. Our meta-analysis indicates an income elasticity of the value of a statistical life from about 0.5 to 0.6. The paper also presents a detailed discussion of policy applications of these value of a statistical life estimates and related issues, including risk-risk analysis.

Keywords: value of statistical life, compensating differentials, safety, risk-risk analysis

JEL Classification: I10, J17, J28

Introduction

Individuals make decisions everyday that reflect how they value health and mortality risks, such as driving an automobile, smoking a cigarette, and eating a medium-rare hamburger. Many of these choices involve market decisions, such as the purchase of a hazardous product or working on a risky job. Because increases in health risks are undesirable, there must be some other aspect of the activity that makes it attractive. Using evidence on market choices that involve implicit tradeoffs between risk and money, economists have developed estimates of the value of a statistical life (VSL). This article provides a comprehensive review and evaluation of the dozens of such studies throughout the world that have been based on market decisions.[1]

These VSL estimates in turn provide governments with a reference point for assessing the benefits of risk reduction efforts. The long history of government risk policies ranges from the draining of swamps near ancient Rome to suppress malaria to the limits on air pollution in developed countries over the past 30 years (McNeill, 1976; OECD, 2001). All such policy choices ultimately involve a balancing of additional risk reduction and incremental costs.

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The proper value of the risk reduction benefits for government policy is society's willingness to pay for the benefits. In the case of mortality risk reduction, the benefit is the value of the reduced probability of death that is experienced by the affected population, not the value of the lives that have been saved ex post. The economic literature has focused on willingness-to-pay (willingness-to-accept) measures of mortality risk since Schelling's (1968) discussion of the economics of life saving.

Most of this literature has concentrated on valuing mortality risk by estimating compensating differentials for on-the-job risk exposure in labor markets. While the early studies assessed such compensating differentials in the United States, much of the more recent work has attempted to estimate risk-money tradeoffs for other developed and some developing countries. In addition, economists have also investigated price-risk (price-safety) tradeoffs in product markets, such as for automobiles and fire alarms.

Use of the economic research on the value of mortality and injury risks in government policy evaluation has been a key benefit component of policy evaluations for a wide range of health, safety, and environmental policies. The policy use of risk valuations, however, has raised new questions about the appropriateness of these applications. How should policymakers reconcile the broad range of VSL estimates in the literature? Should the value of a statistical life vary by income? Should the VSL vary by the age distribution of the affected population? What other factors may influence the transfer of mortality risk valuation estimates from journal articles to policy evaluation in different contexts?

We begin our assessment of this literature with an overview of the hedonic wage methodology in Section 1. This approach motivates the discussion of the data and econometric issues associated with estimating a VSL. Although there continue to be controversies regarding how best to isolate statistically the risk-money tradeoffs, the methodologies used in the various studies typically follow a common strategy of estimating the locus of market equilibria regarding money-risk tradeoffs rather than isolating either market supply curves or market demand curves.

Section 2 examines the extensive literature based on estimates using U.S. labor market data, which typically show a VSL in the range of $4 million to $9 million. These values are similar to those generated by U.S. product market and housing market studies, which are reviewed in Section 3. A parallel literature reviewed in Section 4 examines the implicit value of the risk of nonfatal injuries. These nonfatal risks are of interest in their own right and as a control for hazards other than mortality risks that could influence the VSL estimates.

Researchers subsequently have extended such analyses to other countries. Section 5 indicates that notwithstanding the quite different labor market conditions throughout the world, the general order of magnitude of these foreign VSL estimates tends to be similar to that in the United States. International estimates tend to be a bit lower than in the United States, as one would expect given the positive income elasticity with respect to the value of risks to one's life.

A potentially fundamental concern with respect to use of VSL estimates in different contexts is how these values vary with income. While the income elasticity should be positive on theoretical grounds, extrapolating these values across different contexts requires an empirical estimate of this elasticity. Our meta-analyses of VSL estimates throughout the world in Section 6 imply point estimates of the income elasticity in the range of 0.5 to 0.6.

The meta-analysis also provides a characterization of the uncertainty around the measures of central tendency for the value of a statistical life, i.e., 95 percent confidence intervals for the predicted VSLs. Heterogeneity in VSL estimates based on union status (Section 7) and age (Section 8) indicate that the VSL not only varies by income but also across these important labor market dimensions. The existence of such heterogeneity provides a cautionary note for policy. While policymakers have relied on VSL estimates to an increasing degree in their benefit assessments, as Section 9 indicates, matching these values to the pertinent population at risk is often problematic, particularly for people at the extreme ends of the age distribution.

1. Estimating the value of a statistical life from labor markets

1.1. The hedonic wage methodology

More than two centuries ago, Adam Smith (1776) noted in *The Wealth of Nations* that: "The wages of labour vary with the ease or hardship, the cleanliness or dirtiness, the honourableness or dishonourableness of the employment" (p. 112). Finding empirical evidence of such compensating differentials, however, has been problematic. Because of the positive income elasticity of the demand for safety, the most attractive jobs in society tend to be the highest paid. To disentangle the wage-risk tradeoff from the other factors that affect wages, economists have relied on statistical models that control both for differences in worker productivity as well as different quality components of the job. The primary approach has been hedonic wage and hedonic price models that examine the equilibrium risk choices and either the wage levels or price levels associated with these choices.[2] Market outcomes reflect the joint influence of labor demand and labor supply, but hedonic models do not examine the underlying economic structure that gives rise to these outcomes. For concreteness, we focus on the hedonic wage case.

The firm's demand for labor decreases with the total cost of employing a worker. The cost of a worker may include the worker's wage; training; benefits such as health insurance, vacation, child care; and the costs of providing a safe working environment. Because worker costs increase with the level of safety, for any given level of profits the firm must pay workers less as the safety level rises. Figure 1 depicts two firms with wage-risk offer curves (isoprofit curves) with wage as an increasing function of risk, OC_1 for firm 1 and OC_2 for firm 2. For any given level of risk, workers prefer the wage-risk combination from the market offer curve with the highest wage level. The outer envelope of these offer curves is the market opportunities locus $w(p)$.

The worker's supply of labor is in part a function of the worker's preferences over wages and risk. The labor supply is best characterized subject to several mild restrictions on preferences. Consider a von Neumann-Morgenstern expected utility model with state-dependent utility functions.[3] Let $U(w)$ represent the utility of a healthy worker at wage w and let $V(w)$ represent the utility of an injured worker at wage w. Typically, workers' compensation after an injury is a function of the worker's wage. We assume that the relationship between workers' compensation and the wage is subsumed into the functional form of $V(w)$. Further, assume that workers prefer to be healthy than injured $[U(w) > V(w)]$ and that the marginal utility of income is positive $[U'(w) > 0,\ V'(w) > 0]$.[4]

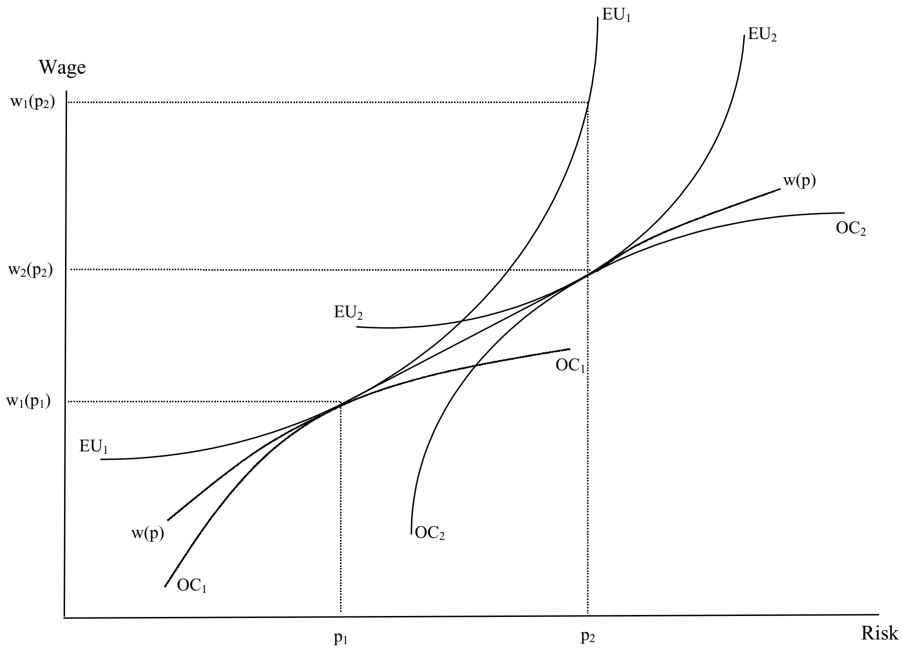

Figure 1. Market process for determining compensating differentials.

Workers choose from potential wage-risk combinations along some market opportunities locus $w(p)$ to maximize expected utility. In Figure 1, the tangency between the constant expected utility locus EU_1 and firm 1's offer curve OC_1 represents worker 1's optimal job risk choice. Likewise, worker 2 maximizes expected utility at the tangency between EU_2 and OC_2. All wage-risk combinations associated with a given worker's constant expected utility locus must satisfy

$$Z = (1 - p)U(w) + pV(w).$$

The wage-risk tradeoff along this curve is given by

$$\frac{dw}{dp} = -\frac{Z_p}{Z_w} = \frac{U(w) - V(w)}{(1 - p)U'(w) + pV'(w)} > 0,$$

so that the required wage rate is increasing in the risk level. The wage-risk tradeoff consequently equals the difference in the utility levels in the two states divided by the expected marginal utility of income.

Actual labor market decisions by workers can be depicted by the wage-risk combinations at the tangencies of the offer curves and expected utility loci at points (p_1, w_1) and (p_2, w_2). All that is observable using market data are these points of tangency. Expanding beyond our two worker example, observations of a large set of workers can show the locus of these workers' wage-risk tradeoffs, depicted by the curve $w(p)$ in Figure 1. Hedonic wage analyses trace out points on this $w(p)$ curve that workers find acceptable.

The observed labor market decisions (p_i, w_i) reflect the joint influence of supply and demand on the market equilibrium. The estimated tradeoff between wage and risk, $\partial w/\partial p$, is a local measure of the wage-risk tradeoff for marginal changes in risk. This estimated slope corresponds to both the worker's marginal willingness to accept risk and the worker's marginal willingness to pay for more safety and the firm's marginal cost of more safety as well as the firm's marginal cost reduction from an incremental increase in risk. For the worker and firm associated with a given labor market decision (p_i, w_i), $\partial w_i/\partial p_i$ reflects both the marginal supply price and the marginal demand price of risk. Econometric models that estimate a linear $w(p)$ curve are estimating an average tradeoff rate across different levels of risk.

The estimated wage-risk tradeoff curve $w(p)$ does not imply how a particular worker must be compensated for non-marginal changes in risk. Consider workers 1 and 2 in Figure 1. Worker 2 has revealed a willingness to accept risk p_2 at wage $w_2(p_2)$ along EU_2. A change in the risk exposure to worker 1 from p_1 to p_2 would require a higher wage compensation to keep worker w_1 on the expected utility locus (EU_1), implying that $w_1(p_2) > w_2(p_2)$ (or alternatively, that $\partial w_1/\partial p_2 > \partial w_2/\partial p_2$). With large changes in risk, a worker's wage-risk tradeoff will not be the same because the relevant tradeoff must be made along the worker's expected utility locus, not the estimated market wage-risk tradeoff.

1.2. Econometrics and data issues in hedonic labor market analysis

Most researchers estimate the wage-risk relationship in labor markets by specifying a wage equation along the lines of the following:

$$w_i = \alpha + H_i'\beta_1 + X_i'\beta_2 + \gamma_1 p_i + \gamma_2 q_i + \gamma_3 q_i WC_i + p_i H_i'\beta_3 + \varepsilon_i$$

where w_i is the worker i's wage rate, α is a constant term, H is a vector of personal characteristic variables for worker i, X is a vector of job characteristic variables for worker i, p_i is the fatality risk associated with worker i's job, q_i is the nonfatal injury risk associated with worker i's job, WC_i is the workers' compensation benefits payable for a job injury suffered by worker i, and ε_i is the random error reflecting unmeasured factors influencing worker i's wage rate. The terms α, β_1, β_2, β_3, γ_1, γ_2, and γ_3 represent parameters estimated through regression analysis.

The personal characteristic variables represented by H_i often include a variety of human capital measures, such as education and job experience, as well as other individual measures, such as age and union status. The job characteristic variables represented by X often include indicators for blue-collar jobs, white-collar jobs, management positions, the worker's industry, and measures of physical exertion associated with the job. These two sets of variables reflect both workers' preferences over jobs as well as firms' offer curves for labor. Some studies interact personal characteristics H_i with the fatality risk p_i to capture how the returns to risk may vary with these characteristics, such as age and union status.

1.2.1. Risk data. An ideal measure of on-the-job fatality and injury risk would reflect both the worker's perception of such risk and the firm's perception of the risk. Because the market opportunity locus reflects both workers' preferences over income and risk and firms' preferences over costs and safety, information on both sets of beliefs would be necessary to appropriately characterize the risk premium. However, very few studies have compiled workers' subjective preferences regarding risks (Viscusi, 1979; Viscusi and O'Connor, 1984; Gerking, de Haan, and Schulze, 1988; Liu and Hammitt, 1999) and there is no available research on firms' risk perceptions. If individuals' and firms' subjective risk perceptions closely reflect objective measures of fatality risk, then such objective risk data could be used instead as a proxy for unobserved subjective risk data.[5] The standard approach in the literature is to use industry-specific or occupation-specific risk measures reflecting an average of at least several years of observations for fatalities, which tend to be relatively rare events.[6]

Measures of job-related fatality and injury risk have included self-reported risks based on worker surveys and objective risk measures derived from actuarial tables, workers' compensation records, and surveys and censuses of death certificates. The choice of the measure of fatality risk can significantly influence the magnitude of the risk premium estimated through regression analysis. The nature of the risk measures also raise questions about possible errors in estimation and the need to correct the econometric specification to address them.

Several early papers on compensating differentials used the University of Michigan Survey of Working Conditions and Quality of Employment Survey data that include several qualitative measures of on-the-job risk. These measures utilize direct surveys of workers and their perceptions of their work environment. For example, Hamermesh (1978), Viscusi (1979, 1980), and Fairris (1989) estimated the hedonic wage equation with a dichotomous measure of injury risk based on a worker's perception of whether his or her job is "dangerous."[7] The survey asked workers if their job exposed them to physical dangers or unhealthy conditions. These studies estimated statistically significant coefficients on this "risk" variable in some of the specifications. Duncan and Holmlund (1983) undertook a similar analysis of compensating differentials with a "danger" variable in a study of male workers in Sweden.

Several papers on the U.S. labor market from the 1970s and early 1980s used actuarial data (Thaler and Rosen, 1975; Brown, 1980; Leigh, 1981; Arnould and Nichols, 1983). These studies all employed a job-related risk measure based on data collected by the Society of Actuaries for 1967. The Society of Actuaries data set provides fatality risk data for 37 occupations. Across these 37 occupations, the annual risk averaged approximately 1 in 1,000. This fatality risk exceeds averages from other data sets by nearly an order of magnitude. To the extent that these data reflect workers in extremely high risk jobs, the estimated wage-risk tradeoffs will suffer from a selection bias. As a result, one would expect these estimates to be lower than found in more broadly representative samples, which has in fact proven to be the case.

Another difficulty is that the Society of Actuaries data do not distinguish fatalities caused by the job but rather reflect the overall fatality rates of people within a particular job category. For example, one of the highest risk occupations based on these actuarial ratings is actors, who typically face few risks other than unfavorable reviews.

Several studies of U.S. and Canadian labor markets have used workers' compensation records to construct risk measures (Butler, 1983; Dillingham, 1985; Leigh, 1991; Martinello and Meng, 1992; Meng, 1991; Cousineau, Lacroix, and Girard, 1992; Lanoie, Pedro, and LaTour, 1995). Only three studies have used workers' compensation data to evaluate compensating differentials in U.S. labor markets, which may reflect the decentralized nature and differences in information collection associated with state (not Federal) management of U.S. workers' compensation programs.[8] In contrast, researchers in Canada can obtain workers' compensation-based risk data from Labour Canada (the labor ministry for the Federal government) and the Quebec government.

For analyses of the United States, the majority of the mortality risk studies have used data collected by the U.S. Department of Labor Bureau of Labor Statistics (BLS). About 80 percent of the U.S. nonfatal injury risk studies summarized below used BLS injury risk data. The BLS has compiled industry-specific fatality and injury risk data since the late 1960s. Through the early 1990s, BLS collected its data via a survey of industries, and reported the data at a fairly aggregated level, such as at the 2-digit and 3-digit Standard Industrial Classification (SIC) code level. The aggregation and sampling strategy have elicited some concerns about measurement error in the construction of the mortality risk variable (see Moore and Viscusi, 1988a).

Concerns about the BLS fatality risk data led the National Institute of Occupational Safety and Health (NIOSH) to collect information on fatal occupational injuries through its National Traumatic Occupational Fatalities surveillance system (NTOF) since 1980. NIOSH compiles these data from death certificates managed by U.S. vital statistics reporting units (NIOSH, 2000). These data are reported at the 1-digit SIC code level by state. Because NIOSH compiles data from a census of death certificates, it circumvents some of the concerns about sampling in the pre-1990s BLS approach. Some have raised concerns, however, about the accuracy of the reported cause of death in death certificates (Dorman and Hagstrom, 1998).

Comparing the BLS and NIOSH fatality risk data over time provides some interesting contrasts. The original NIOSH data set for the fatality census averaged over 1980–1985 has a mean fatality risk nearly 50 percent higher than a roughly comparable BLS data set averaged over 1972–1982.[9] Moreover, the BLS data had greater variation (a standard deviation 95 percent greater than its mean) than the NTOF data, although the NIOSH data also had substantial variation (standard deviation 23 percent greater than its mean) (Moore and Viscusi, 1988a).

Since 1992, the BLS has collected fatal occupational injury data through the Census of Fatal Occupational Injuries (CFOI). The BLS compiles information about each workplace fatality including worker characteristics and occupation, circumstances of the event, and possible equipment involved. The BLS draws on multiple sources such as death certificates, workers' compensation records, and other Federal and state agency reports. The BLS reports these fatality data by industry at the 4-digit SIC level. In contrast to the earlier comparisons of BLS and NIOSH data, more recent years' data on fatality risk collected through the CFOI now show that the BLS measure includes approximately 1,000 more fatalities per year than the NIOSH measure (NIOSH, 2000). Table 1 illustrates the recent national rates of job-related fatalities at the one-digit industry level for the four-year period in which both

Table 1. U.S. Occupational fatality rates by industry, 1992–1995 national averages.

	Fatality rate per 100,000 workers	
Industry	NIOSH (NTOF)	BLS (CFOI)
Agriculture, Forestry, & Fisheries	17.0	23.9
Mining	24.5	26.3
Construction	12.8	13.4
Manufacturing	3.6	3.8
Transportation & Utilities	10.4	10.6
Wholesale Trade	3.5	5.4
Retail Trade	2.8	3.6
Finance, Insurance, & Real Estate	1.1	1.5
Services	1.5	1.8

Sources: Rates constructed by authors based on Marsh and Layne (2001) and BLS (n.d.).

NIOSH and CFOI data are publicly available. In every instance the BLS measure shows a higher risk mortality rate, which in some cases, such as wholesale trade, is quite substantial.

The risk variables used in several of the non-U.S. studies were based on job-related accident and mortality data collected by foreign governments. For example, the data sets used in Shanmugam (1996/7, 1997, 2000, 2001) were from the Office of the Chief Inspector of Factories in Madras. Several of the United Kingdom studies employ data provided by the Office of Population Censuses and Surveys (Marin and Psacharopoulos, 1982; Sandy and Elliott, 1996; Arabsheibani and Marin, 2000) while others used unpublished data from the U.K. Health and Safety Executive (Siebert and Wei, 1994). In their study of the South Korean labor market, Kim and Fishback (1999) obtained their accident data from the Ministry of Labor. Few of these studies indicate whether the mortality risk data were derived from samples or censuses of job-related deaths.

While the large number of studies of labor markets around the world evaluated the compensating differential for an on-the-job death and/or on-the-job injury, very few attempted to account for the risk of occupational disease. Lott and Manning (2000) used an alternative data set to estimate the risk premium for jobs with higher cancer risk associated with occupational exposure to various chemicals (see Section 2).

1.2.2. Wages and related data. Labor market studies of the value of risks to life and health match these risk measures to data sets on characteristics of wages, workers, and employment. Some researchers survey workers directly to collect this information, such as Gegax, Gerking, and Schulze (1991) for the United States, Lanoie, Pedro, and LaTour (1995) for Canada, Shanmugam (1996/7) for India, and Liu and Hammitt (1999) for Taiwan, among others. For the United States, researchers have also used the University of Michigan's Survey of Working Conditions (SWC), the Quality of Employment Survey (QES), the Bureau of Labor Statistics' Current Population Survey (CPS), the Panel Study of Income Dynamics

(PSID), and decennial census data. Similar types of surveys undertaken in other countries have also provided the data necessary to undertake hedonic labor market analysis, such as the General Household Survey in the United Kingdom (e.g., Siebert and Wei, 1994; Arabsheibani and Marin, 2000).

The dependent variable in virtually all labor market analyses has been a measure of the hourly wage. With some data sets, researchers have had to construct the wage measure from weekly or annual labor earnings data. For some data sets, a worker's after-tax wage rate is provided, which can put wage and workers' compensation benefits in comparable terms. While many studies have included pre-tax wages as the dependent variable, this would not likely bias the results significantly so long as workers' income levels and tax rates do not differ substantially. If the regression model includes workers' compensation benefits, then both the wage and these benefits should be expressed in comparable terms (both in after-tax or both in pre-tax terms) to ensure proper evaluation of the benefits' impacts on wages.[10]

Typically, researchers match a given year's survey data on wages and worker and employment characteristics with risk data for that year, or preferably, the average over a recent set of years. Some researchers have restricted their samples to subsets of the surveyed working population. For example, it is common to limit the analysis to full-time workers, and many have focused only on male, blue-collar workers. Restricting the sample in this manner partially addresses the measurement problem with industry-level risk values common to most risk data sets by including only those workers for whom the risk data are most pertinent.

1.2.3. Wage vs. log(wage). Most researchers have estimated the wage equation using linear and semi-logarithmic specifications. Choosing a preferred functional form from these two specifications cannot be determined on theoretical grounds (see Rosen, 1974). To identify the specification with greatest explanatory power, Moore and Viscusi (1988a) employed a flexible functional form given by the Box-Cox transformation. The Box-Cox transformation modifies the dependent variable such that the estimated regression model takes the form:

$$\frac{w_i^{\lambda} - 1}{\lambda} = \alpha + H_i'\beta_1 + X_i'\beta_2 + \gamma_1 p_i + \gamma_2 q_i + \gamma_3 q_i WC_i + p_i H_i'\beta_3 + \varepsilon_i.$$

This approach presumes that a λ exists such that this model is normally distributed, homoskedastic, and linear in the regressors. Note that the case where $\lambda \to 0$ represents the semi-logarithmic functional form and the case where $\lambda \to 1$ represents the linear functional form. The flexible form under the Box-Cox transformation can test the appropriateness of these two restrictions on the form of the model. Using maximum likelihood methods, Moore and Viscusi's estimate for λ equaled approximately 0.3 for their data. While this value is more consistent with a semi-logarithmic form than a linear form, the authors reject both specifications based on a likelihood ratio test. The estimated value of a statistical life based on the Box-Cox transformed regression model, however, differed only slightly from the log(wage) specification. Shanmugam (1996/7) replicated this flexible form evaluation with his evaluation of compensating differentials in India. His maximum likelihood estimate for λ equaled approximately 0.2. While Shanmugam rejected the semi-logarithmic and linear models, he found that the semi-logarithmic functional form also generated results closer to those found with the unrestricted flexible form.[11]

1.2.4. Errors in variables problem with risk measures. Every compensating differential study employs a less than perfect measure of any particular worker's job-related fatality risk. The majority of these studies have used fatality risk measures from the BLS averaged across entire industries. Such an approach, however, suffers from measurement error. As noted above, some researchers have found that the pre-1992 BLS data sets (and NIOSH data sets to a lesser extent) suffer from incomplete reporting. The industry averages constructed by the BLS do not exactly reflect realized industry averages. Further, applying industry averages to individuals may result in errors associated with matching workers to industries due to response error in worker surveys. Mellow and Sider (1983) evaluated several surveys that asked workers and their employers to identify the workers' industry and occupation (among other questions). In their assessment of the January 1977 Current Population Survey, 84 percent of workers and their employers agreed on industry affiliation at the three-digit SIC code level while only 58 percent agreed on the three-digit occupational status. Merging a worker characteristics data set with a risk measure data set based on industry affiliation (or occupation status) can result in a mismatch of worker characteristics and industry risk. Mellow and Sider's statistical analysis of the 16 percent "mismatched" workers by industry affiliation showed that the errors in matching reduced the compensating differential for injury risk by about 50 percent in their samples.

Even with a perfect industry measure of fatality risk and appropriate matching of workers and their industry, measurement error still exists since some workers bear risk that differs from their industry's average. For example, different occupations within an industry may pose different levels of risk. This measurement error can be characterized as:

$$p_i = p_i^* + \eta_i,$$

where p_i reflects the observed industry average fatality risk, p_i^* reflects the unobserved (to the econometrician) fatality risk associated with worker i's job, and η_i reflects the deviation of that job's risk from the industry average. Random measurement error will result in a downward bias on coefficient estimates, and the least squares estimate of the coefficient on fatality risk in this example would be inconsistent:

$$\hat{\gamma}_{1,\mathrm{OLS}} \xrightarrow{p} \left(\frac{\sigma_p^2}{\sigma_p^2 + \sigma_\eta^2}\right)\gamma_1$$

where the signal-noise ratio determines the extent of the downward bias towards zero.

In addition to the downward effect on the risk coefficient, applying industry-level risk data to individual observations may also induce some correlation in the residuals among individuals within industries. Robust (White) standard errors would not appropriately correct for this correlation and result in inappropriately small standard errors. Hersch (1998) and Viscusi and Hersch (2001) employ robust standard errors correcting for within-group (within-industry) correlation.

1.2.5. Omitted variables bias and endogeneity. Failing to capture all of the determinants of a worker's wage in a hedonic wage equation may result in biased results if the unobserved variables are correlated with observed variables. Dangerous jobs are often unpleasant in other respects. Omission of non-pecuniary characteristics of a job may bias the estimated risk premium if an omitted variable is correlated with risk. For example, one may find a correlation between injury risk and physical exertion required for a job or risk and environmental factors such as noise, heat, or odor. While some studies have attempted to control for these unobservables by including industry or occupation dummy variables (see below), a model may still suffer from omitted variables bias.

Several studies have explored how omitting injury risk affects the estimation of mortality risk. Viscusi (1981) found that omitting injury risk resulted in a positive bias in the mortality risk measure for union affiliated workers. Cousineau, Lacroix, and Girard (1992) also found that omitting injury risk may cause a positive bias in the estimation of the coefficient on mortality risk. The high correlation (collinearity) between injury and mortality risks, however, can make joint estimation difficult. Some studies have attempted to estimate regression equations with both types of risk and have found non-significant coefficients on at least one of the measures, including Smith (1976), Leigh (1981), Dillingham and Smith (1984) and Kniesner and Leeth (1991).

While including injury risk in a regression model could address concern about one omitted variable, other possible influences on wages that could be correlated with mortality risk may not be easily measured. Several papers have investigated this bias. Garen (1988) notes that "individuals may systematically differ in unobserved characteristics which affect their productivity and earnings in dangerous jobs and so these unobservables will affect their choice of job risk" (p. 9). One example Garen offers is "coolheadedness," which may make a worker more productive under the stresses of a dangerous job but may not be relevant in a safe job. In this case, an econometrician would prefer to include both the mortality risk variable and the interaction or the mortality risk variable with a variable measuring coolheadedness as regressors in the hedonic labor market model. Failing to include this interaction term results in biased least squares estimation. Garen attempts to address this concern with an instrumental variables technique, although subsequent researchers such as Hwang, Reed, and Hubbard (1992) have noted the difficulty in identifying appropriate instruments for his procedure. Employing this instrumental variables technique, Garen found a mortality risk premium about double what the standard least squares model produced.

The significant increase in the risk premium associated with a method to account for unobserved productivity is consistent with the theoretical and simulation findings in Hwang, Reed, and Hubbard (1992). They estimate that for plausible parameter estimates, models that fail to account for heterogeneity in unobserved productivity may bias estimates of the risk premium by about 50 percent and could result in incorrectly (negative) signing of the risk variable. With the exception of some non-union samples in several studies (e.g., Dorsey, 1983; Dickens, 1984), the empirical literature presents very little evidence of this wrong signing. Siebert and Wei (1994) have also found that accounting for the endogeneity of risk can increase the risk premium compared to a standard least squares approach. Recent theoretical research, however, has also illustrated the potential for over-estimating the risk premium by failing to control for unobservables (Shogren and Stamland, 2002). They note that workers with the ability to avoid injury select into risky jobs while those less able to avoid injury ("clumsy" workers) select into less-risky jobs. They argue that risk premiums could be overestimated by a factor of four with plausible parameter estimates in their simulations. Whether there will be such biases hinges on the monitorability of an individual's safety-related productivity. If these differences are monitorable, as in Viscusi and Hersch (2001), there will be a separating compensating differential equilibrium for workers of different riskiness.

Viscusi and Hersch (2001) note that differences in workers' preferences over risk can affect the shape of their indifference curves and workers' safety behavior and, by affecting firms' cost to supply safety, can influence firms' offer curves. They evaluated the wage-risk (injury) tradeoff of workers with a data set that includes measures of risk preferences (e.g., smoking status) and measures of workers' prior accident history. While smokers work, on average, in industries with higher injury risk than non-smokers, smokers also are more likely to have a work-related injury controlling for industry risk. Smokers also are more prone to have had a recent non-work-related accident. As a result, Viscusi and Hersch find that nonsmokers receive a greater risk premium in their wages than do smokers because the safety effect flattens smokers' offer curves enough to offset smokers' preferences for greater wages at higher risk levels.

To address potential omitted variable bias arising from differences in worker characteristics, employing a panel data set could allow one to difference out or dummy out individual-specific unobservables, so long as these are constant throughout the time period covered by the panel. Unfortunately, very few data sets exist that follow a set of workers over a period of several years. Brown (1980) used the National Longitudinal Study Young Men's sample over 1966–1973 (excluding 1972) with the Society of Actuaries mortality risk data. While he reported results that were not consistent with the theory of compensating differentials for a variety of nonpecuniary aspects of employment, he did estimate a positive and statistically significant coefficient on the mortality risk variable. Brown noted that his estimate of the risk premium was nearly three times the size of the estimate in Thaler and Rosen (1975), which first used the Society of Actuaries mortality risk data.

1.2.6. Compensating differentials for risk or inter-industry wage differentials. Several recent papers have claimed that estimates of risk premiums in this kind of wage regression analysis actually reflect industry wage premiums because the fatality risk variables typically reflect industry-level risk (Leigh, 1995; Dorman and Hagstrom, 1998). Both Leigh and Dorman and Hagstrom evaluate the proposition that risk premiums simply reflect industry premiums by comparing compensating differential models without dummy variables for industry affiliation of each worker with models that include such dummy variables.

Their claim that industry premiums mask as risk premiums in these wage regressions suffers from several deficiencies. First, a large number of studies have included industry dummy variables in their statistical analyses and found significant compensating differentials for risk. For example, the first wage-risk tradeoff study by Smith (1974) employed six industry dummies and yielded a statistically significant compensating differential for risk. Viscusi (1978a) included 25 industry dummy variables in his analysis based on the Survey of Working Conditions *danger* variable (0, 1 variable reflecting a worker's subjective perception of on-the-job risk), although he excluded the dummy variables from the analysis based on the industry-level BLS risk data.[12] In both sets of analyses, *danger* and the BLS risk measure were statistically significant and generated very similar estimates of the risk premium. Freeman and Medoff (1981) found a statistically significant risk premium in their analyses that included 20 industry dummy variables and the BLS injury rate measure. In their evaluation of the U.K. labor market with an occupational mortality risk variable, Marin and Psacharopoulos (1982) found a statistically significant risk coefficient while their SIC code dummies were insignificant. Dickens (1984) estimated regression models with the BLS fatality risk measure and 20 industry dummy variables (1- and 2-digit SIC code industries). For the union sample, he found a positive and statistically significant coefficient on risk. Leigh and Folsum (1984) included 2-digit SIC code industry dummy variables in their wage regressions, and they found statistically significant coefficients on mortality risk in all eight mortality risk models reported. Dillingham (1985) estimated regression models with industry dummy variables (at the 1-digit SIC code level) and without. In both cases, he found statistically significant and positive coefficients on his measure of mortality risk. Moreover, the coefficients were virtually identical (0.0023 vs. 0.0022), although the standard error was higher for the model with industry dummy variables (perhaps related to risk-industry dummy variable collinearity). Cousineau, Lacroix, and Girard (1992) included 29 industry variables in their evaluation of the Canadian labor market that estimated statistically significant coefficients on both injury and mortality risks. Lott and Manning (2000) included 13 industry dummy variables in their evaluation of long-term cancer risks in U.S. labor markets, and found a statistically significant risk premium based on industry-level measures of carcinogen exposure.

Second, inserting industry dummy variables into the regression equation induces multicollinearity with the risk variable. Previous researchers such as Viscusi (1979) have noted this as well. Hamermesh and Wolfe (1990) employed dummy variables for five major industries in their analysis of injury risk on wages. They note that a finer breakdown by industry could be used. A complete set of dummy variables at the 3-digit SIC code level, however, would completely eliminate all variation in the injury risk variable, which is measured at the 3-digit SIC code level (p. S183). While multicollinearity does not affect the consistency of the parameter estimates, it will increase standard errors.

This induced multicollinearity is also evident in the Dorman and Hagstrom results for the models using NIOSH fatality risk data.[13] Dorman and Hagstrom interact the NIOSH fatality risk measure by a dummy variable for union status (and for non-union status in the second set of regressions). Contrary to their hypothesis, including industry dummy variables does not reduce the coefficient in the union-risk interaction models. Inducing multicollinearity does depress the t-statistics slightly, although not enough to render the coefficients statistically insignificant. The models with the non-union-risk interaction reflect the induced multicollinearity, as the t-statistics fall below levels typically associated with statistical significance moving from the standard model to the industry dummy model. While the coefficients in these industry dummy-augmented models fall from their levels in the standard models, they are not statistically different from the standard models' coefficients. Based on the NIOSH fatality risk data, the Dorman and Hagstrom results appear to illustrate that including collinear regressors (industry variables) can increase standard errors but not significantly affect the magnitudes of the parameter estimates.

2. The value of a statistical life based on U.S. labor market studies

The value of a statistical life should not be considered a universal constant or some "right number" that researchers aim to infer from market evidence. Rather, the VSL reflects the wage-risk tradeoffs that reflect the preferences of workers in a given sample. Moreover, transferring the estimates of a value of a statistical life to non-labor market contexts, as is the case in benefit-cost analyses of environmental health policies for example, should recognize that different populations have different preferences over risks and different values on life-saving. If people face continuous safety choices in a variety of contexts, however, the same individual should exhibit the same risk-money tradeoff across different contexts, provided the character of the risks is the same. Researchers have undertaken more than 30 studies of compensating differentials for risk in the U.S. labor market. Some studies have evaluated the wage-risk tradeoff for the entire labor force, while others have focused on subsamples such as specific occupations (e.g., police officers in Low and McPheters, 1983), specific states (e.g., South Carolina in Butler, 1983), blue-collar workers only (e.g., Dorman and Hagstrom, 1998; Fairris, 1989), males only (e.g., Berger and Gabriel, 1991), and union members only (e.g., Dillingham and Smith, 1984). These hedonic labor market studies also vary in terms of their choice of mortality risk variable, which can significantly influence the estimation of a value of a statistical life (for comparison of NIOSH and BLS data, refer to Moore and Viscusi, 1988a; Dorman and Hagstrom, 1998).

Table 2 summarizes the estimated VSLs for the U.S. labor market from the literature over the past three decades.[14] Because some studies provided multiple estimates, in these instances we provide illustrative results based on the principal specification in the analysis. Table 2 provides a sense of the magnitude and range of U.S. labor market VSLs and illustrates the influence of factors such as income and the magnitude of risk exposure as well as specification issues such as including nonfatal injury risk and worker's compensation.[15]

Viscusi (1993) reported that most surveyed studies fall within a \$3.8–\$9.0 million range, when converted into year 2000 dollars.[16,17] While we include more papers from the United States as well as findings from other countries, the general conclusion remains unchanged. Half of the studies of the U.S. labor market reveal a value of a statistical life range from \$5 million to \$12 million. Estimates below the \$5 million value tend to come from studies that used the Society of Actuaries data, which tends to reflect workers who have self-selected themselves into jobs that are an order of magnitude riskier than the average. Many of the studies yielding estimates beyond \$12 million used structural methods that did not estimate the wage-risk tradeoff directly or were derived from studies in which the authors reported unstable estimates of the value of a statistical life. Our median estimated VSL from Table 2 is about \$7 million, which is in line with the estimates from the studies that we regard as most reliable. In terms of methodology, we are more confident in the results presented in Viscusi (1978a, 1979), which include the most extensive set of non-pecuniary characteristics variables to explain workers' wages, and the results presented in Moore and Viscusi (1988a), which include the NIOSH mortality risk data in lieu of the pre-1992 BLS mortality risk data.

A salient research issue of policy importance is the effect of income levels on the wage-risk tradeoff. For example, Hamermesh (1999) notes that as wage inequality has increased

Table 2. Summary of labor market studies of the value of a statistical life, United States.

Author (Year)	Sample	Risk variable	Mean risk	Nonfatal risk included?	Workers' comp included?	Average income level (2000 US$)	Implicit VSL (millions, 2000 US$)
Smith (1974)	Current Population Survey (CPS) 1967, Census of Manufactures 1963, U.S. Census 1960, Employment and Earnings 1963	Bureau of Labor Statistics (BLS) 1966, 1967	0.000125	Yes, significant	No	$29,029	$9.2
Thaler and Rosen (1975)	Survey of Economic Opportunity 1967	Society of Actuaries 1967	0.001	No	No	$34,663	$1.0
Smith (1976)	CPS 1967, 1973	BLS 1966, 1967, 1970	0.0001	Yes, not significant	No	$31,027	$5.9
Viscusi (1978a, 1979)	Survey of Working Conditions, 1969–1970 (SWC)	BLS 1969, subjective risk of job (SWC)	0.0001	Yes, significant	No	$31,842	$5.3
Brown (1980)	National Longitudinal Survey of Young Men 1966–71, 1973	Society of Actuaries 1967	0.002	No	No	$49,019	$1.9
Viscusi (1981)	Panel Study of Income Dynamics (PSID) 1976	BLS 1973–1976	0.0001	Yes, significant	No	$22,618	$8.3
Olson (1981)	CPS 1978	BLS 1973	0.0001	Yes, significant	No	$36,151	$6.7
Arnould and Nichols (1983)	U.S. Census 1970	Society of Actuaries 1967	0.001	No	Yes	NA	$0.5, $1.3
Butler (1983)	S.C. workers' compensation data 1940–69	S.C. workers' compensation claims data	0.00005	No	Yes	$22,713	$1.3
Low and McPheters (1983)	International City Management Association 1976 (police officer wages)	Constructed a risk measure from DOJ/FBI police officers killed data 1972–75 for 72 cities	0.0003	No	No	$33,172	$1.4

(*Continued on next page*.)

Table 2. (*Continued*).

Author (Year)	Sample	Risk variable	Mean risk	Nonfatal risk included?	Workers' comp included?	Average income level (2000 US$)	Implicit VSL (millions, 2000 US$)
Dorsey and Walzer (1983)	CPS May 1978	BLS 1976	0.000052	Yes, significant	Yes	$21,636	$11.8, $12.3
Leigh and Folsom (1984)	PSID 1974; Quality of Employment Survey (QES) 1977	BLS	0.0001	Yes, significant	No	$29,038, $36,946	$10.1–$13.3
Smith and Gilbert (1984, 1985)	CPS 1978	BLS 1975	NA	No	No	NA	$0.9
Dillingham and Smith (1984)	CPS May 1979	BLS industry data 1976, 1979; NY workers' comp data 1970	0.000082	Yes, significant in some specifications	No	$29,707	$4.1–$8.3
Dillingham (1985)	QES 1977	BLS 1976; NY workers' compensation data 1970	0.000008, 0.00014	No	No	$26,731	$1.2, $3.2–$6.8
Leigh (1987)	QES 1977; CPS 1977	BLS	NA	No	No	NA	$13.3
Moore and Viscusi (1988a)	PSID 1982	BLS 1972–1982, NIOSH National Traumatic Occupational Fatality (NTOF) Survey 1980–1985	0.00005, 0.00008	No	Yes	$24,931	$3.2, $9.4
Moore and Viscusi (1988b)	QES 1977	BLS, discounted expected life years lost; subjective risk of job (QES)	0.00006	No	Yes	$31,092	$9.7
Garen (1988)	PSID 1981–1982	BLS 1980, 1981	0.000108	Yes, significant	No	$29,865	$17.3
Viscusi and Moore (1989)	PSID 1982	NIOSH NTOF Survey, Structural Markov Model	0.0001	No	No	$24,611	$10.0
Herzog and Schlottman (1990)	U.S. Census 1970	BLS 1969	0.000097	No	No	$48,364	$11.7

(*Continued on next page*.)

Table 2. (*Continued*).

Author (Year)	Sample	Risk variable	Mean risk	Nonfatal risk included?	Workers' comp included?	Average income level (2000 US$)	Implicit VSL (millions, 2000 US$)
Moore and Viscusi (1990b)	PSID 1982	NIOSH NTOF Survey, Structural Life Cycle Model	0.0001	No	No	$24,611	$20.8
Moore and Viscusi (1990c)	PSID 1982	NIOSH NTOF Survey, Structural Integrated Life Cycle Model	0.0001	Yes	Yes	$24,611	$20.8
Kniesner and Leeth (1991)	CPS 1978	NIOSH NTOF survey 1980–1985	0.0004	Yes, significant in some specifications	Yes	$33,627	$0.7
Gegax, Gerking, and Schulze (1991)	Authors' mail survey 1984	Workers' assessed fatality risk at work 1984	0.0009	No	No	$41,391	$2.1
Leigh (1991)	QES 1972–3, QES 1977, PSID 1974, 1981, Longitudinal QES 1973–1977, CPS January 1977	BLS 1979, workers' compensation data from 11 states 1977–1980	0.000134	No	No	$32,961	$7.1–$15.3
Berger and Gabriel (1991)	US Census 1980	BLS 1979	0.00008–0.000097	No	No	$46,865, $48,029	$8.6, $10.9
Leigh (1995)	PSID 1981, CPS January 1977, QES 1977	BLS 1976, 79–81 and NIOSH 1980–85	0.00011–0.00013	No	No	$29,587	$8.1–$16.8
Dorman and Hagstrom (1998)	PSID 1982	BLS 1979–1981, 1983, 1985, 1986; NIOSH NTOF 1980–1988	0.000123–0.0001639	Yes	Yes	$32,243	$8.7–$20.3
Lott and Manning (2000)	CPS March 1971 and March 1985	Hickey-Kearney carcinogenic exposure 1972–1974, NIOSH National Occupational Exposure Survey 1981–1983	NA	No	No	$30,245	$1.5, $3.0 ($2.0, $4.0)†

†Lott and Manning (2000) estimate represents the value of avoiding a statistical fatal cancer case with an assumed latency period of 10 years (discounted at 3 percent). The reported values from their paper without discounting of this latency period are presented in parentheses.

over the last several decades, so have on-the-job mortality risks diverged. He notes that workplace safety is highly income-elastic. This result is related to the findings in Viscusi (1978b) that the value of a statistical life is increasing in worker wealth. Similarly, Viscusi and Evans (1990) have estimated the income elasticity of the value of statistical job injury risks to be 0.6 to 1.0. The effect of income on the wage-risk tradeoff is evident in a historical evaluation of employment risks as well. Kim and Fishback (1993) estimated compensating differentials for mortality risk in the railroad industry over the period 1893–1909 and found implicit values of statistical life on the order of \$150,000 in today's dollars.[18] Our meta-analysis below examines the role of income differences in generating the variation in VSL estimates.

While most hedonic labor market studies focus on the risk of accidental death or accidental injury, several papers have attempted to explore the effect of occupational disease. Lott and Manning (2000) evaluated the effect of carcinogen exposure on workers' wages within the context of changing employer liability laws. In lieu of the standard mortality risk measures, the authors employ the Hickey and Kearney carcinogen index, which represents worker carcinogen exposure at the 2-digit SIC code level.[19] They find that workers' wages reflect a risk premium for carcinogen exposure. Lott and Manning convert their results into a value of a statistical life assuming that the index is a proportional representation of the actual probability of getting occupational-related cancer, that 10–20 percent of all cancer deaths result from occupational exposures, and that the probability of a worker getting cancer ranges from 0.04 to 0.08 percent per year. We have modified their reported VSL range to account for a latency period.[20] Based on these assumptions, the authors estimate that the value of a statistical life based on occupational cancer would range from \$1.5–\$3.0 million. Assuming that occupational cancers, however, comprise a smaller fraction of all cancer deaths would increase the implicit VSL.[21]

Several early papers in the literature did not find statistically significant compensating differentials for on-the-job mortality risk. For example, Leigh (1981) estimated a risk premium for injuries but not for fatalities. Dorsey (1983) likewise did not find a mortality-based risk premium. The Leigh study coupled the Society of Actuaries mortality data with BLS injury data. The combination of greater measurement error in the data and the high correlation between injury risks and mortality risks probably led to the insignificance of the mortality risk variable. The Dorsey study uses industry-level averages, instead of worker-specific values, as its unit of observation. This averaging across industry for wages and related explanatory variables may have reduced the variation necessary to discern the effects of job-specific influences on wage, such as job risk.

More recent papers by Leigh (1995) and Dorman and Hagstrom (1998) also do not find compensating differentials in many model specifications. As discussed above, we do not find their inter-industry wage differential discussion compelling. Nevertheless, Table 2 includes their results based on the NIOSH data with industry dummy variables.[22]

Some of these analyses of U.S. labor markets investigated the potential heterogeneity in the risk preferences of workers in the labor force in which there is worker sorting by level of risk. The empirical issue is whether the wage-risk tradeoff takes a linear or concave shape. A linear form would imply that an incremental increase of risk in the labor market requires a proportional increase in the wage differential. A concave form, however, would

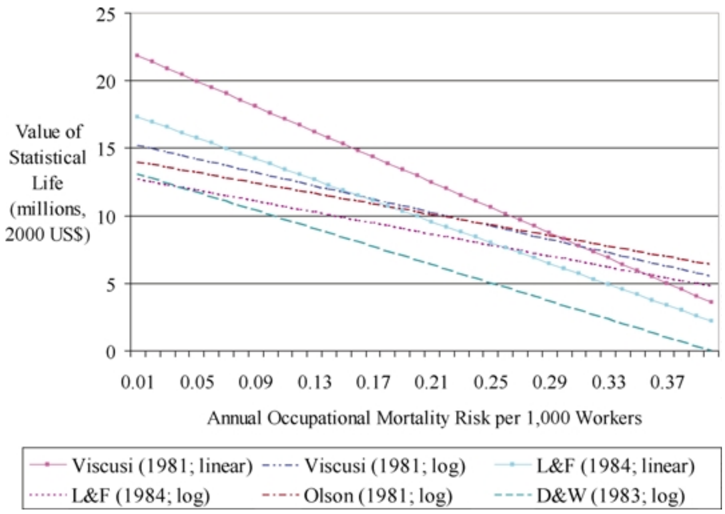

Figure 2. The value of a statistical life as a function of mortality risk.

imply a less than proportional increase in the wage differential, perhaps reflecting sorting by workers based on their risk preferences.

To evaluate the shape of this tradeoff, one can modify the wage equation regression model to include both mortality risk and the square of mortality risk. If the latter term is not significant, then the wage-risk tradeoff is linear for the range of risks and wages covered by the study's sample. If the squared term is significant and negative, then the wage-risk tradeoff takes a concave form. Viscusi (1981), Olson (1981), Dorsey and Walzer (1983), and Leigh and Folsum (1984) all found evidence that the risk-wage tradeoff curve is concave.[23] All four studies include regression models with a quadratic representation of mortality risk.

Figure 2 illustrates how the value of a statistical life varies with mortality risk for a sample of six regression models from these four papers. Viscusi (1981; linear) and Leigh and Folsum (1984; linear) represent regression models where the dependent variable is the hourly wage while the other four lines represent regression models with the logarithm of the wage as the dependent variable. All six models include measures of nonfatal injury risks (probability of a lost-workday accident and, in some cases, duration of lost-workday accident). The slopes of the risk-VSL lines in this figure are similar within the wage-specification type where the wage-based models appear to have a steeper tradeoff than do the logarithm of wage-based models (with the exception of the Dorsey and Walzer model, although this may reflect the fact that the sample in their study faced mortality risks 2 to 3 times smaller on average than the samples in the other studies). Based on these models, populations of individuals who select into jobs with very minor risks (e.g., on the order of 1 in 100,000) have implicit values of statistical life ranging from $12 to $22 million. Increasing the risk ten-fold, to levels that are close to the mean mortality risks in these studies, modestly reduces the VSL into the range of $10 to $18 million. Figure 2 illustrates that very high risks result in small values of statistical lives, although caution should be exercised when considering extrapolations beyond the samples' ranges.

3. Evidence of the value of a statistical life from U.S. housing and product markets

Housing and product market decisions also reflect individual tradeoffs between mortality risk and money. The main methodological difference is that economists typically estimate a hedonic price equation rather than a hedonic wage equation. The underlying theory is essentially the same, as comparison of Rosen (1974) with the wage equation analysis above will indicate.

Table 3 presents the results from eleven studies that evaluated the price-risk tradeoffs for seatbelt use, cigarette smoking, home fire detectors, automobile safety, bicycle helmets, and housing price responses to hazardous waste site risks.[24] The studies in general find an implicit value of a statistical life on the same order of magnitude as the labor market studies, although they tend to be a little lower.

The lower estimates may reflect several characteristics of these studies that distinguish them from the labor market studies. First, some product decisions do not provide a continuum of price-risk opportunities (unlike the labor market that does offer a fairly continuous array of wage-risk employment options) but rather a discrete safety decision. For example, Dardis' (1980) evaluation of smoke detectors represents such a discrete choice. In such a case, the consumer's decision to purchase a smoke detector reveals only the lower bound on the willingness to pay for the reduced risk. Similarly, the study by Jenkins, Owens, and Wiggins (2001) examines the purchase of bicycle helmets. It is interesting, however, that their results show VSLs increasing over the first half of the life cycle.

Second, the types of products considered in some studies may induce selection based on risk preferences. For example, the low estimated VSL for cigarette smokers found by Ippolito and Ippolito (1984) presumably reflects the non-random character of the smoking population. Their research focuses on cigarette smokers, and they estimate a VSL lower than from most product market studies. The lower VSL is consistent with the findings in Hersch and Viscusi (1990) and Viscusi and Hersch (2001) who find that individuals who engage in risky behaviors, such as cigarette smoking and driving without seatbelts, have lower implicit values for injury than do those who do not engage in such behavior.

Third, several studies are based on inferred, instead of observed, price-risk tradeoffs. Consider the seat belt, child seat, and motorcycle helmet studies by Blomquist (1979), Carlin and Sandy (1991) and Blomquist, Miller, and Levy (1996). In these studies, drivers', occupants', or riders' safety is traded off with the time to secure a seat belt or a child seat or to put on a helmet. The authors assume a given time cost—for example, Blomquist assumes that it takes 8 seconds to secure a seat belt. Then this time is monetized at the individual's wage rate (or a fraction thereof in Blomquist, 1979, and Blomquist, Miller and Levy, 1996). Unlike labor market studies where the monetary value of the attribute in question (job wage) is observed, these studies do not observe the actual time drivers take to buckle their seat belts. The amount of time is estimated separately. In addition to time costs, there are other aspects of seat belt or helmet use, such as the costs of discomfort of wearing a seatbelt or a helmet, which would increase the implicit valuation of a statistical life derived by this methodology. While some of these studies attempt to include estimates of these potentially large costs, the estimates are imprecise.

Table 3. Summary of value of a statistical life studies based on tradeoffs outside the labor market, United States.

Author (Year)	Nature of risk, year	Component of the monetary tradeoff	Average income level (2000 US$)	Implicit VSL (millions, 2000 US$)
Blomquist (1979)	Automobile death risks, 1972	Estimated time costs and disutility of seat belts	$38,395	$1.0
Dardis (1980)	Fire fatality risks without smoke detectors, 1974–1979	Purchase price and maintenance costs of smoke detectors	NA	$0.77
Portney (1981)	Mortality effects of air pollution, 1978	Property values in Allegheny County, PA	NA	$1.03
Ippolito and Ippolito (1984)	Cigarette smoking risks, 1980	Monetary equivalent of effect of risk information	NA	$0.90
Garbacz (1989)	Fire fatality risks without smoke detectors, 1968–1985	Purchase price of smoke detectors	NA	$2.56
Atkinson and Halvorson (1990)	Automobile accident risks, 1989	Prices of new automobiles	NA	$5.13
Carlin and Sandy (1991)	Fatality risks with use of children's car seats, 1985	Purchase price of car seats plus time to buckle children, 10 Indiana cities	$24,737	$0.84
Dreyfus and Viscusi (1995)	Automobile safety, 1988	Prices of automobiles	NA	$3.8–$5.4
Blomquist, Miller, and Levy (1996)	Fatality risks associated with use of safety-belts, children's car seats, and motorcycle helmets, 1985	Estimated time costs and disutility of safety devices	NA	$1.7–$9.9
Gayer, Hamilton, and Viscusi (2000)	Superfund sites' cancer risks, 1988–1993	Property values in Greater Grand Rapids, MI	NA	$3.2–$3.7 ($4.3–$5.0)†
Jenkins, Owens, and Wiggins (2001)	Bicycle-related fatal head injury risks, 1997	Purchase price of bicycle helmets	NA	$1.4–$2.9 (5–9 year olds) $1.2–$2.8 (10–14 year olds) $2.1–$4.3 (20–59 year olds)

†Gayer, Hamilton, and Viscusi (2000) estimate represents the value of avoiding a statistical cancer case with an assumed latency period of 10 years (discounted at 3 percent). The reported values from their paper without discounting of this latency period are presented in parentheses.

The studies focused on automobile purchases and home purchases do not suffer from the need to infer the monetary component of the price-risk tradeoff. For example, Atkinson and Halvorsen (1990) and Dreyfus and Viscusi (1995) evaluate the risk-price tradeoff for automobiles. They construct hedonic price models very similar to the hedonic wage models used in labor market analyses. Both studies include car purchase price (annual average for a given model) as the dependent variable and an array of automobile attributes as explanatory variables, such as vehicle size, power, reliability, fuel economy, and safety (fatal accident rate). Just as in the labor market hedonic studies, the coefficient on the safety variable in these automobile price hedonic studies reveals the price-risk tradeoff. Automobile purchases should be less likely to suffer the selection bias of the cigarette smoking study or the discreteness in decision of the fire alarm study.

Gayer, Hamilton, and Viscusi (2000) evaluate the tradeoff between housing prices and cancer risk associated with hazardous waste sites. The authors develop a housing hedonic price model, similar in form to the labor market hedonic studies. The dependent variable is the price of a house sold over a five-year period in the greater Grand Rapids, MI area, and explanatory variables include house characteristics such as number of bedrooms and bathrooms, neighborhood characteristics, property tax rates, measures of proximity to a Superfund hazardous waste site, and calculated cancer risk associated with exposure from the nearest hazardous site. The VSL interpretation from this study is analogous to that of Lott and Manning (2000). The hedonic price model generates the value of avoiding a statistical cancer case, which may not necessarily reflect the value of a statistical life to the extent that some cancers are treatable. If housing decisions are made based on the expectation that cancers associated with hazardous waste site exposure are terminal, then this price-risk tradeoff can be considered comparable to a VSL.[25]

4. The value of a statistical life based on non-U.S. labor market studies

While about 25 wage-risk studies of the U.S. labor market were published in the 1970s and 1980s, only three studies on non-U.S. labor markets appeared in the literature during this period. We have identified another 20 labor market hedonic studies in both developed and developing countries outside of the U.S. context published since 1990. The studies presented in Table 4 include evaluations of wage-risk tradeoffs in labor markets in Australia, Austria, Canada, Japan, and the United Kingdom. More recent work in developing countries has focused on Asia, including analyses of labor markets in Hong Kong, India, South Korea, and Taiwan.

Marin and Psacharopoulos (1982) undertook the first hedonic labor market analysis of job risks outside of the United States in their study of the U.K. labor market. Based on wage and risk data from the 1970s, they found a value of a statistical life of about \$3.5 million. Arabsheibani and Marin (2000) sought to replicate the earlier Marin and Psacharopoulos analysis for the United Kingdom. By employing a similar methodology and more recent wage and risk data from the same sources as in the original study, the authors evaluated the stability of VSL estimates over time. They found, consistent with other studies of the U.K. labor market during the 1980s, a higher value of a statistical life than did Marin and Psacharopoulos. While the evaluation of the whole U.K. labor force yielded

Table 4. Summary of labor market studies of the value of a statistical life, international.

Author (Year)	Country	Sample	Risk variable	Mean risk	Nonfatal risk included?	Workers' comp included?	Average income level (2000 US$)	Implicit VSL (millions, 2000 US$)
Marin and Psacharopoulos (1982)	UK	General Household Survey 1975	OPCS Occupational Mortality Decennial Survey 1970–72	0.0001	No	No	$14,472	$4.2
Weiss, Maier, and Gerking (1986)	Austria	Austrian Microcensus File of Central Bureau of Statistics 1981	Austrian Social Insurance Data on job-related accidents 1977–1984	NA	Yes	No	$12,011	$3.9, $6.5
Meng (1989)	Canada	National Survey of Class Structure and Labour Process 1981	Labour Canada and Quebec Occupational Health and Safety Board 1981	0.00019	No	No	$43,840	$3.9–$4.7
Meng and Smith (1990)	Canada	National Election Study 1984	Labour Canada and Quebec Occupational Health and Safety Board 1981–83	0.00012	No	No	$29,646	$6.5–$10.3
Kniesner and Leeth (1991)	Japan	Two-digit manufacturing data 1986 (Japan)	Yearbook of Labor Statistics (Japan)	0.00003	Yes	No	$44,863	$9.7
Kniesner and Leeth (1991)	Australia	Two-digit manufacturing data 1984–85 (Australia, by state)	Industrial Accidents, Australia Bureau of Statistics 1984–1986	0.0001	Yes	Yes	$23,307	$4.2
Cousineau, Lacroix, and Girard (1992)	Canada	Labor, Canada Survey 1979	Quebec Compensation Board	0.00001	Yes	No	$29,665	$4.6
Martinello and Meng (1992)	Canada	Labour Market Activity Survey 1986	Labour Canada and Statistics Canada 1986	0.00025	Yes	No	$25,387	$2.2–$6.8
Kim and Fishback (1993)	South Korea	Ministry of Labor's Report on Monthly Labor Survey and Survey on Basic Statistics for the Wage Structures	Ministry of Labor's Analysis for Industrial Accidents	0.000485	Yes	Yes	$8,125	$0.8
Siebert and Wei (1994)	UK	General Household Survey 1983	Health and Safety Executive (HSE) 1986–88	0.000038	Yes	No	$12,810	$9.4–$11.5
Lanoie, Pedro, and Latour (1995)	Canada	Authors' in-person survey 1990	Quebec Workers' Compensation Board 1981–1985	0.000126	Yes	No	$40,739	$19.6–$21.7

(*Continued on next page*.)

Table 4. (*Continued*).

Author (Year)	Country	Sample	Risk variable	Mean risk	Nonfatal risk included?	Workers' comp included?	Average income level (2000 US$)	Implicit VSL (millions, 2000 US$)
Sandy and Elliott (1996)	UK	Social Change and Economic Life Initiative Survey (SCELI) 1986	OPCS Occupational Mortality Tables Decennial Supplement 1979/80–1982/3	0.000045	No	No	$16,143	$5.2–$69.4
Shanmugam (1996/7)	India	Author's survey of blue collar manufacturing workers, Madras, India 1990	Administrative Report of Factories Act 1987–1990	0.000104	No	No	$778	$1.2, $1.5
Liu, Hammitt, and Liu (1997)	Taiwan	Taiwan Labor Force Survey 1982–1986	Taiwan Labor Insurance Agency 1982–1986	0.000225–0.000382	No	No	$5,007–$6,088	$0.2–$0.9
Miller, Mulvey, and Norris (1997)	Australia	Australian Census of Population and Housing 1991	Worksafe Australia, National Occupational Health and Safety Commission 1992–93	0.000068	No	No	$27,177	$11.3–$19.1
Siebert and Wei (1998)	Hong Kong	Hong Kong Census 1991	Labour Department	0.000139	No	No	$11,668	$1.7
Liu and Hammitt (1999)	Taiwan	Authors' survey of petrochemical workers 1995	Workers' assessed fatality risk at work 1995	0.000513	Yes	No	$18,483	$0.7
Meng and Smith (1999)	Canada	Labour Market Activity Survey 1986	Ontario Workers' Compensation Board	0.00018	Yes	Yes	$19,962	$5.1–$5.3
Arabsheibani and Marin (2000)	UK	General Household Survey (1980s)	OPCS Occupational Mortality Decennial Survey 1979–83	0.00005	Yes	No	$20,163	$19.9
Shanmugam (2000)	India	Author's survey of blue collar manufacturing workers, Madras, India 1990	Administrative Report of Factories Act 1987–1990	0.000104	Yes	No	$778	$1.0, $1.4
Baranzini and Ferro Luzzi (2001)	Switzerland	Swiss Labour Force Survey, 1995, Swiss Wages Structure Survey, 1994	Swiss National Accident Insurance Company, 1991–1995	0.000059, 0.000064	No	No	$47,400	$6.3, $8.6
Sandy et al. (2001)	UK	SCELI 1986	OPCS 79/80–82/3, HSE 1986–88	0.000038, 0.000045	No	No	$16,143	$5.7, $74.1
Shanmugam (2001)	India	Author's survey of blue collar manufacturing workers, Madras, India 1990	Administrative Report of Factories Act 1987–1990	0.000104	Yes	No	$778	$4.1

a relatively large VSL of about $18 million, regression analyses of subsamples resulted in VSLs ranging up to $68 million (in this case, for non-manual workers). While this result qualitatively conforms to the U.S. findings of lower VSLs for workers in higher risk jobs (see Figure 2), the magnitude of the U.K. compensating differentials seems implausibly large.

The results from several of the studies of the United Kingdom reveal compensating differentials on the order of 10 percent of total worker wage income. One regression result (with a VSL of $69 million) from Sandy and Elliott (1996) implies a compensating differential for mortality risk comprising nearly 20 percent of worker wages. These risk premiums are substantially larger than the compensating differentials evident in other developed countries' labor markets, even those countries with higher per capita incomes. Moreover, risk levels cannot account for the high wage share of compensating differentials as the mortality risk is lower than in many U.S. studies. The large U.K. compensating differentials may reflect correlation between the risk measure and other unobservables that yield substantial returns to the worker.

After the United States, no country has been the focus of more hedonic labor market analyses of wage-risk tradeoffs than Canada. The Canadian studies appear to produce compensating differentials more in line with the U.S. experience than with the evidence from the U.K. labor market. With the exception of Lanoie, Pedro, and LaTour (1995), most Canadian labor market VSLs fall within the range of $3–$6 million. The Lanoie et al. findings of a VSL on the order of $18 million–$20 million may reflect their data collection methodology. They surveyed about 200 workers in the Montreal area and solicited workers' perceptions of risk with a risk information ladder similar to that in Gegax, Gerking, and Schulze (1991), which suffered from using a truncated job risk scale that omitted most job risks in the U.S. economy. Analyses by Lanoie et al. with industry risk measures provided by the Quebec Compensation Board did not yield statistically significant risk coefficients, while the perceived risk measures generated these large VSLs. This result contrasts with the findings of Cousineau, Lacroix, and Girard (1992) who found a statistically significant compensating differential for risk using mortality risk data from the same source on a sample of more than 30,000 Quebec workers.

With the exception of some U.K. studies, the compensating differentials estimated in developed country analyses tend to find risk premiums ranging between 1–2 percent of labor income. These results are broadly consistent with the findings in the Duncan and Holmlund (1983) paper that used Swedish workers' perceptions of danger in lieu of measured industry mortality risks. The authors estimated a statistically significant and positive compensating wage differential for dangerous jobs on the order of about 2 percent of wages. Swedish workers' perceptions of danger yield comparable compensating differentials to measured industry mortality risk in both U.S. and European studies (see Viscusi, 1979 for an example from the U.S. labor market).

Researchers have also evaluated the VSL in several of the newly industrialized countries of Asia, including Hong Kong, South Korea, and Taiwan. Note that these countries have on-the-job mortality risks three to five times greater than the average in Australia, the United States, and the United Kingdom. Further, the average worker earnings are two to four times lower than labor earnings in developed countries.

Kim and Fishback (1999) examined the South Korean labor market over the 1984–1990 period. Unlike many of the studies in developed countries, which employ worker-level data, their unit of observation is at the industry level. Kim and Fishback estimate a VSL of approximately $0.5 million. They note that the estimated VSL is about 94 times the average annual earnings of workers. Siebert and Wei (1998) estimate a VSL for the Hong Kong workforce that is larger than the Korean estimate by about a factor of three. The ratio of VSL to average annual earnings for Hong Kong is about 150. These estimates are of the same order of magnitude as the ratio of VSL to annual earnings evident in the U.S. labor market.

Liu, Hammitt, and Liu (1997) and Liu and Hammitt (1999) estimated the wage-risk tradeoff in Taiwan. The Liu, Hammitt, and Liu study focuses on all non-agricultural workers while Liu and Hammitt base their analysis on in-person surveys of petrochemical workers. In the former case, the authors use 3-digit industry level risk data, while the latter paper uses workers' risk perceptions derived from a survey instrument similar to that in Gegax, Gerking, and Schulze (1991). Workers' risk perceptions in the petrochemical industry yield a mortality risk rate about 35 percent greater than the rate published by the Taiwan Labor Insurance Agency, the data source for the Liu, Hammitt, and Liu study.[26] While petrochemical workers face higher average mortality risk (perceived and measured) than the average for all non-agricultural workers in Taiwan, the higher wages and income associated with petrochemical workers in 1995 relative to the broader workforce in the early to mid 1980s probably explains why Liu and Hammitt estimated a VSL about twice what Liu, Hammitt, and Liu found.

Estimates for the Indian labor market yield a value of a statistical life greater than the VSLs in other developing countries despite the fact that per capita income in India is an order of magnitude smaller than in these countries. Shanmugam (1996/1997, 1997, 2000, 2001) assessed the wage-risk tradeoff in a variety of studies using survey data of manufacturing workers in Madras, India in 1990. The VSL estimates from these studies range by nearly a factor of four, even though they reflect the same wage and risk data, illustrating how a variety of econometric specifications can produce in some cases a range of results.

5. The implicit value of a statistical injury: U.S. and international estimates

Complementing the research on the returns to bearing fatal risks in the workplace, a significant number of studies have evaluated the risk premium associated with bearing nonfatal job risks. The hedonic labor market studies of nonfatal risk employ the same econometric approach as used for mortality risk. As discussed above, some studies that attempt to estimate jointly the effects of fatal and nonfatal risks on workers' wages do not find significant effects of risk on wages for at least one of the risk measures. Fatal risk is highly correlated with nonfatal risk, so joint estimation may result in large standard errors due to collinearity. Omitting one of these variables when estimating the other could result in an upwardly biased estimate of the return to that type of risk.

Table 5 summarizes 31 studies from the U.S. labor market (Table 5(a)) and 8 studies of labor markets outside of the United States (Table 5(b)) that have found statistically significant influences of nonfatal job risk on wages. These studies employ three different

Table 5(a). Summary of labor market studies of the value of statistical injury, United States.

Author (Year)	Sample	Risk variable	Mean injury risk	Fatal risk included?	Workers' comp included?	Average income level (2000 US$)	Implicit value of a statistical injury (2000 US$)
Smith (1974)	Current Population Survey (CPS) 1967, Census of Manufactures 1963, U.S. Census 1960, Employment and Earnings 1963	Bureau of Labor Statistics (BLS) 1966, 1967	NA	Yes, significant	No	$29,029	−$30,934
Smith (1976)	CPS 1967, 1973	BLS 1966, 1967, 1970	NA	Yes, significant	No	$31,027	Nonfatal injury coefficient not significant
Viscusi (1978a, 1979)	Survey of Working Conditions, 1969–1970 (SWC)	BLS non-fatal injury rate 1969 (pre-OSHA)	0.032	Yes, significant	No	$31,842	$25,693–$49,442
Viscusi (1978b)	SWC 1969–1970	BLS non-fatal injury rate 1969 (pre-OSHA)	0.032	No	No	$32,675	$61,537–$63,241
McLean, Wendling, and Neergaard (1978)	Wisconsin Census 1970	Wisconsin Workers' Compensation accident data 1970	0.05	No	No	$34,414	$141,659
Viscusi (1981)	Panel Study of Income Dynamics (PSID) 1976	BLS non-fatal injury rate 1976	0.032	Yes, significant	No	$22,618	$59,238
Olson (1981)	CPS 1978	BLS total lost workday accident rate 1973	0.035	Yes, significant	No	$36,151	$24,009–$32,304
Freeman and Medoff (1981)	CPS May 1973–1975	BLS mean lost workdays per worker 1972–1974	0.701	No	No	NA	Not reported—can't calculate (positive and statistically significant)
Leigh (1981)	PSID 1974, QES 1977	BLS injury rates 1974	NA	Yes, not significant	No	NA	Not reported—can't calculate (positive and statistically significant)
Butler (1983)	S.C. workers' compensation data 1940–69	S.C. workers' compensation claims data	.061 (claims rate)	No	Yes	$22,713	$936/day or $16,848 for an 18-day injury
Dorsey and Walzer (1983)	CPS May 1978	BLS nonfatal lost workday injury incidence rate 1976	0.03	Yes, some specifications	Yes	$21,636	$60,581, $69,235
Smith (1983)	CPS 1978	BLS Work Injury Rate	0.078	No	No	$32,488	$35,485

(*Continued on next page*.)

Table 5(a). (*Continued*).

Author (Year)	Sample	Risk variable	Mean injury risk	Fatal risk included?	Workers' comp included?	Average income level (2000 US$)	Implicit value of a statistical injury (2000 US$)
Dorsey (1983)	Employers' Expenditures for Employee Compensation survey 1977, BLS May 1978, BLS May 1979	BLS	0.036	Yes, not significant	Yes, in some specifications	$33,019	$102,360
Leigh and Folsom (1984)	PSID 1974; Quality of Employment Survey (QES) 1977	BLS nonfatal injury rate	0.074, 0.066	Yes	No	$29,038, $36,946	$99,431–$114,663
Viscusi and O'Connor (1984)	Authors chemical worker survey, 1982	Workers' assessed injury and illness rate	0.1	No	No	$37,642	$17,707–$22,773
Dickens (1984)	CPS May 1977	BLS industry data 1977	NA	Yes	No	NA	Not reported—can't calculate
Viscusi and Moore (1987)	QES 1977	BLS lost workday injury rate, BLS total injury rate	0.038, 0.097	No	Yes	$43,503	$70,650 lost workday accident; $27,950 for nonpecuniary loss-lost workday accident; $45,400 per accident
Biddle and Zarkin (1988)	QES 1977	BLS nonfatal lost workday injury incident rate, 1977	0.037	No	No	$42,170	$168,603 (willingness to accept), $155,582 (willingness to pay)
Garen (1988)	PSID 1981–1982	BLS nonfatal injury rate, 1980–1981	NA	Yes	No	$29,865	$26,953
Moore and Viscusi (1988b)	QES 1977	BLS annual incidence rate of lost workday cases 1973–1976	0.047	Yes, significant	Yes	$31,092	$36,818, $48,349
Hersch and Viscusi (1990)	Authors' survey in Eugene, OR, 1987	Workers' assessed injury rate using BLS lost workday incidence rate scale	0.059	No	No (one state)	$21,897	$72,429 (whole sample); $39,468 (smokers); $118,277 (seat belt users)

(*Continued on next page*.)

Table 5(a). (*Continued*).

Author (Year)	Sample	Risk variable	Mean injury risk	Fatal risk included?	Workers' comp included?	Average income level (2000 US$)	Implicit value of a statistical injury (2000 US$)
Viscusi and Evans (1990)	Viscusi and O'Connor chemical worker survey	Utility function estimates using assessed injury and illness rate	0.1	No	No	$37,802	$23,781 (marginal risk change); $37,030 (certain injury)
Hamermesh and Wolfe (1990)	PSID 1981	BLS 1980, 1981	Incidence: 0.0476; Duration: 0.1571	No	Yes	NA	Not reported—can't calculate (positive and statistically significant)
Kniesner and Leeth (1991)	CPS 1978	BLS lost workday injury rate	0.055	Yes	Yes	$33,627	$60,624
French and Kendall (1992)	CPS 1980 (railroad industry only)	Federal Railroad Administration Injury Rate	0.048	No	No	$46,284	$48,928
Fairris (1992)	SWC 1969–1970	BLS 1969 industry injury frequency rate	0.032	No	No	$33,850	$49,290
Hersch and Pickton (1995)	National Medical Expenditure Survey 1987	BLS total lost workdays per worker per year 1987	0.702†	No	No	$26,345	$120,709 (whole sample); $155,453 (nonsmoker-seat belt users); $83,186 (smoker-nonseat belt users)
Dillingham, Miller, and Levy (1996)	QES 1977	Constructed risk measure	NA	Yes	No	$24,267	$155,435–$242,671 WTP to avoid one year of worklife impairment
Dorman and Hagstrom (1998)	PSID 1982	BLS 1981	0.052	Yes	Yes	$32,243	Nonfatal injury coefficient not significant
Hersch (1998)	CPS March 1994	BLS 1993 (number of cases of days away from work)	0.029 whole sample; 0.022 females	No	No	$28,004	$22,810–$33,723 (females); $12,146–$36,192 (males)
Viscusi and Hersch (2001)	National Medical Expenditure Survey 1987	BLS 1987 injury rate and lost workdays rate	Injury rate: 0.042 nonsmoker; 0.49 smoker	No	No	$31,651 (nonsmokers); $28,316 (smokers)	$47,476–$59,144 (nonsmokers); $20,755–$31,028 (smokers)

†Note that the measure used in the Hersch and Pickton study—probability of losing 1 workday due to injury per worker per year—differs from the measure used in most other studies—probability of a lost workday injury per worker per year.

Table 5(b). Summary of labor market studies of the value of statistical injury, international.

Author (Year)	Country	Sample	Risk variable	Mean injury risk	Fatal risk included?	Workers' comp included?	Average income Level (2000 US$)	Implicit value of a statistical injury (2000 US$)
Cousineau, Lacroix, and Girard (1992)	Canada	Labor, Canada Survey 1979	Quebec Compensation Board	NA	Yes	No	$29,665	$38,104
Martinello and Meng (1992)	Canada	Labour Market Activity Survey 1986	Labour Canada and Statistics Canada 1986	0.063	Yes	No	$25,387	$10,815-$14,456 for injury; $161,210-$191,027 for severe injury
Siebert and Wei (1994)	UK	General Household Survey 1983	Health and Safety Executive (HSE) 1986–88 data	0.0143	Yes	No	$12,810	Coefficient on injury risk not significant in all specifications
Lanoie, Pedro, and Latour (1995)	Canada	Authors' in-person survey 1990	Quebec Workers' Compensation Board 1981–1985	0.099	Yes	No	$40,739	$8,148
Liu and Hammitt (1999)	Taiwan	Authors' survey of petrochemical workers 1999	Workers' assessed injury risk at work 1995	0.0109	Yes	No	$18,483	$49,717
Meng and Smith (1999)	Canada	Labour Market Activity Survey 1986	Ontario Workers' Compensation Board	3.01–workdays lost per worker	Yes	Yes	$19,962	$423 per work day lost
Shanmugam (2000)	India	Author's survey of blue collar manufacturing workers, Madras, India 1990	Administrative Report of Factories Act 1987–1990	0.0729	Yes	No	$778	$150–$560
Shanmugam (2001)	India	Author's survey of blue collar manufacturing workers, Madras, India 1990	Administrative Report of Factories Act 1987–1990	0.0729	Yes	No	$778	$350

measures of nonfatal job risks: the overall injury rate, the rate of injuries severe enough to result in a lost workday, and the rate of total lost workdays. Studies using different measures of nonfatal job risks will generate different risk premiums because the return to the frequency of injuries (the injury rate) will usually differ from the return to the severity of injuries (lost workdays rate). In two studies with specialized data, Butler (1983) constructed an injury rate from South Carolina workers' compensation claims, restricting his risk measure to only the more serious work accidents and French and Kendall (1992) and French (1990) constructed an injury rate for railroad workers based on data collected by the Federal Railroad Administration. As an alternative to these objective measures of risk, Viscusi and O'Connor (1984) and Hersch and Viscusi (1990) used workers' own assessments of risk with risk scales based on the BLS injury rate and the BLS lost workday accident rate, respectively. These authors estimated comparable wage-risk tradeoffs based on subjective risk perception as the other studies based on objectively measured industry-level risk.

These value of statistical injury studies yield a wide range of estimates, reflecting both the differences in the risk measures used as well as whether mortality risk is included in the results. While several studies have very high values of injury, such as McLean, Wendling, and Neergaard (1978), Leigh and Folsum (1984), and Biddle and Zarkin (1988), most studies have estimates in the range of \$20,000–\$70,000 per injury.

The value of statistical injury appears to vary with workers' preferences over risk, consistent with some of the findings based on the mortality risk literature. As a proxy for risk attitudes, several studies have used information about workers' behavior outside of the workplace, such as smoking status and seatbelt use, to identify the effect of risk preferences on wage-risk tradeoffs. Hersch and Viscusi (1990), Hersch and Pickton (1995), and Viscusi and Hersch (2001) all found that smokers have lower injury risk premiums than do nonsmokers. Hersch and Viscusi as well as Hersch and Pickton also found that individuals who do not wear seatbelts have lower injury risk premiums than do individuals who regularly wear seatbelts.

The study by Hersch (1998) is of particular interest because it used gender-specific risk measures. Many previous studies had focused on male samples only because estimates using industry-based measures often failed to yield significant risk premiums for women. Researchers hypothesized that women did not work in risky jobs that would pose health and safety risks. The estimates by Hersch indicate that the nonfatal injury risk for women is over two-thirds the size of that for men and that the wage-injury risk tradeoff rates are similar for men and women.

The evidence outside of the United States, while based on a smaller set of studies, also indicates significant injury risk premiums. For example, the Cousineau, Lacroix, and Girard (1992) result falls within the U.S. range of about \$20,000–\$70,000. However, several other Canadian labor market studies provide some estimates that are lower, such as Martinello and Meng (1992) and Meng and Smith (1999). The value of statistical injury estimates for India are much smaller, likely reflecting the effect of per capita income on wage-risk tradeoffs. The low values of statistical injury are somewhat surprising given that these same studies generated fairly large values of statistical life.

6. The effects of income on the value of a statistical life

The review of the VSLs above shows that developing countries tend to have lower values of statistical life than do developed countries. A variety of factors could account for such an outcome, such as cultural influences on risk preferences and variations in labor market institutions. The dominant cause, however, is most likely that developing countries are poorer, and safety is a normal good, as shown in Viscusi (1978a). The value of a statistical life should increase with per capita income. To assess the relationship between the value of a statistical life and income, we first review several meta-analyses of the wage-risk literature. Second, we provide our estimates of the income elasticity of the value of a statistical life based on the meta-analysis approaches employed in four previous studies with a data set we constructed from our review of the papers presented in Tables 2 and 4. Third, we report income elasticities for a number of specifications in our preferred meta-analytic approach.

Since wage-risk studies employ a measure of income (usually a function of hourly or weekly labor earnings) as the dependent variable, an individual study cannot estimate the effect of income on the premium for bearing mortality risk. The injury risk study of Viscusi and Evans (1990) used experimental data coupled with market evidence to estimate income elasticities of injury risk valuations from 0.6 to 1.0. A meta-analysis of existing VSL studies can facilitate the calculation of the income elasticity for the value of a statistical life. The type of meta-analysis used in the VSL literature attempts to evaluate the VSL (the constructed dependent variable) as a function of a number of studies' characteristics (such as mean income of the sample population, mean mortality risk, and econometric specification).

The published meta-analyses on the value of a statistical life literature vary in terms of their sample construction, explanatory variables, and regression technique. Liu, Hammitt, and Liu (1997) sampled 17 wage-risk studies surveyed in Viscusi (1993) and regressed VSL on income and mean risk. The Liu et al. sample comprised primarily U.S. wage-risk studies. They reported a statistically insignificant income elasticity of 0.53. Miller (2000) developed an international sample including more than 60 wage-risk, product-risk, and contingent valuation studies. Miller employed a relatively small set of explanatory variables, including income. For five models, Miller estimated statistically significant income elasticities ranging from 0.85 to 0.96. Bowland and Beghin (2001) conducted a meta-analysis with a set of 33 wage-risk and contingent valuation studies surveyed in Viscusi (1993) and Desvousges et al. (1995). They matched the data on these studies with a variety of country-specific data on demographics, human capital, etc. Bowland and Beghin employed robust regression with Huber weights to address concerns about the non-normality in the residuals of their data. Bowland and Beghin reported statistically significant income elasticities of 1.7 and 2.3. In contrast to the previous three papers, Mrozek and Taylor (2002) constructed a sample of about 200 observations reflecting multiple VSL estimates from 33 wage-risk studies (eight of which evaluated non-U.S. labor markets). They employed the most extensive set of control variables, including those characterizing a study's sample, risk measure, specification, and earnings. For two models, Mrozek and Taylor impute statistically significant estimates of 0.46 and 0.49 for the income elasticity for the value of a statistical life.

To further explore the relationship between income and WTP, we have conducted a meta-analysis based on the U.S. and international VSLs reported in this paper. Our sample includes the VSLs for 49 studies presented in Tables 2 and 4.[27] Each study yields one observation. Refer to the appendix for a description of the explanatory variables and their summary statistics.

We replicated the results from the four previous meta-analysis studies with our wage-risk study sample (see Table 6). For the Liu, Hammitt, and Liu (1997) model, we replicated their econometric specification exactly. Miller (2000) reported a number of specifications. Only model 3 of Miller's meta-analysis employed per capita incomes converted to US dollars on a purchasing power parity (PPP) basis. Because we constructed all VSLs and annual incomes based on a PPP basis, we replicated his model 3. Note that dummy variables for contingent valuation surveys and wage-risk studies are unnecessary since our data set comprises only wage-risk studies. Bowland and Beghin (2001) reported results for linear, log-linear, and trans-log specifications. While they presented very limited information about most of their control variables, we have attempted to replicate their set of controls. We do not have information on the average age of the sample used in the VSL studies, and we have omitted this variable from our specification. We have proxied for percent of sample in union-affiliated jobs by accounting for whether the VSL study includes union membership as a control and whether the VSL is union-based. Our studies do not provide average educational attainment, so we have proxied these values with national annual average educational attainment for the over-25 population from Barro and Lee (1996). We replicated the robust regressions with Huber weights. Our analyses with log-linear and trans-log specifications, however, yielded insignificant coefficients on income. We only present the results from the linear robust regression model with our data. Mrozek and Taylor (2002) reported results from four specification models. We have focused on their model 2 since model 1 yields virtually identical results and models 3 and 4 are U.S.-specific. We have included all the control variables that Mrozek and Taylor report, with the exception of a dummy variable for white collar-based VSLs. Our sample does not include any white collar-based VSLs.

Three of the four specifications yield statistically significant coefficients on the relevant income variable. We found a comparable point estimate with much narrower bounds than Liu et al. with their specification, perhaps reflecting our larger sample, as their sample is essentially a subset of our sample. We found a smaller coefficient on the income variable than Miller with his specification, although a very comparable coefficient on the Society of Actuaries risk data dummy variable (equivalent to Miller's variable label "risk beyond workplace" with our data). For comparison with Bowland and Beghin's choice of using "marginal willingness to pay" as their dependent variable (apparently equivalent to the VSL expressed in terms of hourly wage instead of annual labor income), we modified our dependent variable accordingly. Imputing the income elasticity with the linear income coefficient in this model requires the sample means of VSL ($3,350 per hour worked, assuming 2000 hours worked per year and $6.7 million VSL) and income ($26,006). With 23 explanatory variables and only 41 observations, the Mrozek and Taylor specification yields very few precise coefficient estimates.

While the reported income elasticities from these four studies vary by a factor of 3, the imputed elasticity point estimates with our data set cover a much smaller range (see Table 7). With these studies' specifications, we found income elasticities from about 0.5 to 0.6. The 95 percent confidence interval upper bounds fall below 1.0 for two of the three statistically

Table 6. Replication of published meta-analyses with Viscusi-Aldy data.

Variable	Viscusi-Aldy Version of Liu et al. (1997) Eq. (2) (1)	Viscusi-Aldy Version of Miller (2000) Model 3 (2)	Viscusi-Aldy Version of Bowland-Beghin (2001) linear model (3)	Viscusi-Aldy Version of Mrozek-Taylor (2002) Model 2 (4)
Dependent variable	log (VSL)	log (VSL)	VSL (expressed in per hour terms)	log (VSL)
log(Income)	0.51*	0.53*	–	–
	(0.15)	(0.17)		
Income	–	–	0.078**	–
			(0.031)	
Hourly wage	–	–	–	0.040
				(0.026)
Mean risk	−0.015*	–	−19.34**	−0.16***
	(0.0057)		(8.56)	(0.075)
Mean risk squared	–	–	–	0.0019
				(0.0015)
Union × mean risk	–	–	–	0.22***
				(0.11)
Dillingham risk	–	–	–	−0.32
				(0.58)
Society of Actuaries risk	–	−1.29*	–	–
		(0.28)		
BLS risk	–	–	1445.18**	–
			(591.47)	
NIOSH risk	–	–	–	0.27
				(0.40)
Education level	–	–	−157.14	–
			(192.32)	
Unemployment rate	–	–	–	0.045
				(0.048)
U.S. national data	–	–	–	0.31
				(0.82)
Non-U.S. study	–	–	–	−0.0048
				(0.81)
Union VSL	–	–	2995.05*	−0.42
			(1086.09)	(0.90)
Union dummy variable	–	–	1000.76	0.43
			(638.37)	(0.40)
Male only sample	–	–	−588.20	–
			(600.82)	

(*Continued on next page*.)

Table 6. (*Continued*).

Variable	Viscusi-Aldy Version of Liu et al. (1997) Eq. (2) (1)	Viscusi-Aldy Version of Miller (2000) Model 3 (2)	Viscusi-Aldy Version of Bowland-Beghin (2001) linear model (3)	Viscusi-Aldy Version of Mrozek-Taylor (2002) Model 2 (4)
Dependent variable	log (VSL)	log (VSL)	VSL (expressed in per hour terms)	log (VSL)
Blue collar sample	–	–	−812.78	−0.68
			(644.54)	(0.49)
Quadratic risk	–	–	–	0.54
				(0.33)
Morbidity variable included	–	–	–	0.11
				(0.32)
log(Dependent variable)	–	–	–	−0.24
				(0.36)
Regional dummy variable	–	–	−757.70	0.16
			(601.16)	(0.35)
Urban dummy variable	–	–	−65.91	0.12
			(804.50)	(0.41)
Workers' compensation	–	–	–	0.10
				(0.45)
Wage in after tax terms	–	–	–	−0.29
				(0.50)
Industry dummy variable	–	–	–	0.081
				(0.27)
Occupation dummy variable	–	–	–	0.0039
				(0.20)
No occupation dummy variable	–	0.48	–	–
		(0.33)		
Job characteristics dummy variable	–	–	–	−0.021
				(0.51)
Constant	10.56*	9.80*	1935.54	15.68*
	(1.49)	(1.78)	(1506.92)	(1.39)
R^2	0.37	0.27	–	0.83
n	46	49	45	41

Specifications (1), (2), and (4) estimated with ordinary least squares.
Specification (3) estimated with robust regression with Huber weights.
Robust (White) standard errors are presented in parentheses for specifications (1), (2), and (4).
Asymptotic standard errors presented in parentheses for specification (3).
*Indicates statistical significance at 1 percent level.
**Indicates statistical significance at 5 percent level.
***Indicates statistical significance at 10 percent level.

Table 7. Income elasticity of willingness-to-pay to reduce mortality risk.

Paper (Model)	Reported elasticity in authors' papers	Viscusi-Aldy version
Liu, Hammitt, and Liu (1997) (Eq. (2))	0.53	0.51* (0.21–0.80)
Miller (2000) (Model 3)	0.89*	0.53* (0.20–0.86)
Mrozek and Taylor (2002) (Model 2)	0.46**	0.52 (−0.18–1.22)
Bowland and Beghin (2001) (linear model)	1.66*	0.61** (0.11–1.10)

Ranges in parentheses represent the 95 percent confidence interval around the point estimate for the income elasticity.

*Indicates elasticity is based on coefficient that is statistically significant at 1 percent level.

**Indicates elasticity is based on coefficient that is statistically significant at 5 percent level.

significant income elasticities estimated by the specifications outlined in these four studies. The apparently large variation in income elasticities in this literature apparently reflects authors' choices of studies for inclusion more so than choices over control variables and regression techniques.

To augment the replications of previously published meta-analysis specifications, we evaluated a large number of specifications. Based on the existing literature, we focused on two regression techniques, ordinary least squares and robust regression with Huber weights. We have varied the control variables from as few as 3 to as many as 18, recognizing that coefficient estimates' precision will decline with the length of the right-hand side of the regression equations given our sample size. We chose to include explanatory variables of the following types: income and mean risk (common to all specifications), type of risk measure, and specification variables.

The estimated coefficient on the income variable is rather stable across both regression techniques and for a wide variation in the number of control variables (see Table 8). For the OLS specifications, the income elasticity varies from 0.49 to 0.60. The 95 percent confidence intervals never range below 0.2 and never exceed 0.95. For the robust regression specifications, the income elasticity varies from 0.46 to 0.48. The 95 percent confidence intervals never fall below 0.15 and never exceed 0.78.[28] The income coefficients in all specifications are statistically significant at the 1 percent level.

Based on the approximately 50 wage-risk studies from 10 countries, we can conclude from these results that the income elasticity for the value of a statistical life is less than 1.0. Across a number of specifications with our data, our point estimates of the income elasticity range between about 0.5 and 0.6. Note that in none of our specifications did the income elasticity's 95 percent confidence interval upper bound exceed 1.0.

Current practice by regulatory agencies is effectively in line with these findings. The U.S. EPA (1999) accounted for income growth into the 22nd century in monetizing the long-term mortality risk reduction benefits from stratospheric ozone protection.[29] In a regulatory context, the U.S. EPA (2000b) also accounted for income growth over a thirty-year period in monetizing the mortality risk reduction benefits from reduced particulate matter pollution

Table 8. Regression models for Viscusi-Aldy meta-analysis.

Variable	(1)	(2)	(3)	(4)	(5)	(6)
Regression technique	OLS	OLS	OLS	Robust with Huber weights	Robust with Huber weights	Robust with Huber weights
log(Income)	0.51* (0.15)	0.49* (0.13)	0.60* (0.16)	0.48* (0.12)	0.46* (0.11)	0.47* (0.15)
Mean risk	−0.015** (0.0057)	−0.053* (0.011)	−0.045* (0.015)	−0.029* (0.0061)	−0.090* (0.018)	−0.11* (0.029)
Mean risk squared	–	0.00022* (0.000052)	0.00016* (0.000055)	–	0.00065* (0.00019)	0.0010** (0.00035)
Society of Actuaries risk	–	–	0.50 (0.87)	–	–	Dropped
NIOSH risk	–	–	0.50 (0.41)	–	–	0.56 (0.41)
Subjective	–	–	−0.69 (0.78)	–	–	−0.16 (0.98)
Union dummy variable	–	–	0.44 (0.32)	–	–	0.50 (0.30)
Male only sample	–	–	0.24 (0.36)	–	–	0.36 (0.34)
Blue collar sample	–	–	−0.016 (0.31)	–	–	−0.23 (0.33)
Quadratic risk	–	–	0.092 (0.27)	–	–	0.20 (0.34)
Morbidity variable included	–	–	0.55*** (0.30)	–	–	0.62** (0.30)
log(Dependent variable)	–	–	0.17 (0.31)	–	–	0.13 (0.41)
Regional dummy variable	–	–	−0.16 (0.25)	–	–	0.16 (0.31)
Urban dummy variable	–	–	0.38 (0.29)	–	–	0.087 (0.39)
Workers' compensation	–	–	−0.57 (0.33)	–	–	−0.28 (0.38)
Industry dummy variable	–	–	−0.46 (0.27)	–	–	−0.37 (0.28)
Occupation dummy variable	–	–	−0.45 (0.31)	–	–	−0.24 (0.33)
Constant	10.56* (1.49)	11.22* (1.30)	9.58* (1.82)	11.05* (1.23)	11.83* (1.12)	11.22* (1.68)
R^2	0.37	0.55	0.72	–	–	–

(*Continued on next page.*)

Table 8. (*Continued*).

Regression technique	OLS	OLS	OLS	Robust with Huber weights	Robust with Huber weights	Robust with Huber weights
n	46	46	46	45	45	44
Income elasticity (95% confidence interval)	0.51 (0.21–0.80)	0.49 (0.23–0.75)	0.60 (0.27–0.94)	0.48 (0.23–0.73)	0.46 (0.24–0.69)	0.47 (0.15–0.78)
Mean predicted VSL, full sample (millions 2000 US$) (95% confidence interval)	5.0 (3.4–7.4)	5.4 (3.7–7.8)	5.9 (2.7–13.9)	5.5 (4.1–7.5)	5.9 (4.4–8.1)	6.2 (2.5–15.7)
Mean predicted VSL, U.S. sample (millions 2000 US$) (95% confidence interval)	5.5 (3.8–8.1)	5.8 (4.1–8.3)	6.9 (3.1–16.2)	6.1 (4.6–8.2)	6.3 (4.8–8.4)	7.6 (3.0–19.4)

Dependent variable: log(VSL).
Robust (White) standard errors presented in parentheses for specifications 1–3.
Asymptotic standard errors presented in parentheses for specifications 4–6.
*Indicates statistical significance at 1 percent level.
**Indicates statistical significance at 5 percent level.
***Indicates statistical significance at 10 percent level.
Note: Estimation with robust standard errors clustered by wage data source yields same significance levels, with the exception of Mean Risk in (1) and (3) and Mean Risk Squared in (3), which are all significant at the 2 percent level, and the industry dummy variable in (3) which is significant at the 8 percent level.

associated with the diesel sulfur standard for heavy-duty trucks. In both these cases, the U.S. EPA employed an income elasticity of 0.4 and conducted sensitivity analyses with a low-end elasticity of 0.1 and a high-end elasticity of 1.0. If our results for the income elasticity apply over time, the point estimate chosen by the EPA is reasonable, although more narrow bounds may be appropriate for the sensitivity analysis.

The meta-analysis regressions can also serve to characterize some of the uncertainty in value of a statistical life estimates. We constructed the mean predicted VSL values presented in the last two rows of Table 8 by first using the estimated coefficients from the meta-analysis regressions to predict the natural logarithm of VSL for each study. Then we converted each predicted log(VSL) to a predicted VSL. We averaged these over all studies that were included in each regression model's sample to produce the average values reported in the table. We constructed the average 95 percent confidence intervals by first estimating the prediction error for each study from the meta-analysis regressions. We then used this prediction error to construct study-specific 95 percent confidence intervals. The values for the lower and upper bounds of these confidence intervals were then averaged over all the studies in the regression sample. The U.S. specific results are based on regression samples that include non-U.S. studies; only the averaging is U.S.-specific.[30]

The mean predicted VSLs from the meta-analysis regression models for the whole sample vary from \$5.0 to \$6.2 million, and from \$5.5 to \$7.6 million for the U.S. sample. An assessment of median predicted VSLs produced very similar results. For most regression models, the 95 percent confidence interval upper bound is double or more than the 95 percent confidence interval lower bound. Note that the small number of degrees of freedom in specifications 3 and 6 yielded very large bounds around the measure of central tendency. While this analysis can characterize some of the uncertainty around the value of a statistical life, several caveats are in order. First, the description of uncertainty presumes that the proper VSL meta-analysis model has been specified. Otherwise, the bounds around the predicted means are not valid. Second, this assessment of uncertainty regards the value of a statistical life constructed from a sample of wage-risk studies of prime-aged workers. Policy applications of these VSLs in benefits transfer should consider appropriate modifications to the VSL point estimate *and* the distribution around it.

7. The effects of union affiliation on the value of a statistical life

Since the U.S. and international evidence from labor markets and other product markets demonstrate a significant wage-risk tradeoff, numerous researchers have explored several factors that may influence the compensating differential for risk. The relationship between union affiliation and the wage-risk tradeoff has received substantial attention in the literature. Most studies of the U.S. labor market find that union affiliation is positively correlated with a greater wage-risk tradeoff while the international evidence is much more mixed.

Workers in union jobs may enjoy an additional premium for bearing risk greater than those in nonunion jobs for several reasons. First, if firms face an upward sloping labor supply curve, then the absence of collective bargaining may result in an inefficiently low level of workplace safety. Viscusi (1980) shows that if the marginal worker's valuation of workplace safety differs from the average worker's valuation, then the firm would provide a suboptimal level of safety. If the marginal worker is willing to accept less of a decrease in wage for an incremental improvement in safety than the average worker, then workplace safety would be too low. If the marginal worker tends to be younger and less experienced while the average worker tends to be older and wealthier, then the average worker with greater wealth and family obligations may have a greater preference for workplace safety than the marginal worker. In light of this inefficiently low provision of safety, unions may bargain over workplace safety in addition to wages and other benefits on behalf of the inframarginal workers who may place greater value on risk reduction than the marginal worker.

A second, and not entirely unrelated phenomenon, is that workplace safety may be a quasi-public good and suffer the common under-provision associated with such goods due to free-riding (Dillingham and Smith, 1984). If a firm provides some level of workplace safety (e.g., a fire extinguishing system), then one worker's consumption of this safety does not preclude another worker from enjoying the same consumption. Since safety is quasi-public, a worker lacks the incentive to truthfully reveal his or her preference for safety, especially since an increase in safety would likely correspond to a decline in the wage (or some other benefit). Collective action can overcome such free-riding. In this case, collective bargaining by a union with a firm could reflect all workers' true preferences for safety, and result in higher aggregate demand for safety by workers than what would be expected in a non-union setting. This higher demand would translate into greater compensating differentials for job-related injury and fatality risk.

Third, if some workers lack adequate information about the safety at their workplace, then they may underestimate the actual risks they face. Workers underestimating their on-the-job risk would demand lower wages than if they held correct perceptions of risk. Unions potentially could provide workers with more accurate information about their on-the-job risks (Viscusi, 1979; Olson, 1981). Unions can take advantage of economies of scale in providing information not available to unorganized non-union workers (Dillingham and Smith, 1984). Unions may also negotiate for mechanisms that increase worker exposure to safety information. For example, Olson noted that a 1976 BLS survey showed that 36 percent of all workers covered by a collective bargaining agreement worked in establishments that sponsored joint firm-worker safety committees.

To evaluate the extent to which these factors influence compensating differentials for on-the-job risk, researchers have taken two estimation approaches (see Table 9). First, some have split their samples into union and non-union subsamples and estimated the wage regressions separately for each subsample. Second, others have included an interaction term, risk variable x union dummy variable, in the wage regression model. In our review of papers evaluating union effects, only Olson (1981) assessed the union-risk premium with both the separate sample regression approach and the risk-union interaction approach. Both approaches yield substantial differences in compensating differentials for union and non-union members.

Regardless of estimation strategy, most assessments of the U.S. labor market found higher risk premiums for union workers than for non-union workers (see Table 9(a)). Of the ten U.S. labor market value of life studies we reviewed that evaluated the role of unions in risk premiums, nine found union workers enjoyed greater compensating differentials for bearing risk than nonunion workers. In contrast to accepted theory, several of these papers found that non-union workers had insignificant or statistically significant *negative* compensating differentials for risk.[31]

Table 9(b) summarizes the rather mixed effects of unionization on premiums for nonfatal risks in the U.S. labor market. In the studies that controlled for fatal risks, the compensating differential for injury risk for nonunion workers often exceeded the differential for union workers, even in the same studies where the union fatality risk premium was greater (e.g., Olson, 1981; Dorsey, 1983; Dorsey and Walzer, 1984). Olson found that union workers enjoyed a greater positive premium than nonunion workers for injury incidence but a negative premium for injury duration (number of lost workdays), in contrast to nonunion workers' positive premium.

In regressions that did not include a variable for fatality risk in the estimation model, the findings reflected the results for fatality risk discussed above. Hamermesh and Wolfe (1990) found that the extra compensating differential for union workers reflected injury incidence, while injury duration was insignificant, similar to Olson. While Freeman and Medoff (1981) could not discern between the statistically significant injury risk premiums for union and non-union workers, Smith (1983), Biddle and Zarkin (1988), Fairris (1992), and Hersch and Pickton (1995) all found greater compensating differentials for nonfatal risks for union than for nonunion workers.

While the majority of the U.S. research illustrates greater risk premiums for union workers, the research on labor markets in other countries often reveals a more ambiguous union

Table 9(a). Summary of union effects in value of a statistical life studies, United States.

Author (Year)	Sample	Risk variable	Mean risk	Nonfatal risk included?	Workers' comp included?	Average income level (2000 US$)	Union implicit VSL (millions, 2000 US$)	Nonunion implicit VSL (millions, 2000 US$)	Specification of union status
Thaler and Rosen (1975)	Survey of Economic 1967 Opportunity	Society of Actuaries 1967	0.001	No	No	$34,663	$1.7–$1.9	Negative	Risk × union interaction
Viscusi (1980)	SWC 1969–1970	BLS 3-digit industry data for 1969	0.000118	Yes	No	$31,842	$5.5–$15.2	Not significant	Risk × union interaction
Olson (1981)	CPS 1978	BLS 1973	0.0001	Yes, significant	No	$36,151	$30.6–$44.2	$5.7–$5.8	Risk × union interaction and separate regressions by union status
Dorsey (1983)	Employers' Expenditures for Employee Compensation survey 1977, BLS May 1978, BLS May 1979	BLS	0.000071	Yes, significant	Yes, in some specifications	$33,019	$9.6	Negative	Risk × union interaction
Dorsey and Walzer (1983)	CPS May 1978	BLS 1976	0.000052	Yes, significant	Yes	$21,636	$11.8, $12.3	Negative	Separate regressions by union status
Dillingham and Smith (1984)	CPS May 1979	BLS industry data 1976, 1979; NY Workers' Comp Data 1970	0.000082	Yes, significant in some specifications	No	$29,707	$6.1, $6.6	$3.1	Separate regressions by union status
Garen (1988)	PSID 1981–1982	BLS 1980, 1981	0.000108	Yes, significant	No	$29,865	Insufficient information provided to calculate	Positive, but smaller than union VSL	Risk × union interaction
Gegax, Gerking, and Schulze (1991)	Authors' mail survey 1984	Workers' assessed fatality risk at work 1984	0.0009	No	No	$41,391	$2.7	Not significant	Separate regressions by union status
Kim and Fishback (1993)	Interstate Commerce Commission data 1893–1909, 1926–32, 1934–45	ICC Fatality Data 1890–1945	0.0006–0.0018	Yes	No	NA	No statistical difference between union and nonunion members		Risk × union interaction
Dorman and Hagstrom (1998)	PSID 1982	BLS 1979–1981, 1983, 1985, 1986; NIOSH NTOF 1980–1988	0.000123–0.0001639	Yes	Yes	$32,243	$18.1	$8.7	Risk × union interaction

Table 9(b). Summary of union effects in value of statistical injury studies, United States.

Author (Year)	Sample	Risk variable	Mean risk	Fatal risk included?	Workers' comp included?	Average income level (2000 US$)	Union implicit value of a injury (2000 US$)	Nonunion implicit value statistical of a statistical injury (2000 US$)	Specification of union status
Biddle and Zarkin (1988)	QES 1977	BLS nonfatal lost workday injury incident rate, 1977	0.037	No	No	$42,170	$319,678	$56,832	Risk × union interaction
Freeman and Medoff (1981)	CPS May 1973–1975	BLS mean lost workdays per worker 1972–1974	0.701*	No	No	NA	No statistical difference between union and nonunion members		Separate regressions by union status
Smith (1983)	CPS 1978	BLS work injury rate	0.078	No	No	$38,138 (males); $25,672 (females)	$74,368 (males); $59,815 (females)	$46,528 (males); $25,672 (females)	Risk × union interaction
Fairris (1992)	SWC 1969–1970	BLS 1969 industry injury frequency rate	0.032	No	No	$33,850	$246,449	Not significant	Separate regressions by union status
Hamermesh and Wolfe (1990)	PSID 1981	BLS 1980, 1981	Incidence: 0.0476; Duration: 0.1571	No	Yes	NA	Insufficient information provided to calculate	Positive, but smaller than union value of injury	Risk × union interaction
Hersch and Pickton (1995)	National Medical Expenditure Survey 1987	BLS total lost workdays per worker per year 1987	0.702*	No	No	$26,345	$222,448	$99,478	Risk × union interaction

*Lost workdays per 100 full-time workers.

Table 9(c). Summary of union effects in value of a statistical life studies, international.

Author (Year)	Country	Sample	Risk variable	Mean risk	Nonfatal risk included?	Workers' comp included?	Average income level (2000 US$)	Union implicit VSL (millions, 2000 US$)	Nonunion implicit VSL (millions, 2000 US$)	Specification of union status
Marin and Psacharopoulos (1982)	UK	General Household Survey 1975	OPCS Occupational Mortality Decennial Survey 1970–72	0.0001	No	No	$14,472	No statistical difference between union and nonunion members		Risk × union interaction
Meng (1989)	Canada	National Survey of Class Structure and Labour Process 1981	Labour Canada and Quebec Occupational Health and Safety Board 1981	0.00019	No	No	$43,840	No statistical difference between union and nonunion members		Risk × union interaction
Meng and Smith (1990)	Canada	National Election Study 1984	Labour Canada and Quebec Occupational Health and Safety Board 1981–83	0.00012	No	No	$29,646	No statistical difference between union and nonunion members		Risk × union interaction
Meng (1991)	Canada	Labour Market Activity Survey 1986	Labour Canada and Workers' Compensation Boards	0.00023 for accidents, 0.00012 for disease	No	No	$25,387	$1.9	$4.2	Risk × union interaction

(*Continued on next page*.)

Table 9(c). (*Continued.*)

Author (Year)	Country	Sample	Risk variable	Mean risk	Nonfatal risk included?	Workers' comp included?	Average income level (2000 US$)	Union implicit VSL (millions, 2000 US$)	Nonunion implicit VSL (millions, 2000 US$)	Specification of union status
Cousineau, Lacroix, and Girard (1992)	Canada	Labor, Canada Survey 1979	Quebec Compensation Board	0.00001	Yes	No	$29,665	$4.4	$2.0	Separate regressions by union status
Martinello and Meng (1992)	Canada	Labour Market Activity Survey 1986	Labour Canada and Statistics Canada 1986	0.00025	Yes	No	$25,387	No statistical difference between union and nonunion members		Risk × union interaction
Siebert and Wei (1994)	UK	General Household Survey 1983	Health and Safety Executive (HSE) 1986–88	0.0000379	Yes	No	$12,810	$11.5	$9.4	Separate regressions by union status
Lanoie, Pedro, and Latour (1995)	Canada	Authors' in-person survey 1990	Quebec Workers' Compensation Board 1981–1985	0.000126	Yes	No	$40,739	$19.6, $21.7	Not significant	Separate regression equations
Sandy and Elliott (1996)	UK	Social Change and Economic Life Initiative Survey (SCELI) 1986	OPCS Occupational Mortality Tables Decennial Supplement 1979/80–1982/3	4.4831E-05	No	No	$16,143	$69.0	$69.4	Risk × union interaction

(*Continued on next page.*)

Table 9(c). (*Continued.*)

Author (Year)	Country	Sample	Risk variable	Mean risk	Nonfatal risk included?	Workers' comp included?	Average income level (2000 US$)	Union implicit VSL (millions, 2000 US$)	Nonunion implicit VSL (millions, 2000 US$)	Specification of union status
Shanmugam (1996)	India	Author's survey of blue collar manufacturing workers, Madras, India 1990	Administrative Report of Factories Act 1987–1990	0.000104	No	No	$778	$0.6	Not significant	Risk × union interaction
Kim and Fishback (1999)	South Korea	Ministry of Labor's Report on Monthly Labor Survey and Survey on Basic Statistics for the Wage Structures	Ministry of Labor's Analysis for Industrial Accidents	0.000485	Yes	Yes	$8,125	No statistical difference between union and nonunion members		Risk × union interaction
Arabsheibani and Marin (2000)	UK	General Household Survey (1980s)	OPCS Occupational Mortality Decennial Survey 1979–83	Reported as −0.006	Yes	No	$20,163	No statistical difference between union and nonunion members		Risk × union interaction
Baranzini and Ferro Luzzi (2001)	Switzerland	Swiss Labour Force Survey, 1995, Swiss Wages Structure Survey, 1994	Swiss National Accident Insurance Company, 1991–1995	0.000059, 0.000064	No	No	$47,400	No statistical difference between union and nonunion members		Separate regressions by union status
Sandy et al. (2001)	UK	SCELI 1986	OPCS 79/80–82/3, HSE 1986–88	0.000038, 0.000045	No	No	$16,143	$5.7, $44.8	$36.1, $74.1	Risk × union interaction

impact on risk premiums (Table 9(c)). Marin and Psacharopoulos (1982) conducted the first analysis of compensating differentials for risk in the U.K. labor market, and found that union affiliation had an insignificant impact on the risk premium. In an attempt to replicate this initial study, Arabsheibani and Marin (2000) also could not find any evidence supporting a union-risk premium based on membership or union strength. In contrast, Siebert and Wei (1994) found higher union risk premiums when accounting for potential endogeneity of risk. Subsequent research by Sandy and Elliott (1996) countered this finding with analysis indicating larger compensating differentials for risk for nonunion members. These four researchers collaborated in a follow-up study, Sandy et al. (2001), which concluded with a qualified claim that nonunion workers enjoy greater risk premiums.

For the Canadian labor market, several analyses have found little support for a positive impact of union affiliation on compensating differentials for risk. In a series of analyses with union x fatality risk interaction terms, Meng (1989, 1991) and Martinello and Meng (1992) found no significant effect for accident risk, while Meng and Smith (1990) found a negative statistically significant coefficient that when combined with the fatality risk coefficient would translate into no compensating differential for union members (in contrast to the positive risk premium for non-union members). In contrast, Cousineau, Lacroix, and Girard (1992) found for a large Quebec sample that union members received larger premiums for both fatality risk and injury incidence, but a smaller premium for injury duration, than nonunion members. Further, Lanoie, Pedro, and Latour (1995) found significant positive compensating differentials for perceived risk for the union sub-sample from their survey of Montreal work establishments.

Finally, the few analyses of developing country labor markets have also found mixed effects of union affiliation on a worker's risk premium. In his analysis of the Indian labor market, Shanmugam (1996/7) included a union x fatality risk interaction term, and found that union members alone enjoy a compensating differential for risk. For the South Korean labor market, Kim and Fishback (1999) could not statistically discern compensating differentials for risk between union and non-union workers.

8. The effects of age on the value of a statistical life

Evaluating wage-risk tradeoffs in labor markets to estimate the value of a statistical life raises the important question of whether life expectancy affects the value of a statistical life. Age affects the duration of life at risk and also may be correlated with other variables that affect one's willingness to bear risk, which are not age effects per se but rather reflect changing preferences over the life cycle. Numerous analyses have shown that the magnitude of the VSL is a decreasing function of age, whereas the value of any particular year of life may increase with age (Rosen, 1988).[32] Wholly apart from life expectancy effects, accounting for the effect of age in the utility function in terms of deteriorating health and quality of life would have generated similar results.[33]

Using this framework, Rosen undertook several simulations based on his previous empirical work (Thaler and Rosen, 1975). He estimated that the value of a statistical life-year, which is equal to the expected consumer surplus for another year, for the average-aged individual in the sample ranges from about \$31,000 to \$130,000, based on discount rates ranging from 0 to 12 percent. Using data on life expectancy by age, Rosen calculated the value of a statistical life for comparable individuals who varied from 36 years to 48 years of age. The 48-year old's VSL is 10 percent less than the 36-year old's VSL.

Several researchers have undertaken direct empirical estimates of the effect of age on the return to risk in hedonic labor market analysis. A simple approach to estimating the effect requires interacting the mortality risk and age variables in the regression model. While some researchers did not identify significant coefficients to this interaction (Meng and Smith, 1990; Shanmugam, 1996/7, 2001), several others found statistically significant and negative estimates broadly consistent with the theory (see Table 10). Research by Thaler and Rosen (1975), Viscusi (1979), Arnould and Nichols (1983) and Moore and Viscusi (1988b) yielded the negative relationship between age and the return to risk. Within the context of a local housing market, Portney (1981) found that the value of a statistical life based on trading off mortality risk associated with local air pollution exposure and housing prices declines significantly with age: an individual under the age of 45 has a VSL 20 times greater than an individual over the age of 65.

Dillingham, Miller, and Levy (1996) employed a modified approach by focusing on the willingness to pay to avoid a fully impaired worklife, where death is the extreme case of impairment. The authors construct a risk variable that reflects injury frequency, severity, and probability of fatality. They assume that a worklife shortened by a fatal injury is equivalent to a worklife shortened by a permanently disabling injury. Their "value of remaining worklife" ranges from \$3.1–\$4.7 million for the whole sample, although it decreases with age. For example, they estimate that a 50-year old values remaining worklife at half the value held by a 30-year old.

Some researchers have proposed a value per discounted expected life-year approach (Moore and Viscusi, 1988b). In lieu of a value of a statistical life, one could adjust the VSL measure for the life expectancy of individuals by essentially annuitizing the estimated VSL. Based on actuarial tables, one could estimate the life expectancy for the average-aged individual in a study and then develop estimates of the life-year value (either by assuming a discount rate, or as in several studies, estimating a discount rate based on revealed preference procedures—see below). Then this life-year could be applied to other situations where life expectancy would differ from the average in the study in question. This approach provides an "age-adjusted" or "life expectancy-adjusted" VSL alternative to the standard VSL. While the life-year approach does address the concern that values of a statistical life should vary with life expectancy, they assume that the marginal value of another year is constant across the age spectrum and across time for a given individual.[34]

Accounting for the effects of age on the value of a statistical life through a life-year approach requires the discounting of future consumer surplus (since, on the margin, the compensating differential should equal the present discounted value of a worker's expected consumer surplus). A variety of papers have imputed workers' or consumers' implicit rates of discount (Moore and Viscusi, 1988b, 1990a, 1990b, 1990c; Viscusi and Moore, 1989; Dreyfus and Viscusi, 1995).[35] While the Moore and Viscusi (1988b) and Dreyfus and Viscusi papers estimate implicit discount rates and marginal discounted life-years based on reduced form models, the other three papers develop structural models to estimate discount rates. Also note that all of these models are based on data from labor markets, except for

Table 10. Summary of age-risk interaction effects in value of a statistical life studies.

Author (Year)	Sample	Average age of sample	Risk variable	Age × risk variable coefficient
Thaler and Rosen (1975)	Survey of Economic Opportunity 1967	41.8	Society of Actuaries 1967	Significant (5%), negative
Viscusi (1979)	Survey of Working Conditions, 1969–1970 (SWC)	39.7	BLS 1969, subjective risk of job (SWC)	Significant (1%), negative
Portney (1981)	Property values in Allegheny County, PA 1978	NA	Air pollution annual mortality rate data, EPA	VSL by age females (males): less than 45: \$1.5 (1.0) million; 45–64: \$0.3 (0.12) million; over 65: \$0.05 (0.05) million
Arnould and Nichols (1983)	U.S. Census 1970	NA	Society of Actuaries 1967	Significant (1%), negative
Moore and Viscusi (1988b)	QES 1977	38.1	Expected life years lost (=BLS fatality risk × discounted remaining life)	Significant (1%), positive (implies risk premium increases with life expectancy, decreases with age, ceteris paribus)
Meng (1989)	National Survey of Class Structure and Labour Process 1981	39.9	Labour Canada and Quebec Occupational Health and Safety Board 1981	Significant (10%), negative
Meng and Smith (1990)	National Election Study 1984	NA	Labour Canada and Quebec Occupational Health and Safety Board 1981–83	Insignificant
Dillingham, Miller, and Levy (1996)	QES 1977	36.5	Constructed risk measure	Declining scale of WTP to avoid impaired worklife as function of age
Shanmugam (1996/7)	Author's survey of blue collar manufacturing workers, Madras, India 1990	34.1	Administrative Report of Factories Act 1987–1990	Insignificant
Baranzini and Ferro Luzzi (2001)	Swiss Labour Force Survey, 1995, Swiss Wages Structure Survey, 1994	NA	Swiss National Accident Insurance Company, 1991–1995	Significant (1%), negative
Shanmugam (2001)	Author's survey of blue collar manufacturing workers, Madras, India 1990	34.1	Administrative Report of Factories Act 1987–1990	Insignificant

Table 11. Summary of imputed discount rate studies, United States.

Year	Author (Year)	Type of study	Sample	Implicit discount rate (%)
1988	Moore and Viscusi (1988b)	Labor hedonic with reduced form discounting model	QES 1977	9.6–12.2
1989	Viscusi and Moore (1989)	Labor hedonic with structural Markov model	PSID 1982	10.7
1990	Moore and Viscusi (1990b)	Labor hedonic with structural life cycle model	PSID 1982	2.0
1990	Moore and Viscusi (1990c)	Labor hedonic with structural integrated life cycle model	PSID 1982	1.0–14.2
1995	Dreyfus and Viscusi (1995)	Automobile hedonic	1988 Residential Transportation Energy Consumption Survey	11–17

the Dreyfus and Viscusi's automobile hedonic study. Rational individuals can implicitly discount their health capital at a different rate than what they face in markets (e.g., real interest rate for a home mortgage) since health status is a non-traded commodity (one cannot "save" good health at age 25 for consumption at age 75). Despite this possibility, these models estimate discount rates that are broadly consistent with the real rates of interest typical workers and consumers face (see Table 11).

9. The application of the value of a statistical life to public policy decisions

At least in the countries with a high level of development, governments recommend or require economic analyses of proposed regulations and public policies.[36] Regulatory agencies in the United States, the United Kingdom, and Canada, have been most prominent in their use of VSL estimates to value the benefits of proposed environmental, health, and safety rules. In some cases, regulatory agencies have modified the VSL to account for distinctive characteristics of the risk and the affected population. In the United States, some environmental laws preclude the promulgation of regulations based on benefit-cost analysis. In these cases, analysts have turned to risk-risk analysis—based in part on an application of the value of a statistical life—to provide some guidance about whether a proposed policy is in fact risk reducing.

9.1. The use of VSLs in government decision-making around the world

9.1.1. United States. Over the past twenty years in the United States, executive orders by Presidents Carter, Reagan, and Clinton have mandated economic impact analyses of all significant Federal regulations (E.O. 12044, E.O. 12291 and E.O. 12866). Beginning with the Reagan Administration, these executive orders vested with the Office of Management and

Budget (OMB) the responsibility for overseeing and coordinating the review of regulatory impact analyses. OMB has published guidelines for all Federal agencies, such as its report with respect to the use of "best practices" in these analyses (U.S. OMB, 1996). The guidance recommends the use of a value of a statistical life to monetize the benefits associated with rules that change the population's mortality risk. While OMB does not recommend a specific VSL or set of VSLs, it does provide a discussion of the issues agencies should consider when choosing a VSL based on the current state of knowledge.

Until the 1980s the dominant policy approach to valuing the benefits of reduced risks of death was based on various human capital measures, such as the present value of lost earnings and medical expenses. These values are lower than the VSL amounts. Typical of this approach was the 1982 analysis by OSHA of its proposed hazard communication regulation. OSHA valued lives saved based on the cost of death, which was the human capital value, because in its view life was too sacred to value. After OMB rejected the regulation, claiming that the costs exceeded the benefits, OSHA appealed the decision to then Vice-President Bush. W. Kip Viscusi was asked to settle the economic dispute between the two agencies. By valuing life properly using a VSL, the estimated benefits exceeded the costs. The regulation was approved the day after his analysis reached the Reagan White House.[37] Thus, the historical impetus for the adoption of the VSL methodology was that these values boosted assessed benefits by roughly an order of magnitude, improving the attractiveness of agencies' regulatory efforts.

The flexibility provided to U.S. agencies in choosing a VSL appropriate to the population affected by their specific rules has resulted in significant variations in the selected VSL both across agencies and through time (see Table 12 and Adler and Posner, 2000). In addition, some regulatory impact analyses have included a range of benefits reflecting different assumptions about the VSL, often reflecting the age profile of the affected population by using a VSL adjusted by the number of life-years saved (e.g., the FDA rule restricting tobacco sales to children, 61 FR 44396, and the EPA rule regulating the sulfur content of gasoline, 65 FR 6698). The U.S. Environmental Protection Agency (EPA), responsible for more costly Federal rule-makings than any other U.S. agency, has developed economic guidelines for its regulatory impact analyses (U.S. EPA 2000a). The EPA guidelines recommend a VSL of \$6.2 million (2000 US\$), reflecting the arithmetic mean of 26 studies reviewed in Viscusi (1992a).

In contrast, the U.S. Federal Aviation Administration (1998) recommends a value of a statistical life of \$3 million in its 2002 economic analyses of regulations.[38] This comparatively low value of life may reflect in part an anchoring effect. The U.S. Department of Transportation was a leader in valuing mortality risk reductions, but began doing so in an era in which the present value of lost earnings was the dominant approach. The agency has slowly increased the value attached to reduced risks of death, but it has continued to lag behind the estimates in the literature.[39]

9.1.2. United Kingdom. In the United Kingdom, the Cabinet Office has likewise provided guidance for economic analyses for the government's regulatory and policy-making agencies (U.K. Cabinet Office, 2000; H.M. Treasury, 1997). While the guidance does not specify the value of a statistical life to be used by agencies, it does recommend careful

Table 12. Values of a statistical life used by U.S. Regulatory Agencies, 1985–2000*.

Year	Agency	Regulation	Value of a statistical life (millions, 2000 $)
1985	Federal Aviation Administration	Protective Breathing Equipment (50 Federal Register 41452)	$1.0**
1985	Environmental Protection Agency	Regulation of Fuels and Fuel Additives; Gasoline Lead Content (50 FR 9400)	$1.7
1988	Federal Aviation Administration	Improved Survival Equipment for Inadvertent Water Landings (53 FR 24890)	$1.5**
1988	Environmental Protection Agency	Protection of Stratospheric Ozone (53 FR 30566)	$4.8
1990	Federal Aviation Administration	Proposed Establishment of the Harlingen Airport Radar Service Area, TX (55 FR 32064)	$2.0**
1994	Food and Nutrition Service (USDA)	National School Lunch Program and School Breakfast Program (59 FR 30218)	$1.7, $3.5**
1995	Consumer Product Safety Commission	Multiple Tube Mine and Shell Fireworks Devices (60 FR 34922)	$5.6**
1996	Food Safety Inspection Service (USDA)	Pathogen Reduction; Hazard Analysis and Critical Control Point Systems (61 FR 38806)	$1.9
1996	Food and Drug Administration	Regulations Restricting the Sale and Distribution of Cigarettes and Smokeless Tobacco to Protect Children and Adolescents (61 FR 44396)	$2.7**
1996	Federal Aviation Administration	Aircraft Flight Simulator Use in Pilot Training, Testing, and Checking and at Training Centers (61 FR 34508)	$3.0**
1996	Environmental Protection Agency	Requirements for Lead-Based Paint Activities in Target Housing and Child-Occupied Facilities (61 FR 45778)	$6.3
1996	Food and Drug Administration	Medical Devices; Current Good Manufacturing Practice Final Rule; Quality System Regulation (61 FR 52602)	$5.5**
1997	Environmental Protection Agency	National Ambient Air Quality Standards for Ozone (62 FR 38856)	$6.3
1999	Environmental Protection Agency	Radon in Drinking Water Health Risk Reduction and Cost Analysis (64 FR 9560)	$6.3
1999	Environmental Protection Agency	Control of Air Pollution from New Motor Vehicles: Tier 2 Motor Vehicle Emissions Standards and Gasoline Sulfur Control Requirements (65 FR 6698)	$3.9, $6.3
2000	Consumer Product Safety Commission	Portable Bed Rails; Advance Notice of Proposed Rulemaking (65 FR 58968)	$5.0**

*This table augments a similar presentation of values of a statistical life used in U.S. regulatory analyses in Adler and Posner (2000) by including more regulations and presenting VSLs in constant year dollars.
**The published summaries of the regulatory impact analyses for these rules do not specify the year in which the reported dollars are denominated. We have assumed that the dollar year corresponds to the date of rule publication for purposes of converting all values into 2000 dollars. Note that the CPSC reported a VSL of $5 million in both its 1995 and 2000 regulations; the difference in values reflects our deflating to 2000 dollars.

consideration of the challenges in applying values estimated in the economic literature to potentially different risk and population contexts of the policy or regulation.

The U.K. Department of the Environment, Transport, and Regions (DETR; formerly Department of Transport) has employed a willingness-to-pay based value of preventing a fatality since 1988 in its regulatory and policy analyses (Chilton et al., 1999). The value of preventing a fatality selected in 1988, \$1.2 million (£500,000 in 1987 prices), is still used by the Department. This value reflects contingent valuation-based estimates of individuals' willingness-to-pay for risk reduction. The U.K. Health and Safety Executive uses the DETR value of preventing a fatality as a starting point for its regulatory impact analyses. The HSE has employed a value of preventing a fatality double the DETR value for cancer-related fatalities, concluding that individuals' dread of the disease significantly outweighs the affects of latency on willingness-to-pay (Andrews and McCrea, 1999).[40]

Whereas U.S. agencies rely on market-based VSL estimates, in the U.K. the emphasis is on contingent valuation estimates. This difference in approach no doubt stems in part from the different character of the empirical evidence in the two countries. There have been dozens of studies of U.S. wage-risk tradeoffs, most of which have been in a reasonable range. There have been far fewer such analyses for the U.K., and the resulting empirical estimates have been much more unstable.

9.1.3. Canada. In Canada, the Privy Council Office published guidelines for benefit-cost analysis in 1995. While these guidelines do not specify one or a set of values of a statistical life, they do note the need for serious consideration of the value of life, determinants of the appropriate VSL, and possible approaches to presenting useful information for policymakers and the public. The value of a statistical life is left to the discretion of agencies promulgating regulations, again much like the U.S. and U.K. approaches.

Transport Canada reviewed the economic analyses for 145 transportation-related projects over 1982–1993 (Blanchard, 1996). The VSL used in these analyses ranged from \$400,000 to \$3.2 million. A recent analysis of a Canadian proposed rule on tobacco products information used a range of the value of a statistical life of \$1.7–\$5.7 million,[41] with higher values for individuals under age 65 (Hara Associates, 2000). A comparable range and age-based VSL differential was employed in an evaluation of a Canadian proposal for cleaner fuels in vehicles as well (Lang et al., 1995).

9.1.4. Multinational organizations. The value of a statistical life has also received attention in multinational contexts. The Intergovernmental Panel on Climate Change (IPCC), established by the United Nations Environment Programme and the World Health Organization in 1988 to provide technical support to participants in global climate change negotiations, discussed issues regarding the value of a statistical life in its 1995 and 2001 assessments of the economic and social dimensions of climate change (IPCC, 1996, 2001). Moreover, the European Commission (EC) began in 2000 a process to prepare guidance for benefits analysis to improve benefit-cost analysis procedures within the EC. The EC effort has focused a substantial amount of time and resources on the value of a statistical life question (EC, 2000).

9.2. Issues pertaining to benefits transfers

The vast majority of the studies reviewed in this paper cover prime-aged workers who have chosen to bear the risk of accidental, immediate death. Many of the studies based their quantitative analyses on samples from the 1970s and 1980s. With the exception of occupational safety policies, most government regulations affect mortality risks with different qualitative characteristics or affect populations with characteristics that differ from those of the samples in these studies. The demographic characteristics of specific groups and the population at large likely differ today (and will differ in the future) from what they were 20 to 30 years ago. The risk-money tradeoffs of those affected by government policy may differ from those values estimated in hedonic market studies.

For example, many environmental regulations address carcinogen exposure. Reduced cancer-related mortality presents several issues that merit consideration when employing a VSL. First, individuals may be willing to pay more to avoid dying of cancer than to avoid an instantaneous accidental death. Revesz (1999) hypothesizes that this "dread" effect of cancer mortality should result in an upward revision to the VSL. This reasoning apparently underlies the higher VSL used by the UK Health and Safety Executive for cancer-related fatalities. The EPA's Science Advisory Board (2000) recommended against any "dread"-related modification to the value of a statistical life on the grounds that the current literature did not support any such change.[42] In particular, contingent valuation estimates of cancer mortality risks have produced values similar to those observed for accidental deaths.[43] The market-based evidence for cancer risks discussed above is similar to the results for accident risks.

Cancer-related mortality also differs significantly from occupational mortality in terms of the timing of the death. The long latency period for cancer (and other chronic health conditions affected by government regulations) should be discounted in the benefits analysis (Revesz, 1999; Science Advisory Board, 2000). Given the rough similarity of the implicit rates of discount discussed in Section 8 to current market interest rates, a benefit-cost analysis could employ one common discount rate for all categories of benefits and costs.

Several researchers have questioned the discounting of benefits of reducing future risks to life when evaluating public policies. Heinzerling (1998, 1999, 2000) has criticized the discounting of statistical lives on moral grounds. She claims that a statistical life cannot be discerned from an actual life, which society would not attempt to price. This concern pertains to both current and future risk reductions and is not a discounting argument per se. While Heinzerling expresses significant concerns with the concept of statistical lives and in their discounting, individuals make private risk-income, risk-time, and risk-risk tradeoffs every day, as evidenced by the literature surveyed in this paper, as well as by simple casual observation.[44] Further, as the studies presented in Table 11 illustrate, individuals value risks in the future less than they value commensurate risks they face today. Both Revesz and Heinzerling raise concerns about discounting statistical lives in future generations (also see Arrow et al., 1996 for a survey of intergenerational discounting).

Failure to discount creates potentially fundamental paradoxes (see Keeler and Cretin, 1983). Suppose that the cost structure for reducing risks to life is unchanged over time. Then it is always desirable to defer any life saving policy and invest the money that would have been spent on risk reduction. Indeed, continual postponement of a life saving effort is always desirable if future benefits are never discounted, but the money spent on these policies continues to grow at the rate of interest.

Similarly, suppose that future life saving benefits are not discounted. If, however, one converts the cost allocations to a terminal value rather than discounting the benefits back to a present value, the effect will be identical in terms of whether the policy passes a benefit-cost test. What the discounting critics generally fail to grasp is that what is being discounted are not lives but rather society's willingness to pay for these future risks to life.

The differences in the age of the population in labor market studies and of the population affected by a regulation should also affect the value of a statistical life. While fewer studies have focused on the effects of age on the return to risk, those that have tend to find that the value of a statistical life declines with age, consistent with the theoretical work. Since these labor hedonic studies include samples where age usually ranges no higher than about 60 years, it may be difficult to extrapolate these results for older populations. This is a significant issue in benefits transfer, since many environment and public health policies (as opposed to worker safety programs) deliver benefits to the elderly. For example, recent air quality regulations promulgated by the U.S. Environmental Protection Agency disproportionately benefit older individuals as illustrated by the average increase in life expectancy of less than 15 years (U.S. EPA, 1999; Science Advisory Board, 1999a). In response to this, the EPA's Science Advisory Board (SAB) has questioned the appropriateness of EPA's choice of a VSL for this population. The SAB notes that it "question[s] the application of a WTP estimate for prime-aged individuals to a population of older individuals and people who are in poor health" (p. 6). The SAB (1999b) has recommended that the EPA revise its VSL in light of this concern.

Several agencies already employ age-adjusted VSLs. As noted above, the economic analysis of a Canadian tobacco regulation employed a smaller VSL for individuals older than 65. The U.S. Food and Drug Administration regularly employs the value of statistical life-years, instead of VSLs, as a way to monetize the health benefits of their proposed rules. The U.S. Environmental Protection Agency has reluctantly conducted benefit-cost analyses with the value of statistical life-years approach. The clear findings in the theory and existing empirical evidence support such age adjustments. Future empirical research should further refine age-specific estimates of the value of a statistical life for use in regulatory analyses.

9.3. The role of risk-risk analysis

While the value of a statistical life can be used to monetize the benefits for risk policy evaluation, most laws do not require that agencies undertake such balancing and some laws in the United States actually preclude the consideration of benefit-cost analysis in setting standards. For example, the courts have interpreted the Clean Air Act such that the U.S. Environmental Protection Agency cannot base ambient air quality standards on benefit-cost analysis or an assessment of the costs.[45] Even if a benefit-cost test cannot be applied, policies that on balance harm individual health presumably should not be adopted. Several forms of risk-risk analysis can account for these various risk effects of policies in an effort to ascertain whether risk regulations on balance are risk reducing. While risk-risk analysis cannot determine if a policy improves societal welfare, it can identify the policies that clearly do not improve societal welfare because they result in a net increase in mortality risk.

Four types of risk-risk tradeoffs could influence the net effect of a policy or regulation on a population's risk exposure. First, a policy may reduce risks of one type while increasing risks of another type. For example, the U.S. Food and Drug Administration considered banning saccharin, the artificial sweetener, in response to an animal study finding that it may be a potential human carcinogen. However, banning saccharin would likely increase the risks associated with obesity. In this case, the U.S. Congress allowed the use of saccharin in foods subject to a warning label on products containing the sweetener (Viscusi, 1994b).

Second, policies to reduce risks may create incentives for individuals to undertake less individual effort to reduce their exposure to risks (moral hazard). These behavioral responses will offset some of the risk reduction of the policy, and could potentially increase net risks. For example, Peltzman (1975) described how drivers responded to mandated safety devices in automobiles by changing their driving behavior. Drivers in "safer cars" drove more recklessly than before. The empirical evidence showed that while the safety measures reduced fatalities among automobile occupants, these gains have been offset at least in part by increases in pedestrian deaths and nonfatal accidents.

Third, risk-reduction policies may result in regulatory expenditures that directly increase fatalities. For example, policies to remove asbestos from buildings may increase asbestos exposure by workers and use of excavation equipment at Superfund hazardous waste sites may result in construction-related injuries and fatalities (Graham and Wiener, 1995). Viscusi and Zeckhauser (1994) illustrate through an input-output analysis the total direct and indirect injury and fatality risks associated with expenditures by industry. Regulations that require a reallocation of resources to industries with higher risks, for example towards construction to build new wastewater treatment facilities or to install pollution control technologies, would result in an offsetting increase in mortality risk.

Fourth, the costs of risk-reduction policies decrease income available to finance other health and safety expenditures. Wildavsky (1980, 1988) noted that the costs of risk-reduction policies reduce national income, some of which would otherwise be used to promote health and safety. This argument focuses on the correlation between income and health, evident in both international cross-sectional data and U.S. time-series data (Viscusi, 1983, 1994b; Graham, Chang, and Evans, 1992; Lutter and Morrall, 1994; see Smith, Epp, and Schwabe, 1994 for a critique of the international evidence).[46] These analyses illustrate that wealthier countries have lower mortality rates associated with greater health and safety investments. Graham, Chang, and Evans (1992) extended the previous analyses on the mortality-income relationship by focusing on the effect of permanent income. They conclude that: "If government regulation reduces the level (or the rate of growth) of permanent income, it is likely to cause smaller health investments and an eventual decline in health status compared to what would have occurred without the economic burdens of regulation" (p. 336).[47]

Keeney (1990, 1994, 1997) formalized Wildavsky's proposition and found that some expensive regulations aimed at reducing mortality risks actually increase mortality risks by reducing national income. Keeney's model yields various estimates for the induced-expenditure associated with an additional fatality that depend on the distribution of the burden of the policy costs. Based on the mortality-income relationships estimated by Kitawaga and Hauser (1973) and Frerichs et al. (1984), Keeney estimates that between \$13.6 million and \$15.2 million of expenditures would induce a fatality, assuming costs are borne proportional to income.[48,49] This research inspired a legal opinion of an OSHA regulation in the early 1990s.[50] U.S. Federal Appeals Court Judge Steven F. Williams wrote that regulations that do not pass a risk-risk analysis would be counterproductive. The U.S. Office of Management and Budget then suggested to OSHA that it consider risk-risk analysis in conducting its regulatory impact analyses.

While the research on the income-mortality relationship influenced several policy discussions in the early 1990s in the United States, the results from these studies point towards a peculiar inconsistency. For the studies with expenditure-induced fatalities on the order of \$5 million per fatality, the expenditure associated with a loss of one life is on par or perhaps below what individuals are willing to pay to reduce the risk of one fatality in the population based on the labor market hedonic studies (Viscusi, 1994b). Moreover, the mortality-income studies also suffer several other potential problems. Some of these do not appropriately account for the simultaneity in the data—higher incomes allow individuals to invest more in health, but poor health often draws down an individual's income (Chapman and Hariharan, 1994).[51] Some of these studies also may suffer from omitted variable bias resulting in misidentifying the relationship between income and health.

As an alternative to deriving the income-mortality relationship from aggregate data, Viscusi (1994a) illustrates how to generate an estimate for the expenditure-induced fatality rate based on the value of a statistical life and the marginal propensity to spend on health. Viscusi shows that

$$\text{marginal expenditure per statistical life lost} = \frac{\text{value of a statistical life}}{\text{marginal propensity to spend on health}}.$$

This approach requires an estimate of the value of a statistical life and an estimate of the marginal propensity to spend on health. The literature surveyed in previous sections provides estimates for the numerator. For the marginal propensity to spend on health, Viscusi estimated this based on an analysis of 24 OECD countries over the 1960–1989 period and a time-series analysis for the United States over the same period (Viscusi, 1992b, 1994a, 1994b). Over an array of specifications for both data sets, the marginal propensity to spend on health ranged from 0.08 to 0.12. This implies that for every dollar increase in national income, an additional 8 to 12 cents are spent on health care. Assuming a marginal propensity to spend on health of 0.1 and a VSL of about \$6 million, the marginal expenditure per statistical life lost would be about \$60 million.[52]

Lutter, Morrall, and Viscusi (1999) extended Viscusi's (1994a) model to account for the effect of income to influence risky behaviors (such as smoking, drinking, and poor diet) as well as the consumption of health care. Their statistical analyses illustrate that higher incomes promote better health habits, including reduced excessive drinking and cigarette smoking and increased exercise. Including the effects of income on risky health behavior reduces the estimated marginal expenditure per statistical life lost from the Viscusi (1994a) by more than a factor of 3 to \$17 million. Lutter et al. note that policies that increase national income would reduce one fatality for every \$17 million increase. The authors also note that since many regulations have costs per life saved of $100 million or more, the expenditures may be wasteful and counterproductive.[53]

If a regulation directly reduces mortality risk with regulatory-induced expenditures yielding a high cost per life saved ratio, then the indirect increase in mortality risk may exceed the direct decrease in risk resulting in an aggregate increase in societal mortality risk. The existing literature varies in terms of a preferred value for the amount of induced expenditures associated with an additional mortality. However, some regulations would clearly not pass a risk-risk analysis with any plausible value for an induced-expenditure mortality. For example, the 1986 OSHA regulation limiting asbestos occupational exposure cost about $250 million per normalized life saved, and the 1987 OSHA regulation limiting formaldehyde occupational exposure cost about $290 billion per normalized life saved (Viscusi, Hakes, and Carlin, 1997).

While the risk-risk literature has focused on mortality risks, many environment and safety regulations provide other kinds of benefits, such as reduced morbidity and injury as well as non-human health related effects such as improved visibility and ecosystem health (Lutter and Morrall, 1994; Portney and Stavins, 1994). A regulation that primarily delivers non-mortality benefits could fail a risk-risk analysis but still pass a benefit-cost analysis. One

Table 13. Evaluation of Risk-Risk Tradeoff for 24 U.S. Regulations, 1986–1998.

Regulation	Year	Agency	Discounted statistical lives saved	Fatalities induced by cost of regulations	Net lives saved by regulations
Toxicity characteristics to determine hazardous wastes	1990	EPA	0.048	−23	23
Underground storage tanks: technical requirements	1988	EPA	1.1	−22	24
Manufactured home construction and safety standards on wind standards	1994	HUD	1.5	−3.2	4.7
Process safety management of highly hazardous chemicals	1992	DOL	220	−42	260
Regulations restricting the sale and distribution of cigarettes and smokeless tobacco to protect children and adolescents	1996	HHS	4,700	−140	4,900
Medicare and Medicaid programs: hospital conditions of participation; identification of potential organ, tissue, and eye donors; and transplant hospitals' provision of transplant-related data	1998	HHS	710	9.2	700
Quality mammography standards	1997	HHS	75	1.4	74
Food labeling regulations	1993	HHS	520	10	510
Childproof lighters	1993	CPSC	95	2.9	92
Standard for occupational exposure to benzene	1987	DOL	4.4	1.8	2.6

(*Continued on next page.*)

Table 13. (*Continued*).

Regulation	Year	Agency	Discounted statistical lives saved	Fatalities induced by cost of regulations	Net lives saved by regulations
Occupational exposure to methylene chloride	1997	DOL	12	5.9	6.2
Occupational exposure to 4,4′ methylenedianiline	1992	DOL	0.7	0.71	−0.01
Asbestos: manufacture, importation, processing, and distribution in commerce—prohibitions (total)	1989	EPA	3.9	4.3	−0.41
National primary and secondary water regulations–phase II: maximum contaminant levels for 38 contaminants	1991	EPA	44	63	−19
Occupational exposure to asbestos	1994	DOL	13	20	−7.1
Hazardous waste management system—wood preservatives	1990	EPA	0.29	0.83	−0.55
Sewage sludge use and disposal regulations, 40 CFR pt. 503	1993	EPA	0.24	2.6	−2.3
Land disposal restrictions for “third third” scheduled wastes	1990	EPA	2.8	30	−27
Hazardous waste management system: final solvents and dioxins land disposal restrictions rule	1986	EPA	1	12	−11
Occupational exposure to formaldehyde	1987	DOL	0.21	4.8	−4.5
Prohibit the land disposal of the first third of scheduled wastes (“second sixth” proposal)	1988	EPA	2.9	66	−63
Land disposal restrictions—phase II: universal treatment standards and treatment standards for organic toxicity, characteristic wastes, and newly listed wastes	1994	EPA	0.16	8.3	−8.2
Drinking water regulations, synthetic organic chemicals—phase V	1992	EPA	0.0061	3.4	−3.4
Solid waste disposal facility criteria, 40 CFR pt. 257 and pt. 258	1991	EPA	0.0049	10	−10

Source: Hahn, Lutter, and Viscusi (2000).

approach could be to convert the morbidity effects into mortality risk equivalents. Alternatively one could focus on policies whose primary intent is to reduce mortality risks. Recognizing this criticism, Hahn, Lutter, and Viscusi (2000) conducted risk-risk analyses on a number of major environment, health, and safety regulations over the 1986–1998 period whose primary benefits were reduced mortality risk (see Table 13).[54] Their analysis focuses on 24 regulations promulgated by the U.S. Environmental Protection Agency (EPA), U.S. Department of Housing and Urban Development (HUD), U.S. Department of Labor (OSHA), and U.S. Consumer Products Safety Commission (CPSC). Thirteen of 24 regulations designed primarily to reduce mortality risks actually increased mortality risks based on the Lutter et al. work finding that $15 million of expenditures induces a fatality. Eight of these thirteen rules failing a risk-risk analysis had at least ten times more expenditure-induced fatalities than lives saved.[55]

10. Conclusion

For nearly thirty years, economists have attempted to infer individuals' preferences over mortality and morbidity risk and income in labor and product markets. The substantial literature that has developed over that time has confirmed Adam Smith's intuition about compensating differentials for occupational hazards in a significant and growing number of countries. In addition to evaluating various international labor markets, the literature has expanded to address a variety of econometric issues, morbidity risk premiums, and factors influencing mortality risk premiums such as union affiliation and age.

While the tradeoff estimates may vary significantly across studies, the value of a statistical life for prime-aged workers has a median value of about $7 million in the United States. Our meta-analysis characterizes some of the uncertainty in estimates of the value of a statistical life, and finds that the 95 percent confidence interval upper bounds can exceed the lower bounds by a factor of two or more. Other developed countries appear to have comparable VSLs, although some studies of the United Kingdom have found much larger risk premiums. Consistent with the fact that safety is a normal good, developing countries' labor markets also have significant, but smaller, values of statistical life. Overall, our point estimates of the income elasticity of the value of a statistical life range from 0.5 to 0.6. Union members in U.S. labor markets appear to enjoy greater risk premiums than non-members, while the evidence in other developed countries is rather mixed. The theoretical and empirical literature indicates that the value of a statistical life decreases with age.

The estimates of the value of a statistical life can continue to serve as a critical input in benefit-cost analyses of proposed regulations and policies. Refining VSLs for the specific characteristics of the affected population at risk remains an important priority for the research community and the government agencies conducting these economic analyses. Improving the application of VSLs in this way can result in more informed government interventions to address market failures related to environmental, health, and safety mortality risks.

Appendix

Table A. Description of variables used in Viscusi-Aldy meta-analyses.

Variable	Description	Summary statistic, Viscusi-Aldy data
VSL	Value of a statistical life (millions, 2000 US$)	$6.7 ($5.6)
Income	Annual labor income (2000 US$)	$26,006 ($12,002)
Mean risk	Average mortality risk of sample	0.0002 (0.0003)
Hourly wage	Hourly wage or hourly equivalent of weekly income (2000 US$)	$13.00 ($6.00)
Union VSL	VSL for union members only (dummy variable)	4/49

(*Continued on next page*.)

Table A. (*Continued.*)

Variable	Description	Summary statistic, Viscusi-Aldy data
Dillingham risk	VSL based on Dillingham (1985) constructed New York workers compensation-based fatality risk measure (d.v.)	1/49
Society of Actuaries risk	VSL based on Society of Actuaries 1967 mortality risk data (d.v.)	2/49
BLS risk	VSL based on BLS mortality risk measure (d.v.)	16/49
NIOSH risk	VSL based on NIOSH mortality risk measure (d.v.)	5/49
Subjective	VSL based on self-reported measure of mortality risk (d.v.)	1/49
Education level	Barro-Lee average educational attainment for population >25 by country (in years)	9.6 (2.0)
Unemployment rate	Annual unemployment rate by year of wage data	7.6 (4.6)
U.S. national data	VSL based on national U.S. worker sample (d.v.)	24/49
Non-U.S. study	VSL based on non-US wage-risk study (d.v.)	22/49
Male only sample	VSL based on male only sample (d.v.)	21/49
Blue collar sample	VSL based on blue collar only sample (d.v.)	15/49
Quadratic risk	VSL based on econometric specification quadratic in mortality risk (d.v.)	7/49
Morbidity variable included	VSL based on study that included an injury risk measure (d.v.)	25/49
log(Dependent variable)	VSL derived from specification with natural logarithm of dependent variable (d.v.)	44/49
Union D.V.	VSL based on specification that included dummy variable for union affiliation (d.v.)	32/49
Regional D.V.	VSL based on specification with regional dummy variables (d.v.)	24/49
Urban D.V.	VSL based on specification with urban/MSA dummy variable (d.v.)	13/49
Workers' compensation	VSL based on specification with workers' compensation variable (d.v.)	11/49
Wage in after tax terms	VSL based on specification with income expressed in after-tax terms (d.v.)	10/49
Industry D.V.	VSL based on specification with industry dummy variables (d.v.)	21/49
Occupation D.V.	VSL based on specification with occupational dummy variables (d.v.)	20/49
No occupation D.V.	Study does not include occupational dummy variables (dummy variable)	29/49
Job characteristics D.V.	VSL based on specification with variables describing job characteristics (d.v.)	10/49

Means (standard deviations) reported for continuous variables.
Share of studies in which variable equals 1 reported for dummy variables.
$n = 49$.

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Notes

1. Past reviews of this literature include Smith (1979), Miller (1990), and Viscusi (1992, 1993, 2000). Several researchers have conducted meta-analyses of this literature, including Liu, Hammitt, and Liu (1997), Miller (2000), Bowland and Beghin (2001), and Mrozek and Taylor (2002). See Hammitt (2002) and Krupnick (2002) for commentaries on the Mrozek and Taylor paper.
2. For more extensive discussion of hedonic analysis, see Griliches (1971), Rosen (1974, 1986), Thaler and Rosen (1975), Smith (1979), and Viscusi (1979).
3. For a discussion of irrational behavior in the presence of mortality risk inconsistent with expected utility theory, refer to Viscusi (1998).
4. Individual risk neutrality or risk aversion, i.e., U'', $V'' \leq 0$ is required to ensure a global maximum.
5. Gaba and Viscusi (1998) compared qualitative and quantitative subjective measures of on-the-job accident risk. They find that for a given level of quantitative risk, a college-educated individual is more likely to report that risk as "dangerous" than an individual with less than college education. For example, for those workers who report their quantitative risk as comparable to an annual injury risk of less than 1 in 20, half of the college-educated described their job risk as "dangerous" while only 19 percent of the less-than-college educated reported their job risk as such. This differences in the "danger" cutoff biases estimates of risk premiums in wages, and this bias has implications distinct from typical measurement error. Gaba and Viscusi find that subjective quantitative measures of risk yielded wage premiums more consistent with estimates based on objective risk measures (BLS). Analysis of the qualitative risk measure (a dichotomous 0-1 "is the job dangerous?" variable) produced a much larger risk premium.
6. Averaging over a period of 5 to 10 years would likely remove any potential distortions associated with catastrophic accidents in any particular year.
7. Viscusi (1979, 1980) includes the danger variable in some regressions, although his research focused on compensating differentials based on BLS measures of injury and fatality risk discussed below.
8. Leigh's (1991) analysis with risk data derived from workers' compensation records in 11 states appears to be the only U.S. study to aggregate such data from more than one state.
9. Averaging fatality risk data over several years can reduce the distortion of a catastrophic event in one year in one industry on the measure of an industry's risk.
10. Most hedonic wage-risk studies have not accounted for labor taxes in their construction of the wage variable or in their interpretation of the value of a statistical life. Exceptions include several papers that have focused on the effects of workers' compensation on the compensating differential for occupational risk (e.g., Moore and Viscusi, 1990a). This reflects the more common use of pre-tax wages in the broader labor market analysis literature.
11. In the product market context, Atkinson and Halvorsen (1990), Dreyfus and Viscusi (1995), and Gayer, Hamilton, and Viscusi (2000) employ Box-Cox transformations to evaluate their respective hedonic price models (the first two focused on automobile prices and the third on housing prices).
12. Viscusi (1978a) excluded the dummy variables out of concern of inducing multicollinearity between an industry-level risk measure and industry dummy variables. Refer to the discussion below on multicollinearity.
13. We focus our discussion here on their NIOSH data-based results for several reasons. First, we believe that the NIOSH data are superior to the 1980s BLS fatality risk data as discussed above. Second, the regression models based on non-NIOSH data in Dorman and Hagstrom have insignificant risk coefficients for all specifications—without industry dummy variables, with industry dummy variables, and with other industry characteristics.

While we are not certain why they did not find significant risk coefficients with the BLS risk data as previous researchers had, it seems moot to argue that a coefficient on a risk variable actually represents an industry effect if that coefficient is not significant.

14. We selected papers for inclusion in this literature review with a modest set of criteria. First, a study should be written in English. Second, a study should be published in either an academic journal or a book. Third, a study should provide enough information to calculate a value of a statistical life. In several cases, information provided in more recent papers (such as per capita incomes reported for some studies in Viscusi, 1993; Mrozek and Taylor, 2002) has been used with reported coefficient estimates to calculate the value of a statistical life for a study. Our aim is to characterize as best as possible the universe of wage-risk studies in the literature, so we have not attempted to purge this assessment of so-called "low quality" studies or to modify the value of a statistical life estimates from such studies. For some studies, we have presented a range of the value of a statistical life estimates and for others we have presented an illustrative point estimate. In the case of the latter, we have focused on the reported results for the whole sample (as opposed to union-only, blue-collar only, managerial-only, etc. samples) based on the econometric specification preferred by the studies' authors.
15. While these estimates have been adjusted to constant year dollars (2000 US$), they have not been adjusted for differences in the samples' income levels. The per capita income data provided in Table 2 coupled with income elasticity estimates in Section 6 can be used to modify the values of a statistical life in Table 2 so that they reflect a common income level.
16. In this paper, we present all VSLs in terms of 2000 US$. All domestic values are converted using the CPI-U deflator series (Council of Economic Advisers, 2002, Table B60). International conversions are made using purchasing power parity exchange rates from the Penn World Table 6.0 (see Summers and Heston, 1991; Aten et al., 2001, and Internet: http://pwt.econ.upenn.edu) and then converted to year 2000 dollars with the CPI-U deflator series.
17. Viscusi (1993) actually reports \$3–\$7 million in December 1990 US\$. This has served as a reference range in several subsequent value of a statistical life studies. Note that we have updated this range to 2000 US\$.
18. Also refer to Fishback and Kantor (1992) for a historical evaluation of compensating differentials for occupational risk in the late 19th century.
19. Smith (1983) also used the Hickey and Kearney carcinogen exposure index to complement his assessment of compensating differentials for injury risk in U.S. labor markets. He found that the index is significantly positively correlated with wages implying a risk premium for workers' operating in environments exposing them to more carcinogens. Further, Smith included a measure of total suspended particulates (TSP) based on the workers' locations and found a statistically significant and positive correlation between TSP and workers' wages. Since TSP has been linked with various respiratory diseases, this could illustrate another premium for workers' bearing long-term risks on the job.
20. The values we report reflect an assumed latency period of 10 years and a real discount rate of 3 percent. Note that the VSL estimates in parentheses in Table 2 represent the values reported by Lott and Manning. Employing a real discount rate of 7 percent, consistent with U.S. Office of Management and Budget (1992) guidance, over a 10-year period effectively reduces the VSL reported by Lott and Manning in half.
21. Meng (1991) attempted to account for occupational disease by including a variable that reflected the rate of heart attacks and industrial disease. However, very modest information about the basis for this variable is provided. It is difficult to discern whether the variable captures long-term work-related risks or simply a selection effect that contaminates the data like the Society of Actuaries data set.
22. The authors also report estimates using the less reliable pre-1992 BLS risk data.
23. Siebert and Wei (1998) found a concave wage-risk relationship with a risk and risk-squared specification for their analysis of the Hong Kong labor market. Meng and Smith (1990, 1999) found the same relationship in their assessments of the Canadian labor market. Baranzini and Ferro Luzzi (2001) likewise found the same relationship in their analysis of the Swiss labor market.
24. All these studies evaluate risk-income tradeoffs in the United States. In our survey of the literature, we found only one study focusing on behavior outside of the United States. Ghosh, Lees, and Seal (1975) study the tradeoff between highway speeds and mortality risk in the United Kingdom. They derive a value of a statistical life estimate of \$0.9 million.
25. We have discounted the reported VSLs assuming that homeowners perceive a 10-year latency period and use a 3 percent discount rate in making their housing decisions.

26. The difference between the mean subjective mortality risk and the objective mortality risk reported by the Taiwan Labor Insurance Agency is statistically significant at the 1 percent level.
27. Studies for which we do not have an income measure were omitted. We did not include the Baranzini and Ferro Luzzi paper on the Swiss labor market since we became aware of it in the spring of 2003, well after completing this analysis.
28. We also estimated a regression with the Mrozek and Taylor set of control variables, with an important exception of replacing the hourly earnings variable with the natural logarithm of annual income. With an OLS specification, we estimated an income elasticity of 0.76 with a 95 percent confidence interval of 0.20–1.32 ($n = 41$). In the robust regression with Huber weights specification, we estimated again a point estimate for the income elasticity of 0.76, but with a much tighter 95 percent confidence interval of 0.73–0.79 ($n = 38$). The income variable was statistically significant at the 5 percent level in the OLS regression and significant at the 1 percent level in the robust regression. The iterative weighting process in robust regression with this much larger set of control variables effectively eliminated several low-VSL studies relative to specification 6 in Table 8, which may have resulted in the larger elasticity.
29. The Science Advisory Board (2000) has supported EPA's adjustment of the VSL for income growth.
30. We also calculated the median predicted VSL values for the full and U.S. samples. For the full sample, the median predicted VSL never exceeded the mean predicted VSL in any of the six regression models by more than 14 percent, and the mean predicted VSL never exceeded the median predicted VSL by more than 15 percent. For the U.S. sample, the median predicted VSL exceeded the mean predicted VSL in all six regressions, but never by more than 7 percent.
31. For example, Dorsey (1983) estimated a statistically significant negative coefficient on the fatality risk variable and a statistically significant positive coefficient on the union x risk interaction variable. This combination (with the union x risk coefficient greater in magnitude than the fatality risk coefficient) results in a positive compensating differential for union workers and a negative differential for non-union workers.
32. Rosen's research yields results similar to the Jones-Lee (1976), Jones-Lee, Hammerton, and Phillips (1985) and Shepard and Zeckhauser (1982, 1984) finding that the value of a statistical life takes an inverted-U shape with respect to age. Also refer to Garber and Phelps (1997) for an assessment of quality-adjusted life-years (QALYs) in the cost-effectiveness literature. Jenkins, Owens, and Wiggins (2001) provide some evidence that the value of a statistical life is increasing over childhood through the pre-retirement years.
33. Modifying utility functions in this way would be analogous to constructing QALY measures, common to valuations based on health status in the health economics literature. The QALYs are usually based on stated preference methods (see Cutler and Richardson, 1997).
34. Based on Rosen's life cycle model, one would expect that the marginal value of another year is greater for an elderly person than for a middle-aged person but that the value of all future years declines with age for a given individual. Accounting for health status may counter the effect of increasing marginal values for a one-year life extension for an elderly person. If health status is decreasing in age, then it may be ambiguous whether the marginal value of another year increases with age.
35. Baranzini and Ferro Luzzi (2001) also conducted such an analysis of the Swiss labor market with the same method as used by Moore and Viscusi (1988b). Consistent with the results for the U.S. labor market, they found that the value of a statistical life is increasing in life expectancy. In Switzerland, they estimate that a 50-year old values his life less than one-half as much as a 35-year old.
36. For example, refer to the guidance to improve regulatory decision-making provided by the OECD (1995) to its member countries.
37. For a review of the substance of this analysis, see Viscusi (1992a).
38. Refer to Table E-1 at Internet: http://www.api.faa.gov/economic/EXECSUMM.PDF and http://www.api.faa.gov/economic/742SECT2.PDF.
39. The low VSL used by the agency also reflects a political dimension as well. In the early 1990s when Viscusi prepared the report that was subsequently published as Viscusi (1993) for the Federal Aviation Administration, that branch of the agency favored a higher value of life than was later mandated for use throughout the department.
40. Note that the Science Advisory Board to the U.S. Environmental Protection Agency considered the effects of latency and dread within the context of cancer-related mortality. The SAB (2000) concluded that the literature

does not currently support a modification of the value of a statistical life to reflect dread. In contrast, the SAB did recommend that economic analyses account for latency by discounting future mortality to the present time consistent with the approach to other categories of benefits and costs.

41. The conversion to U.S. dollars for the Hara Associates analysis was based on the annual market exchange rate reported by the Federal Reserve (refer to Internet: http://www.federalreserve.gov/releases/g5a/current/) because the Penn World Table does not provide conversions for the most recent years.
42. While Revesz suggests other adjustments to the value of a statistical life based on the qualitative characteristics of the risk, such as involuntariness of the risk, the Science Advisory Board (2000) noted that the existing literature does not justify adjustments for these effects.
43. See Magat, Viscusi, and Huber (1996).
44. This discussion implicitly acknowledges an important aspect of most occupational, safety, and environmental health risks: these risks are relatively small. In cases of certain, or near-certain death, the empirical economic evidence and this argument are not relevant (see our discussion in Section 1).
45. For example, the 1997 final rule for the national ambient air quality standard for ozone introduced the section on the regulatory impact analysis with the following: "As discussed in Unit IV of this preamble, the Clean Air Act and judicial decisions make clear that the economic and technological feasibility of attaining ambient standards are not to be considered in setting NAAQS, although such factors may be considered in the development of State plans to implement the standards. Accordingly, although, as described below, a Regulatory Impact Analysis (RIA) has been prepared, neither the RIA nor the associated contractor reports have been considered in issuing this final rule" (62 FR 38856).
46. This relationship is also revealed within a cross-section of the U.S. population. Viscusi (1978b) found that work-related risk exposure decreases with worker wealth.
47. Portney and Stavins (1994) question whether the income losses from regulations would reduce the population's health status. They claim that the nonlinear relationship between income and health and the modest impact of most regulations on the economy would not likely have a significant effect on health status. While individual regulations may involve small costs as a share of the economy, it is important to note that environmental regulations alone cost about 1–2 percent of U.S. economic output (U.S. OMB, 2001).
48. We have updated the reported values in Keeney (1990) to 2000 U.S. dollars using the CPI-U deflator.
49. The Keeney estimates are near the high end of 11 studies cited in Lutter and Morrall (1994) on the income gains necessary to avert one fatality. They are very similar to Lutter and Morrall's estimated range of \$11.4 million to \$15.2 million for the United States.
50. Refer to *UAW v. OSHA*, United States Court of Appeals for the District of Columbia Circuit, 89-1559.
51. A recent paper by Gerdtham and Johannesson (2002) attempts to address this problem by controlling for initial health status in regressions of mortality risk on income (with other relevant controls) for a study focusing on Sweden in the 1980s and 1990s. They find that an income loss ranging from \$7.5–\$10.8 million would induce an expected fatality in Sweden. The range reflects variation in the progressivity of the burden of the regulation.
52. Viscusi (1994a, 1994b) selected a VSL estimate of \$5 million (in 1992 US\$) because it represented the midpoint of the basic range for VSLs of \$3 million–\$7 million in Viscusi (1993). In 2000 US\$, the midpoint would be \$6.1 million.
53. Refer to Morrall (1986) and Tengs et al. (1995) for lists of regulations' cost-effectiveness for the United States. Tengs et al. found that for 124 environmental regulations (primarily toxin control), the median cost per life-year saved is \$3.3 million. About 15 percent of the environmental regulations exceeded \$100 million per life-year saved. Converting these life-year values to statistical life values would result in a significant number of regulations with incredibly exorbitant cost-effectiveness measures. Life-saving regulations in other sectors of the economy (e.g., health care, transportation) had much lower costs per life-year saved. Refer to Ramsberg and Sjoberg (1997) for an evaluation of the cost-effectiveness of lifesaving interventions in Sweden.
54. Hahn, Lutter, and Viscusi only considered regulations with reduced mortality risk benefits comprising at least 90 percent of total monetized benefits.
55. Note that the construction of discounted statistical lives saved reflects both an accounting for life-years saved and discounting for latency.

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APPENDIX I

VSL Studies Used in Three Meta-Analyses
with original citations

Hedonic Wage Studies			
Study	Mrozek & Taylor	Viscusi & Aldy	Kochi et al.
Arnould & Nichols, 1983		√	
Arabsheibani & Marin, 2000			√
Arthur, 1989	√		
Berger & Gabriel, 1991	√	√	
Brown, 1980	√	√	
Butler, 1983	√	√	√
Cousineau, Lacroix, and Girard, 1992	√		√
Desvousges & Banzhaf, 1995	√		
Dickens, 1984			√
Dillingham, 1979	√		
Dillingham, 1985	√	√	√
Dillingham & Smith, 1983	√		
Dillingham & Smith, 1984		√	
Dorman & Hagstrom, 1998	√	√	
Dorsey, 1983			√
Dorsey & Walzer, 1983	√	√	√
Ehrenberg & Schumann, 1982	√		
Fisher & Violette, 1989	√		
Garen, 1988	√	√	√
Gegax, Gerking, and Schulze, 1991	√	√	√
Georgiou, 1992			√
Gill, 1998			√
Hammitt, 2000	√		
Hammitt & Graham, 1999	√		
Herzog & Schlottmann, 1990	√	√	√
Jones-Lee, 1974	√		
Kniesner & Leeth, 1991	√	√	
Leigh, 1987		√	√
Leigh, 1991	√	√	
Leigh, 1995	√	√	√
Leigh & Folsom, 1984	√	√	√
Liu, Hammit and Liu, 1997	√		
Liu & Hammit, 1999	√		
Lott & Manning, 2000		√	
Low & McPheters, 1983	√	√	
Mann, 1994	√		
Manning & Mullahy, 2001	√		
Marin & Psacharopoulos, 1982	√		√
Marsh & Layne, 2001	√		
Martinello & Meng, 1992	√		√
Meng, 1989	√		√
Meng & Smith, 1990	√		√
Miller, Calhoun, and Arthur, 1989	√		
Miller, 1990	√		
Miller, 2000	√		
Miller, Mulvey, and Norris, 1997			√
Moore & Viscusi, 1988a	√	√	√

Hedonic Wage Studies			
Study	Mrozek & Taylor	Viscusi & Aldy	Kochi et al.
Moore & Viscusi, 1988b	√	√	
Moore & Viscusi, 1990a	√	√	
Moore & Viscusi, 1990b		√	
Neumann & Unsworth, 1993	√		
Olson, 1981	√	√	√
Office of Management and Budget, 1996	√		
Sandy & Elliot, 1996			√
Scotton & Taylor, 2000			√
Siebert & Wei, 1994			√
Smith, 1974	√	√	√
Smith, 1976	√	√	√
Smith & Gilbert, 1984		√	
Smith & Gilbert, 1985		√	
Smith & Huang, 1995	√		
Smith & Osborne, 1996	√		
Thaler & Rosen, 1975		√	
Thaler & Rosen, 1976	√		
United States Environmental Protection Agency, 1997	√		
United States Environmental Protection Agency, 1999	√		
United States Nuclear Regulatory Commission, 1995a	√		
United States Nuclear Regulatory Commission, 1995b	√		
Viscusi, 1978	√	√	√
Viscusi, 1979		√	√
Viscusi, 1980	√		√
Viscusi, 1981	√	√	√
Viscusi, 1992	√		
Viscusi, 1993	√		
Viscusi & Moore, 1989		√	
Vodden, Meng, Smith, et al. 1993	√		
Wooldridge, 2000	√		

Selected CV Studies			
Study	Mrozek & Taylor	Viscusi & Aldy	Kochi et al.
Buzby, Ready, and Skees, 1995			√
Carthy, et al., 1999			√
Corso, Hammitt, and Graham, 2000			√
Desaigues & Rabl, 1995			√
Gerking, et al., 1988			√
Hammitt & Graham, 1999			√
Johannesson, Johansson, and Lofgren, 1997			√
Johannesson, Johansson, and O'Conor, 1996			√
Jones-Lee, 1989			√
Kidholm, 1995			√
Krupnick, et al., 2000			√
Loomis & duVair, 1993			√
Ludwig & Cook, 1999			√
Miller & Guria, 1991			√
Persson, Norinder, Hjalte, et al., 2001			√
Viscusi, Magat, and Huber, 1991			√

Hedonic Wage Studies

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Appendix J

Bibliography of New VSL Studies

Recent Value of Statistical Life Literature

(compiled via an EconLit search on 3/16/04 of studies published between 1995 and 2003 and cross-referenced against versions of the Mrozek and Taylor; Kochi, et al; and Viscusi and Aldy meta-analyses)

Studies that likely provide empirical VSL estimates

Baranzini, Andrea; Ferro Luzzi, Giovanni. 2001. The Economic Value of Risks to Life: Evidence from the Swiss Labour Market. *Swiss Journal of Economics and Statistics* 137(2): 149-70.

Abstract: In this paper, we use the hedonic approach to estimate the value of a statistical life based on the 1995 Swiss Labour Force Survey (SLFS) and the 1994 Swiss Wage Structure Survey (SWSS). Roughly, the value of a statistical life in Switzerland ranges from CHF 10 to 15 million (6.5-9.5 million current US$). However, more important than the absolute value, our estimates should be taken as an indicator of the value of a statistical life, which is of an order of magnitude higher than previous studies on the value of life in Switzerland, none of which is based on the hedonic approach. Our study confirms previous literature, since the value of statistical life varies with risk level, union coverage, age, and model assumptions. In particular, by separating between individuals with union coverage and those without, we find a slightly higher (but not significantly so) risk premium for the former, which runs counter the idea that in Switzerland unions bargain for safety measures. Finally, along the lines of Moore and Viscusi (1988), we take into account the discounted life years lost, and find a discount rate not significantly different from zero with SLFS and even negative with the SWSS sample.

Black, Dan A.; Kneiser, Thomas J. 2003. On the Measurement of Job Risk in Hedonic Wage Models. *Journal of Risk and Uncertainty* 27(3): 205-20.

Abstract: We examine the incidence, form, and research consequences of measurement error in measures of fatal injury risk in U.S. workplaces using both Bureau of Labor Statistics and National Institute of Occupational Safety and Health data. Of the various measures examined the NIOSH industry risk measure produces implicit value of life estimates most in line with both economic theory and the mode result for the existing literature. Because we find non-classical measurement error that differs across risk measures and is not independent of other regressors, innovative statistical procedures need be applied to obtain statistically improved estimates of wage-fatality risk tradeoffs.

Madheswaran, Subramaniam; Shanmugam, Kumarapalayam R. 2003. Impact of Trade Unions on the Compensation for Job Risks: Evidence from the Indian Labor Market. *RISEC: International Review of Economics and Business* 50(1): 121-41.

Abstract: This paper examines the workers' behavior in choosing their job risks and the role of trade unions in influencing the wage-risk trade-off. The empirical results provide a strong support for the efficiency of labor market in deriving the optimal risk level. The calculated value of statistical life is Rs.15.55 million and Rs.5.49 million and the estimated value of statistical injury is Rs.5598 and Rs.2059 for the union and non-union sector workers respectively. Comparison of our estimated value of life with those from developed nations indicates that our value is lower than the values from developed nations. The estimated results may have important implications for policy and can be used to value reductions in risk of death achieved by industrial safety programs or environmental health programs.

Perreira, Krista M.; Sloan, Frank A. 2002. Living Healthy and Living Long: Valuing the Nonpecuniary Loss from Disability and Death. *Journal of Risk and Uncertainty* 24(1): 5-29.
Abstract: This analysis uses three valuation approaches--risk-risk tradeoff, paired risk-dollar comparison, and utility function estimation--to estimate the nonpecuniary cost associated with disability in late life. In addition, we obtain an estimate of the value of life using a paired risk-dollar comparison. The data were obtained from interviews with 548 persons using an iterative computerized questionnaire. Respondents reported a median value of life of $12 million. They were willing-to-pay .7-1.4 million to avoid disability in late life or approximately $47-$95 thousand for each year of disability over age 62. The results were robust to the valuation technique employed.

Persson, Ulf, et al. 2001. The Value of Statistical Life in Transport: Findings from a New Contingent Valuation Study in Sweden. *Journal of Risk and Uncertainty* 23(2): 121-34.
Abstract: This article presents the results of a contingent valuation study from Sweden aimed at estimating the value of a statistical life (VOSL) in road traffic safety. Data on respondents' own subjective risk was collected by use of visual aids presented in a mail questionnaire. The relationship between willingness-to-pay (WTP) and absolute risk reduction was estimated by using a non-linear, least absolute deviation estimation method. This study generated an income-adjusted VOSL of SEK22.3 million (US$2.6 million). Analysis of WTP's sensitivity to probability variation indicates that in future studies, valuing risk reductions in road traffic, the magnitude of absolute risk and relative risk reductions to consider should be in perceptible range. On addition it should also be possible for respondents to compare the magnitudes of different risk reductions.

Viscusi, W. Kip. 2003. Racial Differences in Labor Market Values of a Statistical Life. *Journal of Risk and Uncertainty* 27(3): 239-56.
Abstract: This article constructs measures of job fatality rates for black and white workers using information on job-related fatalities from 1992-1997. The fatality rates for black employees are somewhat greater than those for whites. Each of these groups receives significant compensating wage differentials for fatality risks, controlling for nonfatal risks and expected workers' compensation benefits. The implicit value of a statistical life is lower for black workers than for whites. These results in conjunction with evidence that blacks receive less annual compensation for fatality risks than do whites imply that black and white workers face different market offer curves that are flatter for blacks than for whites.

Other Estimates (e.g., Illness, Injury, Healthy Life-Years, Jury Awards)

Abelson, Peter. 2003. The Value of Life and Health for Public Policy. *Economic Record* 79(0): S2-13.
Abstract: Expenditure on health and safety is a substantial part of GDP, but public agencies in many countries, including Australia, have only qualitative views about the value of life and health. Also, despite considerable work by economists on the value of life and health in recent

years, some important issues, such as the value of a healthy life-year, remain unresolved. This paper presents a framework for valuing life and health. It then draws on international and Australian research to estimate possible values for life, healthy life-years, and various chronic and acute health states for public policy purposes in Australia.

Lalive, Rafael. 2003. Did We Overestimate the Value of Health? *Journal of Risk and Uncertainty* 27(2): 2003.
Abstract: Adam Smith's idea that wage differences reveal preferences for risk rests on strong theoretical foundations. This paper argues, however, that the dominant approach to identify compensating wage differentials--regressing individual wages on aggregate measures of risk--may lead to arbitrary estimates of these risk differentials. In a dataset with information on both, the incidence of illnesses or injuries across firms and industries, I calculate an implicit value of one injury or illness of about (1990) USD 18,800 pursuing the dominant approach. In contrast, regressing wages on the incidence of risk across firms produces a value of one injury or illness of about USD 11,300.

Smith, Stan V. 2000. Jury Verdicts and the Dollar Value of Human Life. *Journal of Forensic Economics* 13(2): 169-88.
Abstract: Most economic studies estimate the value of life to range from two to four million dollars. These estimates have served as a standard for governments and corporations in gauging the cost of safety, and for juries in determining awards for the loss of enjoyment of life. Better research on the value of life should give judges greater comfort in allowing economists to testify to such values. This study compares the value of life reported in the economic literature with the value of life implied by jury awards in drunken driving cases. The author uses regression analysis and the present value of impairment, to estimate how juries value life, and concludes that juries value life at $2.3 million to $4.9 million. While jury awards vary considerably, the regressions explained up to 50 percent of the variation in awards, providing further evidence that juries are rational in their deliberations on such matters.