

# **Response to Peer Review Comments on “Colorado River Total Value Survey: Draft Final Report, August 2016”**

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The purpose of this report is to detail how peer review comments on the report “Colorado River Total Value Survey: Draft Final Report, August 2016” were considered and addressed in preparing the final report for the study. The peer review comments are shown, followed by a brief statement of how the comment was incorporated in the final report.

## **Response to Comments of Dr. John Loomis**

### **Overall Assessment**

This is a carefully conducted study in terms of survey implementation (use of Dillman repeat mailing, addressed based sample, and especially a formal non-response follow up check by phone), reweighting of the sample data to adjust for non-response, and rigorous statistical analysis to evaluate the robustness of the results. Overall, the conclusions are supported by the results presented. I think the use of the willingness to pay (WTP) estimates to provide a ranking of EIS alternatives is a productive way to illustrate the policy relevance of the results. I do have several suggestions for improving the clarity of the exposition and for strengthening the confidence the authors and readers can have in the results of the study.

**Author’s Response:** The authors appreciate this validation of their overall methods and work.

### **Executive Summary.**

Page 8. Defining the “local area sample”. While this sample strata is referred to repeatedly during the next several pages of the Executive Summary, it was not clear to me what the area was (I was originally assuming it was the four state area Welsh, et al. used). It is not until page 21 did I see it was an 8 county area. I think this needs to be defined here on page 8.

**Author’s Response:** An explicit definition of the sample strata has been added as suggested.

Page 9. Correction for National Park visitation. I think this is appropriate and certainly worked well in the Haeferle, et al. (2016) report you cite. An option to using your “non-

response check” estimate of National Park visitation, the University of Wyoming Survey Research and Analysis Center (Taylor, et al. 2011) provides a national estimate of 47% of the U.S. public visited a National Park in the last two years. This is the most valid estimate I know of since the respondent had to name the Park unit and the interviewer had to verify it was on the list of official NPS units. Using this estimate is an option you might want to consider instead of or in addition to (for sensitivity analysis) your non-response check.

**Author’s Response:** We considered using the Taylor estimate of park visitation as a weighting factor. In the end we used the same statistic from our non-response survey. The two statistics were marginally different (47% vs. 59%) for the national sample. However, after considering the resulting statistical difference using the alternative weight would make, we decided to use the value from the current study for consistency of question wording (these questions were identical in the mail and phone non-response surveys).

Page 9. Minor. The WTP utility model description (next to last line of Page 9) refers to “choice occasion” which sounds more like recreation model terminology than a non-use value survey. Perhaps “choice alternative”.

**Author’s Response:** Thank you. Change was made as suggested.

Show an example Choice set WTP question format from the survey (e.g., page 86 of your report) and a table of the alternative levels of the four attributes (cost, beaches, native fish, trout populations) before jumping into Analysis Methods on Page 9. Many readers will not have seen the survey, or be familiar with discrete choice conjoint and not realize different people got asked different levels of the four attributes. If they see the WTP question and the table of the attribute levels, then they may have a better sense why all this statistical modeling is necessary (although I do think you could reduce the amount of detail on statistical modeling in the Executive Summary—there is a risk you could lose people at that point and they never go on).

**Author’s Response:** We have included samples of the DC survey question and a table showing alternative attribute levels on pages 11 and 12 of the final report.

Page 17, Table ES7. Minor point. Negative numbers are shown with (parens) rather than negative signs. I think negative signs will be more obvious to readers, especially since later you use parens for the standard errors.

**Author’s Response:** We have changed the representation of negative numbers in the report to –XXX format.

Page 18, Table ES8. Perhaps this will be cleared up in the main body of the report, but you note on the bottom of Page 17, that the marginal values using continuous percentage changes are lower than the discrete attribute levels. You don't offer an explanation here (perhaps you do in the report—I haven't got that far yet, but reviewing the Executive Summary as a stand alone which it may be for the vast majority of readers). So if you have a reason for the difference in marginal values it would instill more confidence to explain why the reader might expect lower values for the continuous percentage attributes than the discrete ones. My thoughts are that it appears you did not include the “trout” attribute in the continuous case but you did in the discrete. Further, while I like the continuous model, it imposes a more restrictive functional form that the marginal values are the same across all levels of the attribute, while the discrete model is more like a piece wise regression which allows the slope to change with different levels of the attributes (I do realize Figures ES1 and ES2 suggest the slope of the two attributes seems fairly linear, but the coefficients don't look all that similar in Table 5 ES5. Perhaps it is the scale of the graph.). Anyway just a thought on explaining why one might expect differences in marginal values between the discrete attribute model and the continuous one.

**Author's Response:** Thank you for your thoughts on this. We have included additional discussion and explanation of this point on pages 20 and 69 of the report.

Table ES11, Conservative Aggregate NEV of Alternatives. I really like the display of the ranking of the alternatives by WTP for each sample. I think that is a very productive way to think about the policy relevance of the WTP results. From a “decision analysis perspective” the policy choice in the EIS it is which alternative to pick as the Preferred Alternative. This involves ranking alternatives, and the exact magnitude of the dollar amounts aren't as critical as they are in NRDA. In fact, this ranking approach helps blunt the “hypothetical bias” criticism of stated preference methods: if there is any hypothetical bias, as long as it is the same across all the estimates of WTP for the EIS alternatives, then the relative ranking of the net economic value of the EIS alternatives will be unaffected. If you think this is a valid point, I am not sure if you want to put that here in the Executive Summary, in the main body of the report, as a footnote somewhere, or wait to use it to respond to the usual criticism of any stated preference study such as this.

**Author's Response:** We have included this valid point in the text.

8. Page 22. Model Validation. I think this is a good way to close out the Executive Summary. It could be strengthened if you want by:

Bullet #1. add at the end of the sentence: this indicates households were paying close attention to the dollar amount they were asked to pay, and took the dollar amount seriously.

Bullet #5. The *binary WTP* question format was a referendum format *that used taxes as payment vehicle* recommended by Arrow, et al. *blue ribbon commission on contingent valuation*. The added text provides a little more detail and some context to those who might not know much of anything about discrete choice WTP questions or what Arrow, et al. is. (*bold italics* are suggested additions to text).

**Author's Response:** We have made the edits as suggested.

### Main Report

Page 30. Minor. Describing the questions as “choice occasions”. How about choice tasks or choice sets?

**Author's Response:** We have replaced “occasions” with “tasks.”

Address based sample: this is certainly a strong feature of the sample design. Page 38 and in the Page 8 of the Executive Summary: You might mention that the Haefele, et al. study was also on National Parks. Readers might wonder why you compare your study to Haefele, et al. unless you mention they are both about National Parks. Also it would be good to spell out CSU as Colorado State University here and the Executive Summary, as to many readers, CSU could very well be California State University.

**Author's Response:** Noted and done.

Page 42, footnote #7, “true-false” questions. The second point that people thought “reducing daily fluctuations in the amount of water released from the dam will reduce the amount of hydropower”. Those people that mistakenly believe this statement is true probably would have lowered their WTP for alternatives involving reducing fluctuations and make your WTP estimates for those alternatives lower than they would be if their perceptions were correct.

**Author's Response** noted.

Page 46, Figure 9. You might mention that the 64% of respondents that agreed “I think my taxes will increase....” demonstrates consequentiality. In particular, as Carson and Groves, 2007: 191-192 (EARE) note, the respondent needs to believe the if the agency implements a particular alternative that the specific quantity will be provided and the stated price will be assessed. The payment vehicle must be such that you cannot opt out of it (e.g., taxes). It seems that since you have a taxes payment vehicle and 2/3 of respondents believe that their taxes will increase you have met the conditions for consequentiality, hence minimizing hypothetical bias. Further, since your question format

is binary it is incentive compatible. Mitani and Flores, (2014) demonstrate that if respondents believe they will have to pay, this reduces hypothetical bias. Further, if you have evidence from any questions that respondents believe that their responses to the survey will have an influence on the management of the Colorado River through Grand Canyon, this would further reinforce the notion that your survey has consequentiality, and hence minimum hypothetical bias (Vossler and colleagues—references at the end of the review). I think these are points worth mentioning here and in the Executive Summary.

**Author's Response:** We have included additional text to reflect these points on page 50 of the final report.

Page 59. Tables 14 and 15. You could cite the Champ, et al. JEEM 1997 and Champ and Bishop (2001) that indicate that if respondents are certain about their responses these articles demonstrate there would be a good match between respondent stated WTP and actual cash WTP. As such there would be minimal hypothetical bias. This is worth mentioning.

**Author's Response:** reference to these studies and further discussion of the points raised have been included in the final report.

Page 60, Table 17. The low percentage of responses agreeing with the first question in Table 17 (I voted for the proposed plan because I thought it would increase the chances the government would do the same thing in a river basin closer to my home) indicates that respondents are just valuing the Colorado River through the Grand Canyon NP. Thus this would indicate the survey has face validity in that the respondent is valuing what the researcher intended and not something larger. This is worth mentioning.

**Author's Response:** This point has been added on page 63 of the final.

Page 60, Table 17. The high responses to the question “I voted for the proposed plan more for future generations than for myself” suggests a high level of bequest value—this might be worth mentioning.

**Author's Response:** This point has been added on page 63 of the final.

Page 61. Minor. Instead of saying lower “odds” why not say something less technical like lower likelihood, or even lower probability (although I realize this is not strictly correct—but it more understandable).

**Author's Response:** Changed in test

Page 63, Table 19, Page 65, Table 20, Page 66 Table 21. All of these use (parens) to represent negative numbers instead of a negative sign. I think a negative sign would be clearer.

**Author's Response:** Throughout the report negative values are now represented with (-XXX) notation.

What is the difference between the coefficients in Table 19 and Table 21? It seems they are the same coefficients, but this is not obvious without careful inspection due to different rounding. And I think it would be worthwhile to say they are the same logit equation, but that you are showing the same coefficients from Table 19 along with marginal values in Table 21.

**Author's Response:** Noted.

Page 70. Section 4.4.3. Zero Values for Non Respondents. I would have expected that what you were talking about was a zero \$ value for the percentage of the original sample that did not respond to the survey at all (that is what Haeefe, et al. did—so 82% of the U.S. households got assigned a zero value). But this section actually addresses a different (but also important topic): assigning an explicit value of zero \$ value to the percent of *respondents* that said they were voting for the No Action Alternative/Existing Management Plan because they were opposed to more taxes or government spending. This is not what Haeefe, et al. did.

**Author's Response:** Thank you, This has been clarified in the text.

Page 70. Handling the percentage of the sample that did not return the survey. The first sentence of Section 4.4.3 indicates that your reweighting of the sample by National Park visitation took care of non-response bias so you applied the resulting marginal values per household in Table 24, to the entire number of U.S. households. If that the correct interpretation, you might at least footnote it.

**Author's Response:** We have added footnote #10 to clarify this point.

Section 4.6 Model Validation. You might add the additional points suggested for Model Validation in Executive Summary (specifically comment #8, in Section A. Executive Summary). This would further strengthen case that you have a valid model.

**Author's Response:** Done.

## Survey Appendix A.

You need to fix the survey example of the choice question on page 86 and 87 as on these pages the survey example shows “cost to your HH”, instead of spelling out *household* like you do on page 57 where you show what the choice question looks like in the final survey.

**Author’s Response:** Thank you.

## **Appendix B. Glen Canyon Pretest Report**

Page 98. It appears that in the Pre-test that an internet version was offered as well as mail. Is that true? If so that might explain a little of the reduced response rate with the main survey, since I believe only a mail survey option was offered. In two recent surveys of mine (including the Haeefe, et al.—although the discussion of this was dropped during the multiple rounds of final editing) about 20% of the total completed surveys were completed on-line via internet.

**Author’s Response:** The survey (both pre-test and final) was only administered by mail.

Page 100 true-false results tabulation versus Main Report Figure 5, Page 43. The table on page 100 presenting the wording of the true-false question and the percent correct provides much more information than Figure 5 on page 43. I would suggest using the format of pretest table to report the results of the true and false questions in the main report.

**Author’s Response:** Noted.

## References

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Vossler, Christian A., Maurice Doyon, and Daniel Rondeau. 2012. Truth in consequentiality: Theory and field evidence on discrete choice experiments. *American Economic Journal: Microeconomics* 4:145–71.

Vossler, Christian A., and Mary Evans. 2009. Bridging the gap between the field and the lab: Environmental goods, policy maker input and consequentiality. *Journal of Environmental Economics and Management* 58:338–45.

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**Author's Response:** We appreciate the careful review of the draft report and all suggested edits and additional citations.

## **Response to Comments of Dr. Lucas Bair**

Thank you for the opportunity to review the report. Overall, it reads well and provides sufficient detail to inform on the management alternatives in the LTEMP EIS. The methods are consistent with standard practice and results are well validated. Suggested edits are provided in track changes throughout the report.

**Author's Response:** Thank you. We appreciate the very careful edit of the document and the many suggestions made within the document for clarifying wording, formatting, and general exposition of the results. We have included these suggestions throughout the document in the Final version of the report.

Two general suggestions:

1. Annotating the WTP equations would make the information much more approachable. I have provided examples in the report.

**Author's Response:** We have adopted your first suggestion for annotating the WTP equations in order to make them more approachable and understandable.

2. Consistent nomenclature should be used in the report. For example; a) the two samples should be defined (e.g., National Sample, Local Sample) and used consistently in the text and tables throughout the report, b) specific titles should be used when referring to various EIS documents (e.g., LTEMP DEIS, LTEMP FEIS).

**Author's Response:** Thank you, We have searched the report in order to ensure that appropriate nomenclature is used to identify documents produced in the EIS process. The exception is when the overall EIS process is referenced, rather than a specific document.