

Comments from individual members of the Science Advisory Board Environmental Economics Advisory Committee to assist meeting deliberations. These comments do not represent consensus SAB advice or EPA policy.
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**Section-by-Section Compilation of Environmental Economics Advisory
Committee (EEAC) Member Comments on the Committee's Draft (5-5-16) VSL
Report
(As of 6/14/16)**

The following EEAC member comments are grouped according to each section of the Committee's draft (5-5-16) VSL report. The compilation contains all comments received from Committee members as of 6/14/16. The Committee Chair has highlighted specific comments for discussion and has provided suggestions (in underlined text) for addressing other comments.

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Overarching Comments

Suggestion: discuss whether overarching recommendations section should be added to address these comments.

Terminology (Jim Opaluch). Do we recommend somewhere that EPA replace the term “Value of a Statistical Life” with “Value of Risk Reduction”. I think this is important because it is easy for non-specialists to misunderstand that VSL is *really* a value of risk reduction. But many will interpret it as placing a value on life, no matter how carefully you try to explain it. And even before you get a chance to explain what VSL is, you have already lost a significant fraction of your audience, who are morally opposed to putting a dollar value on life, and reject the concept of VSL without understanding it.

This also raises the question of whether we should recommend that VSL only be used in the case of small risks applied to a large population. For example, I don't believe that VSL should not be applied to a case of a 100% probability of death of a specific individual, as the life is no longer a “statistical” one.

Adjustment versus conceptual issue associated with logic of benefits transfer (Kerry Smith) I feel there is a difference between putting estimates in a common year dollars versus transforming the estimate to reflect a particular model. These issues arise in constructing the data for the meta- analysis. The white paper and our responses to them muddle the reactions. This type of treatment is a mistake and in my opinion must be changed if I am to agree with the report. The first type of adaptation is simply for consistency. Adjustment for income growth, combining Hicksian and Marshallian estimates as if they are equivalent without theoretical justification, applying something other than sample based weights to construct weighted estimates for a population that original estimate was not intended to represent are modeling assumptions that should be separately identified and justified. They should not be part of transformations to data prior to a meta-analysis. This way the assumptions being made are not properly vetted.

Sources of Risk Tradeoff measures (Kerry Smith) -- There is discussion of preference calibration and citation to applications of the method in the context of VSL work without recognizing the general point that I believe was being made. That is, outside of environmental economics and labor market applications --economists have attempted to reconcile different estimates of risk preferences with other parameters. Chetty's paper in the December 2006 AER is an example where the coefficient of relative risk aversion is related to labor supply elasticities; other economists --Murphy and Topel, Hall and Jones, Weitzman have all done similar "transfer" of efforts to reconcile evidence. This was not a plug for preference calibration but rather a recognition with a general model there are connections between parameters being measured. Does EPA want to build this into the research design they adopt for developing and

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evaluating the ways they select risk estimates and the way they conduct benefit transfers. This is a generic question --not a proposal they need to look at a specific study.

Developing a data platform (Kerry Smith) -- The CPS and COFI are public data sets that can be combined with appropriate attention to assuring confidentiality. Given the importance of the VSL for policy --EPA should commit to establishing a consistent framework for how risk measure should be distinguished --whether by industry and occupation and at what level of detail. This would be an ongoing activity --not a collection of estimates by EPA staff. The effort is akin to the effort in BLS on generic development of data and indexes for the CPI. This dimension of the recommendation for estimating VSL's; posting data bases; examining protocols for revising indexes etc.

Primary Research Questions (Kerry Smith)-- The relationship between Marshallian estimates of MRS of wage hedonic and Hicksian approximate estimates of this ratio has not been worked out to my knowledge. It depends on the nature of the CV question. The committee chair asked for a reference to literature --if there is none it does not means they can simply be combined until something is published. For me it means they cannot be combined until we can establish this relationship --another example of why burying benefit transfer issues in data construction causes us to overlook issues

Introduction

Page 8, line 5. (Jim Opaluch) I recommend that we say “estimates” rather than “takes into account”.

Suggestion: incorporate change.

Page 8, line 18. (Jim Opaluch) change literature to literatures and add comma, so it reads “... stated preference and hedonic wage study literatures, and...”

Suggestion: incorporate change.

Page 8, lines 23-31. (Jim Opaluch) Fix the 19 charge questions and six topic areas as I discuss above in my comments on the Executive Summary.

Suggestion: incorporate change.

Page 8, line 34. (Jim Opaluch) We should also report the conference calls.

Suggestion: conference calls will be reported in the final report.

Section 3.1.1 – Meta-Analysis Dataset (Lead Writer – Kevin Boyle)

Issue – Clarification of guidance on study validity. (Mary Evans – also see comments from Jim Opaluch below). The guidance on the issue of validity generally needs clarification. In addition, the recommendations with respect to construct validity seems contradictory. In this section,

the report notes that “a scope failure, while reason for concern, does not mean a value estimate is invalid” and “failure of construct validity does not necessarily imply validity.” However, later the report recommends eliminating estimates from the Viscusi, Huber, and Bell study on the basis that they do not “provide clear evidence of study validity (i.e., sensitivity to scope).

I’m concerned about two of the additional validity dimensions suggested (the 3rd and 4th) as they require a significant degree of subjectivity to determine what is meant by “clearly”.

To discuss

Page 9. (Jim Opaluch) I like listing the full charge questions.

Suggestion: No change needed.

Page 9, line 17. (Jim Opaluch) Should we emphasize that this was work of another committee? e.g. “A previous committee of the SAB ...”

Suggestion: incorporate change.

Page 9, line 26. 9 (Jim Opaluch) the word “that” is repeated.

Suggestion: incorporate change.

Page 10, lines 3-8. (Jim Opaluch) The definitions here should be more explicit with the definitions. For example, we could say “Construct validity confirms whether a study uses a methodology that is scientifically credible for measuring the quantity that the study is intended to measure.” “Construct validity tests whether estimates in a study conform to expectations, so the estimated values of commodities vary with factors that are expected to affect value (e.g., the scope of the commodity), and do not vary with factors that are expected not to matter (e.g., procedural invariance). Criterion validity confirms whether estimates are consistent with other measures that are presumed to be the “true” measure.

Suggestion: incorporate text to make definitions more explicit.

Page 10, paragraph on lines 10-17. (Jim Opaluch) I recommend provide more explicit definitions in the previous paragraph, and only indicate the consequences in this paragraph. For example, we could say:

“There is no perfect study and no absolute test of validity. Content validity only reduces the likelihood and/or size of an error by ensuring the underlying methodology is a scientifically sound approach for measuring the intended quantity. Similarly, construct validity contributes to the credibility of an estimate by ensuring it conforms with theoretical expectations of the measure. Criterion validity is the strongest concept of validity as it speaks directly to bias. However, the outcome of a test of criterion validity is only credible if there is an available “true”

measure that can serve as the criterion. It is rare that we have a “true value” criterion against which to gauge an estimate from a study. ...”

One other point on this paragraph. Although construct validity is most commonly discussed within the context of stated preference studies, it applies to all studies, not just stated preference studies.

Suggestion: Incorporate suggested changes.

p. 10, line 21, (Richard Carson) see section below which explicitly rejects the need to find no order effect in order for a study to be valid.

Page 10, line 31. (Jim Opaluch) “effects”

Suggestion: Incorporate change.

Page 10, line 33: (Stephen Swallow) Revise to read “...willingness to pay to increase for a larger...”

Suggestion: incorporate change.

Page 10, line 34: (Stephen Swallow) Revise to read “...stated preference question was placed...”

Suggestion: incorporate change.

Page 10, lines 32-33. (Jim Opaluch) “It is logical to expect a larger willing to pay for a larger reduction in risk ...”

Same paragraph. I recommend we reorder the sentences in this paragraph. How about:

Examples of tests of construct validity include scope tests and tests for question ordering effects, among others. A scope effect occurs when individuals are willing to pay more for a more inclusive commodity, as compared to a less inclusive commodity. For example, it is logical to expect an individual to be willing to pay more for a larger reduction in risk than for a smaller risk reduction. Therefore, a study fails to demonstrate construct validity if the results do not show higher willing to pay for a larger risk reduction. In contrast, the value of a given commodity should not depend on the ordering of questions in a survey. Therefore, a study fails to demonstrate construct validity if the value of risk reduction varies depending on the ordering of questions.

Suggestion: incorporate changes.

Page 10, line 44. (Kerry Smith). “Thus, failure of a test of construct validity typically requires additional investigation to understand if the failure is evidence of invalidity or that the stated preference estimate is valid and unbiased.”

As I have suggested before --we cannot omit consequentiality; work by Carson and Groves, Carson Groves and List and Vossler --all published emphasize the importance of consequentiality for the incentive properties of CV studies; I realize that most of the stated preference research was done before this result was widely known. Nonetheless I think we need to highlight it.

p. 10, lines 46-47, (Richard Carson) words “and unbiased” should be dropped. Unbiasedness is not a standard condition for validity. Many valid estimates are useful if the direction of the bias is known and for decision problems it is common to minimize mean square error rather than use the minimum variance unbiased estimator.

Suggestion: Agree to drop “unbiased”.

P 11, line 10. (Kerry Smith) “In order to strengthen the assessment of study validity and better inform a weight of evidence decision to include or exclude a study, the SAB recommends that the EPA expand the consideration of evidence of validity to include answers to the following key questions:...”

What about the consequentiality of the choice question itself --this is Carson and Groves and Carson Groves and List point; Vossler has also shown its importance in other contexts.

To discuss

P 11, line 11. (Kerry Smith) “...the SAB recommends that the EPA expand the consideration of evidence of validity to include answers to the following key questions:”

One could put here a question --what procedures were used to enhance likelihood of being perceived as consequential.

p. 11, lines 23-31 and later points in the draft, (Richard Carson) - Issue – requirement to use only peer reviewed studies. The discussion of studies not appearing in peer reviewed journals is often confusing. The recommendation should be that EPA commission formal peer reviews of reports and theses/dissertations that contain potentially useful estimates. This is a relatively low cost way to increase the set of available studies and there are numerous reasons (given later in the draft) that studies do not end up in a usable form in peer-reviewed journals. Peer-review should be seen by EPA as a way of insuring quality and this is sometimes at odds with journal publication practices that favor novelty. (Carson)

To discuss: Does the Committee want to recommend that only peer review studies be used in the analysis? The report currently mentions the importance of considering whether a study is peer reviewed, but calls for the use of gray literature studies that have been reviewed by EPA.

Page 11, line 2. Issue: Clarification of guidance on study validity – (Jim Opaluch) I’m not sure why we say “not every study needs to conduct ... a construct validity test. Shouldn’t we just say “not every study conducts a construct validity test.”

Same paragraph. We need to be more clear here that we are moving from construct to content to criterion validity to avoid confusing the reader. I think it would be good to simplify this discussion.

More generally, I think the section entitled “Evidence of Study Validity” should be better organized, and we should provide clear guidance. As it stands, the section kind of goes back and forth, and appears to have contradictory statements—some of which sound contradictory, but are not necessarily contradictory). For example, the sentence on page 11 lines 5-6 seems to contradict the first part of the sentence starting on page 11 line 2, which says “not every study needs to ... conduct a construct validity investigation.” The sentence saying “it is logical to expect” a scope effect seems to be contradicted by the sentence on page 10 lines 37-39 about non-negative wtp.

I would also argue that the logical underpinnings of the VSL approach fail if we do not impose the expected utility theory, linear-in-probabilities form as a maintained hypothesis. If we do not impose expected utility theory linear-in-probabilities, then we can't define the “commodity” to be valued as a statistical life, which is calculated by taking a small change in risk and multiplying by the size of the affected population. The notion of a statistical life is inherently linear in probabilities. For example, a 1-in-a-million mortality risk applied to 1 million people is one statistical life. And a 1-in-10-million mortality risk applied to a population of 10 million people is also one statistical life. If risk values are not linear in probabilities, then these two cases should not both be treated as equivalent in terms of the social risk (one statistical life) we should not use VSL at all. Using VSL but not imposing linearity in probabilities is not logically consistent. Questioning the linearity-in-probabilities is a good academic exercise, but should not be recommended to EPA unless we are prepared to recommend that EPA drop a statistical life as the social risk metric.

But the situation might not seem as bad as it might seem, as many sensible social and individual objectives imply that the risk management problem should be treated as one that is linear in probabilities. For example, if we want to allocate a given highway safety budget across projects (e.g., improved lighting, crash barriers, etc) so as to minimize fatalities, then probabilities of the various highway risks should be treated linearly our investment decision. Similarly, if an individual wants to minimize the risk of accidental death from multiple possible accidents (highway death, falls at home, etc.), then each risk should be treated as linear in probabilities. In both cases, adopting VSL as the decision criterion would lead to the proper decisions.

In particular, I would argue against a strict adherence to a criterion of non-negative for the marginal value of risk reduction, as it implies satiation with respect to risk reduction, especially when comparing across risks. Risk reduction is not like other commodities. I might like both chicken and fish, without my utility function being linear in either one. I might become satiated with chicken, and now prefer fish. But this is different than the case with two different risks.

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Extending this same logic across risks means I'm safe enough from automobile accidents, so I don't care about reducing risk of automobile fatality. Now I only care about dying from a fall.

Reduction of one particular risk is not like ice cream or other commodities, where satiation is reached fairly quickly. I would argue that much more significant consideration is that risk preferences vary across people. Morbidity considerations aside, it is not unreasonable for us as a society to have an objective based on minimizing total fatalities (i.e., linear in risk), rather than objective with different types of fatalities each have their own non-linear risk weights (e.g., $\ln(\text{risk}_i)$ or $\exp(\text{risk}_i)$).

To discuss

Page 11, line 15. (Jim Opaluch) I fear that a random sample might be too much to ask for, if by random we mean a "representative" sample. For example, just about any survey will have non-respondents, and potential for nonresponse bias. And it is difficult or impossible to obtain a truly representative sampling frame. I also fear that a "clearly specified population" might include cases of a highly specialized one (e.g., members of a church or other organization). It might be better to say the sample be a "broad representation of a general population".

Suggestion: incorporate change.

Page 11, line 19-20. (Jim Oplauch) How strongly should we push for a binary choice survey?

To discuss

Page 10-11. (Jim Opaluch) All the discussion of evidence of study validity focusses on stated preference surveys. We should be sure to say something about revealed preference, hedonic wage models.

To discuss

P 12, line 40. (Kerry Smith) "It is important that the knowledge and assessment of study validity evolve through time as research progresses. Future updates of the VSL should consider advancements in the literature pertaining to study design, conduct, and testing relating to validity. An example of this is the current evolution in the literature extending incentive compatibility through consequential survey designs (Carson, Groves and List 2015)."

Why isn't this brought up in the context of the validity of CV; the first introduction of this discussion was by Carson and Groves in 2007; Google scholar suggest 800+ citations so this insight is well recognized –published 9years ago; and available long before that; I think this recommendation does not go far enough –need to assess the bias in earlier work –that point would be consistent with the discussion in earlier sections.

To discuss

Page 12, lines 20-22. (Jim Opalauch) What if we recommend different degrees of compliance with validity standards, and one could examine sensitivity to inclusion of studies. For example, 1. complies with validity standards, 2. marginal, 3. does not comply, 4. no test feasible. We might then provide one or more examples each.

To discuss

Page 12, line 26. (Jim Opaluch) Do we mean “individual estimates within a study” rather than “observations in a study”?

Suggestion: incorporate change.

Page 12, line 30-32. (Jim Opalauch) I’m not sure precisely what is intended by this sentence. An example might be useful here. Perhaps we should reorganize lines 30-36 to clarify our point here.

To discuss

Page 12, line 41. (Kerry Smith) “Future updates of the VSL should consider advancements in the literature pertaining to study design, conduct, and testing relating to validity. An example of this is the current evolution in the literature extending incentive compatibility through consequential survey designs (Carson, Groves and List 2015).”

I am glad to see this but it needs to be moved up with the questions and added to the executive summary.

Page 13, line 6. (Jim Opaluch) I recommend we say “EPA must fully document ...” rather than “... could be improved by identifying ...” I also recommend we say that the VSL calculations and analysis must be fully transparent, so they can be replicated by an independent third party analysis. Some of this documentation might be done in an appendix.

Suggestion: incorporate change.

P 13, line 6. (Kerry Smith) “The SAB finds that the White Paper could be improved by identifying: (1) all criteria for including studies in the meta-analysis, and (2) all manipulations of value estimates that were performed to convert the estimates to a homogenous metric that would support the meta-analysis (e.g., manipulation of the value estimates in a study that has a well-defined baseline risk and risk change but is not consistent with the other studies that are included in the meta-analysis).”

My comments distinguished manipulations that don’t affect validity to those that do –such as adjustment by income elasticity, treatment of CV estimates –mixing of Marshallian and Hicksian measures; discounting assumptions to get an annual value—etc; this comes up later –should there be links to issues that are related.

To discuss specific changes to be made to the text

P 13, line 18. (Kerry Smith) “Therefore, the SAB recommends careful documentation of studies that meet or do not meet validity criteria as evidenced in the answers to the key questions listed above.”

There is clear evidence in Carson and Groves and the Carson, Groves List paper that consequentiality is required to conclude that CV is incentive compatible; for me this means there needs to be a discussion of potential biases associated with nearly all the CV studies used by EPA.

To discuss

Page 13: (Stephen Swallow) Should we discuss the extent to which SP studies should be eliminated due to evidence of question ordering effects? Other studies may not have tested for order effects, and studies with order effects could have provided empirical parameters allowing an adjustment to control for order effects.

To discuss

Page 13: (Stephen Swallow) Criteria for “implausibly large or small (negative)” estimates should be elaborated. This criteria creates the possibility of a bias from a subjective judgment, while possibly excluding studies offering valid estimates on the tail of a valid distribution.

To discuss

Page 14. (Jim Opaluch) Bullets at top of page. We should also recommend that all calculations and adjustments in estimates need to be fully documented, so they can be replicated by an independent party.

Suggestion: incorporate change.

P 14, line 10. (Kerry Smith) “All future updates of the VSL should simultaneously consider whether the conditions for investigating study validity should be updated.”

I am not sure what this suggestion means; does it mean new evidence implies look at old accepted studies or what?

To discuss if we should eliminate this suggestion

Section 3.1.2 – Construct of Risk Variable in Hedonic Wage Studies (Lead Writer – Wayne Gray)

Issue – recommendation that hedonic wage studies use either occupation-by-industry or (just) occupation risk measures (Mary Evans). Before I’m comfortable recommending that included hedonic wage studies use either occupation-by-industry or (just) occupation risk measures, I’d like to see research investigating the differences in the estimated VSLs using the three primary CFOI risk measures: (i) industry, (ii) occupation, (iii) occupation-by-industry. Kip

Viscusi compares (i) and (iii) in his 2004 Economic Inquiry paper and I believe he may have another paper in which he compares all three but unfortunately I can't seem to recall the citation. Perhaps another SAB member recalls it. My primary concern is that the occupation-by-industry risk measures are available only for those researchers with a data agreement with the BLS. The necessary data to construct the industry risk measures and occupation risk measures are publicly available but historically many hedonic wage studies have used the industry risk measures. As a result of these two features, limiting the sample of hedonic wage studies to those that use either occupation or occupation-by-industry risk measures has the potential to severely limit the number of studies (and furthermore to limit the studies to a small group of researchers).

To discuss

Page 15, line 8. (Jim Opaluch) Add comma: "... existing research, and either..."

Suggestion: incorporate change.

Page 15, line 11 (Jim Opaluch) Drop the word "is".

Suggestion: incorporate change.

p. 15, lines 15-16, (Richard Carson) worker misperception of risk, which encompasses limited awareness, could lead to over or under estimation of VLS estimates.

Suggestion: incorporate change.

P 15, line 25. (Kerry Smith) "The hedonic VSL is related to a Marshallian willingness to accept which could overstate the Hicksian willingness to pay measure."

I would say extend of difference in this context –risk depends on the model

To discuss Comment not clear.

P 15, line 28. (Kerry Smith) "The SAB notes that a simple algebraic formula (and some assumptions) could identify how large an adjustment would be needed to convert the Marshallian measure to the Hicksian one."

Assumptions about the nature of preferences and format of treatment of risk.

To discuss

P 15, line 29. (Kerry Smith) "This approach is described in Smith, et. al. (2006). An assessment of the magnitude of this adjustment could be conducted immediately to determine the importance of this issue."

For simple models readily –I would remove immediately.

Suggestion: incorporate change.

P 15, line 33. (Kerry Smith) “The SAB also notes that existing hedonic wage studies are often based on different models or data from different sources and therefore it may be difficult to find future published hedonic wage studies that carefully apply existing hedonic wage models to new data.”

The point is that publication depends on innovations in methods –rarely new data.

Suggestion: incorporate point.

P 15. Line 35. (Kerry Smith) “The EPA should consider applying a consistent hedonic wage model to the available years of data, combining an industry and occupation risk measure from the U.S. Bureau of Labor Statistics Census of Fatal Occupational Injuries (CFOI) with the U.S. Bureau of Labor Statistics March Current Population Survey (CPS) wage information, and generating measures of VSL on a consistent basis. Estimates for future years should be added as the data become available. This research would be relatively inexpensive to conduct, and could be done by EPA staff or by other researchers.”

My recommendation was that these data be placed on a public web site; that the definitions of variables and construction procedures be well documented; EPA staff can certainly use them but I was more concerned about setting up a mechanism to make data publicly available.

Suggestion: incorporate point.

P 16, line 19. (Kerry Smith) “An assessment should be conducted to determine the magnitude of the adjustment needed to convert the hedonic (willingness to accept) VSL to a Hicksian willingness to pay measure.”

Note since these are approximations to MRS in case of CV; make assumptions about expected utility etc; difference will not be the same as conventional understanding for price or quality changes.

Discuss to determine what text change is needed.

P 16, line 22. (Kerry Smith) “A consistent hedonic wage model should be applied to the available years of data, combining an industry and occupation risk measure from the U.S. Bureau of Labor Statistics Census of Fatal Occupational Injuries (CFOI) with the U.S. Bureau of Labor Statistics March Current Population Survey (CPS) wage information, and generating measures of VSL on a consistent basis.”

I was recommending data –not necessarily measurement of VSL.

My recommendation was that EPA develop a web based link to these data and update the available data bases as they become available as a mechanism to encourage new VSL estimates and provide a convenient basis for evaluating new research strategies and for benefits transfer

I felt this should be part of the executive summary and profiled as a feasible activity that is given high priority.

Discuss to determine what text change is needed.

Section 3.1.3 – Estimates of Value of Immediate Risk Reduction (Lead Writer – Reed Johnson)

p. 17, line 3, (Richard Carson) this statement should be checked. It is inconsistent with the statement in the original paper (p. 363) that the study passes a scope test.

Suggestion: discuss whether Committee wants to say that Viscusi, Huber, and Bell (2014) does not provide clear evidence of study validity.

Page 17, line 3: (Stephen Swallow) Does SAB want to give EPA specific guidance on whether this means EPA should remove Viscusi et al. (2014) from the analysis, given earlier comments indicating that there are criteria for assessing validity other than sensitivity to scope (Pages 9-14)?

Discuss whether Committee wants to say that Viscusi, Huber, and Bell (2014) does not provide clear evidence of study validity.

P. 17, line 13. (Reed Johnson) “The SAB recommends broadening the scope of studies the EPA uses to derive values for reducing both mortality and morbidity risks. There are a significant number of published studies that estimate willingness to pay for improved health and reduced health risks (see studies listed in Appendix B of this report). There also is a burgeoning literature on benefit-risk and risk-risk tradeoff preferences in health and health care that could provide a basis for enriching the evidence base on risk preferences and providing support for benefits-transfer applications (see studies listed in Appendix C of this report).”

As noted, this same point applies elsewhere. Given realistic constraints, there isn't much room for improving estimates if the literature is defined as narrowly as EPA has done for many years. This isn't the only area of applied economics that has an interest in valuing health risks.

Suggestion: no revision indicated.

P 17, line 26. (Reed Johnson) Other Concerns about the Estimation of Willingness to Pay for Reduced Risk of Immediate Death.

Good summary of the discussion.

Suggestion: no revision indicated.

Page 17, lines 35-36: (Stephen Swallow) What is the EPA action that SAB could recommend? Do enough studies with this focus exist?

To discuss

p. 17, lines 38-39, (Richard Carson) sentence should be rephrased to say: “People may not be able to precisely evaluate long-latency risks, particularly when there is considerable uncertainty regarding timing of conditions.”

Suggestion: incorporate change.

Page 17, line 38: Issue – Statistical Life Years (Stephen Swallow) Is this a conjecture? Is there citable evidence? What is the actionable recommendation here?

Suggestion: this text does not provide recommendation, it lists concerns about EPA’s approach to estimation of willingness to pay for reduced risk of immediate death. Committee should discuss to determine whether change in text is needed and recommendation we want to make.

Page 18, lines 15-17: Issue – Statistical Life Years - (Stephen Swallow) While conceptually, this recommendation is sound (as are the comments on page17 lines 29-46), what is the practical approach that EPA should take? Can SAB explicitly say that it is more likely than not that sufficient literature exists on which to base the estimates in relation to statistical life-years lost? This leans toward a recommendation to replace the focus on valuation of immediate death. Failure to account for pain and suffering (morbidity and disability; quality of life changes) would imply a downward bias on the individuals’ losses due to causes of illness or impending death. Wouldn’t standard discounting of a delayed but otherwise immediate death also imply a downward bias?

Discuss to determine whether recommendation and supporting text should be removed or changed.

P 17, line 34. (George Van Houtven) “Discounting does not correctly account for the effect of time on VSL. Dying immediately means fewer years of life, not just a delay in a financial payment. A more correct construct would be the value of statistical life-years lost rather than the present value of a future statistical death.”

As mentioned above, I do not agree with this statement.

To discuss

P 17, line 44. Use of discount rate and recommendation concerning statistical life years. (Matthew Neidell) “The SAB finds that the selection of a three percent discount rate is arbitrary and recommends that the EPA use a more correct construct such as the value of statistical life-years lost rather than the present value of a future statistical death.”

Again, I don’t recall this. Do we have good estimate of value of statistical life-years lost? Even less of this than ordinary VSL.

To discuss

Discounting - P 18, line 15. (George Van Houtven) “Discounting does not correctly account for the effect of time on VSL. The SAB recommends that EPA use a more correct construct such as the value of statistical life-years lost rather than the present value of a future statistical death.”

Same comment.

To discuss

Page 18, line 45: (Stephen Swallow) Revise to: “...maybe subject to non-responses biases that would require EPA to address in calculation of a representative estimate of value.”

Suggestion: incorporate change.

Section 3.1.4 – Empirical Studies (Lead Writer – Stephen Swallow)

(Mary Evans) Recommendation concerning expanding set of studies used by EPA. Suggest that somewhere in this section, we note that the published empirical literature has moved in recent years towards studies that employ experimental or quasi-experimental methods for identification. Neither the stated preference studies nor the conventional hedonic wage studies fall within these categories. As a result, the number of published studies in either of these categories is likely to fall further over time. I am in favor of encouraging the EPA to expand the set of studies to those that employ experimental or quasi-experimental methods. Some of the citations in this section do so (Laura Taylor and Jonathan Lee also have a relevant paper that has an R&R at AEJ: Policy).

I am not in favor of suggesting that EPA use hedonic wage studies that apply data other than the CFOI. Instead I prefer the broader suggestion above.

Suggestion: Incorporate Change

Page 18, line 27. (Jim Opaluch) Do we really want to say there has been lack of significant growth in the literature, or do we want to say there is a lack of growth in the number of studies used by EPA? Elsewhere we indicate that there is significant growth in the literature on health studies, which is more appropriate than occupational fatalities.

Suggestion: incorporate change.

P 18, line 28. (Reed Johnson) “Therefore, the EPA may need to commission more studies or create other incentives for new studies in order to improve the prospect for a deeper literature to support future reviews of VSL.”

I would remove this advice everywhere it appears. The report should acknowledge the scarcity of and slow growth in directly relevant studies and just recommend that EPA cast a wider net. I see no realistic option for improving the credibility of health and mortality risk estimates used by the agency.

To discuss

Page 18, lines 27-34. (Jim Opaluch) Shouldn't we list additional studies that EPA should use, prior to indicating that EPA should fund additional research.

Suggestion: incorporate change.

Page 19, bullets. (Jim Opaluch) Again, doesn't make sense first to recommend EPA consider additional available studies, then go on to recommend that EPA commission new studies.

Suggestion: incorporate change.

Page 19 line 3 (and line 25). (Jim Opaluch) What is meant by "more detail or information"?

Suggestion: discuss to determine how text should be changed.

Page 19, line 25. (Kerry Smith) "The White Paper should contain more detail or information to allow readers to assess how the reliance on published studies, particularly other meta-analyses (including studies that drew from international data), might lead to results that differ due to publication bias, lags in publication, or other concerns."

Need to assess if there is enough variation in these dimensions to allow a separate assessment of their effects; could consider --tracking selected studies from working paper thru to publication and changes in results over time.

Page 19, line 28. (Jim Opaluch) how to address publication bias and lags in publication - I agree we need to provide more specific guidance. I'm not sure what we are recommending here.

Discuss to determine how to clarify the recommendation (or remove it).

P 19, line 25. (Kerry Smith) "The White Paper should contain more detail or information to allow readers to assess how the reliance on published studies, particularly other meta-analyses (including studies that drew from international data), might lead to results that differ due to publication bias, lags in publication, or other concerns <<Chair's note: a sentence could be added to suggest how EPA should do this.>>"

Consider tracking evolutions of a sample of working papers from first appearance to ultimate publication.

Discuss to determine how to clarify the recommendation.

Page 19, lines 30-31. (Jim Opaluch) We should also indicate that EPA should provide a rationale for excluding categories of studies (other than hedonic wage and stated preference)

Suggestion: incorporate change

Page 19-20. (Jim Opaluch) We need to clarify this sentence. I'm not sure what it means.

Discuss. Sentence that needs clarification is not identified.

Section 3.1.5 – Population Weighting in EPA’s Analysis (Lead Writer – Stephen Swallow)

Page 20, line 2: (Stephen Swallow) Revise to read “...of the studies draw on data that do not include some portion of...”

This changes the meaning to avoid the suggestion that perhaps the studies necessarily had data and excluded the data, rather than they may have just not had data on some portion of the population. The former may be the case in some studies, but not all.

Suggestion: incorporate change.

Page 20, lines 3-4. (Jim Opaluch) Excludes both older and younger individuals. This is especially important, since EPA regulations will tend to affect future environmental exposures, which will disproportionately affect individuals who are younger today. This raises a question, especially given an expectation of changing demographics. Assuming different demographic categories of individuals have different VSLs (e.g., young individuals vs. old) does this reflect fundamentally different risk preferences, or is there a cohort effect, whereby as young people age, their preferences become more like older individuals.

Suggestion: Change to indicate that older and younger individuals are excluded and include text raising the question about different risk preferences vs cohort effect.

Page 20, line 4. (Jim Opaluch) The last phrase seems awkward here. What is meant by “above a standard that each study set”.

Suggestion: remove “above a standard that each study set.”

P 20, line 21. (Matthew Neidell) “Weighting by population shares is common but may not cover all of the potential sources of selection bias, particularly for survey-based studies. The White Paper should more explicitly address the implications of selection bias. “

I don’t quite get this comment.

To discuss

Page 20, lines 13-14. (Jim Opaluch) Again, I recommend that EPA should provide adequate information for a reader to replicate their results.

Suggestion: incorporate comment.

P 20, line 25. (Kerry Smith) “Weighting approaches should to give much greater consideration to details of the specific studies being weighted. Population weighting and benefit-transfer weighting may involve different principles and relevance.”

I don't know what benefit transfer weighting means.

The white paper gives impression that weighting is comparable to using approximations for sampling weights --in some cases that is correct --eg the CV studies; however for hedonic wage studies that is not the case. I have commented repeatedly that in this case weighting in NOT sample weighting. It is more properly consider a benefits transfer. This discussion comments on issue but is not explicit enough in my opinion. I feel this distinction is important.

Discuss to determine how to clarify.

Page 20, lines 38-40. (Jim Opaluch) This sentence is really difficult to read.

Suggestion: edit.

P 20, line 41. (Kerry Smith) “Weighting to adjust for income differences in the populations (or time periods) in individual studies should be done after determining the estimates to be drawn from a particular study time period. Income adjustments should be addressed in the process of aggregating across studies to derive an estimate for a representative population. In this regard, there should be an explanation in the White Paper of how Hicksian and Marshallian measures of VSL should be aggregated with a consistent measure of income to account for income effects. <<Chair’s note: it would be helpful to provide a method and citation.>>”

This is confusing –if it is an attempt to reflect one of my comments; my points with respect to income were as follows:

- 1. Adjusting both VSL estimates derived from wage hedonic and CV studies for income must be consistent with the income concept relevant to each model; with wage hedonic models income is endogenous; so is it nonwage income that is adjusted?*
- 2. With CV this is not the case and expected utility is being held constant in the concept that is measured so analysis of proper treatment of income needs to reconcile these modeling assumptions before applying some adjustment.*

The comment on income applies to the measure of income used income elasticity as well; the analysis is an extension of Smith et al paper cited earlier.

To discuss

Page 20, line 42-43. (Jim Opaluch) I suspect that for small risks (e.g., 1 in a million), there will be a very small difference between Hicksian vs. Marshallian, but I agree it would be worthwhile showing this.

Suggestion: no change needed.

Page 21, line 14. (Jim Opaluch) We should be consistent, and either Capitalize both “Population” and “Census” or capitalize neither of them.

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Suggestion: capitalize the words.

Page 21, line 38. (Kerry Smith). “The weighting process is more complex for the hedonic wage studies. For the Viscusi and Aldy (2007) study, VSL measures were constructed for each of 5 age groups. Although separate hedonic wage models were estimated for 1998, the weights appear to be for 2013 for the entire population. No adjustment was made to account for the difference between those who are working and those who are not for a variety of reasons. As a result, in this case the weights appear not only to be for the wrong year but the wrong population. This approach mixes a benefit transfer issue (assuming non-workers have the same VSL as workers) with the construction of a population mean based on a sample. The SAB has similar concerns about the EPA’s weighting of the Aldy and Viscusi (2008) estimates and the weighting of any of the other hedonic wage estimates based on sub-populations. “

This captures my comments noted earlier but I feel it deserves more attention earlier as indicated and in the executive summary --especially when we discuss whether the procedures are adequate.

Page 22, line 3. (Jim Opaluch) It might be worth determining whether there are large variation in VLS across subpopulations, relative to variation across individuals, in order to decide how great an effort should be placed on weighting subpopulations.

Suggestion: incorporate comment.

Page 22, lines 5-36: (Stephen Swallow) Recommendation to improve the population weighting approach - Do members of the EEAC to SAB who are most knowledgeable about the available empirical literature believe that there is sufficient literature focused on subpopulations to allow this more complex approach to succeed in providing some improvement to derivation of a representative estimate of value for the national population?

Or is the recommendation on Page 23, lines 14-16 sufficient to cover this concern?

To discuss

p. 22, line 6, (Richard Carson) drop “more complex”.

Suggestion: incorporate change.

P 22. Line 8. (Kerry Smith) “The analysis would mix benefit-transfer and statistical benefit transfer in a more comprehensive, potentially more consistent way and enable the use of a wider spectrum of available studies to derive VSL estimates for subpopulations.”

I don't understand the mixing of benefit transfer and statistical benefits transfer; my point was that the weighting is described as if it were sample weighting and it is not that in many of the situations; it is really a procedure properly treated as making benefits transfer assumptions.

p. 22, line 10, (Richard Carson) change to “standard benefit-transfer and statistically oriented benefit-transfer approaches”.

Suggestion: incorporate change.

p. 22, line 28, (Richard Carson) change to “on narrower groups (e.g., specific sub-populations)”.

Suggestion: Incorporate change.

Page 22, lines 32-33: (Stephen Swallow) Revise to clarify and read: “...while relaxing the requirement for a national focus at the level of the original studies drawn upon in support of a nationally representative population estimate.”

Suggestion: incorporate change.

p. 22, line 33, (Richard Carson) change to “Although this approach could”.

Suggestion: incorporate change.

Page 22, line 40 (and Page 23, line 11). (Jim Opaluch) Again, I recommend we are stronger and more specific here. EPA should provide sufficient documentation to allow an independent party to replicate their analysis. This might be done in an Appendix. (They might also provide a spreadsheet that has the actual formulas that were used to do the calculations).

Suggestion: incorporate comment.

P 23, line 4. (Kerry Smith) The White Paper should:

- “Adjust for income differences in the populations (or time-periods) in individual studies after determining the estimates to be drawn from a particular study-time-period (income adjustments should then be addressed in the process of aggregating across studies to an estimate for a representative population).
- Explain how Hicksian and Marshallian measures of VSL should be aggregated with a consistent measure of income to account for income effects.”

These two points are related –see my earlier comment on income adjustments –this should be modified to reflect it –it committee agrees.

Revise text

P 23, line 8. (George Van Houtven) Explain how Hicksian and Marshallian measures of VSL should be aggregated with a consistent measure of income to account for income effects.

I don't follow this point.

To discuss

Section 3.1.6 – Estimation of Standard Errors (Lead Writer – JunJie Wu)

p. 23, line 35, (Richard Carson) change to “provide sufficiently detailed”.

Suggestion: incorporate change.

P.23, line 44. (Matthew Neidell) “Under some assumptions, one can calculate the standard error of a VSL estimates (\widehat{VSL}) based on its 95% confidence interval using the following formula:

$$se(\widehat{VSL}) = \frac{\overline{VSL} - \widehat{VSL}}{t_{0.025}(n)} \quad (1)$$

where \overline{VSL} is the upper bound of the 95% confidence intervals reported by the authors, and $t_{0.025}(n)$ is read off as the 2.5 percent point of the t -distribution with n degree of freedom. The White Paper should present the formula it uses to translate confidence interval to standard error estimates. “

If no SE is given but it is derived from a certain %CI, doesn't this mean the SE is essentially given? It's an exact formula. This seems like overkill.

Discuss to determine whether text should be changed.

p. 25, line 14, (Richard Carson) change “fails to” to “does not”.

Suggestion: incorporate change.

p. 25, line 15, (Richard Carson) insert “enough” after “detailed”.

Suggestion: incorporate change.

p. 25, lines 15-16, (Richard Carson) change “In fact ... only” to “There are only”.

Suggestion: incorporate change.

p. 25, line 17, (Richard Carson) drop “does not discuss”.

Suggestion: incorporate change.

p. 25, line 18, (Richard Carson) change “estimates at all” to “are not discussed at all”.

Suggestion: incorporate change.

p. 25, line 26, (Richard Carson) change “the bootstrap” to “this”.

Suggestion: incorporate change.

p. 25, line 31, (Richard Carson) change “why re-sample” to “why”

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Suggestion: incorporate change.

p. 25, line 31, (Richard Carson) change “In fact, the” to “The”.

Suggestion: incorporate change.

p. 25, line 33, (Richard Carson) change “4” to “four”.

Suggestion: incorporate change.

p. 25, line 39, (Richard Carson) change “problems” to “issues”.

Suggestion: incorporate change.

p. 26, line 33, (Richard Carson) change “fails to” to “does not”.

Suggestion: incorporate change.

Page 26, lines 33-36. (Jim Opaluch) I recommend we say that EPA document precisely how the standard error is estimated, so that an independent party could replicate the calculations.

Suggestion: incorporate change.

P 26, line 33.(George Van Houtven) Rewrite as follows: “The white paper fails to provide detailed information about how the standard errors of the VSLs taken from the source studies were is calculated in situations where one is not reported in the original study.”

Suggestion: incorporate change.

p. 26, line 38, (Richard Carson) change “proposes” to “suggests”

Suggestion: incorporate change.

P 26, line 39. (George Van Houtven) Rewrite as follows: “The SAB proposes an alternative, perhaps theoretically better, way to calculate standard errors for each non-parametric VSL estimator produced by the meta-analysis.”

Suggestion: incorporate change.

Section 3.2.1 – Overall Methodology for Analyzing the Data (Lead Writer – George Van Houtven)

p. 27, line 17, (Richard Carson) change “narrower” to “narrow”.

Suggestion: incorporate change.

p. 27, line 38, (Richard Carson) drop “very”.

Suggestion: incorporate change.

Page 27, line 36. (Jim Opaluch) Throughout, I think we should recommend that EPA provide detailed documentation, adequate to allow an independent party to replicate the results.

Suggestion: incorporate change.

Page 28, line 6-7. (Jim Opaluch) EPA should use a theoretical model to show the difference between Hicksian and Marshallian measures. The standard Willig result shows that the two converge as the ratio of consumer surplus to income goes to zero. Consider a simple example with a (Marshallian) VSL of \$10 million, a single risk level applicable to an entire population, and adopt the expected utility maximization assumption of linear-in-probability risk preferences (which is also the maintained assumption that is implicit in use of statistical life as a risk metric). Then the annual Marshallian compensation needed by an individual to accept a yearly 10^{-6} mortality risk is \$10 for each individual facing the risk. Using Willig's 2nd order approximation:

$$C \approx A + \eta A^2/2m_0 \text{ or } |C-A|/A \approx \eta A/2m_0$$

where C is Hicks compensating variation, A is Marshallian surplus, η is income elasticity, and m_0 is income. For example, consider the case of the error for a single individual facing a risk of 10^{-6} , assuming an income elasticity of 1 and an initial income of \$40k, the Willig error of approximation is on the order of 0.000125 ($=1 * \$10 / (2 * \$40k)$)

In comparison, in a case of a one percent mortality risk, the error is roughly 56% (C = \$225 thousand, A = \$100 thousand). (Note I used Willig's 2nd order approximation in these calculations, which really shouldn't be applied as the risk gets high)

Assuming risks relevant to EPA analyses are on the order 10^{-3} , errors of using Marshallian surplus should be trivial relative to other errors. Of course, it goes without saying, however, that WTA and WTP might differ greatly in the case of policies affecting much higher risk levels (e.g., farm workers who handling pesticides, residents living adjacent to a hazard waste facility, homeowners with high levels of radon, etc.)

As I mentioned above, this raises the question of whether we should recommend that VSL only be used in cases of small risks applied to a large population. For example, VSL should not be applied to a case of a 100% probability of death of a particular individual, where the life is no longer "statistical".

It is important to note that the above discussion applies to the hedonic wage studies, but is not directly applicable to policies of most direct relevance to EPA. Most EPA policies (e.g., reduction in air pollution) affect quasi-public goods. So, for example, an increase in EPA standards for air pollution affect the quantity of the public good (air quality), not the price. Hanemann's 1991 article shows that WTA and WTP need not be close when examining quantity changes. Of course, Hanemann's result is of direct relevance for the difference between Marshallian and Hicksian surplus measure since Hicksian WTA and WTP bound Marshallian consumer surplus.

Of greatest relevance here, Hanemann's results show that the difference between the Hicksian WTA and WTP measures (and hence Marshallian surplus) need not be small if the change in surplus is small relative to income, so long as the public good of concern has no close market substitutes. But if we view overall mortality (or morbidity) risk as the "commodity", one could expect that there exists market goods and services that are close substitutes for changes in environmental quality. For example, for risks associated with changes in drinking water quality, the bottled water would be a close substitute for quality of drinking water. If we view mortality risk more generally as the "commodity", any market good that reduces mortality risk (e.g., a healthy diet, medicines, precautionary checkups, etc.) could substitute for increases in mortality risk due to changes in environmental quality (e.g., air quality).

I believe this confirms that Willig type conclusions apply to EPA policies that result in "small" changes in risk (e.g., smaller than 10^{-3}), as is typical of most EPA policies at the national level. And we should recommend that VSL not be applied in cases of "large" mortality risks.

To discuss and revise

P 28. Line 12. (Kerry Smith) "The parametric meta-regression analysis should include specifications with an income measure as an explanatory variable. This income measure should be selected to approximate as closely as possible the average disposable household income of the sample used in the primary study."

Issue of income endogeneity needs to be considered for wage hedonic studies.

To discuss

Page 28, line 38. (Jim Opaluch) I'm not sure exactly what is meant by "consider, discuss and ... include". Shouldn't we say that EPA should make necessary adjustments, and should provide detailed documentation of the adjustments so an independent party could replicate the calculations.

Suggestion: revise as suggested.

Section 3.2.2 – Grouping Samples for Analysis (Lead Writer – Daniel Phaneuf)

Page 29, line 15. (Jim Opaluch) The word "that" is repeated.

Suggestion: make correction..

Page 29, line 15-16. (Jim Opaluch) Should we say "data sets" here rather than samples?

Suggestion: revise as suggested.

p. 29, line 23, (Richard Carson) an example of different group assignment is needed here. Suggest "(e.g., grouping studies that used the same dataset or the econometric approach together)".

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Suggestion: revise as suggested.

p. 30, line 5. (Richard Carson) replace “, if not the literal equation” with “and the specific equation”.

Suggestion: revise as suggested.

Section 3.2.3 – Addressing Sampling and Non-Sampling Errors (Lead Writer – Mary Evans)

Page 30, line 8: (Stephen Swallow) Revise to:...recommends that, without compromising best known, science-based practice for quantitative estimation, transparency should also be included as a criterion for selecting an estimator.

This change would be parallel to a change suggested in the Executive Summary, page 4 line 22.

Suggestion: incorporate change.

Section 3.2.4 – Non-Parametric and Parametric Approaches for Estimating Value of Statistical Life (Lead Writer – Andrew Plantinga)

Page 30, line 30-31. (Jim Opaluch) I don’t think we need to repeat the charge question, as we do in part in the first sentence. I recommend we write this as “The SAB finds that additional information is needed in the White Paper, especially to explain their use of the nonparametric approach.”

Suggestion: incorporate change.

Page 30, lines 41-42. (Jim Opaluch) We should recommend that EPA emphasize the rationale that using the mean of means avoids giving too much weight to studies that report multiple estimates. (See my comment on page 3, above, in response to the Executive Summary, page 4 line 40).

Suggestion: incorporate change.

p. 31, lines 10–11, (Richard Carson) this suggestion will not work as written. A revision that better reflects the committee’s discussion is “One suggestion is to include indicator variables for a study having specific major contributors to the VSL literature as a co-author.”

Suggestion: revise as suggested.

Page 31, line 12. (Jim Opaluch) As I recall, EPA showed a graph of results over time, and while there *is* evidence of a time trend, but it is not very compelling. We should look this over again and determine whether we should weaken this statement. Note that inclusion of a time trend could have very significant results when applied within the EPA policy domain. Many of the policies EPA is required to assess have mortality risks that extend over long time horizons and

that have long latency periods. Even a modest increase in VSL per year could have a great impact on estimated benefits for policies many years in the future.

To discuss

Page 31, line 15-17. (Jim Opaluch) "... or could simply be a statistical artifact that arises due to the relatively small number of studies" I think we also should stress that the selection of a time trend could have very important implications for policy assessments, and that EPA should do further study on this issue.

Suggestion: incorporate change.

P 31, line 21. (Kerry Smith) "The first two approaches would place greater weight on more recent studies, which could be appropriate if one believes that methodologies are improving over time. However, if one believes that methodologies are getting better, estimates should simply be taken from the most recent study."

Need to distinguish meta-analysis for explanation of factors influencing results and meta equations as predictive equations; in the later role it is hard to justify any use of a time trend for predicting VSL.

Discuss and determine whether text should be revised as suggested by Jim Opaluch.

p. 31, lines 22-23, (Richard Carson) drop "However, ..., recent study."

Suggestion: revise as suggested.

p. 31, line 24, (Richard Carson) change "one should pick ... the best." to "there are conditions under which it is optimal to rely solely on the study the utilizes the best methodology."

Suggestion: revise as suggested.

Page 31, Lines 20-27: (Stephen Swallow) Most of this discussion implies concerns about the validity of studies of a different vintage, when line 16 also correctly points out that a time trend could simply reflect changes in preferences of the population. Some of the concerns about methodology might be better identified as influences on the variance for a given estimate, rather than necessarily implying a bias or a direct concern about validity (convergence of the methods a study uses toward generating an estimate of the "true" value sought). Also, methodologies may not necessarily be trending toward "better" rather than simply offering alternative perspectives or the advantages of methodological plurality in identifying an unknown value. Could these lines simply be revised to read something like:

"The White Paper should include a discussion of the implications of including or excluding a time trend in terms of beliefs about validity of the studies used and their methodologies and the potential that a time trend could capture unobserved changes in preferences of the underlying population."

Also, if EPA has a target-year for which the VSL estimate is to be established, would it not be appropriate to set the time variable to that target year (2013)?

Or does the EEAC intend to make the specific recommendation to drop the time trend variable as implied by lines 26-27?

To discuss

Page 32, line 2. (Jim Opaluch) The specification of a time trend could have very significant implications for the results of a policy analysis for risks that extend many years into the future. EPA should carry out a careful assessment of whether there is, in fact, a time trend on historic VSL estimates, if so, whether the trend is likely to continue into the future, or whether the trend is simply a statistical artifact of having a relative small number of studies to work with.

Suggestion: incorporate the recommended change.

P 32. Line 2. (Kerry Smith) “EPA should be consistent in its treatment of the time trend time trend in VSL estimates. If it is controlled for in the parametric model, it should be controlled for in the non-parametric models.”

I would argue NO ROLE for time trend in predicting VSL.

Discuss and determine whether text should be revised as suggested by Jim Opaluch.

Section 3.3.1 – Proposed Estimates of Value of Statistical Life (Lead Writer – Kerry Smith)

p. 32, line 36, (Richard Carson) change “explicit and not buried in” to “explicitly stated and not simply subsumed in”.

Suggestion: incorporate suggested change.

Page 32, lines 22-37. (Jim Opaluch) Should we recommend that EPA also report the range of estimates, based on the difference formulations (Stated preference only, hedonic wage only, balanced; mean-of-group-means, simple mean); parametric, nonparametric?

Suggestion: discuss whether to incorporate this change.

Page 32, line 34-37. (Jim Opaluch) Should we recommend that EPA consider testing the reasonableness assumption of non-workers having the same risk preferences as workers using data from stated preference models? Presumably some respondents to the stated preference surveys do not work, are retired, etc.

Discuss whether to incorporate change.

Page 32, line 39-40. (Jim Opaluch) Should we give a brief rationale here? “... does not seem appropriate because ...”

Discuss what the rationale should be.

P 32, line 39. (Kerry Smith) *These are the points I mentioned earlier --when presented here they seem to contradict the executive summary and comments made earlier; the same points need to be raised with respect to responses to earlier questions. I don't understand why this didn't happen. It is as if the person writing the first part was not aware of this part*

P 32, line 41. (Kerry Smith) "Building in" the income elasticity and growth assumptions as maintained hypotheses before constructing the mean mixes a benefit transfer decision with an adjustment for household income across different studies. More specifically, income adjustment could involve: (1) adjustment for differences in the income across different samples that could hypothetically alter the risk tradeoff;..."

See my earlier comments –answers to earlier questions seem to contradict this comment –all the answers should be consistent on the adjustment using the income elasticity and the measure of income and the report should cite later places where issue is discussed in detail.

Discuss what changes are needed.

Page 33, line 3. (Jim Opaluch) Of course, this requires forecasting economic conditions years and decades into the future, but presumably EPA would assume some rate of economic growth, and could do sensitivities analyses on this. This also requires developing reliable income elasticities of VSL, which the data do not currently seem to support.

To discuss

Page 33, Line 6: (Stephen Swallow) "discuss the income used". This phrase is unclear. Income elasticity? Income variables? Income value? Income growth?

To discuss

Page 33, lines 7-8. (Jim Opaluch) I think we can use the Willig argument above to rule out the importance of Hicksian vs. Marshallian effects so long as we are looking at small changes in individual risk (e.g., 10^{-6}). With a VSL of 10 Million and a risk of 10^{-6} per year (or per lifetime), income compensation would be on the order of \$10, which is a tiny fraction of annual (or lifetime) income. This might not be the case for a larger risk, such as a hypothetical case of a 1% risk faced by a population in the immediate vicinity of a superfund site.

To discuss

P 33, line 7. (Kerry Smith) "In addition the SAB notes that adjustment for income with the stated preference measures would need to be different because these are derived from Hicksian welfare measures <<Chair's note: can we provide a citation for methods that could be used for this kind of adjustment?>>."

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General details discussed in Smith et al economic letters 2003; and smith et al land economics 2002.

Suggestion: include citation.

p. 33, line 15, (Richard Carson) something is missing or confused in this sentence.

Suggestion not clear

P. 33, line 18 (Kerry Smith) “The documentation of income adjustment to VSL should be clarified in the White Paper. Adjustment of VSL estimates by an income elasticity of VSL and index of income growth (based on GDP per capita) does not seem appropriate. However, conversion of VSL to inflation adjusted dollars would be appropriate. “

This statement contradicts earlier statements where the committee approved the EPA approach

p. 33, line 19, (Richard Carson) change “by an” to “by both an”.

Suggestion: incorporate suggested change.

p. 33, line 20, (Richard Carson) change “would be appropriate” to “is appropriate”.

Suggestion: incorporate suggested change.

Page 33. (Jim Opaluch) Key Recommendations. Should we add a recommendation that EPA report a range of estimates, based on the full range of specifications (stated vs. revealed vs. balanced; mean-of-group-means vs. simple mean; etc.)?

Discuss whether to incorporate this additional recommendation.

Section 3.3.2 – Influence Analysis (Lead Writer – Kerry Smith)

Page 33, line 31-32. (Jim Opaluch) “... and given the relatively small number of VSL estimates”

Suggestion: incorporate change.

P 33, line 36. (Kerry Smith) “Looking at the mean of group means in the White Paper, the two most influential studies are Corso Hammitt and Graham (2001) at -13.8% and Chestnut, Rowe, and Breffle (2012) at -11.1.”

Edit –replace “looking at” with “considering”

Suggestion: incorporate change.

Page 33, line 37-38. (Jim Opaluch) The Chestnut, Rowe and Breffle number is of the wrong sign. It should read +11.1%.

Suggestion: Incorporate change.

p. 33, lines 37-38, (Richard Carson) way text is written suggests that one of these numbers should be positive.

Suggestion: change -11.1 to +11.1.

p. 33, line 42, (Richard Carson) change “technic” to “technique”.

Suggestion: incorporate change.

P. 34, line 15. (Matthew Neidell) “Influence analysis of the maximum likelihood stated preference estimates indicates that Corso, Hammitt and Graham (2001) at -22.8 is well over two times more influential than the second most influential study. The EPA should consider using a robust estimation technique that limits the influence of this observation.”

A median analysis?

Discuss whether revision is needed.

P 34, line 20. (Matthew Neidell) “The EPA should consider the potential for using regression diagnostic indexes (Belsley et al. 1980; Cook and Weisberg 1982; Belsley 1991) for the parametric modeling of VSL.”

A concern with this is the small sample size and potential for data mining. If it doesn’t pass a certain diagnostic, it’s not clear there is enough power to resolve the issue.

Discuss whether revision of text is needed.

Section 3.4.1 – Protocol for future Revisions of Value of Statistical Life (Lead Writer – Jim Opaluch)

P 35, line 4. (Reed Johnson) “Given the importance of VRR, high priority should be assigned to increasing the pool of high quality studies to support the VRR meta-analysis. This is particularly important due to the small number of data sets to support hedonic price estimates, and the relatively small number of stated preference studies currently included in the meta-analysis.”

As noted, EPA has no resources for doing this and doesn’t expect to have any resources for doing this in the foreseeable future.

Suggestion: no change indicated.

P 35, line 24. (Reed Johnson) “The EPA could even consider the feasibility of sponsoring its own refereed journal that focuses on analyses of direct relevance to meeting the agency’s needs.”

None of this is going to happen. Al regrets not moving on the journal when he had a chance, but sees no prospect in the future.

Suggestion: no change indicated.

Page 36, lines 6-8. (Jim Opaluch) Should we recommend that EPA consider updating estimates more frequently if it identifies a significant number of studies that could be added by extending the categories of studies to be included (e.g., transportation safety, risk-risk tradeoffs; peer reviewed studies outside of non-peer reviewed journals, etc.). Or is it likely to take EPA 5 years to do any sort of assessment of studies to add?

Discuss whether updates more frequent than five years should be recommended.

Page 36, line 10-11. (Jim Opaluch) I strongly believe that VSL estimates from articles in peer reviewed journals would not necessarily be expected to be of higher quality than any and all available manuscripts outside of peer reviews journals. This is especially true since many papers in peer reviewed journals are published primarily due to some innovative conceptual or methodological contribution to the body of knowledge, not necessarily because of the high quality of its empirical approach. For example, it would generally be difficult to publish an article in a peer reviewed journal simply because it is a competent, but routine empirical analysis that employs standard methods and is based on a representative national sample. But such a study could be very useful observation for estimating VSL as part of a larger body of literature. Even if potentially publishable, a study of this sort might not even be submitted for peer review, especially if it were carried out by a consulting company that does not view peer review publication as a high priority.

To discuss

p. 36, lines 17-35, (Richard Carson) this text should be expanded to clearly emphasize that EPA can set up a formal peer review process for studies (e.g., reports to government agencies by consulting firms) that may be informative on the value of a VSL.

To discuss

P 36, line 20. (Kerry Smith) “Rather, a quality controlled peer review process should be established. For example, EPA might ask the SAB to organize a process to review research results outside of traditional peer reviewed journals, both to identify appropriate reviewers (possibly including SAB members), and to determine whether or not studies that undergo peer review are judged to “pass” the review process, and therefore qualify for inclusion.”

This sounds good but on reflection could easily be criticized as not hands off from perspective of agency staff; journal review process has advantage of being “detached” even though EPA staff in reality have greater experience and understanding of these issues than most journal referees.

To discuss

P 36, line 29. (Matthew Neidell) “A major challenge to relying only on publications in peer reviewed journals is that economics journals rarely publish articles that contain routine empirical analyses without some sort of innovation or other improvement in the state-of-the-

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art. As a consequence, many analyses could provide satisfactory estimates of VRR, but may not be submitted to peer-reviewed journals, or may be rejected for publication because they do not improve upon the state-of-the-art. This may be particularly relevant for analyses carried out by consulting companies, for whom publication of research results in peer-reviewed journals may or may not be of high priority.”

I don't think there was agreement here. I fear that using non-peer reviewed papers opens up a whole new can of worms. Peer review is flawed, but it is an accepted standard such that deviating from would be a major departure. As noted by Phaneuf, editor of JAERE, a high quality paper on VSL is still highly valued by the profession.

To discuss

Page 36, line 31 and line 33. (Stephen Swallow) Revise to “...state-of-the-art in economic theory or empirical methodology.”

Could add: In contrast to some other disciplines, the field of economics places a low priority on improvements in the state-of-the-inventory of empirical knowledge, which severely discourages production of studies serving a primary function of recording value estimates useful for policy analysis.”

Suggestion: incorporate the change.

Page 36, line 33: (Stephen Swallow) revise as for line 31.

Suggestion: incorporate the change.

p. 36, line 34, (Richard Carson) change “companies” to “companies for government agencies”.

Suggestion: incorporate the change.

P 36, line 37. (Reed Johnson) ” Information from Other Economic Studies of Risks

The SAB recommends that the EPA consider whether useful information can be extracted from other studies that could improve estimates of VRR and its characteristics (e.g., latency, morbidity).”

Another source of data is the huge literature on health-care cost-effectiveness analysis. These studies use measures of health-related quality of life that often fall short of utility-theoretic standards, but could nevertheless be useful.

There is a comprehensive searchable database of 5000 such studies that is managed by Tufts University. <https://research.tufts-nemc.org/cear4/Default.aspx>

The problem could turn out to be too many studies, rather than too few.

Suggestion: incorporate changes suggested.

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P 36, line 42. (Reed Johnson) “For example, EPA might consider using **the** results of a risk-risk **study** studies that employed a stated-preference approach, wherein respondents were asked to choose whether to undergo treatment (e.g., a risky surgery) that has a stated risk of immediate mortality versus a given risk of cancer, which involves stated risks of both long term morbidity and subsequent mortality

Suggestion: incorporate changes.

P 36, line 46. (Reed Johnson) “EPA might also use the results of **a study** studies that asked respondents to choose whether to undergo treatment that has a stated risk of morbidity (e.g., paralysis, chronic pain, etc.) versus foregoing treatment, in which case they face a stated mortality risk.”

See Hauber AB, Fairchild AO, Johnson FR. Quantifying benefit-risk preferences for medical interventions: an overview of a growing empirical literature. Appl Health Econ Health Policy. 2013;Aug;11(4):319-29.

I SEE THIS IS CITED IN APPENDIX C. MIGHT WANT TO CALL THIS OUT IN THE REPORT REFERENCES.

Suggestion: incorporate changes suggested.

P 37, line 32. (Reed Johnson) Open Data Initiatives

Good recommendation.

Suggestion: no change needed.

P 37, line 44. (Kerry Smith) “Project Open Data (U.S. Office of Management and Budget and U.S. Office of Science and Technology Policy 2016) provides an excellent framework for making data available in order to improve the information obtained from available studies.”

I had argued for EPA establishing online access to merged data from every year on a part of its web page to encourage research on the topic; this places burden on researchers –my proposal was for EPA to do some data construction from public sources in house.

Suggestion: Insert text referring to recommendation that EPA simplify periodic updating of hedonic wage estimates of VRR by creating an archive of wage data and perhaps other data from the U.S. Census Bureau’s demographic supplement to the Current Population Survey, matched with data from the U.S. Bureau of Labor Statistics Census of Fatal Occupational Injuries (CFOI) in standardized form, and perhaps other data sets.

Page 38, lines 9-13. (Jim Opaluch) There is an editorial problem with this sentence. It should read something like:

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“Federal grants and contracts could require that data collected under the contract be published to Data.gov in standard format (U.S. General Services Administration 2016), unless there is a compelling reason that the data not be published.”

We might also add a sentence to the effect:

“Such a policy might allow exceptions, and be subject to possible censuring of individual variables, observations, etc. as necessary to ensure protection of confidentiality.”

Suggestion: incorporate the change.

p. 38, “line 25, (Richard Carson) change “correct” to “adequately take into account”.

Suggestion: incorporate the change

p. 38, lines 37-38, (Richard Carson) it may be worth nothing that making the data publically available after a reasonable amount of time for the original authors to write papers for journal publication would work fine given the nature of the process of updating the VSL estimate.

Suggestion: incorporate the change.

Page 38, Lines 38-40: (Stephen Swallow) For example, EPA could learn from the policies established by the National Science Foundation program for Long Term Ecological Research.

Suggestion: incorporate the change.

P 38, line 46. (Kerry Smith) “For example, the EPA might simplify periodic updating of hedonic wage estimates of VRR by creating an archive of wage data and perhaps other data from the U.S. Census Bureau’s demographic supplement to the Current Population Survey, matched with data from the U.S. Bureau of Labor Statistics Census of Fatal Occupational Injuries (CFOI) in standardized form, and perhaps other data sets.”

My point above is captured her —seems the two discussions need to be coordinated; recognizing that unfunded mandates –such as the open data initiative may not accomplish objectives when the real research dollars to support initiatives in these areas have declined.

This is the point that should be in executive summary --more likely to promote research than calls for special issues of journals or conferences.

Suggestion: incorporate the suggested revision.

p. 39, (Richard Carson) there is a shift here to “VRR” rather than “VSL” which is generally used elsewhere.

Discuss whether to use VRR or VSL terminology.

Page 39, lines 26-28. (Jim Opaluch) Should we add that the EEAC might be asked to coordinate reviews, or at least provide input on the review process?

Discuss whether non-peer reviewed papers should be recommended.

p. 39, lines 27-28, (Richard Carson) this sentence should be modified to be explicit that EPA will need to screen for potentially useful unpublished studies and then run a formal peer review process.

Discuss whether to revise.

p. 39, line 31, (Richard Carson) change “improve estimates of VRR and its characteristics” to “improve understanding of the nature of VSL estimates and how they relate to underlying characteristics”.

Suggestion: incorporate the change.

Section 3.4.2 – Valuing Reductions in Risks of Cancer (Lead Writer – Richard Ready)

P 40, line 11. (Reed Johnson) “The SAB has previously concluded that “research suggests that people are willing to pay more for mortality risk reductions that involve cancer than for risk reductions from accidental injury and proposes a placeholder value that could be used for this cancer differential while the Agency pursues long-term research to differentially value other types of risks” (U.S. EPA SAB 2011).”

EPA is not going to do this. I don't see any point in recommending or assuming they will. The report would be more useful if it focused on ways to enrich the existing studies with studies in related areas.

Suggestion: no change needed, this text refers to previous SAB recommendations.

P 40, line 14. (Reed Johnson) “The motivation behind a potential cancer differential is that a death from cancer is preceded by a significant period of morbidity, while a death from accidental injury may not be. According to this motivation, a cancer death can be thought of as two events, a period of morbidity followed by an early death.”

“period of morbidity” is too simplistic. It isn't just that people feel sick before dying, but treatment typically is accompanied by surgery, chemotherapy, and radiation that have serious, debilitating side effects. The experience of the final death also is traumatic for family and friends as well as the affected individual in ways that sudden accidental death is not.

Suggestion: Expand text to incorporate points raised.

Page 40, lines 17-20. (Jim Opaluch) We might add something like “... higher values would likely be associated with reductions in risk of mortality that also includes longer and/or more severe periods of morbidity.”

Suggestion: incorporate the revision.

P 41, line 10. (Reed Johnson) “Based on available studies, the SAB concludes that there is not sufficient evidence at this time to justify a non-zero cancer differential.”

This is not the appropriate conclusion. SAB can conclude that the evidence that EPA has looked at does not justify a non-zero differential, but these studies simply were not adequately designed or powered to detect such a differential. The health-preference literature clearly shows that people care how they die.

Suggestion: Discuss to determine if text change is needed.

Page 41, lines 13-15 and page 41, line 3. (Jim Opaluch) I don’t think it is wise to recommend that EPA simply add together VRR for mortality risk plus morbidity risk. The VRR for an otherwise healthy individual who dies suddenly at some future date would likely be much higher than mortality component of VRR associated with an illness that also involves a long period of grave morbidity, and causes death at that same future date. This also seems to be in contradiction to the available empirical evidence, which we argue does not support a cancer differential.

I recommend instead that we state that it is logical to expect that VRR for morbidity plus mortality should be larger than the VRR for instantaneous death at the same future date Or more generally, we might argue that VRR for longer and/or more severe morbidity followed by mortality should be higher than VRR for shorter and/or less severe morbidity followed by mortality at that same future date. But the current literature is inadequate to support an estimate. We recommend that EPA support future research on this issue.

Discuss whether to incorporate the revision suggested by Jim Opaluch.

P. 41, line 13. (Reed Johnson) “The SAB recommends that, instead of adopting a nonzero cancer differential, the EPA consider using existing methods to value the morbidity that occurs prior to an early death, and add that estimated morbidity value to conventional estimates of the value of the associated mortality. “

This is just inadequate.

Discuss to determine text change needed.

P 41, line 22. (Matthew Neidell) “The EPA should encourage and support ongoing research on whether willingness to pay to reduce the risk of an early death preceded by a period of morbidity is correctly valued by summing the value of the morbidity plus the value of the mortality. At this time, the SAB does not have evidence to suggest that that approach would over- or under-state the true willingness to pay.”

See recent paper by Viscusi in Journal of Health Economics

Discuss to determine change in text needed. Obtain full citation for Viscusi paper.

P. 41, line 22. (Reed Johnson) “The EPA should encourage and support ongoing research on whether willingness to pay to reduce the risk of an early death preceded by a period of morbidity is correctly valued by summing the value of the morbidity plus the value of the mortality.”

It is pointless to keep recommending this.

Suggestion: no change indicated.

P 41, line 25. (Reed Johnson) “Gray literature studies, studies conducted outside the United States, and studies that do not directly estimate VSL, such as risk-risk tradeoff studies and risk-benefit studies, could be assessed to determine whether there is evidence that the VSL for different mortality risks differs, after having controlled for the value of associated morbidity. <<Chair’s note: can we provide citations to relevant studies?>>”

See references provided above.

Suggestion: incorporate suggested references.

Section 3.5.1 – Income Elasticity of the Value of Statistical Life (Lead Writer – Matthew Kotchen)

Page 42, lines 22-24. (Jim Opaluch) I think we need to drop the word “Even” from this sentence. It should read “If EPA chooses to exclude these studies from the analysis, the agency ...”

Suggestion: incorporate the change.

p. 42, line 24, (Richard Carson) drop “(not enough)”.

Suggestion: incorporate the change.

Page 42, line 26-28 and lines 44-46. (Jim Opaluch) This might be a promising avenue to pursue. But I think we need to recommend that EPA first establish that there is a conceptual rationale for linking income elasticity of VRR to that for related goods and services, prior to the agency exploring ways to follow this route. Frankly, I am skeptical of whether this approach is applicable in sufficiently general cases. For example, I suspect one can show that it is straightforward to use income elasticity for related market goods in the case where the market good is a perfect substitute for the risk reduction, and the “trading ratio” is known (e.g., one unit of medication offsets 1 unit of a toxin in the drinking water). But even in the case of perfect substitutes, I suspect the trading ratio is necessary derive an income elasticity of VSL from income elasticity of a market good. The relationship between the two income elasticities would seem to become far more complex when there is a set related market goods and services that are less-than-perfect substitutes for the risk reduction. It might be worth putting an effort into this, but we should most definitely NOT recommend that EPA simply adopt an

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income elasticity for related market goods and services without first establishing a conceptually defensible approach for doing so.

Discuss whether revisions in text are needed.

P 42, line 26. (Matthew Neidell) “One area to explore further, in the absence of explicit studies, is the possibility of using estimates of the income elasticity for other related goods and services to infer estimates of the income elasticity of VSL. <<Chair’s note: it would be helpful to provide some examples and citations to clarify what types of goods and services>>

Any goods and services. It would just be needed to tell us something about how the utility from consumption changes with income.

Discuss whether change suggested by Richard Carson below will clarify the text.

P 42, line 26. (Reed Johnson) “One area to explore further, in the absence of explicit studies, is the possibility of using estimates of the income elasticity for other related goods and services to infer estimates of the income elasticity of VSL. <<Chair’s note: it would be helpful to provide some examples and citations to clarify what types of goods and services>> “

There were a number of suggestions raised in the discussion, but I’m not sure we reached a consensus on this point.

To discuss

P 42, line 26. (Kerry Smith) “One area to explore further, in the absence of explicit studies, is the possibility of using estimates of the income elasticity for other related goods and services to infer estimates of the income elasticity of VSL. <<Chair’s note: it would be helpful to provide some examples and citations to clarify what types of goods and services>>”

I had recommended the Murphy and Topel to signal a larger literature –Chetty AER dec2006; Hall and Jones QJE 2007; coefficient of relative risk aversion can be linked to labor supply elasticity and to income elasticity of VSL.

AS I noted earlier we need to recommend that the analytics of the relationships be developed first before suggesting other goods and services --how do they relate to the risk and the associated tradeoff measure.

Suggestion: include suggested citations.

p. 42, lines 26-31, (Richard Carson) change “One area ... of VSL.” to “One area to explore further is the possibility of using income elasticities for consumer products that can be used to reduce environmental risks such as bottle water and suntan lotion.”

Suggestion: incorporate the change.

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p. 42, lines 44-46, (Richard Carson) EPA should explore the possibility of using income elasticities for consumer products that can be used to reduce environmental risks.”

Suggestion: incorporate the change.

P 42, line 44. (Kerry Smith) “In the absence of explicit studies, the EPA should consider the possibility of using estimates of the income elasticity for other related goods and services to infer estimates of the income elasticity of the value of statistical life.”

Issue to answer chairs question –income elasticity of various form of health insurance –not sure of the other types of goods see attached NBER paper.

Suggestion: revise text to indicate that income elasticity of various forms of health insurance could be examined.

Section 3.5.2 – Analysis of Very Low Income Elasticity Estimates (Lead Writer – Matthew Neidell)

p. 43, (Richard Carson) If the recommendation is to not use the income elasticity estimate from Hammitt and Robinson, then the issue of how to deal with the negative/zero income elasticities in the studies they consider is moot. If there is a need to make a statement here: there are two consistent approaches to imposing the constraint that income elasticities should be positive— impose the restriction in the specification of the functional form or use a Bayesian approach with a prior that has no support over the non-positive axis. This would require re-estimating the model on the original data. Otherwise, current meta-analysis practice is to include the negative/zero estimates under the guise that all sources of sampling variation should be included.

To discuss

P 43, line 2. (Reed Johnson) “Analysis of Very Low Income Elasticity Estimates”

I believe it was George who advocated leaving all the estimates in, but calculating with and without zeros would handle that.

To discuss

Page 43, line 11. (Jim Opaluch) “highly implausible” might be better than “highly unlikely” here.

Suggestion: incorporate the change.

Page 43 lines 11-19 and lines 26-27. (Jim Opaluch) From a scientific or methodological perspective, there is no rationale for simply excluding zero/low estimates of income elasticity. At the same time, these estimates do seem to be highly implausible. And note that intuition

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suggest that the value of reducing risk is likely to be a luxury good, especially for risks within the purview of EPA which primarily occur many years (or decades) into the future.

Could this problem be explained, in part, by a correlation between wealth and age? Older individuals tend to be more wealthy, especially when excluding retired individuals. But they also may be less concerned about risks of mortality that occurs many years in the future.

Another possible issue is the difference between estimates that have narrow confidence intervals around zero, versus cases where statistical power is such that while we cannot reject the hypothesis that the value is zero, but it may also not be possible to reject the hypothesis that the true value is 1, or some other more plausible value. It would be worth looking at the estimates in more detail to see the extent to which “zero” estimates arise due to inadequate statistical power, which precludes our rejecting the hypothesis that true value is zero, but also precludes rejection of the hypothesis that the true value is a more plausible number (like 1).

To discuss

Section 3.5.3 – Study Selection Criteria and Alternative Approaches for Estimating Income Elasticity of VSL (Lead Writer – Richard Carson)

P 44, line 36. (Kerry Smith) “To estimate the income elasticity of VSL, variation in income is needed. However, there has been relatively little change in median income over the last two decades.”

Especially true for groups who are represented in the samples used for hedonic wage studies.

Suggestion: incorporate this point in text.

Page 44, line 36-40. (Jim Opaluch) This makes a good point. There could be an aggregation issue involved.

Suggestion: no change needed.

P 44, line 37. (Kerry Smith) “Changes in per capita income have been more pronounced, but much of the change has been in the two tails of the income distribution. This calls into question what the appropriate income variable is if a causal relationship is needed.”

Sentence is not clear; perhaps I wrote it –but issue is what is the concept of income; in interpreting estimates of risk tradeoff expressed by VSL for wage hedonic –income is endogenous; so need a structure to work out income elasticity; if discussing this as a benefit transfer question –take VSL as the estimated economic “parameter” how do we adjust for income growth –this could be described as a separate issue –for me these two issues are not properly distinguished in white paper or in our recommendations.

To discuss

Page 44, line 43. (Kerry Smith) “It is well known that estimates based on cross sectional data measure what would be expected to happen to an individual’s VSL if that individual swapped income with someone else in the current income distribution. In contrast, income elasticity of VSL estimates based on a time series measure provide an estimate of how VSL statistics would shift if the entire income distribution rises or falls. The EPA’s use of income elasticity of VSL estimates to adjust VSL estimates over time generally calls for a time series-based measure.”

The income elasticity of VSL related to coefficient of relative risk aversion; I mentioned this in earlier comments; connection is noted in Evans and Smith paper on income elasticity; but I did not see that discussion in this draft

Page 45, lines 8-23. (Jim Opaluch) Can these arguments be used to advocating for ignoring empirical estimates, at least until further research is available, and instead using an alternative approach to specifying income elasticity of VSL? Expenditures on related goods and services might be a promising approach if we can provide conceptual justification. Or an *ad hoc* assumption of an income elasticity of 1 might be better than using zero based on empirical estimates that are here argued to be specious.

To discuss

P 45, line 15. (Matthew Neidell) “It has long been known that in order to adequately measure income, a very large set of questions about specific types of income and monetary transfers is required. Furthermore, from a theoretical perspective, income is not the correct variable that should help determine the risk-wage tradeoff but rather the correct variable is medium term discretionary wealth.”

I don’t recall this, but could be my memory. Not sure why this construct (“medium term disc wealth”) is the right one.

To discuss

P 45, line 21. (Matthew Neidell) “In this case, the presence of classical measurement error is known to bias the estimate of the income elasticity of VSL downward, a result that has considerable support in the broader literature on income elasticities.”

It is true that classical measurement error will bias down, but we don’t know that it’s classical. For example, if the negative IEVSL estimates are due to measurement error, then it can’t be classical (otherwise it would be a larger negative value in truth)

To discuss

P 45, line 25. (Reed Johnson) “Methodologies for Estimating Income Elasticity of VSL”

I reacted negatively to the section in the ES on this. This discussion is much more nuanced. It might help if a little more of the complexity carried over into the ES.

Expand text in executive summary.

Page 45, lines 34-37. (Jim Opaluch) As I recall, the EPA analysis of available studies provide some support for a time trend in VSL, but only very weak support as indicated by the graph they showed. I don't believe that including an income variable was of much help. Plus there are all the problems we indicated with income measures.

Suggestion: no change is indicated.

Page 46, line 5. (Jim Opaluch) I would use the phrase "systematically different". Random differences across individuals would not be a problem.

Suggestion: incorporate change.

Page 46, lines 10-30. (Jim Opaluch) At a minimum, this approach might be useful for providing insights into the plausibility of income elasticity of zero versus one, whether or not the analysis provides a compelling point estimate of income elasticity.

Suggestion: include this point in the text.

p. 46, line 14, (Richard Carson) change "Cost of Funds Index (COFI)" to "Census of Fatal Occupational Injuries (CFOI)". This should be checked elsewhere in the document.

Suggestion: incorporate change.

P 46. Line 15. (Matthew Neidell) "It would be possible, however, to take one of the currently preferred VSL model specifications that can be estimated by combining the U.S. Census Bureau's Annual Social and Economic Supplement to the Current Population Survey (CPS) with COFI data. By holding the methodology and data sources used to estimate the VSL constant, it should be possible to use the income variation over the last two decades to obtain a defensible income elasticity of VSL estimate. Each annual cross section of the CPS, can be used to produce a VSL estimate."

I like this idea but see two issues. One, how do we decide on the accepted model for estimating the VSL in a given cross section? Two, how do we account for changes in VSL over time due to shifts in preferences that are not income related?

To discuss

p. 46, line 17, (Richard Carson) change "COFI" to "CFOI".

Suggestion: Incorporate change.

P 46, line 27. (Matthew Neidell) "The income elasticity of VSL estimate(s) to be used in assessing regulations could be updated at regular intervals simply by adding VSL estimates based on more recent years of the CPS, with earlier time period perhaps given less weight in determining the income elasticity of VSL estimate."

How is this weight determined?

To discuss

Page 47. (Jim Opaluch) I'm not sure I am comfortable with the following argument, but one could make the case that an income elasticity of zero is not plausible, and given the many problems with existing empirical estimates, low estimates of income elasticity in the extant literature could be specious. We might make the argument for an income elasticity of 1 (or some other number) While *ad hoc*, such an estimate is more plausible than the very small number you get from averaging estimates from the available literature, and might be used until a more defensible estimate is available. As I said above, I'm not entirely comfortable with this argument.

To discuss

Section 3.5.4 – Income Elasticity of the Value of Non-fatal Health Effects (Lead Writer – Reed Johnson)

p. 47, lines 21-30, (Richard Carson) as written this is somewhat inconsistent with other recommendations involving the income elasticity. More specifically, the same notion of private consumer goods that can be used to reduce environmental risks was raised earlier in terms of the income elasticity of the VSL. It may be difficult to find a consumer good that reduces an environmental morbidity risk that does not also influence an environmental mortality risk.

To discuss

Page 47, Line 23-24: (Stephen Swallow) Elsewhere, we recommend consideration of income elasticities for other goods as a foundation for assessing the income elasticity of VSL. This comment seems to conflict with earlier recommendation.

To discuss

Page 47, lines 28-29. (Jim Opaluch) As I indicated elsewhere, this seems like a potentially promising approach, but I think it needs some sort of theoretical or empirical justification.

To discuss

P 47, line 28. (Kerry Smith) "The SAB recommends that the EPA explore the income elasticity of expenditures on private health care products as a better proxy for the income elasticity of non-fatal health risks. <<Chair's note: can we provide citations?>>"

Obvious alternatives affected by incentives created by policy –identification problem.

To discuss

Page 47. lines 36-38. (Jim Opaluch) Absent some theoretical or empirical justification, this smacks of applying the income elasticity of one good for another good, which the previous sentence rejects. One might be able to use a simple conceptual model of averting expenditures to show conditions under which the income elasticities might be expected to be the same.

Discuss to determine whether text change is needed.

Appendices

P B-1. (Reed Johnson) APPENDIX B Bibliography on Willingness to Pay in Health and Health Care [partial]

Plus 5,000 health and health-care cost-effectiveness studies!

Suggestion: no change indicated.

Executive Summary

General (Jim Opaluch). In general, it would be good to provide a more explicit link between the text of our comments and the charge questions as listed in Appendix A. We refer to 19 charge questions, but Appendix A lists 17 (with question 1 divided into a, b and c). I recommend we refer to 17 charge questions.

The numbered items on page 1, lines 23-29 are a bit confusing because we indicate there are 19 charge questions, but we list 6 items. We might reword this to say something like "... 19 charge questions, organized into 6 topics focusing on ...". Or we might just drop the list of 6 items altogether to avoid confusing the reader, since we don't refer to them anywhere below.

The bold headings correspond to the charge questions, but this might not be clear to a reader. I recommend we number each of the bold headings, with the first three being listed as "1a. Meta-analysis dataset: Evidence of Validity of the Stated Preference Study", etc.

Another suggestion which is purely stylistic. I recommend that we start a new paragraph under each of charge question that separates EPA's request from our recommendation. For example, on Page 1 lines 42-46, I recommend that we start a new paragraph with "The SAB finds that ...", and we do this throughout the charge questions.

Suggestion: incorporate recommended edits.

Page 1, line 5. (Jim Opaluch). I recommend we change the wording "takes into account" so the sentence reads "The method estimates ..."

Suggestion: incorporate change.

Page 1, line 19. (Jim Opaluch) The phrase "on income elasticity of VSL" makes the sentence confusing. It sounds like the sentence says "the income elasticity of VLS discuss options ...".

Can't the phrase be dropped, and so the sentence reads "The report and technical memorandum discuss options ..."?

Suggestion: incorporate change.

P 1, line 37. (Kerry Smith) "In addition, the EPA should clarify how their criteria of validity were applied to all of the studies that were considered for use in the analysis."

Why is there not discussion of the issue of consequentiality; see Carson Groves and List JAERE paper—mentioned in my comments; I don't see how this can be overlooked.

Suggestion: revise to make consistent with any changes in main report.

Page 1, line 38 (Kerry Smith). "The SAB finds that the evidence of study validity considered by the EPA is appropriate but incomplete. To strengthen the assessment of study validity, the agency should consider a broader set of criteria for 36 validity. In addition, the EPA should clarify how their criteria of validity were applied to all of the 37 studies that were considered for use in the analysis."

I think the executive summary should profile the key role that consequentiality now plays in contingent valuation surveys. I believe there is now a consensus that this is a key requirement for respondents to have incentives to respond truthfully.

Page 1, line 40. (Jim Opaluch) Should read "Construction" rather than "Construct".

Suggestion: incorporate change.

Page 1, line 44. (Jim Opaluch) This 2nd part of the sentence (following "and") is a bit confusing. It would be easier to read if it said "EPA used *hedonic wage studies that have risk estimates that are differentiated by ...*"

Suggestion: incorporate change.

Page 2, lines 13-14. (Jim Opaluch) I don't recall a discussion of using studies for morbidity risks, and I'm not sure how these studies would be used to estimate VSL. There was discussion of using studies of risk-risk tradeoffs, but I would think these need to be mortality risk tradeoffs, rather than morbidity risk tradeoffs, unless someone has an idea of how to compare morbidity risk vs. mortality risk. I also recall we recommended that EPA consider studies that value mortality risk in the transportation (e.g., highway fatalities).

Suggestion: discuss points to be included in the executive summary based on any revisions needed in text.

P 2. Line 17. (Matthew Neidell) "The SAB also finds that discounting does not correctly account for the effect of time on VSL. The EPA should use a more correct construct such as the value of statistical life-years lost rather than the present value of a future statistical death."

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I don't recall this being our conclusion.

Suggestion: discuss points to be included in the executive summary based on any revisions needed in text.

P 2, line 17. (George Van Houtven) “The SAB also finds that discounting does not correctly account for the effect of time on VSL. The EPA should use a more correct construct such as the value of statistical life-years lost rather than the present value of a future statistical death.”

I am not convinced that this is indisputable “a more correct construct”

Suggestion: discuss.

Page 2, lines 18-20: (Stephen Swallow) Should this be elaborated slightly in Exec Summary (with check for consistency in main report): Is this statement reflecting the concern that a delayed death may involve a qualitative change in quality of life for that delay (pain and suffering) that standard discounting does not address? If so – or if it reflects something similar – perhaps that sense should be included here.

Suggestion: elaborate as indicated.

Page 2, lines 25-26. (Jim Opaluch) I may not be recalling the discussion correctly, but my interpretation was that there was not significant growth in the literature used by EPA since 2011. The point is there is literature out there that EPA might have missed, including the studies referred to in the following sentence, studies in transportation fatalities, possibly studies of risk-risk tradeoffs, possibly including refereed papers in the gray literature, etc.

Suggestion: discuss whether text should be revised.

P 2, line 28. (Kerry Smith) “However, the SAB also recommends that the agency consider commissioning more studies or creating other incentives for new studies to improve the prospect for a deeper literature to support future reviews of VSL.”

I specifically recommended setting up a web page with data that would allow wage hedonic studies to be done; this recommendation is too vague.

Suggestion: revise to incorporate recommendation about web page.

Page 2, line 29: (Stephen Swallow) Such other incentives likely need to link to the job performance criteria (e.g., academic tenure and promotion criteria relative to the types of publications economics researchers would be expected to produce) for researchers who might produce such studies, such as establishing recognition for peer-reviewed publication of studies providing rigorous valuation information rather than focusing on new theoretical or methodological innovations primarily; this could include mechanisms to recognize state-of-the-art valuation or replication as noteworthy elements of a career portfolio.

Suggestion: no change indicated.

Page 2, line 42. (Jim Opaluch) I would make this stronger by saying “However, additional information is needed in the White Paper *to explain in detail precisely* how the weights were calculated ...” In some places in the White Paper, EPA provide a general indication of how something is done, but does not provide sufficient detail to understand exactly what they did (e.g., see our discussion on Page 3, starting on line 11, about calculating standard errors on VSL when they are not reported in the original study)

Suggestion: incorporate change.

Page 2. Discussion of Population Weighting. (Jim Opaluch) I recall we also indicated that, while studies other than those of a national population might be useful, EPA should not include studies of highly specialized groups. For example, regional studies might be useful if there are multiple regional studies across the nation. But it would not be a good idea to include a single study of one particular community. It is important to note that, by definition, hedonic wage studies are based on self-selected samples. For example, people who choose dangerous occupations likely have different systematically different risk preferences than those that choose safer occupations. This means, as we look at the cross section, those individuals who accept higher levels of risk for an offered wage premium will tend to be those with a lower WTA, and those who reject the wage premium will tend to have a higher WTA. Strictly speaking, the estimated VSL calculated from this data is not applicable VSL for an involuntary risk faced by the population as a whole (e.g., mortality risk from particulates).

Suggestion: incorporate these points in the main report and executive summary.

Page 3, line 1-2: (Stephen Swallow) Earlier material (last bullet of Letter to Administrator) suggests SAB will recommend that there is not an adequate basis for use of an income-elasticity, instead suggesting considering VSL estimates over time to derive an implied income elasticity. Does this recommendation place these two lines (points 3 and 4) of the Exec Summary in conflict?

Suggestion: discuss and revise as necessary.

P 3, line 3. (Kerry Smith) “...the EPA should explain how Hicksian and Marshallian measures of VSL were aggregated.”

Aggregated is probably the wrong word here –perhaps combined.

Suggestion: incorporate change.

P 3, line 5. (Kerry Smith) “The EPA should also consider undertaking future work to investigate the possibility of developing a more complex set of subpopulation weights that build upon what is known about the subpopulations covered in each of the available studies.”

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The weighting needs to consider the objectives of the sampling associated with the original studies; for the case of the studies based on hedonic wage models with CPS or other surveys that focus on hours worked and earnings/wage rates; weighting mixes a benefit transfer decision with construction of a population average based on the sampling criteria used to compose the sample that is the basis for the model.

Suggestion: revise executive summary and main text to incorporate point raised.

Page 3, Line 7, (Stephen Swallow) Chair’s note: I believe this sentence is referring to the reality that even the studies identified by EPA as nationally representative often involve a focus on a subset of subpopulations of the national population. For example, one hedonic wage study drew data only from a few age groups of workers (e.g. 40-year olds), thereby leaving out data for other age groups; and most or all hedonic wage studies focus only on data related to workers, eliminating retirees.

EPA should develop a procedure for obtaining a reasonably representative estimate of the VSL distribution by building from studies that offer a scientifically valid estimate for some subset(s) of the national population, by using benefit transfer or other approaches to apply this information to broader segments of the population and building up to an estimate of national value. This procedure should make clear to what extent the values of some subset(s) of the national population may be omitted from the estimation of a representative value or value-distribution of VSL.

Suggestion: incorporate suggested clarification in the main report and executive summary.

Page 3, line 7. (Kerry Smith) “Population Weighting.”

My comments suggested a distinction between weighting that is actually more properly considered a set of benefit transfer assumptions and the use of sample weights. This is not reflected in this summary and should be. Right now it is misleading. What is described as weighting attributes values to populations that are not represented by the samples involved --so it should not be described as weighting. I cannot agree with this summary.

P 3, line 11. (George Van Houtven) “In the White Paper, the EPA attempts to estimate the standard errors of the VSL when the original studies do not report them. The SAB was asked to comment on whether the methods used to estimate these standard errors are appropriate and scientifically sound. The SAB finds that the white paper does not provide detailed information about how the standard error of the VSL is calculated when the original studies do not report it.”

Like the more detailed discussion of this topic, this summary paragraph should more clearly distinguish between two topics addressed: (1) how the std errors of the INPUT VSLs from the

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source studies were calculated and (2) how the std errors of OUTPUT VSL estimates from the meta-analysis were calculated.

Suggestion: incorporate suggested change.

Page 3, line 32. (Jim Opaluch) The word “paper” in White paper should have a capital P.

Suggestion: incorporate change.

P 3, line 36. (Kerry Smith) EPA should “conduct non-parametric and parametric analyses without adjusting VSL values to account for differences in income but include an income measure as an explanatory variable in the parametric meta-regression.”

This is particularly important since GDP per capita was used as the measure of income; for some groups income has not increased –this discussion seems to have been dropped.

Suggestion: add sentence indicating that it is important since GDP per capita was used as the measure of income and for some income groups this has not increased.

Page 3, line 39. (Kerry Smith) “Overall Methodology for Analyzing the Data.”

I argued the measure of growth based on GDP and the failure to recognize that there has not been income growth were serious issues. These are overlooked. This is a separate issue from the Hicksian/Marshallian issue. Adjustment for income is part of benefits transfer and must reflect income measures consistent with how the VSL was estimated. These are not details. They are fundamental to the methodology. So I cannot agree with conclusion that methods are scientifically sound given these adjustments are considered as part of the methodology.

Page 3. line 44 and following lines. (Jim Opaluch) Should this refer to estimates that are obtained from the same dataset, rather than the same sample? The word “sample” suggests that the same individuals are involved, but not necessarily that the estimate are from precisely the same data. At a minimum, I would state that the fact that the estimates are derived from the same data as part of the rationale for grouping the estimates and treating them as a single observation.

Suggestion: incorporate change.

Page 4. Line 15. (Jim Opaluch) I recommend we start a new paragraph with “Additional information is needed...”

Suggestion: incorporate change.

Page 4, line 19. (Jim Opaluch) We should indicate that the actual equation should be included. I recommend we drop “if not the” and instead the sentence should read “The report should include the precise equation that is used by EPA and citations that establish the validity of the

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basic approach.” I recommend that the report allows a reader to understand precisely how calculations are done, so one can replicate the methods used by EPA.

Suggestion: incorporate change in the executive summary and main report.

Page 4, line 21-22. (Jim Opaluch) I recommend we say that documentation should be sufficient to allow a reader to know precisely how to replicate the calculations.

Suggestion: incorporate change.

Page 4, Line 22: (Stephen Swallow) Revise to:...recommends that, without compromising best known, science-based practice for quantitative estimation, transparency be applied as a criterion for selecting an estimator.

Suggestion: incorporate change.

Page 4, line 30. (Jim Opaluch) I recommend we start a new line with “The SAB finds ...”. Also, I again recommend that EPA be told to document their calculations with sufficient detail to allow a reader to know precisely how to replicate the calculations.

Suggestion: Incorporate change.

Page 4, line 35. (Jim Opaluch) I recommend we change the sentence to read “More importantly, EPA should also justify use ...” I don’t believe having a smaller standard error is a compelling rationale for the mean-of-group-means approach. For example, imagine an extreme case where there is a single study with a large number (e.g., 100) of VSL estimates that all are virtually identical, and a small set of other studies, each with a single estimate. In this case, it is conceivable that a simple mean has a smaller standard error than a mean-of-group-means. But it puts excessive weight on a single study. Comparing the sizes of the standard errors is not a particularly compelling argument to me.

Suggestion: incorporate change.

P 5, line 9. (Kerry Smith) “The SAB also recommends that the documentation of income adjustment to VSL be clarified in the White Paper. Adjustment of VSL estimates by an income elasticity of VSL and index of income growth (based on GDP per capita) does not seem to be appropriate. However, conversion of VSL to inflation adjusted dollars would be appropriate.”

This comment should be noted in the earlier discussion and did not make it into the summary; should GDP per capita be the income measure –I would say no –it is not income!

Suggestion: include this point in the discussion of the overall methodology for analyzing the data.

Page 5, line 12. (Jim Opaluch) Should we recommend a specific deflator that should be used? CPI seems to make more sense than the GDP implicit price deflator.

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Suggestion: discuss whether text should be revised.

Page 5, line 12. (Kerry Smith) “The SAB also recommends that the documentation of income adjustment to VSL be clarified in the White Paper. Adjustment of VSL estimates by an income elasticity of VSL and index of income growth (based on GDP per capita) does not seem to be appropriate. However, conversion of VSL to inflation adjusted dollars would be appropriate.”

This point is part of the overall methodology and should be made earlier as indicated by my earlier comment. I don't feel meta analysis with this adjustment process can be accepted and I feel we should make the point earlier and more forcefully. The income measure should correspond to what is most relevant for the sample used to estimate the VSL. The discussion here treats the issue as a detail. I feel it is central to the construction of the data for the meta analysis.

Page 5, line 20. (Jim Opaluch) New paragraph starting with “The SAB agrees ...”?

Suggestion: incorporate the change.

P 5, line 35. (Kerry Smith) “EPA should: (1) consider whether estimation of VSL and its various attributes should be a high priority topic for EPA grants and fellowships, sponsored conferences, special issues of journals, and awards...”

What happened to the web site with data –seems to me better than sponsoring a journal.

Suggestion: include recommendation for the website in the executive summary discussion.

P 5, line 40 (Kerry Smith). “Criteria for Inclusion and Exclusion of VSL Estimates in Future Analyses.”

“I had proposed establishing a web site where EPA routinely committed to merging the CPS and COFI risk data to increase access and explore possibility of removing confidentiality barriers -- why was this dropped as a high level recommendation?”

P 5, line 43. (Matthew Neidell) “... (2) the EPA should not restrict studies used for updating VSL to those published in peer-reviewed journals (studies outside of the peer-reviewed journals should be considered for inclusion following a transparent and rigorous peer review process) <<Chair’s note: this statement should be discussed by the Committee>> ;”

Agreed that we should discuss. Those opposed to this approach, including myself, felt very strongly.

Suggestion: discuss points to be included in the executive summary based on any revisions needed in text.

P 5, line 43. “ (Kerry Smith) ...(2) the EPA should not restrict studies used for updating VSL to those published in peer-reviewed journals (studies outside of the peer-reviewed journals should be considered for inclusion following a transparent and rigorous peer review process)...”

Who does the peer review and who manages this process?

Suggestion: revise executive summary to reflect discussion of this issue.

P 5, line 43. (Kerry Smith) “... the EPA should not restrict studies used for updating VSL to those published in peer-reviewed journals (studies outside of the peer-reviewed journals should be considered for inclusion following a transparent and rigorous peer review process)”

I don't agree; peer review is essential. Issue is how is that review obtained?

Page 5, line 46. (Jim Opaluch) I think it makes sense to include papers outside of the peer reviewed literature, following a transparent and rigorous peer review process. This process could easily be better than the journal peer review process, in that the review could focus specifically on use of the estimates for policy purposes. Peer review generally focuses on a paper’s intellectual contribution to the literature, not so much on quality of empirical estimates. But we might want to make a specific recommendation on how such a process be administered. For example, EEAC might administer the peer review process.

Suggestion: discuss the points to be included in the executive summary based on any revisions needed in text.

Page 6, Line 2, item (4): (Stephen Swallow) Revise to: “...(4) the EPA should not exclude studies based on non-national samples from use in updating VSL as long as there is a set of studies that as a group is representative of the nation as a whole can be used to either develop a representative estimate for the nation as a whole or to improve the representation of VSL values of subpopulations that are underrepresented or omitted from studies used to otherwise estimate a representative value for the nation as a whole; and...”

Suggestion: incorporate change.

Page 6, line 4. (Jim Opaluch) I also recall that some EEAC members indicated that studies of very specific samples (e.g., a specialized category of individuals in a small community) should not be included. Studies that are generally representative of State-wide or regional populations could be appropriate if they are as a group generally representative of the nation.

Suggestion: If changes in main report are made revise executive summary to be consistent.

P 6, Line 4. (Kerry Smith) “...(5) the EPA should consider a long term strategy of requiring that a more inclusive set of research results, and even whole data sets, be made generally available for use by the research community and by government agencies.”

This does not capture the fact that data do exist and could be combined every year with the risk information.

Suggestion: revise text in executive summary and main report to include point raised.

P 6, line 10. (Reed Johnson) “The SAB was asked to comment on whether the selection criteria for identifying studies for valuing reductions in risks of cancer mortality should differ from those used in the current White Paper. The SAB was also asked whether the literature supports a non-zero differential between valuation of cancer and other mortality risk. Based on EPA’s current study-inclusion criteria available studies, the SAB concludes that there is not sufficient evidence at this time to justify a non-zero cancer differential. The SAB recommends that, instead of adopting a non-zero cancer differential, the EPA consider using existing methods to value the morbidity that occurs prior to an early death and add that estimated morbidity value to conventional estimates of the value of the associated mortality. The EPA currently values morbidity from cancer in cases where the cancer is not fatal, but does not value morbidity in fatal cancer cases. The EPA should value cancer morbidity regardless of whether that morbidity leads to an early death. This recommendation also applies to other environment-related mortality risks, including cardio-pulmonary disease. In addition, the EPA should encourage and support ongoing research on whether willingness to pay to reduce the risk of an early death preceded by a period of morbidity is correctly valued by summing the value of the morbidity plus the value of the mortality. At this time, the SAB does not have evidence to suggest that approach would over- or under-state the true willingness to pay.”

This response assumes that morbidity and mortality are separate and additive outcomes. Although linked clinically, there is considerable literature on patients’ willingness to accept tradeoffs between quality and quantity of life. People also care how they die. There may not be sufficient evidence in the small number of studies included in the EPA analysis, but that doesn’t mean there isn’t sufficient evidence in the general health literature.

Suggestion: If changes in main report are made revise executive summary to be consistent.

Page 6, line 16. (Jim Opaluch) I don’t think we want recommend that EPA add together morbidity plus mortality values, as the two are not independent. The value applied to mortality would likely be lower for someone suffering from a long illness. I agree with the statement that follows on lines 22-23, but I think we should be more definitive on that. It is unlikely that value to reduce risk of early death preceded by a period of morbidity is correctly valued by adding together morbidity plus mortality.

Suggestion: If changes in main report are made revise executive summary to be consistent.

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P 6, line 17. (Matthew Neidell) “The EPA currently values morbidity from cancer in cases where the cancer is not fatal, but does not value morbidity in fatal cancer cases. The EPA should value cancer morbidity regardless of whether that morbidity leads to an early death. This recommendation also applies to other environment-related mortality risks, including cardio-pulmonary disease. In addition, the EPA should encourage and support ongoing research on whether willingness to pay to reduce the risk of an early death preceded by a period of morbidity is correctly valued by summing the value of the morbidity plus the value of the mortality. At this time, the SAB does not have evidence to suggest that approach would over- or under-state the true willingness to pay.”

I don't recall a lot of this, but again could be my memory. There is a recent study by Viscusi on morbidity and mortality in the journal of health economics.

Suggestion: discuss points to be included in the executive summary based on any revisions needed in main report text.

P 6, Line 21. (Kerry Smith) “EPA should encourage and support ongoing research on whether willingness to pay to reduce the risk of an early death preceded by a period of morbidity is correctly valued by summing the value of the morbidity plus the value of the mortality. At this time, the SAB does not have evidence to suggest that approach would over- or under-state the true willingness to pay.”

This should be the lead off comment –no basis for adjustment.

Suggestion: revise paragraph to make this the lead off comment.

P 6, line 26. Income Elasticity Literature (Reed Johnson)

There were a number of suggestions raised in the discussion, but I'm not sure we reached a consensus on this point.

Suggestion: If main body of the report is changed revise executive summary to reflect changes.

p. 6, lines 34-38, (Richard Carson) this is an incomplete characterization of the recommendation. The recommendation should be to look at the income elasticity of private consumer goods that can be purchased by the public to reduce environmental risks.

Suggestion: incorporate suggested change.

P 6, line 33. (Kerry Smith) “Very few studies have been conducted on the income elasticity of the value of statistical life. The SAB therefore recommends that the EPA consider the possibility of using estimates of the income elasticity for other related goods and services to infer estimates of the income elasticity of the value of statistical life.”

I do not agree.

Suggestion: discuss and revise to reflect any changes in main report.

Page 6, line 34. (Kerry Smith). “The SAB therefore recommends that the EPA consider the possibility of using estimates of the income elasticity for other related goods and services to infer estimates of the income elasticity of the value of statistical life. Going 36 forward, the EPA should support more research to provide methodological guidance and empirical estimates in this important area.”

I think this recommendation must be more speculative; there is not a direct parallel between income elasticity of other goods --perhaps for averting behaviors; until the theoreticla connections are developed --direct use of the income elasticity should not be suggested. First develop the proposed assumptions for an analytical connection --then consider using the appropriately restricted values. as it stands this is simply too vague.

Page 6, lines 34-36. (Jim Opaluch) I’m not sure I agree with this. I recall the discussion, but I’m not convinced that it is a sensible approach. I’d suggest the wording be even weaker than is present here, as the recommendation that “EPA consider ...” seems like we are recommending that they might just go ahead and do this. I’d prefer first using the sentence on lines 37-39. Then we might add income elasticity for related goods as a possible research topic. I’d prefer that EEAC revisit the concept of using income elasticities for related goods at a later date with more research in hand, rather than recommending that EPA consider it. But if this is the case, why not simply recommend that EPA fund studies on income elasticity of risk reduction. Logically, this also introduces that rather controversial topic of having a VSL that varies over income groups, which seems like a non-starter from a policy perspective.

Page 6, Lines 35-37: (Stephen Swallow) This recommendation on how to identify the income elasticity runs (partially?) in conflict with recommendations elsewhere that suggest not using elasticity estimates to adjust values or using VSL estimates over different points in time to identify an approximation of the income elasticity (possibly as related to changes in wealth between those points in time). For example, does this conflict with Page 7, lines 21-22?

Suggestion: Discuss and revise to make it reflect the main body of the report.

P 6, line 40. (Reed Johnson) Analysis of Very Low Income Elasticity Estimates

I believe it was George who advocated leaving all the estimates in, but calculating with and without zeros would handle that.

Suggestion: discuss points to be included in the executive summary based on revisions needed in main report text.

P 6, line 46. (Kerry Smith) “The SAB finds that it is highly unlikely for the income elasticity of VSL to be zero or negative. However, to address the issue of low/zero estimates, the SAB recommends that, instead of calculating an unweighted mean of income elasticity of VSL

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estimates, the EPA should use standard errors of individual income elasticity of VSL estimates to calculate a weighted mean.”

But we did make comments that estimates are random variables and the decision to drop zero or negative values implicitly makes an assumption distribution of the estimates is one sided. I think many members of committee would not agree.

Suggestion: discuss points to be included in the executive summary based on revisions needed in main report text.

pp. 6-7, (Richard Carson) analysis of very low income elasticity estimates. There is an inconsistency with this recommendation since we later recommend that the income elasticity estimate not be used (either with or without the zero/negative point estimates). It is not clear what lines 1-5 on p. 7 are trying to say. More generally, zero/negative point estimates are not unexpected given the known downward bias due to measurement error and sampling variation. There are two consistent approaches to imposing the constraint that income elasticities should be positive—impose the restriction in the specification of the functional form or use a Bayesian approach with a prior that has no support over the non-positive axis. This would require re-estimating the model on the original data. Otherwise, current meta-analysis practice is to include the negative/zero estimates under the guise that all sources of sampling variation should be included.

Suggestion: discuss points to be included in the executive summary based on any revisions needed in text.

Page 7, line 5: (Stephen Swallow) Revise to “...estimates to assess their influence, using the estimates of non-negative the income elasticity drawn from scientifically sound studies as legitimate information relative estimating the central tendency of income-elasticity estimates.”

Suggestion: incorporate revision.

Page 7, line 14. (Jim Opaluch) New paragraph starting with “Robinson and Hammitt (2015)...”

Suggestion: incorporate change.

P 7, line 17. (Kerry Smith) “The SAB finds that neither of the two alternatives put forward in Robinson and Hammitt (2015) and described in EPA’s technical memorandum represent an adequate basis for providing an estimate of the income elasticity of VSL for policy purposes. Therefore the SAB recommends that the EPA consider the alternative approach of using the preferred VSL model specification to obtain and compare VSL estimates at different points in time and use that to obtain the implied income elasticity of VSL.”

The letter to the Administrator does not appear as strong as this.

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Suggestion: revise letter to reflect the statements in the executive summary and main report.

P 7, line 20. (Reed Johnson) Study Selection Criteria and Alternative Approaches for Estimating Central Income Elasticity of Value of Statistical Life

“Therefore the SAB recommends that the EPA consider the alternative approach of using the preferred VSL model specification to obtain and compare VSL estimates at different points in time and use that to obtain the implied income elasticity of VSL.”

I believe this was Richard’s suggestion, but I didn’t sense complete consensus on this point, either. A lot of things change over time and it isn’t clear you can isolate income effects without some careful controls for other factors. If it isn’t possible to disentangle other effects, it isn’t clear this is a superior solution.

Suggestion: discuss points to be included in the executive summary based on any revisions needed in text.

P 7, line 26. (Reed Johnson) “The EPA’s Technical Memorandum recommends using the income elasticity of VSL to estimate income elasticity for the value of non-fatal health risks. The SAB was asked to comment on whether this represents an appropriate and scientifically sound approach given the available data. The SAB does not **fully** support using the income elasticity of VSL to estimate income elasticity for the value of non-fatal health risks because it is conceptually incorrect to apply income elasticity for one good to some other good.”

“fully” is too weak.

Suggestion: remove “fully.”

Page 7, lines 31-32. (Jim Opaluch) Is there a conceptual justification for using income elasticity of expenditures as a proxy for income elasticity of values of risk reduction? It seems to make intuitive sense that the income elasticity of expenditures for risk reduction reflect the income elasticity of value of risk reduction, but it would be good to provide a stronger theoretical justification for this recommendation. I’m concerned that from a theoretical perspective, the “technology” side (the marginal product) of health care products becomes embedded in income elasticity of expenditures. Is there any literature on the topic?

Suggestion: discuss points to be included in the executive summary based on any revisions needed in main report text.

Page 7, line 31. (Kerry Smith) “The SAB recommends that the EPA explore use of the income elasticity of expenditures on private health care products as a better proxy for the income elasticity of non-fatal health risks.”

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In this context as in the case of income elasticity of VSL --there needs to be the necessary analytical modeling to derive the relationship of income elasticity of related products and income elasticity of a transformation of MRS of risk of nonfatal health effects; without this connection the proposal is ad hoc.

Page 7, lines 29-31 (Stephen Swallow) versus Page 6, lines 35-36: Are these recommendations fully consistent (not in conflict)?

Suggestion: discuss and identify changes needed.

p. 7, lines 31-32, (Richard Carson) see the p. 6 qualification above that the goods at issue here are specifically consumer products that can be used to reduce environmental risks.

Suggestion: incorporate suggested revision.

P 7, line 31. (Kerry Smith) The SAB recommends that the EPA explore use of the income elasticity of expenditures on private health care products associated with serious health effect as a better proxy for the income elasticity of non-fatal health risks.

Challenge here is treatment of insurance in the estimation –so this will be very difficult.

Suggestion: no change indicated.

P 7, line 31. (Reed Johnson) The SAB recommends that the EPA **explore** use of the income elasticity of expenditures on private health care products as a better proxy for the income elasticity of non-fatal health risks.

Not a great recommendation. How should they “explore”?

Suggestion: change “explore” to “consider using.”

Letter to the Administrator

Page 1, line 31. (Jim Opaluch) There is a potential confusion with using 19 charge questions, numbered 1 through 17 (with 1a, 1b and 1c). I recommend we refer to 17 charge questions throughout the document.

Suggestion: incorporate change.

Page 1, line 43. (Jim Opaluch) This seems too vague. How about something like “As explained in the attached report, more detailed information ...” But also, more substantively, I think we should make the case throughout the whole report that EPA needs to provide detailed documentation of all calculations, adequate to allow an independent party to replicate those calculations. Much of this could be contained in one or more Appendices, and EPA might provide spreadsheets with the formulas used to do the calculations.

Suggestion: incorporate change.

Page 1, line 45. (Jim Opaluch) This is very specific, and not explained in manner that is understandable to a reader, which makes it appear rather mysterious. Either the statement should be made more understandable, or we should be more general (e.g., "SAB has several suggestions to refine some of the concepts, as explained in the attached report ...,"

Suggestion: make statement more general.

Page 2, line 4. (Jim Opaluch) There appears to be an editorial problem here. Maybe we should say "... EPA should also clarify ..."

Suggestion: incorporate suggested editorial change.

Page 2, line 9. (Jim Opaluch) Also the transportation literature on reduced risk for highway fatalities. I like the wording Reed used, "there is useful information that can be extracted".

Suggestion: incorporate suggested wording.

P 2, line 23. (Kerry smith) "However, additional information is needed in the White Paper to explain how the weighting was actually done and how the studies were brought together for the aggregate estimate. "

My comments on this were stronger; I questioned the validity of the procedures used.

Suggestion: revise to change the statement as suggested.

Second page, lines 38-39 (Stephen Swallow): Based on the knowledge of members of the committee regarding VSL literature, would the committee have a way to construct a recommendation for how EPA might construct an estimate by building up from samples (or studies) that are not explicitly national in the manner of those studies that EPA identified as national in scope? Many of the studies qualified as national nonetheless have gaps, due to selection of samples focused on specific age ranges (rather than the whole continuum of ages) or focused on workers rather than non-workers (e.g., excluding retirees or people otherwise not in the labor force). Thus EPA is already drawing together estimates by building up from imperfectly representative studies – while seeking and using best available information. Can the committee recommend that EPA develop a process to draw on available, more narrowly-focused studies to build a representative (or more completely representative estimate of value and distribution of VSL value) for at least some subset of the broader, national population?

And should EPA provide a summary of weaknesses in the final estimates developed, in relation to identifying explicitly those groups or subpopulations whose values might be under represented in the foundation for the value estimate and distribution obtained through this analysis? Example groups might be non-workers (e.g., retirees omitted from hedonic wage studies) or workers in non-hazardous occupations. Is EPA identifying clearly and explicitly the groups that are or are not proportionately represented through the use and aggregation of knowledge from existing, qualified studies?

Comments from individual members of the Science Advisory Board Environmental Economics Advisory Committee to assist meeting deliberations. These comments do not represent consensus SAB advice or EPA policy.
DO NOT CITE OR QUOTE

Suggestion: no change. This seems quite detailed for discussion in the letter to the Administrator.

P 2, line 42. (Kerry Smith) “The EPA report and technical memorandum on the income elasticity of VSL provide reasonable summaries of the income elasticity literature. However, the summary of the literature indicates that there is not an adequate informational basis for deriving a consensus estimate of the income elasticity of VSL. Therefore the SAB recommends that the EPA consider using the preferred VSL model specification to compare VSL estimates at different points in time and use that to obtain the implied income elasticity of VSL”

There is no discussion of the fact that for the group most likely to be represented in the wage hedonic models there was NO INCOME GROWTH; I realize that this is a BENEFIT TRANSFER ASSUMPTION; however it should have been highlighted in a separate bullet point.

Suggestion: add additional statement about no income growth problem.

Typos and edits

Exe summary, page 6, line 44: “...(1) whether this [is] an appropriate...”

P 6, line 44. “The SAB was asked to comment on: (1) whether this was an appropriate and scientifically sound choice, and (2) how very low, non-zero, mean reported income elasticity results should be addressed in the EPA’s analysis.”

Exec summary page 7, line 15: remove comma after “their analysis”.

p. 10, line 32: add “higher”

Page 15, line 11: delete “is” before “notes”.

Page 15, line 23: delete “s” on “populations.”

P 20, line 35. Weighting approaches should **to** give much greater consideration

Page 20, line 25: Delete “to” from “to give.”

P 20, line 35. Weighting approaches should **to** give much greater consideration

Page 22, line 14: Insert “the” before “whole” to read: “...representativeness of the whole population.”

Page 22, line 18: Add a period mark.

Page 23, Line 30: Revise to insert “to” before “the methods” to read: ...is related to the methods...”

Page 33, Line 38: Typo: Table 10 of the White paper shows this number as +11.1, which also conforms to the sense of the sentence on lines 35-36 here.

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DO NOT CITE OR QUOTE

Page 35, Line 1: "...regularly..." rather than "...regular..."

P 36, line 42. "For example, EPA might consider using **the** results of a risk-risk **study** studies that employed a stated-preference approach, wherein respondents were asked to choose whether to undergo treatment (e.g., a risky surgery) that has a stated risk of immediate mortality versus a given risk of cancer, which involves stated risks of both long term morbidity and subsequent mortality

Page 42, Line 24: Delete "is" before "that very little."

p. 46, line 11: Cost of Funds Index (COFI) should read "Census of Fatal Occupational Injuries" (CFOI)