

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 DO NOT CITE OR QUOTE.

**Committee Member Comments on the Draft (5-5-16) SAB Review of the EPA’s Proposed Methodology for Updating Mortality Risk Valuation Estimates for Policy Analysis**

**(As of 6/2/16)**

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***Dr. Richard Carson***

Throughout the report there is inconsistent capitalization of “White Paper” and confusion over adjusting estimates to take account of inflation (CPI adjustment) versus adjusting estimates to take account of changes in real income.

An overarching point that is not clear in this draft report, but underlies many of the comments and recommendation, is that there is not likely to be a single true VSL value but rather one that depends on the specific population from which data is obtained and the specific characteristics of the risk reduction. This is why it is important to obtain a good estimate of the variance of the VSL estimate to be used that takes this heterogeneity into account.

**Executive Summary**

**p. 6, lines 34-38**, 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.

**pp. 6-7**, 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.

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

**Section 3.1.1**

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

**p. 10, lines 46-47**, 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.

**p. 11, lines 23-31**, and later points in the draft. 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.

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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.

### **Section 3.1.2**

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

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

**p. 17, lines 38-39**, 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.”

### **Section 3.1.5**

**p. 22, line 6**, drop “more complex”.

**p. 22, line 10**, change to “standard benefit-transfer and statistically oriented benefit-transfer approaches”.

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

**p. 22, line 33**, change to “Although this approach could”.

### **Section 3.1.6**

**p. 23, line 35**, change to “provide sufficiently detailed”.

**p. 25, line 14**, change “fails to” to “does not”.

**p. 25, line 15**, insert “enough” after “detailed”.

**p. 25, lines 15-16**, change “In fact ... only” to “There are only”.

**p. 25, line 17**, drop “does not discuss”.

**p. 25, line 18**, change “estimates at all” to “are not discussed at all”.

**p. 25, line 26**, change “the bootstrap” to “this”.

**p. 25, line 31**, change “why re-sample” to “why”

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**p. 25, line 31**, change “In fact, the” to “The”.

**p. 25, line 33**, change “4” to “four”.

**p. 25, line 39**, change “problems” to “issues”.

**p. 26, line 33**, change “fails to” to “does not”.

**p. 26, line 38**, change “proposes” to “suggests”

### **Section 3.2.1**

**p. 27, line 17**, change “narrower” to “narrow”.

**p. 27, line 38**, drop “very”.

### **Section 3.2.2**

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

**p. 30, line 5**. replace “, if not the literal equation” with “and the specific equation”.

### **Section 3.2.4**

**p. 31, lines 10—11**, 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.”

**p. 31, lines 22-23**, drop “However, ..., recent study.”

**p. 31, line 24**, 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.”

### **Section 3.3.1**

**p. 32, line 36**, change “explicit and not buried in” to “explicitly stated and not simply subsumed in”.

**p. 33, line 15**, something is missing or confused in this sentence.

**p. 33, line 19**, change “by an” to “by both an”.

**p. 33, line 20**, change “would be appropriate” to “is appropriate”.

### **Section 3.3.2**

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

**p. 33, line 42**, change “technic” to “technique”.

### **Section 3.4.1**

**p. 36, lines 17-35**, 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.

**p. 36, line 34**, change “companies” to “companies for government agencies”.

**p. 38, line 25**, change “correct” to “adequately take into account”.

**p. 38, lines 37-38**, 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.

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

**p. 39, lines 27-28**, 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.

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

### **Section 3.5.1**

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

**p. 42, lines 26-31**, 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.”

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

### **Section 3.5.2**

**p. 43**, see comments on pp. 6-7, above. 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, then see comment on pp. 6-7 above on statistically valid approaches.

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### **Section 3.5.3**

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

**p. 46, line 17**, change “COFI” to “CFOI”.

**p. 47, lines 21-30**, 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.

*Dr. Mary Evans*

### **Section 3.1.1. Evidence of validity**

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 3<sup>rd</sup> and 4<sup>th</sup>) as they require a significant degree of subjectivity to determine what is meant by “clearly”.

### **3.1.2. Construct of the risk variable in hedonic wage studies**

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).

### **3.1.4. Empirical studies**

I 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.

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**Typos:**

p. 10, line 32: add “higher”

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

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*Dr. Wayne Gray*

I'll be unable to participate in the conference call, but I spent some time reviewing the draft report, paying particular attention to the section I was responsible for writing up (charge question 1b on the construct of the risk variable in hedonic wage studies). It all looked fine to me (one minor typo on p. 15, line 11 - the extraneous "is").

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***Dr. Matthew Kotchen***

I have read the draft report and have no written comments and to submit at this time.

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***Dr. Matthew Neidell***

### **Executive Summary**

**P 2, Line 17.** “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 don't recall this being our conclusion.*

**P 5, line 43.** “... (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.*

**P 6, line 17.** “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.*

### **Section 3.1.3**

**P 17, line 44.** “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.*

### **Section 3.1.5**

**P 20, line 21.** “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. “

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*I don't quite get this comment.*

### **Section 3.1.6**

**P.23, line 44.** “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.*

### **Section 3.3.2**

**P. 34, line 15.** “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?*

**P 34, line 20.** “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.*

### **Section 3.4.1**

**P 36, line 29.** “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-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.”

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*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.*

### **Section 3.4.2**

**P 41, line 22.** “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*

### **Section 3.5.1**

**P 42, line 26.** “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.*

### **Section 3.5.3**

**P 45, line 15.** “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.*

**P 45, line 21.** “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)*

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**P 46, Line 15.** “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?*

**P 46, line 27.** “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?*

***Dr. James Opaluch***

**Letter to the Administrator**

**Page 1, line 31.** 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.

**Page 1, line 43.** 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.

**Page 1, line 45.** 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 ...,"

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

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

**Executive Summary**

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.

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.

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

**Page 1, line 19.** 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 ..."?

**Page 1, line 40.** Should read "Construction" rather than "Construct".

**Page 1, line 44.** This 2<sup>nd</sup> 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 ...*"

**Page 2, lines 13-14.** 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).

**Page 2, lines 25-26.** 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.

**Page 2, line 42.** 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)

**Page 2. Discussion of Population Weighting.** 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).

**Page 3, line 32.** The word “paper” in White paper should have a capital P.

**Page 3, line 44 and following lines.** 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 estimates 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.

**Page 4, line 15.** I recommend we start a new paragraph with “Additional information is needed...”

**Page 4, line 19.** 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 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.

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

**Page 4, line 30.** 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.

**Page 4, line 40.** 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.

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

**Page 5, line 20.** New paragraph starting with “The SAB agrees ...”?

**Page 5, line 46.** 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.

**Page 6, line 4.** 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.

**Page 6, line 16.** 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.

**Page 6, lines 34-36.** 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 7, line 14.** New paragraph starting with "Robinson and Hammitt (2015)..."

**Page 7, lines 31-32.** 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?

### **Introduction**

**Page 8, line 5.** I recommend that we say "estimates" rather than "takes into account".

**Page 8, line 18.** change literature to literatures and add comma, so it reads "... stated preference and hedonic wage study literatures, and..."

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

**Page 8, line 34.** We should also report the conference calls.

### **Section 3.1.1**

**Page 9.** I like listing the full charge questions.

**Page 9, line 17.** Should we emphasize that this was work of another committee? e.g. "A previous committee of the SAB ..."

**Page 9, line 26.** the word “that” is repeated.

**Page 10, lines 3-8.** 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.

**Page 10, paragraph on lines 10-17.** 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.

**Page 10, line 31.** “effects”

**Page 10, lines 32-33.** “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.

**Page 11, line 2.** 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. 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 of 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  $\exp(\text{risk}_i)$ ).

**Page 11, line 15.** 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”.

**Page 11, line 19-20.** How strongly should we push for a binary choice survey?

**Page 10-11.** 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.

**Page 12, lines 20-22.** 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.

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

**Page 12, line 30-32.** 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.

**Page 13, line 6.** 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.

**Page 14.** 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.

### **Section 3.1.2**

**Page 15, line 8.** Add comma: “... existing research, and either...”

**Page 15, line 11** Drop the word “is”.

### **Section 3.1.4**

**Page 18, line 27.** 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.

**Page 18, lines 27-34.** Shouldn’t we list additional studies that EPA should use, prior to indicating that EPA should fund additional research.

**Page 19, bullets.** Again, doesn’t make sense first to recommend EPA consider additional available studies, then go on to recommend that EPA commission new studies.

**Page 19 line 3 (and line 25).** What is meant by “more detail or information”?

**Page 19, line 28.** I agree we need to provide more specific guidance. I’m not sure what we are recommending here.

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

**Page 19-20.** We need to clarify this sentence. I'm not sure what it means.

### **Section 3.1.5**

**Page 20, lines 3-4.** 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.

**Page 20, line 4.** The last phrase seems awkward here. What is meant by "above a standard that each study set".

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

**Page 20, lines 38-40.** This sentence is really difficult to read.

**Page 20, line 42-43.** 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.

**Page 21, line 14.** We should be consistent, and either Capitalize both "Population" and "Census" or capitalize neither of them.

**Page 22, line 3.** 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.

**Page 22, line 40 (and Page 23, line 11).** 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).

### **Section 3.1.6**

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

### **Section 3.2.1**

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

**Page 28, line 6-7.** 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 2<sup>nd</sup> 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,  $\eta$  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 2<sup>nd</sup> 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.

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**Page 28, line 38.** 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.

### **Section 3.2.2**

**Page 29, line 15.** The word "that" is repeated.

**Page 29, line 15-16.** Should we say "data sets" here rather than samples?

### **Section 3.2.4**

**Page 30, line 30-31.** 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."

**Page 30, lines 41-42.** 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).

**Page 31, line 12.** 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.

**Page 31, line 15-17.** "... 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.

**Page 32, line 2.** 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.

### **Section 3.3.1**

Page 32, lines 22-37. 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?

**Page 32, line 34-37.** 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.

**Page 32, line 39-40.** Should we give a brief rationale here? "... does not seem appropriate because ..."

**Page 33, line 3.** 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.

**Page 33, lines 7-8.** 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.

**Page 33.** 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.)?

### **Section 3.3.2**

**Page 33, line 31-32.** "... and given the relatively small number of VSL estimates"

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

### **Section 3.4.1**

**Page 36, lines 6-8.** 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?

**Page 36, line 10-11.** 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.

**Page 38, lines 9-13.** 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.”

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

### **Section 3.4.2**

**Page 40, lines 17-20.** 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.”

**Page 41, lines 13-15 and page 41, line 3.** 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.

### **Section 3.5.1**

**Page 42, lines 22-24.** 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 ...”

**Page 42, line 26-28 and lines 44-46.** 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 income elasticity for related market goods and services without first establishing a conceptually defensible approach for doing so.

### **Section 3.5.2**

**Page 43, line 11.** “highly implausible” might be better than “highly unlikely” here.

**Page 43 lines 11-19 and lines 26-27.** 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 suggest that the value of reducing risk is likely to be a luxury good, especially for risks within the purvue 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 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).

### **Section 3.5.3**

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

**Page 45, lines 8-23.** 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.

**Page 45, lines 34-37.** 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.

**Page 46, line 5.** I would use the phrase “systematically different”. Random differences across individuals would not be a problem.

**Page 46, lines 10-30.** 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.

**Page 47.** 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.

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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.

#### **Section 3.5.4**

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

**Page 47, lines 36-38.** 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.

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***Dr. Andrew Plantinga***

I had time to consider in depth the charge questions I was assigned to. I feel that my comments, and the discussion of the SAB, is accurately reflected in the report.

*Dr. Kerry Smith*

### **Overarching Comments**

**1. Adjustment versus conceptual issue associated with logic of benefits transfer** --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.

**2. Sources of Risk Tradeoff measures** -- 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 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.

**3. Developing a data platform** -- 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.

**4. Primary Research Questions** -- 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

### **Letter to the Administrator**

**P 2, line 23.** “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.*

**P 2, line 42.** “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.*

### **Executive Summary**

**P 1, line 37.** “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.*

**P 2, line 28.** “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.*

**P 3, line 3.”**...the EPA should explain how Hicksian and Marshallian measures of VSL were aggregated.”

*Aggregated is probably the wrong word here –perhaps combined.*

**P 3, line 5.** “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.”

*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*

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*construction of a population average based on the sampling criteria used to compose the sample that is the basis for the model.*

**P 3, line 36.** 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.*

**P 5, line 9.** “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!*

**P 5, line 35.** “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.*

**P 5, line 43.** “...(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?*

**P 6, Line 4.** “...(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.*

**P 6, Line 21.** “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.*

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**P 6, line 33.** “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.*

**P 6, line 46.** “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 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.*

**P 7, line 17.** “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.*

**P 7, line 31.** 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.*

### **Section 3.1.1.**

**P 11, line 10.** “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.*

**P 13, line 18.** “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*

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*needs to be a discussion of potential biases associated with nearly all the CV studies used by EPA.*

**P 12, line 40.** “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.*

**P 13, line 6.** “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.*

**P 14, line 10.** “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?*

### **Section 3.1.2.**

**P 15, line 25.** “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*

**P 15, line 28.** “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.*

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**P 15, line 29.** “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.*

**P 15, line 33.** “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.*

**P 15, Line 35.** “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.*

**P 16, line 19.** “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.*

**P 16, line 22.** “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.*

#### **Section 3.1.4.**

**P 19, line 25.** “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.>>”

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

### **Section 3.1.5.**

**P 20, line 25.** “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.*

**P 20, line 41.** “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.*

**P 23, line 4.** 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.*

### **Section 3.2.1.**

**P 28, Line 12.** “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.*

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#### **Section 3.2.4.**

**P 31, line 21.** “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.*

**P 32, Line 2.** “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.*

#### **Section 3.3.1.**

**P 32, line 41.** “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.*

**P 33, line 7.** “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?>>.”

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

#### **Section 3.3.2.**

P 33, line 36. “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”*

### **Section 3.4.1.**

**P 36, line 20.** “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.*

**P 37, line 44.** “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.*

**P 38, line 46.** “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 here—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.*

### **Section 3.5.1.**

**P 42, line 26.** “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.*

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**P 42, line 44.** “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.*

### **Section 3.5.3.**

**P 44, line 36.** “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.*

**P 44, line 37.** “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.*

### **Section 3.5.4.**

**P 47, line 28.** “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.*

***Dr. Stephen Swallow***

**Letter to Administrator**

**Second page, lines 38-39:** 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?

**Executive Summary**

**Page 2, lines 18-20:** 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.

**Page 2, line 29:** 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.

**Page 3, line 1-2:** 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?

**Page 3, Line 7, 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.

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

**Page 6, Line 2, item (4):** 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..."

**Page 6, Lines 35-37:** 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?

**Page 7, line 5:** 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."

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

## **Main report**

### **Section 3.1.1**

**Page 10, line 33:** Revise to read "...willingness to pay to increase for a larger..."

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**Page 10, line 34:** Revise to read "...stated preference question was placed..."

### **Section 3.1.3**

**Page 17, line 3:** 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)?

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

**Page 17, line 38:** Is this a conjecture? Is there citable evidence? What is the actionable recommendation here?

**Page 18, lines 15-17:** While conceptually, this recommendation is sound (as are the comments on page 17 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?

**Page 18, line 45:** Revise to: "...maybe subject to non-responses biases that would require EPA to address in calculation of a representative estimate of value."

### **Section 3.1.5**

**Page 20, line 2:** 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.

**Page 22, lines 32-33:** 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."

**Page 22, lines 5-36:** 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?

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### **Section 3.2.3**

**Page 30, line 8:** 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.

### **Section 3.2.4**

**Page 31, Lines 20-27:** 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?

### **Section 3.3.1**

**Page 33, Line 6:** “discuss the income used”. This phrase is unclear. Income elasticity? Income variables? Income value? Income growth?

### **Section 3.4.1**

**Page 36, line 31 and line 33.** 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.”

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**Page 36, line 33:** revise as for line 31.

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

#### **Section 3.5.4**

**Page 47, Line 23-24:** 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.

#### **Typos:**

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

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

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

**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.

**Page 35, Line 1:** “...regularly...” rather than “...regular...”

**Page 42, Line 24:** Delete “is” before “that very little.”

#### **Regarding EPA response to EEAC requests for information, transmitted May 11, 2016:**

**Page 13:** 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.

**Page 13 again:** 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.

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***Dr. George Van Houtven***

### **Executive Summary**

**P 2, line 17.** “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”*

**P 3, line 11.** “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 source studies were calculated and (2) how the std errors of OUTPUT VSL estimates from the meta-analysis were calculated.*

### **Section 3.1.3**

**P 17, line 34.** “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.*

**P 18, line 15.** “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.*

### **Section 3.1.5**

**P 23, line 8.** 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.*

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.

### **Section 3.1.6**

**P 26, line 33.** 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.”

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

### **Typos**

**P 15, line 11.** The SAB ~~is~~ notes that the..

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