Publication selection and the income elasticity of the value of a statistical life

Hristos Doucouliagos, T.D. Stanley, W. Kip Viscusi

School of Accounting, Economics and Finance, Deakin University, Melbourne, Australia
Department of Economics and Business, Hendrix College, Conway, AR, USA
Vanderbilt University Law School, Nashville, TN, USA

Abstract

Estimates of the value of a statistical life (VSL) establish the price government agencies use to value fatality risks. Transferring these valuations to other populations often utilizes the income elasticity of the VSL, which typically draw on estimates from meta-analyses. Using a data set consisting of 101 estimates of the income elasticity of VSL from 14 previously reported meta-analyses, we find that accounting for potential publication bias the income elasticity of value of a statistical life is clearly and robustly inelastic, with a value of approximately 0.25–0.63. There is also clear evidence of the importance of controlling for levels of risk, differential publication selection bias, and the greater income sensitivity of VSL from stated preference surveys.

© 2013 Elsevier B.V. All rights reserved.

1. Introduction

The value of a statistical life (VSL) is the most important parameter used in monetizing the value of health, safety, and environmental risks. For example, the largest benefit component of regulations under the Clean Air Act consists of the reduction of mortality risks valued by VSL (EPA, 1997). Regulatory policies to reduce mortality risks from medical devices and transportation safety improvements can have large benefits when evaluated at large VSLs. Thus, it is essential to get the value of a statistical life right in order to allocate public resources efficiently (Viscusi and Aldy, 2003). Ideally, these values should reflect the willingness to pay of the population that will benefit from a mortality risk reduction.

Although estimates of the VSL vary greatly, most research surveys report the VSL to be somewhere between $6 million and $10 million U.S. dollars. This range in the literature has been reflected in similar clustering of the VSL estimates used by government agencies since the mid-1990s. Meta-analyses of the VSL literature have played a prominent role in agencies’ selection of the VSL.

Policy analysts generally face a benefits transfer problem in that the population in any VSL study may not be representative of the population affected by the policy. While adjusting for population characteristics remains a controversial issue (U.S. EPA, 2010; Viscusi, 2011), agencies have incorporated income elasticity adjustments into their analyses. The U.S. EPA (2010) adjusts its VSL estimates over time to account for the effect of rising income levels on the pertinent VSL. The U.S. Department of Transportation (2011) policy guidance document adopted an income elasticity of

1 Viscusi (2009) provides the VSL amounts used to assess 40 major government regulations, all of which include estimates of the VSL in the $6–10 million range after 1996.

2 Adjustments for age are less common and generated controversy in the U.S. (Viscusi, 2009), but Canada and the World Bank have adopted age adjustments to the VSL (Hammitt and Robinson, 2011).
VSL of 0.55 based on the meta-analysis results in Viscusi and Aldy (2003). More recently, the U.S. Department of Transport guidelines (2013) now use an elasticity of 1.0, splitting the difference between the meta-analysis results in Viscusi and Aldy (2003) and the quantile estimates in Kniesner et al. (2010). Because most VSL estimates are for the U.S. and other developed countries, assessing the pertinent income elasticity is essential to estimating the pertinent VSL in other countries based on the existing literature (Hammitt and Robinson, 2011). Such modifications are especially sensitive to the magnitude of the income elasticity when applied to low-income countries or populations.

This paper integrates, explains, and corrects 101 meta-analytic estimates of the income elasticity of the VSL. Consistent with the VSL meta-analysis in Doucouliagos et al. (2012), we find that once the estimated VSL’s income elasticities are corrected for observed publication selection bias, the average reported estimate is greatly reduced. We find that the VSL is a normal good, but not a luxury good. Correcting for potential publication bias yields an overall income elasticity of approximately 0.6 or as low as 0.25 when further adjusted for common misspecification biases.

2. Calculating the value of a statistical life and its income elasticity

The VSL can be estimated in a variety of ways from the choices that workers and citizens make regarding an actual or hypothetical increased risk of death. Most estimates are produced by wage-risk studies based on the tradeoffs implied by the estimated regression coefficients of the risk of fatal injury from a hedonic wage model. In this primary research literature, the dependent variable is usually the worker’s log wage, and one of many independent variables is the fatality risk. Because wages comprise much of worker income, these hedonic regressions usually cannot be further employed to estimate how VSL varies with income.

Evans and Smith (2010) note that the income elasticity of VSL can be measured in one of four ways: meta-analyses of hedonic wage studies, stated preference studies, single country comparisons of VSL at different points in time, or cross-country comparisons of VSL estimates. An additional approach involves the use of quantile regressions (Evans and Schaur, 2010; Kniesner et al., 2010). Nevertheless, the most common way to estimate the sensitivity of VSL to income is to ascertain how the VSL varies across studies. Because researchers use different samples of workers, their average income will naturally vary from study to study. Thus, a meta-analysis, which collects all comparable estimates of VSL, can provide that broader perspective needed to estimate the income elasticity of the VSL (Viscusi, 2012).

We conducted a comprehensive search for prior meta-analyses that report the income elasticity of VSL or which report regression results that could be converted into an income elasticity. The search was conducted using various search engines, including Econlit, JSTOR, Proquest, ScienceDirect, Scopus, and Google Scholar. We also pursued references in prior meta-analyses. Our search strategy and subsequent meta-analysis follows the MAER-NET guidelines for meta-analysis of observational data (see Stanley et al., 2013). This search strategy revealed 14 meta-studies from which it was possible to derive or calculate the income elasticity of VSL. These 14 meta-studies jointly report a total of 101 estimates of the income elasticity, and its standard error, of the value of a statistical life (see Meta-Analysis References and Appendix 1). In some cases, the estimates are derived from the same meta-study using the same data but a different specification. In other cases, different original studies are used to generate estimates of the income elasticity.

While these meta-analyses all incorporate a broad set of VSL studies, there is still great variation among these meta-regression estimates of the income elasticity of VSL, which range from −0.26 to 4. For theoretical and practical reasons, researchers should expect there to be some heterogeneity in the income elasticity. But, as we will demonstrate, much of the variation across studies arises from differences in methodology and publication selection. It is important for policymakers to have a clearer, more narrow, estimate of the average income elasticity of VSL and how it might vary with the choices that researchers make.

Generally, VSL estimates can be calculated by a two-step process (Hammitt and Robinson, 2011). In the first step, an overall VSL is estimated from a hedonic wage model or a contingent valuation survey of what people are willing to pay (WTP) for a hypothetical risk reduction. After an overall VSL value is estimated, it can be adjusted for the particular circumstances to which the policy will apply. This adjustment can primarily be made for differences in income from the sample used to estimate overall VSL value and the incomes of those likely affected by the new policy. These adjustments are therefore very sensitive to the income elasticity of VSL, and this sensitivity is especially compounded when applied to low-income countries (Hammitt and Robinson, 2011). Because most VSL estimates come from developed nations with relatively high incomes, it is essential to have an accurate income elasticity estimate to extrapolate to low incomes. Among the 14 existing meta-analyses of VSL that we include on our study, 94% of the VSL estimates come from developed nations.

There are basic economic reasons to believe that the income elasticity will be greater than 1.0 and higher for low-income workers (Kaplow, 2005; Viscusi, 2010; Hammitt and Robinson, 2011). When VSL is income inelastic and extrapolated from a high-income sample to a low-income country (or population sub-group), the analyst gets values of VSL that seem to be too high given the expected lifetime income and consumption choices of very low-income workers. In these cases, it is possible for the ratio of the VSL to a worker’s discounted expected stream of future income to be much greater than in developed countries. For this reason, some analysts arbitrarily use a value of one for VSL’s income elasticity, which gives more modest estimates of VSL for low-income groups.

The primary way to estimate the income elasticity of VSL, η, has been to use meta-regression analysis. As already noted, we have found 14 such meta-analyses containing 101 estimates of the income elasticity of the value of a statistical life. Because primary studies that report estimates of VSL also typically report the average incomes of the sample of workers surveyed, it is rather easy to estimate η from a meta-regression of VSL estimates on average income. Generally, these meta-regression estimates are inelastic. For example, Viscusi and Aldy (2003) report income elasticities ranging between 0.50 and 0.60, while Doucouliagos et al. (2012) estimate VSL’s income elasticity to be only 0.2. Likewise, contingent valuation surveys also tend to give inelastic estimates of the income elasticity of VSL (Hammitt and Robinson, 2011).

An exception to these low estimates of the inelastic income elasticity for VSL is the value based on quantile regressions (Viscusi, 2010; Kniesner et al., 2010). Quantile regression analyses of large

---

3 Several VSL meta-studies could not be included as they did not include income in their meta-regressions.

4 An argument can be made that income elasticity might vary between groups or countries, e.g., some groups or countries can have greater taste for safety, regardless of income.

5 How one should undertake such an extrapolation to less developed countries will also depend on the extent to which the income elasticity varies for much lower income populations, which is beyond the scope of our study.
national surveys such as the Panel Survey of Income Dynamics yield separate estimates of the VSL obtained for different income groups along with the associated income elasticities. This procedure yields an average income elasticity of 1.44, but individual quantile estimates become larger (smaller) for lower (higher) incomes (Kniesner et al., 2010).

To maximize comparability of the income elasticity values included in our analysis, we include only the income elasticity estimates from meta-regressions. These studies provide the largest number of empirical estimates of income elasticity of VSL. Understanding what causes these estimates to vary from study to study is important in itself, and combining all of these estimates provides a better assessment of the overall average income elasticity and its variation.6

The one limitation of these meta-analysis estimates is that they are drawn from the average VSL and income estimates of the samples used. Hence, they may not be representative of the entire range of income (Viscusi, 2011). In contrast, estimates from quantile regressions use a broader range of incomes and can thereby estimate the distribution of the income elasticity. However, a quantile regression estimate is based on a single sample, which might contain some idiosyncratic feature or errors that might, as a result, produce an outlier (Viscusi, 2012).7

3. Meta-regression of the value of a statistical life

Meta-regression analysis is the regression analysis of all previously published or reported comparable regression estimates of a given parameter. It is a type of meta-analysis specifically developed for empirical economics (Stanley, 2001). Because the VSL is such a key policy parameter, there have been more meta-regression analyses of VSL, 14, than for any other economic research topic (see Meta-Analysis References). The advantage of meta-regression analysis (MRA) is that it minimizes random sampling (or estimation) error by averaging across many VSL estimates. Furthermore, MRA can objectively and statistically control for any idiosyncratic research approach, technique, or other dimensions that might be suspected of inducing bias. For example, some datasets do not contain sufficient information to control for all of the many worker characteristics that have been shown to be important in explaining workers’ wage. Omitting a relevant variable is well known to potentially create a bias in the regression coefficient of interest, in this case the coefficient on fatality risk. Experience has shown that controlling for omitted-variable bias is essential, and acknowledging these potential biases does much to explain the wide variation among reported empirical economic estimates (Stanley and Doucouliagos, 2012).

Generally, meta-analysts find that VSL is between $6 and $10 million (US dollars) and that it is income inelastic. However, 29% of the income elasticities are greater than one. Aside from Doucouliagos et al. (2012), none of these meta-studies corrected for publication (or reporting) bias, and this correction makes a huge difference, reducing VSL to less than $2 million in that study.

3.1. Publication bias

Publication bias is a form of selection bias where by researchers choose which estimate to report based on statistical significance and having the ‘correct’ sign. (“Studies that find insignificant and wrong-signed values of compensating wage differentials have a more difficult time getting published” (Hwang et al., 1992, p. 855). Typically, for this literature, a log wage equation is estimated where the coefficient on the fatality risk variable is used to estimate VSL. If a researcher gets a negative coefficient, she is unlikely to report it believing that a negative VSL must be a signal of some important estimation or misspecification error. Even insignificant, positive VSLs are likely to be under-reported on the belief that reviewers and editors might use statistical significance as one criterion for publication (Card and Krueger, 1995). Publication bias is somewhat of a misnomer. Most researchers have already incorporated the perceived professional importance of statistical significance and will be less likely to report negative and/or insignificant VSL estimates even in their unpublished reports and working papers.

Nor are reviewers and meta-analysts immune to further selection bias. Doucouliagos et al. (2012, p. 199) give two examples:

For example, Kluve and Schaffner (2008) note that some studies in their literature search found negative or statistically insignificant estimates and these were excluded from their meta-analysis. In their dataset, Bellavance et al. (2009) include studies that report both negative and positive VSL estimates, but only positive estimates are included in their meta-analysis.

In this paper, we document that publication selection (or reporting) bias is also found among the meta-regression estimates of VSL’s income elasticity. Correcting this selection bias transforms an average income elasticity that is not significantly less than one to an elasticity that is significantly inelastic with a value of approximately 0.6 or less.8

3.2. A graphical illustration of publication selection bias

The best way to illustrate, literally, publication selection or reporting bias is through a funnel graph. The funnel graph is a scatter diagram of an estimate’s precision (the inverse of the estimate’s standard error or 1/SE) and the magnitude of this estimate (horizontal axis). Funnel plots have been widely used to detect publication bias among clinical trials on medical treatments. As the name suggests, a funnel graph should resemble, roughly, an inverted funnel when there are no selection or reporting biases. The symmetry of a funnel graph is quintessential. Alternatively, when a significant number of values are missing from one side or the other (that is, when the funnel is skewed or asymmetric), this may indicate selection bias.

Fig. 1 presents the funnel plot for all 101 meta-analytic estimates of VSL’s income elasticity, $\eta$. Clearly, this graph is skewed to the right, indicative of the suppression of negative and, perhaps, insignificant income elasticities. Also, there might be selection for elastic values (e.g., $\eta > 1$). For theoretical reasons, some researchers may wish to find income elasticities greater than or equal to one, or at least not significantly less than one. In spite of how revealing you might find this graph, the inherently subjective nature of visual interpretation means that the graph alone cannot establish publication bias or determine the corrected income elasticity. Rather, objective statistical testing is required. Fortunately, a series of tests and estimates have been developed for exactly this purpose.

6 This type of meta-analysis of prior meta-analyses is known as meta-meta-analysis. See Stanley and Doucouliagos (2012) for details.

7 The problem lies not in meta-analysis as a technique but in the estimates available to apply to it. Thus, over time when more quantile regression studies become available, it will be possible to apply meta-analysis to the distribution of income elasticities from these quantile regressions.

8 Selection for income elasticities can occur either as a result of selection for positive and statistically significant VSL estimates or as a deliberate process of selecting both VSL and income elasticity estimates. As in the case of VSL estimates, however, the existence of bias does not mean that all authors have engaged in this process.
3.3. Meta-regression models of selective reporting

A simple meta-regression model of estimated effects and their standard errors can be used to model and test for publication (or reporting) bias:

\[ \eta_i = \beta_0 + \beta_1 SE_i + \epsilon_i, \]

where \( \eta_i \) is an individual estimate of the income elasticity of VSL and \( SE_i \) is its standard error, \( \beta_1 SE \) models publication selection bias, and estimates of \( \beta_0 \) serve as corrections for publication bias. The essential idea is that studies with larger SEs (and generally smaller samples) will need to engage in more intensive selection to find a statistically positive \( \eta_i \). See for example Stanley (2005, 2008) and Stanley and Doucouliagos (2012). Note that as \( SE \to 0 \), \( \epsilon \) -> \( \beta_0 \). The funnel-asymmetry test (FAT), \( H_0: \beta_1 = 0 \), is a low power test for the presence of selection (Egger et al., 1997), while the precision-effect test (PET), \( H_0: \beta_1 = 0 \), indicates whether there is a genuine effect beyond publication selection (Stanley, 2008).

This FAT–PET–MRA, Eq. (1), is never estimated by OLS because empirical economics estimates always contain great heteroskedasticity, as directly evidenced by the wide variation found among the reported standard errors, \( SE_i \). Weighted least squares, WLS, estimates of MRA model (1) can be obtained either dividing MRA (1) by \( SE_i \):

\[ t_i = \beta_1 + \beta_0 \left( \frac{1}{SE_i} \right) + \nu_i \]

or by using a WLS statistical package on MRA (1) with \( 1/SE_i^2 \) (the inverse variance) as the weights.

Column 1 of Table 1 reports our meta-meta-estimates for the weighted least squares version of MRA model (1). Note first that there is clear evidence of both reporting selection (reject \( H_0: \beta_1 = 0; t = 3.61; p < .001 \)) and a genuinely positive overall income elasticity of VSL (reject \( H_0: \beta_0 = 0; t = 6.99; p < .001 \)). Of course, there could be other reasons, for the funnel asymmetry. Perhaps it is the result of heterogeneity in model specifications that is coincidentally correlated with SE or maybe it is some type of small-sample bias. To allow for heterogeneity in how the models were specified, we embed these selection terms into a multiple MRA that also controls for important model specification choices as well as difference across countries, measures, and data—see Section 5, below. In spite of these controls, evidence of selection or small-sample bias remains. Regardless of the source of the bias, bias is bias, and we seek to minimize its influence on this important policy parameter. Fortunately, accommodating and correcting either publication bias or small-sample bias is precisely what these MRA models of publication selection do.

The estimated intercept, \( \hat{\beta}_0 \), from the FAT–PET–MRA (1) is an estimate of the overall income elasticity corrected for publication bias. Note how it is only half as large as the simple reported average of these 101 elasticity estimates (0.90). Although the average value of this literature is not significantly less than one \( (t = -1.46; p > .05) \), \( \hat{\beta}_0 \) is much less than one \( (t = -8.55; p < .001) \).

The income inelasticity of VSL is also borne out by a very simple correction for publication bias, focusing on the most precisely estimated income elasticity values. The measure, which we designate as the Top 10, is calculated by discarding 90% of research with the lowest precision and averaging the remaining 10% most accurate estimates, those at the top of the funnel graph. The Top 10 value is 0.61. Although Top 10 was developed merely to highlight how publication selection creates a statistical paradox, simulations show that it provides a rather good corrected estimate, one that is typically better than \( \hat{\beta}_0 \) (Stanley et al., 2010). Regardless, correcting for publication selection (or reporting or small-sample) bias makes a difference in whether the VSL may be considered a luxury or a necessity, and this has important policy implications when, for example, extrapolating to low incomes.

Unfortunately, \( \hat{\beta}_0 \) is known to be biased downward when there is a true nonzero effect (Stanley, 2008; Stanley and Doucouliagos, 2013). This bias is due to using a linear approximation to represent a complex non-linear relation between \( \eta_i \) and \( SE_i \) (for details see Stanley and Doucouliagos, 2013). In the place of MRA model (1), simulations show that the simple MRA that substitutes \( SE_i^2 \) for \( SE_i \) offers an improved, less biased, corrected estimate (Stanley and Doucouliagos, 2012, 2013). That is, \( \hat{\beta}_0 \) in Eq. (3) below,

\[ \eta_i = \gamma_0 + \gamma_2 SE_i^2 + \nu_i \]

typically provides a better corrected estimate. This estimator is sometimes called ‘PEESE’ from precision-effect estimate with standard error (Stanley and Doucouliagos, 2012, 2013). Table 1 columns 3 and 4 estimates MRA (3), above, for this meta-regression income elasticity data and finds \( \hat{\beta}_0 = 0.62 \), with a 95% confidence interval of 0.44–0.80. The value of PEESE is larger than \( \hat{\beta}_0 \), as expected, but still much less than one \( (t = -11.1; p < .001) \). Note how the PEESE is virtually identical with the simple Top 10, suggesting that 0.6 is, approximately, a robust overall estimate of the income elasticity of the value of a statistical life for this research literature. This figure lies within the upper range of the income elasticity derived by Viscusi and Aldy (2003).

Some have pointed out that theory requires VSL’s income elasticity to be at least one. For example, there is a relationship between the income elasticity of VSL and the coefficient of relative risk aversion (CRRA, Kaplow, 2005). Estimates of CRRA in conjunction with

---

9 When MRA model (1) is reported with cluster-robust standard errors, column 2 Table 1, evidence of publication bias is less clear. The funnel-asymmetry test (FAT) is widely known to have low power, and Egger et al. (1997) recommend using \( \alpha = .10 \). Using this larger significance level and cluster-robust standard errors confirms publication selection for statistically significant positive income elasticities. In any case,
that model imply that VSL should be greater than 1.10,11 Others see elastic values to be necessary to extrapolate the VSL to low-income applications. Given such professional priors, it is possible that some of the reported elasticities are selected to be greater than one, or at least not statistically less than one. Therefore, it would be prudent to allow for differential publication selection to accommodate the selection of elastic values should this research contain such selection. The simplest way to allow for two types of reporting selection is to expand the publication bias term to:

$$
\eta_i = \beta_0 + \beta_1 E_i + \beta_2 Elastic_i \cdot E_i + \xi_i,
$$

where $Elastic_i$ is a dummy variable indicating whether an income elasticity is statistically significantly less than one ($Elastic_i = 0$) or not ($Elastic_i = 1$).12 The term $\beta_2 Elastic_i \cdot E_i$ in MRA model (4) allows for differential publication selection for income elasticity, while $\beta_1 E_i$ accounts for potential selection for statistically positive or negative income elasticity found among inelastic reported estimates.

Table 1, columns 5 and 6, give the MRA results for this differential publication bias model (4). These findings confirm the need to make this further accommodation of differential selective reporting. First, we easily reject $H_0$: $\beta_2 = 0$ ($t = (10.6; 39.3); p < .001$). Second, the explanatory power increases greatly when differential publication selection is allowed, increasing the adjusted $R^2$ from 11% to 58%. Allowing more or less publication (or reporting) selection in terms of whether a reported VSL elasticity is significantly less than one or not explains nearly one half of the observed variation among these 101 income elasticities. But then, a rise in explanatory power is not unexpected as $\beta_2 Elastic_i \cdot E_i$ is defined in a way that is related to large income elasticities. The negative estimate coefficient for $\beta_1$ is likely an artifact of how we have reclassified the research as those with an income elasticity significantly less than one ($Elastic_i = 0$). Thus, the set of estimates that have $Elastic_i = 0$ are much less likely to be significantly positive. Our reclassification therefore creates its own selection bias in a negative direction for those estimates where $Elastic_i = 0$, because many significantly positive estimates will be selected out of this group. However, note that the sum of these two publication selection terms ($\beta_1 + \beta_2$), is quite close to what is reported in columns 1 and 2 of Table 1 for the overall publication selection effect.

When there is no publication selection bias ($SE_i = 0$), this model of differential publication (or reporting) selection gives virtually an identical corrected estimate of VSL’s income elasticity as Top 10 and PESEE, 0.63.

Columns 2, 4, and 6 of Table 1 provide the first robustness check for our MRA results. Others will be discussed below. Because the typical meta-analysis of VSL reports multiple income elasticities (over 7, on average), there might be some dependence among estimates in the same study. This is especially true when there is a pattern of selective reporting as indicated by all of our tests. One acceptable way to accommodate potential dependence within studies is to calculate cluster-robust standard errors—see columns 2, 4 and 6 of Table 1. Although the $t$-values change in all cases, the overall implications about publication bias or the size of the corrected income elasticity do not. Another way to deal with potential dependence of having multiple estimates per study is to use fixed and random-effects unbalanced panel models, which we use in the multiple meta-regression models reported in Section 5, below.

Although these simple meta-regression results are very revealing, we must also be sure that they remain robust when other potential explanatory variables are considered. Perhaps what appears as publication selection bias is merely chance correlation with how VSL is measured or how the MRA model is specified? It is conventional practice in meta-regression analysis to embed these models of publication selection into larger explanatory MRAs (Stanley and Doucouliagos, 2012). Section 5 considers multivariate MRAs and further investigates the robustness of the above findings. First, however, we illustrate how meta-regression estimates of the income elasticity of VSL can be so different and how they might be selected for either statistical significance or, alternatively, to be elastic.

### 4. Estimating the income elasticity of the value of a statistical life

To illustrate the wide variation that one can obtain for VSL’s income elasticity, we use data from one of the 14 meta-analyses of VSL (Bellavance et al., 2009). Their meta-analysis contains 39 estimates of VSL from hedonic wage equations, which Doucouliagos et al. (2012) used to correct the VSL estimate for publication selection. It is important to note that obtaining a VSL income elasticity was not the objective of Doucouliagos et al. (2012). It was reported
as merely an ancillary byproduct. The point to this qualification is that there was no intentional selection for either the size or signification of the income elasticity. No doubt, other meta-analysts also did not explicitly or knowingly select for positive or significant income elasticities. However, perhaps a few MRAs produced negative or insignificant income elasticities, and those meta-analysts used this as evidence that their MRA was misspecified. In any case, if a researcher chooses to do so, it would be easy to get virtually any value he wanted.

Table 2 explicitly models VSL to estimate VSL’s income elasticity. Because log-log models are the most straight-forward and the most often used, we begin with the log-log version of the MRA model reported in Doucouliagos et al. (2012, Table 3 column 1). Indeed, 65% of the meta-analysis estimates of VSL’s income elasticity use a log-log MRA. Oddly, this MRA gives a negative, though insignificant, income elasticity when applied to Bellavance’s et al. (2009) 39 estimates of VSL (see column 1 Table 2). If taken seriously, this would imply that VSL does not depend on income and need not be adjusted when applied to low-income countries.

The next columns of Table 2 merely change the set of moderators variables included in the MRA to see how robust (or sensitive) the income elasticity estimate is to changes in exact model specification. Column 2 of Table 2 omits the time trend and yields a statistically significant negative income elasticity. If instead, the meta-analyst fails to allow for publication selection bias, which 97% of this literature fails to do, VSL’s income elasticity becomes significantly positive and larger than one (column 3 Table 2). If, on the other hand, the researcher omits both a time trend13 and publication selection with or without accounting for the presence of compensation insurance (Compensation), one obtains significantly positive but inelastic values (see columns 4 and 5 of Table 2). Without employing entirely unreasonable specifications, the researcher may easily get a significantly negative income elasticity, an elasticity that is not significantly different than zero, a significantly positive one that is not statistically less than one, or a significantly positive income elasticity that is clearly and statistically less than one. All of these widely dispersed estimates of VSL’s income sensitivity are compatible with the existing VSL research, any one of which might be obtained by an honest researcher.

Nor is this merely an academic exercise. The majority of reported meta-regression VSL income elasticity estimates throughout this literature omit all or most of these important explanatory variables. Therefore, how do we know which estimates to believe or which estimates come from correctly specified models of the value of a statistical life? Largely, we do not. Researchers should routinely use specification tests, but editors and reviewers do not require their usage; thus, they are rarely reported.14 Furthermore, with many tests to choose from and their well-known low power, these tests are also likely to give a wide variety of results, easily confirming any a priori notion the researcher has about VSL.

Nonetheless, we report the results from Ramsey’s RESET in Table 2. Recall that Ramsey’s RESET test is sensitive to omitted-variable, misspecification, and simultaneity biases, among others. Because of the low power of such tests, rejections of proper specification are to be believed more than a failure to find improper specification. Log-log models that use all of these variables or that do not contain a time trend are rejected as misspecified (p < .05). The exception is when all moderator variables are omitted from explaining VSL other than income (Column 5 Table 2), which is a common approach in this literature. However, we can be confident that this simple MRA model is misspecified because it omits several relevant explanatory variables—Compensation, time trend, and publication or reporting selection. In contrast, the model used by Doucouliagos et al. (2012, Table 3 column 1) that includes all of these variables when only income is a logarithm easily passes RESET (F(3,31) = 0.63; p ≻ .05).

5. Modeling heterogeneity and bias among VSL’s income elasticity estimates

As clearly demonstrated in the above section, VSL’s income elasticity is highly sensitive to unavoidable modeling choices. Thus, like all other meta-analyses in economics, it is important to ensure that our findings about the overall magnitude of η are robust to potential heterogeneity and misspecification bias. Doucouliagos and Stanley (2009) and Stanley and Doucouliagos (2012) offer a very general MRA model that can accommodate any complexity of genuine heterogeneity, misspecification biases, selection biases, and study-effects, S_t:

\[ \eta_{\text{VA}} = \beta_0 + \beta_1 Z_{\text{VA}} + \beta_2 S_{\text{VA}} + \sum \delta_j S_{\text{VA}} K_{\text{VA}} + S_{\text{VA}} + \xi_{\text{VA}}. \]  

Where the Z-variables account for heterogeneity and filter out potential misspecification biases, and the K-variables allow for potential differential selection. In this application, we find that the K-variable, Elastic, \( S_{\text{VA}} \), from Eq. (4) is particularly important.

---

13 There is no theoretical reason for a time trend to exist in income elasticities. Theoretically, it is possible that over time VSL becomes more elastic, less elastic, or time invariant.

14 Viscusi and Aldy (2003) is an exception to this.
Table 3

Moderator variables and inclusive MRA results.

<table>
<thead>
<tr>
<th>Moderator variable</th>
<th>Definition</th>
<th>MRA coefficient (t-value)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Publication selection</td>
<td>is the standard error of the reported estimated elasticity</td>
<td>0.50 (1.14)</td>
</tr>
<tr>
<td>SE Elastic</td>
<td>allows differential publication selection</td>
<td>1.54 (5.83)</td>
</tr>
<tr>
<td>Model specification variables</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log Log</td>
<td>= 1, if the MRA model is in a log–log form</td>
<td>0.10 (0.678)</td>
</tr>
<tr>
<td>Log Lin</td>
<td>= 1, if the MRA model is in a log-linear form</td>
<td>−0.44 (−1.53)</td>
</tr>
<tr>
<td>Linear</td>
<td>= 1, if the MRA model is linear</td>
<td>−0.44 (−0.48)</td>
</tr>
<tr>
<td>Risk</td>
<td>= 1, if the MRA model includes a measure of risk</td>
<td>−0.39 (−3.95)</td>
</tr>
<tr>
<td>Education</td>
<td>= 1, if the MRA model includes an education variable</td>
<td>0.22 (0.24)</td>
</tr>
<tr>
<td>Cluster robust</td>
<td>= 1, if the MRA model reports cluster-robust standard errors</td>
<td>0.08 (0.73)</td>
</tr>
<tr>
<td>Robust</td>
<td>= 1, if the MRA model is estimated by a robust regression</td>
<td>−0.001 (−0.01)</td>
</tr>
<tr>
<td>Random</td>
<td>= 1, if the MRA model is estimated by random-effects</td>
<td>−0.03 (−0.42)</td>
</tr>
<tr>
<td>OLS</td>
<td>= 1, if the MRA model is estimated by OLS</td>
<td>0.02 (0.20)</td>
</tr>
<tr>
<td>Data</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Stated preference</td>
<td>= 1, if all estimates come from stated-preferences studies</td>
<td>1.25 (2.44)</td>
</tr>
<tr>
<td>Wage Risk</td>
<td>= 1, if all estimates come from wage-risk studies</td>
<td>0.15 (1.15)</td>
</tr>
<tr>
<td>Developed</td>
<td>= 1, if all estimates come from developed countries</td>
<td>−0.03 (−0.92)</td>
</tr>
<tr>
<td>US</td>
<td>is the proportion of estimates that come from the US</td>
<td>0.09 (0.23)</td>
</tr>
<tr>
<td>Europe</td>
<td>is the proportion of estimates that come from Western Europe</td>
<td>−6.58 (−2.69)</td>
</tr>
<tr>
<td>Canada</td>
<td>is the proportion of estimates that come from Canada</td>
<td>−1.45 (−1.89)</td>
</tr>
<tr>
<td>Australia/NZ</td>
<td>is the proportion of estimates that come from Australia or NZ</td>
<td>−6.52 (−1.63)</td>
</tr>
<tr>
<td>Japan</td>
<td>is the proportion of estimates that come from Japan</td>
<td>7.08 (2.19)</td>
</tr>
<tr>
<td>Developing</td>
<td>is the proportion of estimates from developing countries</td>
<td>1.03 (0.57)</td>
</tr>
<tr>
<td>Ave. year</td>
<td>is the average year of the data used</td>
<td>−0.008 (−0.20)</td>
</tr>
<tr>
<td>Intercept</td>
<td></td>
<td>16.64 (0.21)</td>
</tr>
</tbody>
</table>

However, we do not stop at a multivariate explanation of heterogeneity and selection. To allow for potential dependence of reported estimates in the same study we also estimate MRA model (5) with fixed and random study-specific effects in an unbalanced panel design. These effects are represented by $S_t$ in Eq. (5). Lastly, like our above MRAs, we accommodate heteroskedasticity by using weighted least squares.

Table 3 lists 21 moderator (or explanatory) variables and their estimated MRA coefficients when all are included in a single meta-regression model. The variables listed in Table 3 can be classified in three broad categories: publication selection, model specification or potential misspecification bias, and data.

Because there is substantial multicollinearity, we also report the general-to-specific (G-to-S) MRA results in Table 4. G-to-S begins by including all 21 moderator variables coded. The variable that had the largest p-value is removed, one at a time, until all remaining explanatory variables are statistically significant. Because of limited degrees of freedom and high multicollinearity, some simplification of MRA models is necessary. The general-to-specific approach is the least objectionable way to do so (Charemza and Deadman, 1997).

As we saw in Section 4, the income elasticity can be highly affected by the functional form of the MRA model used. For this literature, log-log models give significantly larger (more elastic) estimates—MRA coefficient = 0.20; t = 4.33; p < .001; see column 1 Table 4. Consistent with the simple MRA models of publication selection reported in Table 1, we again find differential publication selection bias (MRA coefficient Elastic SE = 1.79; t = 6.84; p < .001). Other important effects are: whether the estimating MRA controls for risk levels (t = −5.52; p < .001), whether estimates are exclusive from stated-preference studies (t = 3.85; p < .05) or wage-risk studies (t = 2.54; p < .05), as well as regional effects (Japan and Europe). This multiple MRA model and its individual effects are robust—see Table 4. Because researchers typically report multiple estimates per study, it is necessary to control for the potential dependence of estimates within studies. Column 1 of Table 4 does so by computing cluster-robust standard errors, while columns 2 and 3 treat this research data as an unbalanced panel using either random study effects (RE-panel) or fixed study effects (FE-panel), respectively. Note how all effects are robust to different estimation methods, except that one effect becomes statistically insignificant when estimated by a fixed-effects panel. However, for our data, the Hausman test accepts the RE-panel MRA, column 2 ($\chi^2(8) = 6.28, p > .05$). In case a few ‘outlier’ estimates of VSL’s income elasticity might be driving our results, we also estimate this MRA model using robust regression and obtain virtually the same findings in column 4 of Table 4. Of special note is the fact that differential publication selection (Elastic SE), controlling for risk (Risk), and Stated Preference have robust and statistically significant effects even when all moderator variables are included (Table 3). Furthermore, when these three effects are accommodated, all potential systematic variation is explained ($\chi^2(97) = 88.9, p > .05$), and this is also true for the MRA model reported in Table 4 ($\chi^2(93) = 71.1, p > .05$). Lastly, our multiple MRA easily passes RESET ($F(6,84) = 1.35; p > .05$). Thus, we can have confidence that our meta-regression model provides an acceptable explanation of the systematic pattern found among reported income elasticities of the value of a statistical life.

This table cannot be regarded as reliable because they are based on extrapolations well beyond our MRA sample. In particular, the largest proportion of estimates that come from western Europe is 0.286 for any of our MRA VSL income elasticities with mean 0.053. For Japan, its largest proportion is 0.385 with mean 0.074.

15 Six variables have variance-inflation factors (VIF) larger than 100.
16 Although these regional coefficients would imply that VSL is income elastic in Japan and has a negative income elasticity in western Europe, such inferences cannot be regarded as reliable because they are based on extrapolations well beyond our MRA sample. In particular, the largest proportion of estimates that come from western Europe is 0.286 for any of our MRA VSL income elasticities with mean 0.053. For Japan, its largest proportion is 0.385 with mean 0.074.

17 This test is applied to the t-value WLS-MRA, Eq. (2), but one with the added moderator variables. When we have only sampling error and no excess heterogeneity, we would expect the t-values to have a variance of one (Higgins and Thompson, 2002; Stanley and Doucouliagos, 2012). This gives a chi-square test for whether or not the variance is one after the systematic variation from the moderator variables is factored out.
As an additional robustness check, we replace the potentially endogenous Elastic SE variable that we constructed from elasticity estimates not significantly less than one, with the exogenous, SE², used by the PESEE-MRA, Eq. (3), in column 5 Table 4. Overall, the findings remain largely the same; however, meta-analyses that use only wage risk estimates, WageRisk, or a double log specification are no longer significantly different from meta-analyses that use a mix of estimates or other specifications.

But what does this MRA model say about the magnitude of VSL’s income elasticity? What values should be substituted into the MRA reported in Table 4? With a multiple MRA, one must make some allowance for the appropriate values of the moderator variables. Although reasonable researchers might have some differences of judgment regarding the appropriate values of this research’s explanatory variables, we think that most of these choices are uncontroversial, and we explore plausible variation in professional judgment below.

First, the differential publication term, Elastic SE, must be driven to zero, because it represents a bias. Next, Risk should be set to one, because researchers should account for the risk level. Our meta-analysis clearly shows that risk level affects income elasticity estimates, and the failure to control for risk level, therefore, would seem to cause omitted-variable bias. From our exploration of the Bellavance et al. (2009) wage-risk data, RESET provides clear evidence that the log-log MRAs are misspecified (recall Section 4), implying that LogLog should be set to zero. This leaves only the values of Stated Preference, WageRisk, and the regional variables in question. For those, one might be purposefully ambivalent and substitute in their sample average values. Substituting these values into the MRA model reported in column 1 Table 4 gives an overall VSL income elasticity of 0.25 (CI: 0.17; 0.34) and is close to the value (0.20) reported by Doucouliagos et al. (2012, p. 202). If you believe that there is nothing wrong with using the log-log specification, we could instead substitute the sample mean in for LogLog, and VSL’s income elasticity increases to 0.38 (CI: 0.30; 0.47). On the other hand, if we use the multiple MRA that substitutes SE² for Elastic SE, column 5 Table 4, the implied income elasticity is nearly the same, 0.37, (CI: 0.25; 0.49), or 0.47 if LogLog is set to its sample mean. To attempt to make these predictions as large as possible, one might consider only U.S. stated preferences studies, 0.42 (CI: 0.22; 0.62). Even here, we obtain an inelastic estimate, but one that is consistent with both our simple publication-bias corrected estimates and the values preferred by Viscusi and Aldy (2003). To justify an income elasticity that is one or larger, the meta-analyst would need to purposefully choose a likely biased log-log estimate, consider only stated preferences studies that contain mostly Japanese surveys and none from Europe. Defensible applications of our multiple MRA findings imply that the overall income elasticity is much less than one, and even less than what the simple MRA models of publication selection (or small-sample bias) suggest (recall Table 1). Thus, we have robust evidence that income elasticity of the value of a statistical life is less than one.

### 6. Conclusions

Meta-analysis has become a common method from which to derive VSL estimates. Recent evidence suggests that it is important to correct meta-analysis for publication selection bias. This results in significantly lower estimates of VSL. Another important parameter estimate is the income elasticity of VSL. Here too, meta-analysis have been widely used to derive estimates of this important policy parameter. Again, however, it is important to correct the evidence base for possible publication selection, small-sample, or other potential sources of bias. We show that correcting for potential, perhaps inadvertent, selection bias yields an income elasticity of VSL that is significantly less than 1, practically and statistically. Although all of our preferred estimates of the income elasticity fall between 0.25 and 0.63, most are in the upper range (0.5; 0.63). The Top 10 estimates of 0.61 and the PESEE estimate of 0.62 were remarkably similar. In any case, our findings are consistent with the estimates from Viscusi and Aldy (2003) that have been used by at least some U.S. agencies. Viscusi and Aldy (2003) derived their estimate by constructing a range of income elasticities using specification from prior meta-analysis as well as their own specifications. We adopt a different methodology and yet find a similar magnitude, though wider range, of VSL’s income elasticity. A series of studies has questioned the prior findings of income inelastic VSL. However, a meta-meta-analysis of 101 estimates from 14 prior meta-studies of VSL’s income elasticity in developed countries provides robust results that VSL is indeed income inelastic.

### Appendix 1. Studies included in the meta-meta-analysis, chronological order

---

18 Relying on stated preference studies exclusively, however, is problematic. VSL estimates from stated preference studies tend to be lower than those from revealed preference studies (such as those from hedonic wage regressions). Viscusi (2012, p. 3) notes that this arises in part because stated preference studies “address the hypothetical risks that people may not view as a real threat” whereas hedonic wage regressions report estimates based on actual observed behavior.
References


Meta-Analysis References


