APPENDIX 1. Responses to Peer Reviews

Restoration of a Desert Lake in an Agriculturally Dominated Watershed: The Walker Lake Basin

What follows are the authors responses, in italics, to peer reviewer comments. To assist the peer review process we provided the reviewers with a series of questions, listed below.

Questions for peer review:

In addition to any general comments please address the following questions.

1. Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation?

2. Is the paper clear, well organized and concise?

3. Are the methods appropriate, current, and described in sufficient detail?

4. If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct?

5. Are all tables and figures necessary, clearly labeled, and readily interpretable?

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper?

7. Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted?

8. Can the paper be published with:
    i. Minor revisions
    ii. Moderate revisions
    iii. Major revisions
    iv. Too flawed to be published, even with major revisions

Contents

Past Elevation and Ecosystems of Walker Provide a Context for Future Management Decisions

Neither reviewer was aware that this chapter served as an introductory chapter to 300+ pages of specific research projects on Walker River and Walker Lake I responded to their comments accordingly.

Response to Reviewer 1

The manuscript presents results from a comprehensive, integrated ecology research project for the Walker River and Walker Lake that is based on process-oriented modern sampling and a review of existing literature providing abundant paleo-data. Walker Lake faced extreme variations in lake level and thus changes in water volume and salinity during its 30,000 year-long history. This paper reviews the effects of these drastic changes on ecosystems and I find this a novel and very important approach. Interestingly, the author comes to the conclusion that the Walker River is the key to species survival in Walker Lake. I also find especially important that the papers stresses the consequences and feedbacks of human impact on the lake level caused by ground water pumping because this complexity and large-scale changes often are under-estimated.

This paper is an important and innovative contribution to aquatic ecology because it combines paleo-data with management aspects of a lake-river system that should be published. I would suggest, however, that minor to moderate revisions are made and will provide a few more detailed comments below. I am aware that some of the questions arising to me may result from not knowing the entire volume or misunderstandings. Partly my suggestions derive from curiosity because I find this paper so interesting, and I would like to learn more about this topic.

1.Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation? Yes.

2. Is the paper clear, well organized and concise? In general yes. I do miss, however, a brief overview of the main aspects controlling the climate in the region, a map with all sites discussed in the text, especially from the sites discussed in the section "Drought conditions at Walker Lake". Also, it would be helpful to have all sampling sites presented on a map. The climate scenario for each salinity state should be clearly emphasized. Maybe there could be a statement at the end of each salinity state as it is been done for the "Fresh waters" section?

Added brief paragraph on Walker Lake (Hawthorne) climate.

Added a figure (new Fig. 1) with map of Lake Lahontan high stands and localities mentioned in text. For those localities outside of the map borders, I described locations in text.

Climate scenarios for each salinity state are included.

3) Are the methods appropriate, current, and described in sufficient detail? This contribution does not contain a specific method section. Is there a separate chapter that describes the methods in more detail?

This chapter does not have a methods section. Methods sections are included for each project in subsequent chapters.

4. If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct? In the 2nd paragraph (page 1) the author states that "recommendations for…. Monitoring plan statistically tracking the….. are provided" I have difficulties identifying a monitoring plan that statistically tracks the environmental condition. Maybe this could be made clearer and emphasized?

The sentence referring to the monitoring plan is deleted.

5. Are all tables and figures necessary, clearly labeled, and readily interpretable? Table 1.2 does not present any ostracode species for "this study" although it is indicated in the text (page 8, 3rd paragraph) that *L. ceriotuberosa* is the only abundant ostracode living in the lake today.

Ostracode analysis was not part of the current study. Reference (Bradbury et al. 1989) added in text for clarification that it was that study that noted the ostracode species was living in the lake today.

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper? Yes.

7. Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted? It is stated that the focus of this paper is to present the composition of ecosystems during times of different salinities states on page 4 (2nd paragraph) and also on page 6 (3rd paragraph), maybe this could be reduced to one statement?

Although the timing of past phases of low and high salinities is apparently not of specific interest and thus not provided in detail, I think the paper would profit from giving the reader a general idea when phases of extremely high and low salinities occurred, even if there is dating insecurity. For example, can a more specific timing be given in the section "High salinity alkaline waters" (page 7, 3rd paragraph) for "at least twice" and "at time"? Also, page 8 (2nd paragraph) "the same core depth": Is here a more specific information about the age of the core depth possible? And, page 8 (bottom line), which "two time periods"? Giving approximate ages would also provide more consistency for the entire paper, because ages are given in the very detailed discussion in the section "Drought conditions at Walker Lake".

In the section "Historic change in taxa" I miss information on, for example, ostracodes and chironomids. Please make sure that all taxa are discussed.

Paragraphs combined.

Time periods included where appropriate. Clarified text describing that radiocarbon dating of cores is problematic and dating errors exist. As stated above, ostracodes are not part of current study. Chironomids are discussed in subsequent chapter, I believe.

8) Can the paper be published? The paper should be published with minor to moderate revisions.

<u>Response to Reviewer 2</u>

1. Unfortunately I did not find this chapter very informative on the relation of lake volume and/or river discharge to the ecology of either system. The simple fact is that if you want to restore Walker Lake to a more pristine (early historical condition) you have to 1: increase the discharge of the Walker River at the lake mouth, and (2) decrease the flux of irrigation return (which adds nutrients such as N and P to the lake). Again the simple fact is that the lake has transited from a system that overturns once a year in January, mixing to the bottom to one that can now chaotically mix during the summer as the thermocline reaches the bottom. The latter state causes the lake to be eutrophic because nutrients can be mixed throughout the lake more than once a year. In addition, the ratio of the volume of the epilimnion (where living organic matter is created) to the hypolimnion (where dead organic matter collects) has greatly increased with time. This means that more organic matter is being concentrated over the ever decreasing lake bottom area. Such an increase in the density of organic matter causes the system to become anaerobic and anaerobic bacteria to dominate. This type of bacteria is only about 10% as efficient as aerobic bacteria in breaking down organic matter and returning it to the lake as DIC. Thus the systems spirals to a higher eutrophic state and the bottom waters of the lake become depleted in oxygen. With respect to fish, they need cool water and oxygen. If the cool deep waters become depleted in oxygen the fish cannot find a place to live and go belly up, etc. etc.

> *I agree with statement. No changes to text necessary. The reviewer clearly thought that this chapter was a stand-alone chapter when he reviewed it.*

2. Generally I see this report as a cataloging without criticism of past work on the lake, most of which has little bearing on the evolution of the lake's ecology. The section on the chemical evolution of Walker Lake is not up to snuff. First if you are going to simulate the chemical evolution of the lake, you need to state what chemical model you are employing; i.e., Phreeque, EQ3, etc. Secondly, all these models are equilibrium models; i.e., calcite will precipitate when its IAP (saturation state) is reached. Great Basin lakes are not, I repeat not, equilibrium systems. The lake may exceed its saturation state with respect to a mineral by several orders of magnitude. The demonstration of this problem is that monohydrocalcite (which is not stable under any earth conditions) has precipitated from Walker Lake during the historical period when equilibrium concepts suggest that calcite or aragonite should have precipitated. In any case, forget the classification scheme shown on Fig. 1.1. Again the point to be made is that both carbonate and calcium (NOT CALCITE!!!) are discharged to the lake. At some point some form of calcium carbonate begins to precipitate (and that point will be quickly reached after the lake begins to form) and because the molar concentration of Ca in the river is much less than the molar concentration of carbonate/bicarbonate, a Rayleigh

fractionation process will deplete the Ca to very small levels. There will remain some Ca in solution because it is complexed to other chemical species. An equilibrium or non-equilibrium model would have told you that non-complexed Ca should have been at very low levels for the last several thousand years.

Chapter is general overview. Criticism and analysis of published literature could be included on the timing of high or low lake stands or climate or diversion controls on lake levels, but as stated in text, this is not the direction or intent of this chapter.

Chemical evolution discussion is meant to be a general discussion of snapshots in time of the lake geochemistry, not a detailed geochemical model of lake solutes. I included a sentence stating that this is a general overview and this simplified scenario does not account for non-equilibrium processes, species of calcium precipitated, or calcium complexed to other chemical species.

Reviewed text for misuse of word "calcite" and replaced with carbonate and calcium where appropriate. Agree with last part of paragraph 2 and my text is consistent with reviewer's statement.

Added reference in text to Leach and Benson study regarding pollution.

We don't know what the chemistry of the Lahontan high stand was, but there is a pretty good indication based on ostracode and mollusk (my research) modern and paleo studies that it was fresh.

3. Projects on the Walker River may have been few but the program run by Leach and Benson measured the chemistry of the river at numerous sites during both the high flow and low flow periods and demonstrated the problem with irrigation return as a pollutant.

Added statement in text which includes evaporation and humidity at lake in addition to temperature as part of climate influence.

4. We do not know what the chemistry of the highstand Lake Lahontan was like. It could have very well been quite saline. The volume of the lake is not a balance between Sierran snowpack and temperature over the lake. River discharge is a function of the snow pack, but evaporation at the lake surface is not a simple function of temperature; in fact, evaporation is mostly a function of humidity of the air over the lake which is dependent on T. Because cloudiness in the historical period is small during the warm season, rates of lake evaporation tend to be relatively constant (+/- 15%) in an annual sense.   For this reason, I would suggest that the evaporation rate during the Holocene has been relatively invariant. Again dilution is the solution to pollution (read lake salinity). Salinity is almost a perfectly inverse function of lake volume. The amount of TDS added to the lake annually is tiny. The only other major contributor is the flux of salts from a brine some 10s of meters below the center bottom of the lake.

Agree with author. This is discussed in the Thomas report.

5. The discussion of the taxa in the lake is horribly incomplete. There is just no data for most of the time frame discussed except for data on cods and diatoms and that data is

completely flawed from the long core taken from the lake. Only the Livingstone cores have produced good data and the interpretations from the types of diatoms and ostracodes are not based on ecological knowledge of the critters; they are based on analogies where the critters have been previously noted. For example is critter A was previously found in a saline pond, the former workers would posit finding it in Walker implies Walker was saline. Maybe, maybe not!

Taxa discussion is incomplete because it is based on what is known about prehistoric and historic taxa. Current taxa are discussed in subsequent chapters. Flawed core record is discussed in text, which is why dates greater than ~5,000 years ago are not specified. Ecological knowledge of critters is based on modern data from greater than 600 localities throughout the U.S., not analog studies.

6. With regard to river diversion, while one has to acknowledge the possibility, I am yet to be convinced of the fact and the timing of such.

The timing of river diversion is referenced to specific published studies. These studies report different time periods for diversion to Adrian Valley. I make the point that the exact timing of diversion (or cause of low lake levels) is not as important to this chapter as is the effect of low lake levels on taxa.

7. The author should point out how much water has been consumed by irrigation practices and she should also calculate how long it would take given various increases in river discharge to bring the lake back up to whatever level (read salinity) is desired. One cannot simply say that lake productivity has increased when confronted with increasing stored organic matter over time in the lake's sediment. It is reasonable to suspect this to be true but the amount of residual OM is a function of production-respiration.

A calculation of irrigation water consumed or increased river flow for a specified lake level are not pertinent to this chapter. Sentence on lake productivity is deleted.

8. One should not swallow stump data in lakes whole. If the stumps in Tenaya represent a change in climate, then that change is about 90% drier than today. If the stumps in the West Walker represent as Stine suggests a complete cessation of flow, then the snowpack in the Sierra was essentially zero for many decades. Something is wrong with this picture!! For example, Pyramid would have fallen several tens of meters and I doubt the cui ui would still be with us.

There is a problem with the tree stump data. The point in the paper is that there were substantial droughts in the past and future droughts will affect Walker Lake taxa.

9. The author should discuss Sierran tree-ring data and also the Mono Lake climate modeling study carried out by Nick Graham and Malcolm Hughes which pointed out that runoff during the middle-12th and late-13th centuries was decreased by about 30%. Also Benson has shown that discharge to Pyramid Lake (which implies Walker Lake also was decreased by ~30% between 8 and 3 cal ka.

Added reference to Graham et al.

PROJECT A: INSTREAM AND LAKE AQUATIC HEALTH

Lake Aquatic Health

<u>Response to Reviewer 1</u>

General Comments:

In general, the report read very well and was structured appropriately. The main objectives were succinctly developed in the Executive Summary and the subsequent individual segments were organized, complete, and relatively capable of being viewed as stand alone documents. Some of the information was very repetitive, but this was unavoidable given the need to provide appropriate context for each segment of the report.

> *This was a consequence of our desire to provide a limnological overview at the beginning, followed by subsequent chapters with expanded detail and further discussion.*

The report provides useful information regarding the current limnological conditions of Walker Lake and the potential threats of a continued water level decline. The developed database appears to be appropriate and sufficiently documented for the stated goals. The ecological model development appears appropriate and model results are somewhat similar to the provided observations in the lake. However, I disagree with the assertion that the model was validated given the information provided and I am concerned that future applications of the model may leave managers with inappropriate conclusions, primarily regarding implications for the biological community (see Arhonditsis and Brett 2004).

> *We agree overall with the reviewer's sentiments that the current status of the model should be clarified to indicate its current level of development. We have added caveats throughout the chapter to indicate the current model status and the need for further refinement before the model is suitable to inform management decisions. The support for model development for this project was very limited in terms of budget and also in terms of time, due to reliance on field data collected through other tasks.*
>
> *The reviewer's comments were very helpful for improving the current report and also for supporting arguments for future refinement of the model.*

The algal & zooplankton community focus of the Food Web section was well placed for compatibility with the stated primary goals of the report. The information provided on the fish community in Walker Lake was very limited and provides little utility for future understanding of how water quality changes will influence fish population dynamics.

Although the developed model was not fully corroborated and the fish community was given short shift, the report adequately addresses the stated primary goals. Ecological model development and corroboration is a difficult and time consuming process, rarely complete to the satisfaction of any reviewer. After minor revision, this manuscript will be

suitable for publication as a report. I have inserted a few general concerns into the review format provided and some specific comments will follow those concerns.

1. Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation? Yes, the paper is cohesive and very easy to read. I see no compelling reasons to adjust the structure or format of the document.

Agreed. We feel a major reedit or reorganization of the material is not necessary at this time.

For the most part, the conclusions appear to support the data. However, as stated previously, the provided data was insufficient to support the conclusion regarding corroboration of the model. The authors appropriately held data in reserve for model corroboration, but the actual corroboration analysis was deficient (e.g. see Haefner 1996). The authors state that "The temporal patterns were **very similar** to those reported by Horne."

The wording of this section was adjusted to more accurately reflect the model performance. An additional paragraph was also added to this (limnology) section of the report describing limitations in the current version of the model and the need for model refinement before using the model to inform management.

But similarity is not sufficient and temporal similarity is a very broad scale from which to make a comparison. One graph illustrating the simulated vs. observed results and a simple statistical correlation analysis would provide the minimum support necessary for referring to the model as "corroborated" and it would probably still be more appropriate to refer to the model as consistent with observations rather then corroborated.

We agree, and our wording in the text has been adjusted to reflect this distinction.

2. Is the paper clear, well organized and concise? Yes.

3. Are the methods appropriate, current, and described in sufficient detail? For the most part the methods are appropriate, current and adequately described. I feel the model goes beyond its capabilities and its utility is overstated. However, large scale ecological models are traditionally deficient (Arhonditsis and Brett 2004) and with a discussion of the appropriate caveats this model should be acceptable for publication as a report.

The utility of the model was restated and caveats were inserted throughout the chapter.

Many of the figure heads for the graphs lacked appropriate detail and the method used to interpolate between points was not provided (with exception of the food web section). However, these minor issues are easily addressed.

We have added more detail to the figure heads for many graphs and, where relevant, have indicated the interpolation methods used.

4. If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct? In addition to my previous concerns

regarding model corroboration I also did not feel the model adequately reflected the vertical extent of the dissolved oxygen profiles as suggested (page 84, figure 5). I would be troubled if this model was used to assess the amount of suitable habitat available for zooplankton or fishes during the summer. It appears the model under estimates the amount of available DO in the water column around the thermocline (pg 84 fig 5., figs 15-17 compared to pg 26 fig 9; similarly suggest lower than observed DO values between 5 &15 meters during the summer). The thermocline is an important area of the lake for biotic organisms, primarily mobile ones. Given the data available, I don't think the model can, nor needs to be recalibrated to address this apparent DO bias at the thermocline as long as the documentation addresses the issue. Similarly, I would have liked to see more cautious language used regarding the model and its utility.

Same as above, we have emphasized the model's limitations, especially related to DO dynamics which are in great need of further refinement (with subsequent funding).

5. Are all tables and figures necessary, clearly labeled, and readily interpretable? No. Most figures have inadequate figure headings for interpretation of the graphs. Each figure head should provide sufficient information for each graph to be interpreted in the absence of the text. Some of the figures have axis errors. These will be identified below in the specific comments.

Again, we have added detail to the figure heads for most graphs and, where relevant, have indicated the interpolation methods used. Axis errors have been corrected.

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper? Overall the references appear sufficient. I would like to see more development of the model corroboration section and this would probably require the inclusion of appropriate references regarding some of the assumptions taken.

We appreciate the references provided by the reviewer and we have requested reprints. We will expand this section in future drafts as time allows.

7. Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted? Yes. All of the parts are appropriate in length and content.

We agree, despite some repetition in text and content between chapters.

8. Can the paper be published? With minor revisions.

Most comments and minor revisions have been incorporated as indicated herein.

Some specific comments: (suggested changes are underlined)

Pg. ii, Line 8, Because so many organizations …

Fixed

Pg. ii, Line 10. …users might ask.

Fixed

Pg. 8 Lines 6-8. Hanging sentence that could be joined with the previous paragraph.

Done

Pg. 9. Many single sentence paragraphs. Consider grouping a few together.

Done

Pg. 21. Figure 4. Nice graph, but could include the method used to interpolate between data points? Linear interpolation?

Done

Pg 22. Line 2. Move Figure 6 to the end of the sentence in parenthesis.

Unclear what the reviewer meant by this comment. There didn't seem to be a sentence in parenthesis from this section.

Pg 22. Figure 5. The secchi depths would be easier to use if the water depths were inverted similar to previous graphs.

This figure was revised to reflect the useful comments of these reviewers.

Pg. 26. Figure 9. The scale in the legend could use a few more reference numbers. Again, what method of interpolation was used?

Interpolation method has been provided in the chart caption. Legend scale is equivalent to Figures 4 and 11, which seemed to be OK, so we didn't make any changes to Figure 9.

Pg. 43. Fig 20, 21. Is the y-axis correct? Very useful graphs with nice contours, legends, and scale.

The y-axes have been corrected in Figures 20, 21 and 22.

Pg. 57. Line 8. …page XX for…

This has been corrected.

Pg. 57. Line 15 …were the most current available.

This has been corrected.

Pg. 77. Figure 1. The text is unreadable in many places.

Fixed

Pg 108. Line 9. mesh size of net?

we have modified the sentence as follows;

Next, vertically integrated zooplankton tows from near the bottom to the surface were collected using a 50-cm-diameter zooplankton net with 80-micron mesh

Pg 112. Line 4-5. inappropriate carriage return.

Fixed

Pg. 123. Line 7. These <u>the</u> profiles showed…

Fixed

Pg. 123. Line 9. The DO values <u>in</u> the evening…

Fixed

Pg 124. Line 3. …and <u>respiration</u> rates…

Fixed

Pg. 125. Line 17. …uses in <u>the</u> this category of…

Fixed

Pg. 129. Line 8. …strategies could affected during…

Fixed

Pg 134. Line 4. …of the lake <u>from way</u> from…

Fixed

Pg. 137. last line. …the <u>fifth</u> power, divided…

Fixed

Pg. 142. Figure 4. Title needs adjusting.

Fixed

Pg 146. Line 5. The lowest and highest values are identical.

Fixed

Pg 146. Line 8. little variation but an increase <u>in</u> winter (November).

Fixed

Pg. 147. Figure 8. No replication? Error bars?

No replication was placed on this graph since the data was generated from 1 primary location. It is common to determine zooplankton densities by counting subsamples from the same field sample. Placing error bars around these means would be considered pseudoreplication.

Pg. 149. Figure 11. Label axis.

Fixed

Pg 150. Line 12. Please provide the sample size. Note that Fulton's K condition factor is most appropriate for comparisons within a system not among systems. Wr relative weight is a more appropriate condition index for among system comparisons (Kruse and Hubert 1997), or use the slope and intercept method (Cone 1989).

This is correct however since very little if any information is available for cutthroat trout from other ecosystems with the same parameters for comparison we used the Fulton's K for comparison. We correct our manuscript by noting these comparisons are not always accurate and interpretation should be conducted with caution.

Pg. 152. Line 3. …are highly patchy in ….

Fixed

Pg. 152. Line 11. …tui chub morphotype is slightly more

Fixed

References:

Arhonditsis G.B. and M.T. Brett. 2004. Evaluation of the current state of mechanistic aquatic biogeochemical modeling. Marine Ecology Progress Series. 271:13-26.

Cone, R. S. 1989. The Need to Reconsider the Use of Condition Indexes in Fishery Science. T. Am. Fish. Soc. **118**(5): 510-514.

Haefner, J.W. 1996. Modeling Biological Systems; Principles and applications. Chapman and Hall. New York, NY. USA.

Kruse C.G., and W.A. Hubert. 1997. Proposed standard weight (Ws) equation for interior cutthroat trout. North American Journal of Fisheries Management. 17: 784-790.

Response to Reviewer 2

Summary

The DRI/UNR team did a very nice job in expanding and focusing the limnological study of Walker Lake. They added a number of activities that were lacking from previous work and overall took a very professional approach. As the salinity of Walker Lake continues to increase large ecological changes are only matter of time. If there are to be serious consideration to new management actions, this limnological information is essential.

In addition to the standard limnological parameters that would be part of most limnological surveys, the DRI/UNR team also included interesting sections related to bacterial function groups, food web dynamics, data management, dissolved oxygen budgets and simulation modeling.

Before stating my general and specific comments there are two points to be made. First, the authors recommend that additional monitoring field work be done in support of this project. I agree and want to point out that even though a model was calibrated for this study, its use seemed to be most appropriate to develop preliminary ideas for management and generate hypotheses. I would have to speak with the authors, but it appears as though the model needs more work before it is used to guide the details of an extensive restoration effect. As noted elsewhere in this review, the authors should be commended for carrying out the initial develop of this model, it will have significant utility down the road.

> *We completely agree with the reviewer regarding the utility of the current version of the model. We have adjusted the wording throughout the report and added several caveats to make it clear that the model is still in the early stages of development and that much refinement is necessary before it can be used to guide management decisions. We would like to point out that the modeling task was a small component of the overall project scope and also that it was developed under severe time constraints due to reliance on data from other tasks.*
>
> *We have attempted to address the reviewer's questions and comments below. However, many of these helpful thoughts cannot be properly addressed without a much more detailed investigation and further model refinement. We greatly appreciate the thoughtful review and we hope to address all of these issues through future work.*

Second, the presentation of some very good information is somewhat awkward in the sense that the text is written as a series of individual manuscripts with not enough integration. There is considerable over-lap in information presented and it would be very useful to tie it together in a more comprehensive manner.

Reviewer 1 noted that this was unavoidable given the need to provide appropriate context for each segment of the report. Although we agree that it would be useful to eliminate some of these redundancies, at this time it would require an effort beyond the funding currently available for this portion of the project.

Note: I found it very difficult to review this large a document using the questions for peer review supplied by the Academy. I hope this does not cause any trouble and that what I provide below will be sufficient.

General Comments on all Chapters

1. There was significant overlap between the various sections, with some of the same data presented as many as three times. While the report was not difficult to read and follow, and the writing for each section was clear, the document as a whole lacked a progressive flow through the material. It would benefit from a more complete effort to integrate all the material.

The same as above. Repetition was a consequence of our desire to provide a limnological overview at the beginning, followed by subsequent chapters that would provide more detail and further discussion. We agree it would be useful to fully integrate all of the material for a more progressive flow, but that effort is impractical at this time.

2. I did not see a discussion related to the possible impacts of TDS-related toxicity to sensitive life stages of resident fish. Have bioassays been performed such as those done in Pyramid Lake?

In the discuss we added sentence that points out the physiological impacts of salinity levels.

3. There have been no recent comprehensive limnological surveys of Walker Lake – therefore this activity was needed for management decisions. I also appreciated the attempt to begin modeling to (1) help inform information and data gaps and (2) develop working (initial) hypotheses about how the lake will respond to changes in water supply.

We appreciate this acknowledgement of the utility of the current model.

4. In light of comment #3 above, can the authors state the appropriateness of their model for policy decisions. Does the model need more development or can it be used now to make decisions regarding the development of additional sources of water for Walker Lake.

We have added text to address this issue at several places throughout the report. The reviewer is correct to point out the limitations of the current model in guiding decisions.

5. Specific citations should be made to direct reader to location of other data bases used.

In Table 1 and in the database methods section we reference a new Appendix in the Walker Lake Database User Manual that provides sources and contact info for the data entered into this database.

6. Document should provide a review of historic data vis-à-vis QA/QC, methods used and ultimately the reliability of past data.

This refers to the collection of detailed QA/QC information from historic agency sampling programs that were not part of this project. That would be an effort well beyond the limited scope and funding provided for the database development task.

7. What QA/QC guidelines were followed for the DRI/UNR sampling. This is not provided in the first chapter on the contemporary limnology. If it appears elsewhere, please make a notation.

Standard QA/QC procedures for sampling and analysis by DRI and UNR have been indicated and cited within the text. Additional information is available in subsequent chapters where sampling methods and results are discussed in greater detail.

8. Document lacked literature citations in many sections and there was little reference to other saline terminal lake. Given the large literature available from nearby Pyramid Lake and Mono Lake, there are sources of out side information that need to be incorporated.

This project was developed to provide an assessment of the current Walker Lake limnological conditions for modeling purposes and to provide a reference against which changes resulting from future water rights acquisitions and delivery could be evaluated. It was not intended to be a comparative study of saline lakes in the region. However, when this material is developed for publication in a peer-reviewed journal, we anticipate adding references to work on other saline terminal lakes.

9. Team did a nice job to develop and manage the database.

We appreciate the recognition that considerable effort went into developing the Walker Lake database and populating it with as much information as possible from agency monitoring programs as well as from the shorter term studies implemented as part of this project.

10. In the opening chapter on contemporary limnology, there is very little discussion in the Results/Discussion section. This is in part due to the fact that latter 'chapters' cover topics with more specificity; however, this is part of comment #1 (above) – a more complete job to integrate the 'chapters' will improve the presentation of material.

The same as above. While we agree it would be useful to fully integrate all of the material for a more progressive flow, that effort would exceed the funding available at this time. The intent of this chapter was to provide a limnological overview at the beginning, followed by subsequent chapters with further detail and more discussion.

11. The modeling results should be removed from the contemporary limnology chapter. There is a full chapter dedicated to the modeling study. Since not enough information can be given in the contemporary limnology chapter it unnecessarily detracts from the modeling effort. i.e. the reader is left with too many questions after reading the first chapter and must wait for the modeling chapter. At the least, please refer to the fact that more information is given in the later chapter on modeling.

This comment is really best addressed by the UNR/DRI management. The modeling description was initially intended to be part of the contemporary limnology section because it was only a small subset of the lake ecology component and it was intended to support the development of the monitoring plan. The modeling chapter was originally intended to be an

appendix to provide details for curious readers about the model's development, input data, etc. It was later "upgraded" to a chapter but can just as easily be "downgraded" or dropped.

We have added text at the beginning of the modeling results sections to indicate that more detailed information is provided in a subsequent chapter on the model.

12. The chapter on Contemporary Limnology ends with this sentence "Ultimately, the Walker Lake ecological model will help to optimize future water deliveries in terms of lake benefits, which is critical for developing sound management strategies." I was not clear on whether the authors believe that the model is currently ready to contribute to management decisions/policy or whether they consider it a good beginning but that more needs to be done.

We consider the model to be a "good beginning" and the wording has been adjusted.

13. More discussion comparing the DRI/UNR sampling results to previous data would be useful. I understand that there is not always a good historical database, but when possible it would be useful.

14. Recommendations for the enhanced/continued monitoring program should be expressed in terms of specific needs, based on model results, management questions and general limnological standards of protocol.

Many of the monitoring recommendations provided were based on model results and sensitivity analysis, as well as from management questions related to limnological conditions and water quality trends. No change made.

15. I see this entire report as the starting point for further considerations of management approaches.

Agreed and noted in the conclusions.

16. This report highlights the need for a science and monitoring plan to support targeted research.

Agreed and noted in the conclusions.

Specific Comments

Contemporary Limnology of Walker Lake, Nevada

1. There should be a table early on in the document that provides information of lake characteristics such as, maximum and mean depth, volume, bottom area, etc.

These characteristics keep changing as the lake level drops. The USGS has conducted a bathymetric survey, but the data have not been entered into a GIS database, so constructing these relationships would be difficult at this time. There is a chart from the USGS shown in the modeling chapter (Stone et al.) which illustrates the relationship between different Walker Lake volumes and surface area. (We had thought to do a similar one which shows the relationship between depth and volume, but did not have time after incorporating all other suggested changes to the document.)

2. Table 1 should be expanded (perhaps as an Appendix) to give more information on the specifics of the monitoring program and data availability.

Table 1 is intended to provide a short summary of the monitoring programs and data available in the Walker Lake database. The caption has been revised to reflect this, and we reference appendices (A and F) in the database User Manual that contains additional information as suggested by the reviewer.

3. Page 9 – Were the additional profiles at WL3 taken primarily for salinity profiles?

Text was revised to clarify that DO and temperature profiles were also collected at this site.

4. Page 12 – It would be helpful to state what was the minimum size limit for plankton cell size that was observable.

Done.

5. Page 13 – Paragraph on microbial approach could be expanded and more literature citations would be helpful.

Expanded this paragraph slightly and added some references. The two paragraphs following this one now also serve to better define the microbial approach.

6. Page 13 – Last paragraph – the cultivation-based approaches tell you what is there and more specifically what will grow in the test. They do not account for actual environmental conditions nor do they speak to the importance of these functional groups to the lake's microbial ecology. I believe the authors used these tests in an appropriate manner, i.e. they gave a first understanding of what might be there. This information can be used in the future to better understand their role(s).

This isn't really a request for changes, but rather a comment supporting our use of the cultivation techniques.

7. First chapter on contemporary limnology should at least give a brief over view of the food web work by Chandra at al.

We reference the food web chapter in our contemporary limnology discussion, but the UNR/DRI project management team had decided early on that these would be treated as separate chapters without any attempt at integration or overview. No change made.

8. Explain why 1-D approach to WQ modeling was justified in Walker Lake. I don't disagree, but it should be justified more in the document.

A 1-D approach was selected for this study because a more advanced 2-D or 3-D approach was not feasible with the resources allocated for the modeling effort. However, we feel the 1-D model was appropriate for investigating general trends in lake limnology and that with further refinement it can provide helpful guidance to management decisions. The 1-D model will also allow for long-term (i.e. 30 year) simulations after specific flow scenarios are developed. Long-term simulations are not feasible with more advanced hydrodynamic models.

Finally, we chose CAEYDM for this project because it can easily be coupled with a 3D hydrodynamic model should additional resources become available.

9. Page 20 – Second paragraph in Results/Discussion is more appropriate in Methods section.

Done

10. Page 20 – Figure 4 does not contain DO data as indicated in text.

Text has been revised to correctly indicate temperature in Figure 4 and DO in Figure 9.

11. Page 21-21 – Figure 5 does not present annual Secchi depths; the x-axis on Figure 5 (time) should distinguish the relative times, i.e. each full 12 month period should be represented by the same distance on the axis; in Figure 5 why were only two dates selected for representation?

This Figure was revised to reflect the useful comments of these reviewers

12. Page 23 – For the general reader it would be useful to explain why the samples have a similar ionic character regardless of when it is sampled.

The lake has very high TDS, is generally well mixed, and seasonal thermal stratification does not significantly change the major-ion chemistry. That is, the proportions of the major ions do not change with time or temperature. Note, however, this is not the case for dissolved oxygen, nutrients concentrations, and algal populations, all of which are affected by limnological processes such as thermal stratification. Change made to text.

Also, is the increased in TDS in December part of a seasonal affect or does it relate to long-term increases.

Walker Lake and Pyramid Lake are terminal lakes. As such, river water flowing into the lake carries dissolved constituents to the lake, and evaporation removes water from the lake but leaves the dissolved constituents behind. Thus, all terminal lakes have increasing TDS (and salinity and specific conductance) over time. Change made to text.

What are toxic levels of TDS for the fish in Walker Lake?

There is not a specific value of TDS that is "toxic" to fish in Walker Lake. Rather, TDS is one of many factors that affect fish health in the lake. For example, if the Tui chub, an important food source for Lahontan Cuthroat Trout in Walker Lake, cannot reproduce in the lake because of say insufficient dissolved oxygen, the number of LTC in Walker Lake may decline. It is important to note though, that higher levels of TDS correlate with decreases in LTC numbers, for example "It is obvious that as TDS has risen, both non-acclimated and acclimated LCT survival has diminished." (http://www.leg.state.nv.us/73rd/Interim/Studies/Treasures/exhibits/19140H-1.pdf).

No change made.

As noted above, Pyramid Lake and others have seen a similar increase in salinity as seen in Figure 8 – references to this being a more wide-spread phenomenon would be useful.

See response to comment 2b (above) about TDS in December

13. Page 25 – Would groundwater inflow only be expected during the period April-July?

Ground water would flow into the lake year round; however, during the April through early July time frame, increased ground water flow would occur because of annual spring time snow melt and runoff. Thus, it should be easier to identify increased ground-water input to the lake during this time period. Change made to text.

14. Page 27 – Explain why there was such a difference in October. Do these monthly values represent a depth-weight mean of the water column?

We added a few sentences of text to better explain Figure 10 and the features/differences observed in this chart.

15. Page 27-28 – There is no discussion of the relationship between DO and nutrients nor of N:P ratios.

Our focus was on developing the Walker Lake database and model. We provided an overview of dissolved oxygen and nutrient dynamics in this context, but did not have time to more fully develop the discussion to infer relationships between dissolved oxygen and nutrient concentrations or nutrient ratios. No change made.

16. Page 30; Figure 12 – Chlorophyll sampling in the spring needs to be intensified to capture the onset of the spring bloom.

Recommendations for long-term monitoring now include more frequent sampling in early spring to capture the onset, distribution and dynamics of phytoplankton blooms.

17. Page 34-38 – The presentation on bacteria is significantly more detailed that the discussion for anything else in this chapter.

This was necessary to convey the findings in this chapter (since no separate treatment follows). Information was condensed as much as possible without detracting from results (note that comment #5, above, had requested additional information).

18. Page 39 – With regard to the concept of metal-drive respiration, how speculative is it at this point (conceptual or demonstrated. What would have to be done to show its importance in Walker lake and what are ecological consequences. Citations from the literature would be particularly helpful here.

Metal-driven respiration has been demonstrated in other systems. To understand whether it is important to Walker Lake would require measurements of metal cycling rates in isolates and in-situ. This is now noted in the text.

19. Page 40 – the comment that "overall, the activity of alkaliphilic iron-reducing bacteria in Walker Lake, a group that was not even known to exist prior to our study, may be of benefit to fish" needs further details and explanation.

This process has been better described in the text. We have also focused on the net effect to organic carbon and oxygen consumption, directly, rather than on subsequent potential benefits to fish. (Also see our response to comment #21, below.)

20. Page 42 – A series of questions about the model (a) how was 'new flow' added to lake in the model (as surface flow that could stratify, full mixed with lake water, etc.),

All new flow was added via the Walker River using the river's current geometry. This has been noted in the text.

(b) since the dissolved oxygen results are critical to this investigation, it would be useful to know more about the mechanistic features of the DO component of the model,

We have added a reference to the CAEDYM science manual, which provides a complete description of the DO component of this model.

(c) how would the area of the bottom (m^2) that is both oxygenated and de-oxygenated change as a result of new water addition (important if benthic food production is important as stated),

The 1-D model cannot explicitly determine the portion of the lake bottom that is oxygenated or deoxygenated. This information could be inferred by comparing the model's forecast for the elevation of anoxic conditions with the lake bed area above and below that elevation. Such an analysis was beyond the scope of the modeling effort but could be addressed in the future. However, we would suggest further model refinement before going through the trouble.

(d) what are ecological ramifications to the rather rapid change in salinity predicted with new water added (15 to 11 mg/L) or with no water added (15 to 21 mg/L), and

The hypothetical high and low flow scenarios tested in the study were at the extreme ends of the observed flow record and such drastic shifts are not expected to occur with new water acquisitions, so we did not include a discussion of ecological effects resulting from these scenarios.

(e) how confident are the authors that the modeled increase in density stratification is feasible and not just a by-product of the 1-D model.

The density stratification only occurred under an extremely high flow condition and the simulated density stratification was quite weak. It is possible that a more sophisticated 3D simulation may yield different results. However, these results suggest that some density stratification could occur under such an extreme condition, and this is reasonable based on similar conditions in other systems. This has been noted in the text for clarification.

21. Page 50 – This page includes a comment on the possible beginning of hypereutrophy while earlier there was discussion of metals-drive oligotrophy. Are these states related?

Nutrient driven eutrophy and metal-driven oligotrophy are different process that produce opposite effects on the same condition (trophic state). Each could work to offset the net effect in one direction or the other. While the potential for metal-driven oligotrophy is raised, we do not have the data to support it. It would be interesting if the proposed high rates of metal

cycling were helping to keep lake eutrophication in check (relative to lake trophic condition without metal cycling), but that is purely speculative at this point.

User Manual: Walker Database Version 1

22. As noted above, an Appendix with a more detailed summary of data availability would be useful. What data were not included in the DRI/UNR effort?

We added Appendix A to the Walker Lake Database User Manual that provides sources and contact info for the data entered into this database. We also added Appendix F to show what data were collected by which organizations on specific sampling dates from 2000 through 2008.

23. Are the methodologies used for the various historic and current data sets available?

Methods used by the Walker Lake sampling program in this project were discussed in the document. The methods used by historic data collection efforts were not available in our data compilations and would likely take considerable effort to collect and verify. This would be a topic to develop if the Walker Lake Database is to receive future support for continued updates, quality assurance, distribution and use.

 Ecological Model for Walker Lake, Nevada

(note a number of specific questions regarding the ecological model were presented above since the model was first presented in the chapter on contemporary limnology)

24. Page 77 – Graph of bottom area versus lake elevation would be useful for discussions related to benthic food webs.

We agree this would be an interesting addition and we will try to create a figure for future drafts. Figure 1 partially addresses this concept but to develop a true relationship between bottom area and lake elevation would require a GIS analysis using the 3D bathymetric data collected by the USGS and that would require additional resources.

25. It would be helpful to have a table for the modeling parameters, variable and initial input data that was used in this modeling effort.

Hundreds of modeling parameters and variables are required for the DYRESM-CAEDYM model and references to the modeling documentation, which include tables for this information, have been added. Default values were used for nearly all model parameters. Descriptions of all data sources, including boundary conditions, initial conditions, and calibration data can be found in the text.

26. Page 81 – Explain why calibration/validation was only done for surface elevation, dissolved oxygen and temperature. I understand these are three very important forms of model output for the questions being asked, but given the argument that biology plays such and important role with regard to dissolved oxygen, biological parameters for photosynthesis and respiration and especially bacterial metabolism would appear to be very important for DO.

We agree that the calibration could be improved through additional parameters, however, DO, temp, and water elevation were the only consistent observations available to us during the calibration period. Additional data describing algal and microbial communities have since become available and could be used to improve the model calibration through future work.

27. Page 83 – The statement is made that CAEDYM requires little to no calibration and that there was reasonable agreement between simulated and measured DO. Please quantify what is meant be reasonable – the middle depths in Figure 5 suggest a 2 ppm difference. This might not be ecologically meaningful, but an explanation would help. Was Figure 5 a typical example? While the CAEDYM authors may claim little need for DO calibration, were they dealing with system so strongly influenced by microbial physiology.

We have expanded the text in this area. As alluded to by the reviewer, the statement regarding little to no calibration is that of the CAEDYM developers. It was not possible to move beyond a qualitative description of model performance to a quantitative calibration metric, such as root-mean-square error or a Nash-Sutcliff coefficient, because the simulation results and observed data are not available at commensurate elevations. Thus, a visual comparison of the general trends was used.

There are a host of reasons that could contribute to discrepancies between modeled and observed data including simplified representations of algal, microbial, and zooplankton communities in addition to the lack of fish component within the model or the lack of a meteorological monitoring station on the lake.

28. Page 84 – Good to show the agreement between the modeled output and the results of Horne et al. (1994).

This is difficult to complete because only a small portion of Horne's raw data is available to us. The model was parameterized based on the figures provided by Horne and the descriptions within the body of the text. Likewise, the evaluation of model performance was made by comparing simulated results with the overall observations reported by Horne (i.e. temperature and DO patterns over time). Such information does not readily lend itself to a direct graphical comparison, unlike the results from temperature and DO profiles collected by the DRI and UNR teams and reported numerically as a function of depth.

29. For the model sensitivity analysis what is the consequence of running this test for one year only. Might the model show increased/decreased sensitivity to varying parameters over time?

Due to time and funding constraints we could only run the full sensitivity test for one year. However, in future work we would like to run extended simulations for those parameters that showed a high degree of sensitivity.

30. Page 85 – In the model analysis, it appeared as though only a few parameters demonstrated any degree of significant sensitivity (e.g. air temperature, solar radiation, wind speed). Does this find have implications for the role that biological processes have in Walker Lake and their influence on DO? Does this imply that DO is physically driven?

We don't feel that this conclusion can be drawn from the sensitivity analysis alone given the relatively crude nature of the model at this point. However, the reviewer's questions are excellent examples of hypotheses that can be generated based on current model results which can be tested through future research or further model refinement.

31. Page 92 – First paragraph. Even though a very wide range of values for bottom sediment nutrient conditions were used in the model no noticeable change in DO profiles were observed. At the same time the model did reveal some near-bed changes in nutrient levels. Discuss how this might affect lake microbial ecology, vis-à-vis in-lake DO profiles and concentrations.

It is difficult to speculate on this aspect because at this point we know next to nothing about nutrients or microbial communities associated with the lake bed sediments. Thus, even though the model didn't show great sensitivity to the sediment nutrient concentrations, it is critical that data be collected on this aspect.

32. Page 102 – Were the recommendations for model improvement based on the sensitivity analysis. What guided the formulation of these recommendations? The reason I ask this is that a number of the recommended monitoring parameters did not show up as being critical in the sensitivity analysis.

The sensitivity analysis was used to guide the recommendations but other issues were also taken into account. For example, the model showed a low level of sensitivity to sediment nutrients. However, no data is currently available for this parameter and thus our understanding of the lake's limnology and the overall model setup would be improved by having real data for sediment nutrients.

33. Does the depth of insertion for freshwater affect mixing and therefore lake profiles? For example will a density difference between the saline water in the lake and new freshwater result in a density stratification? Explain the quantitative connection between expanded microbial/bacterial monitoring and improving model performance.

The depth of insertion should make a difference and this would make for an interesting point for additional sensitivity analyses. It was assumed that all additional freshwater will be delivered from the Walker River and the elevation and streambed angle were set as the existing conditions for the river. The elevation should be adjusted dynamically if the simulated lake level rises substantially.

34. Expand discussion of model results and ramifications to fish.

Due to the limitations of the current incarnation of the model, as mentioned by the reviewer above, along with the hypothetical nature of the flow scenarios, we hesitate to speculate on ramifications of model projections on fish. Ultimately, a refined model could provide information regarding available fish habitat during the summer DO and temperature "squeeze".

35. Discuss pros and cons of modeling out only over a 5-year period.

A brief description of this has been included. A much longer simulation period is desirable (i.e. 20 to 30 years), but resources for this initial modeling effort, along with substantial

uncertainty attributed to the simplicity of the current model and lack of specific flow scenarios, do not justify longer simulations at this point in time.

Walker Lake: Hypolimnetic Oxygen Deficit Assessment and Associated Limnological Factors

36. Page 108 – If lake was 28 m deep, why sample only to 22.5 m.

We have modified the text to indicate the depth at the reference site WL3 and have also learned through reviewing the bathymetry document that the 28 meter maximum depth from this document was actually measured in 2005 (Lopes and Smith 2007).

37. Page 113 – Define values of DO associated with hypoxia.

We have defined the DO values associated with hypoxia in the text (comment 37. Page 113) and added a reference where this is defined;

38. Page 115 – Explain why HOD was calculated in the bottom 8 meters of the lake and not higher. Is this determined by depth of hypolimnion in each year?

This was the top of the hypolimnion. To mitigate confusion, we have expanded the text to include a definition of HOD and how it is calculated;

39. Page 115-116 – I don't know if there is a simple answer, but does the conclusion that HOD represents the very minimum estimate of net productivity affect conclusions regarding what it would take to increase DO as a result of biological changes. The statement implies that you could get as severe a DO depletion even if net productivity were to be significantly reduced. Can this be discussed further?

I am just a little confused on the matter that if lake productivity is sufficient to drive and create extensive anoxia and HOD is a large under-estimate of net productivity, why would there appear to be a relationship between oxygen deficit and lake level. Would it be useful to calculate HOD per unit volume and see how that changes with time. It is not that I necessarily doubt the conclusions, I bring this up because DO is such an important aspect of all this work, a more detailed discussion would be useful to the reader especially if policy decisions will emerge from this work.

The calculation of HOD per unit volume could be useful, but the simple matter is the ratio of the hypolimnion volume (and thus the content to oxidize organic matter) to the lake's productivity is the ratio that drives the total oxidation deficit. However, when primary productivity is much greater than hypolimnion oxygen, the oxygen is consumed and then other oxidants come into play in order to degrade the reduced organics (e.g. sulfate, nitrate, iron, etc.). Thus, we feel that for the most part, the text adequately covers the topic. We agree that DO is an important aspect of this work. We have added text and split the second paragraph in the section to address the concerns raised.

40. The influence of morphometric scaling based on the relative size of the epilimnion and hypolimnion results from varying lake elevation can be very important as the authors indicate. Can the model be used to test some of the hypotheses generated in this chapter?

Data generated by the model could be used to investigate morphometric scaling hypotheses. However, the limitations of the 1D hydrodynamic model must be kept in mind and further validation of the model would be desirable before such testing is conducted.

41. Page 119 – It is stated that "Thus- the relative contribution of production in littoral zones may be on the increase as the lake level continues to decline." The report would benefit from an explanation of the ecological affects of this and how it relates to the fishery in Walker Lake.

We have determined that it is unclear from the literature whether or not this would in turn lead to an increase or decrease in the productivity, so have removed the comment "Thus- the relative contribution of production in littoral zones may be on the increase as the lake level continues to decline." from the document.

42. Page 120-124 – The measurement of primary productivity using the 14C method (as used here) has been successfully applied in Pyramid Lake, Mono Lake and nearby Lake Tahoe. Was this technique used to measure in situ rates of productivity on a regular basis.

We have included further explanation in the text and included two more references included in the text.

43. Page 124 – Sorry if I missed it, but was the model used to evaluate the risk of Walker Lake becoming polymictic?

Determining whether there is a risk of the lake becoming polymictic was not a specific objective of the model. Rather the model was used to evaluate general changes in the lake's limnology under different flow scenarios. Upon further refinement, the model could be used to investigate the probability of various mixing patterns developing under a range of flow scenarios.

44. Page 125 – While I agree that the Nodularia blooms will likely remain, it would benefit the report if the authors could more fully support the statement on page 125 that "All observations, data and analysis indicates that large nuisance blooms and deepwater hypoxia will continue or increase in occurrence and magnitude".

This is a general statement that is meant to convey that as long as the positive internal loading is maintained, the large phosphate will remain an issue and cyanobacterial blooms will be expected to continue. We have added the following reference to the text.

Whitton, B. A. and M. Potts. [eds.]. 2000. The ecology of cyanobacteria: their diversity in time and space. Kluwer Academic Publishers.

45. Page 125 – Discussions regarding nutrient/biomass removal are premature in light of the findings presented in the report. I see this entire report as the starting point for further considerations of management approaches.

Yes, we agree that this is as starting point and we discuss some approaches that could be taken in the future based on our observations. We discuss nutrient/biomass removal and deep water oxygenation as possible management approaches.

The Contemporary Ecology and Food Web Energetics of Walker Lake

46. Page 133 – Please state the concentration where salinity becomes adverse to fish.

In the discuss we added sentence that points out the physiological impacts of salinity levels.

47. Page 133 – Last sentence before Methods – what about bottom production; earlier it is stated that it largely supports the contemporary fishery.

Fixed

48. Page 135 – Please explain more about the 2003 and 2004 phytoplankton data that appears in Figures 5 and 6. Where did this come from and how does it relate to the current DRI/UNR findings. There appears to be a significant difference in the timing of Nodularia between the two time periods. Is this correct? Were the 17-station synoptic samplings done in 2003-04?

These statements are correct and have been corrected in the text. For example we have placed in the statement, "This data was obtained prior to the initiation of this research project via one of the authors in this chapter."

49. Page 136 – Define edible phytoplankton.

Fixed

50. Page 138 – If this report is to be read by non-technical readers, a brief overview of the stable isotopic methodology would be helpful. If not most technical readers will understand.

51. Page 147 – Figure 8 needs dates in the caption.

Fixed

52. Page 150 – What levels of freshwater need to be added for the other fish species to exist in Walker Lake.

This point was not addressed due to the lack relevant scientific or published literature

53. Page 151 – the statement is made that, "A continuous monitoring program by the state of Nevada's Department of Wildlife suggests limited recruitment of young of the year tui chub due to increasing saline condition and low freshwater flows entering the lake (Solberger personal communication)." If this document is to be used for policy decisions, this statement should be supported and presented in more detail.

This point was not addressed due to the lack relevant scientific or published literature

54. Reference to Chandra et al. (2008) is missing from citations.

Added

Instream Aquatic Health

<u>Response to USFWS comments</u>

Editorial changes and clarifications were made throughout the document as suggested by reviewer. The more extensive comments and the subsequent responses are list below.

1) page 74: Are epidendric sites still representative of the sandy reaches? I understand the issue with sampling sand sections, but downstream of the confluence is dominantly a sand system, so how do samples represent the sandy reaches?

We agree that the epidendric assemblages do not represent the periphyton communities as a whole in the lower reaches due to the dominance of sandy habitat. The epidendric samples do represent the richest targeted habitat that was available at these lower sites. The initial evaluation of epissammic periphyton assemblages showed that the sand-associated assemblages were largely dominated by a few taxa (Amphora, Achnanthes) that are specialized in attaching to sand grains. The dominance of these taxa was likely attributed to the physical environment (i.e. constantly shifting sand) and not entirely to water chemistry. The comparison of the RTHs is the approach most commonly used to compare habitats across differing environmental regimes and seemed to be the most appropriate for the aims of this longitudinal assessment.

2) page 76: There is some mention of the impact of irrigation on flows etc. but there is no discussion of how agricultural practices may be impacting the sampling results. Are TKN and Phosphorus associated with fertilizers?

Nitrate and ammonium are typically the forms of nitrogen that increase in agriculturally impacted streams. However, the increase in TKN could be due to the organic load from filamentous algae consistently growing to eutrophic levels at select sites (EWB, EWA, WA). Increasing concentrations of phosphorus in Great Basin streams have largely been attributed to watershed geology (e.g. increased volcanic ash deposits), although fertilizer inputs may also increase these measured concentrations in the Walker River. The largest apparent point sources of phosphorus appear to be coming from the reservoirs (Bridgeport and Weber).

3) page 76: The Walker River Paiute Tribe has been fallowing during both sampling seasons in conjunction with work being completed on Weber Dam, both impact flows at the SHRZ sampling site.

We are not sure what the reviewer is requesting. Drawing any direct connections between tribal activities above WA and the periphyton data at the time of collection would seem to be highly speculative based on the current study.

4) page 76: The lack of any sort of recommendations based off the results seem odd. Aren't there target periphyton populations for a "healthy" system?-this information will be valuable for monitoring future water acquistions and restoration activities.

We have indicated that select sites have eutrophic levels of algal accrual based on the measured standing stocks (i.e. biomass). A "target periphyton population" with regard to community composition is beyond the scope of the current work as no predictive model

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(observed/expected) was constructed. However, generally a decrease in eutrophic taxa would indicate improved conditions as well as an increase in sensitive taxa (e.g. Cymbella). Moreover, good practices necessitate maintaining low-levels of biomass relative to flows and temperature. Data suggest some reaches are at high/moderate risk of having low dissolved oxygen at night. Exactly which reaches are presently affected cannot be determined from the present study.

5) page 76: If you are going to keep using the Truckee as a reference for comparing results you should clearly state the differences between the two systems, specifically regarding flow management and anthropogenic activity. Questions could be raised on the validity of comparing these systems.

The Truckee is not being used as a "reference" system in our discussion. It is referred to for comparison as it is the only eastern Sierra river that there are adequate studies of periphyton dynamics. The Truckee basin is largely impacted by urban and municipal land uses (Truckee, CA and Reno/Sparks, NV) while the Walker basin is mostly developed for agricultural uses. The Truckee is tightly regulated to maintain fairly constant flows throughout the year while the Walker resembles a more natural hydrograph. However, the consistent base-flow for the Truckee below Reno/Sparks is largely driven by the discharge of wastewater effluent. The above description of flow and land-use differences were added to the text within the methods section regarding the selection of algal-based metrics.

6) page 76: Graphs used throughout the text (beginning Fig.3) could be potentially confusing. Just make sure it is clear to the reader what the graph is showing.

We have made adjustments to graphs symbolism that should help clarify the locations of the sampling points with regard to the east fork, west fork and main-stem. The changes should help clarify the trends observed and subsequently reported in the text. Also the nomenclature for sites has been changed throughout to be consistent with the other reports.

Response to Reviewer 2

General comments: As this review questionnaire is laid out, it appears that this is supposed to be a manuscript for publication rather than a project report? To me, it reads like a project report. To be submitted as a publication it needs more substance in introduction to pertinent literature and formulation of hypotheses or expectations based on concepts of longitudinal zonation or patch dynamics theory, other current concepts regarding organization of stream communities.

1. Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation? There IS a story here, but it is not quite yet in a cohesive form. Among other things, the data analyses/presentations would benefit from being placed in the context of a reference condition comparison. Now this might not be feasible, but other streams of similar size and physiography in the region might be contrasted (lower Carson, upper, middle and lower Truckee), and examined in terms of the Lahontan Region IBI that I have produced, or the other sites on the West Walker that I have sampled (report available online at Calif State Water Resource Control Board). No reference is made to these other data sets, or to those collected on the lower Truckee by EPA. There is a great deal of data here and excess detail in data presentation that all show a gradient from upstream to downstream. This seems a foregone

conclusion, so what is the significance? How is this related to impairment vs zonation or patch structure? Seems to me that the most important and interesting observation is that sites degraded in summer and fall conditions have the potential for communities of higher integrity during spring. Protecting water quality by maintaining higher flows (lower temps) thus appears to be one of the management implications of this study to loss of biological integrity. In fact it might be possible to use the higher flow conditions as a reference of sorts and measure the relative seasonal departures among sites as a means of scoring the extent of impairment from site to site.

Reference condition was not discussed because the focus of this work was to determine relationships between benthic macroinvertebrate community structure and discharge. The text was modified to clarify this focus, and to address issues regarding other BMI data sets in the Walker Basin. Most of the work conducted by this reviewer has been at high elevations, which is largely irrelevant to work conducted by this study because of cold temperatures, low nutrient content, and a more natural hydrograph than in the lower Walker Basin. Reference to work recently conducted by Tetra Tech working for NDEP to establish an IBI for the Truckee, Carson, and Walker Rivers was added to the report. This work found that Walker River benthic communities show that the river is in 'fair' condition.

2. Is the paper clear, well organized and concise? Again, this is in more of a report format so needs more organization along the lines of a scientific publication.

Yes, this is a report and substantial changes (including shortening the length) are required before it is submitted for publication.

3. Are the methods appropriate, current, and described in sufficient detail?

Another issue is comparability of sampling methods, and the approaches outlined here do not match any standard method in use in the region, so may not be applicable in any case to other regional data. That said, the methods used were repeatable and sampled an impressive diversity of fauna from differing habitat types and with great seasonal resolution over a 2-year time span. As a baseline then, this is a rich data set for contrasts within itself and for a restricted application to this watershed. The very short reach-lengths described for the physical habitat surveys may not be an appropriate geomorphic representation of habitat, but they do serve the purpose of correlation with the biota. One problem with this though may be that since the sample quadrats for inverts were pooled, the habitat data (depth and current) related other than as a composite mean? This will cause loss of resolution with microhabitat associations. Taxa counts of 300 are fine, but sources since Vinson and Hawkins (1996) have concluded that 500 is a better representation of diversity for a fixed-count sample. How samples were normalized (resampling routine?) to 300m is not explained. Community metrics need reference to more primary literature that has evaluated metric sensitivity.

Data accumulated during this project were intended to address the question, 'How will Walker River benthic communities respond to increased flow?' Data that were compiled can be analyzed in a number of ways (including those that are traditionally used for bioassessment), and we elected to conduct analyses that revealed information that indicated specific environmental elements that influence BMI communities, including many that are relevant to effects of changes in discharge. Habitat metrics (depth and current velocity) were a composite mean, which is appropriate for data analysis that was conducted and the purpose

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of this study. Yes, these methods are not appropriate to determine microhabitat use…this is not an intent of this study.

4. If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct? The analyses are appropriate to the data set (CCA etc), but are repetitive and need condensing. The descriptive community metrics could be presenting in a table rather than graphs, and thereby include more metrics than presented. The ordinations are confusing and also excessive. Finding a way to summarize this information will be crucial to developing a publishable manuscript.

Many comments by this reviewer focused on redundancy in data analysis and in figures. These comments would be appropriate if this report was being prepared as manuscript for publication, but we believe they have little relevance for a report, which should provide greater detail that can support management interpretation. Comments to shorten and focus information will be incorporated when the report is shortened for ms submission. The level of detail in the report in context of graphs vs. tables showing results from the multivariate analyses is typical for presentations in the peer reviewed literature.

5. Are all tables and figures necessary, clearly labeled, and readily interpretable? See above comments. The tables contain many mis-spellings of taxa names. Why is the table not taxonomically organized? It is difficult to follow. Isoperla is a stonefly, not a mayfly.

These issues have been resolved.

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper? The report does need more context to relevant literature on physical and spatial patterns of community organization. Since the RCC of Vannote, there has been much published on the topic of longitudinal pattern and templates.

Reviewer comments are correct but this report the discussion was minimally broadened to include discussion of other ideas regarding biotic changes along the river continuum. This will be fully developed when the report is revised for publication.

7) Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted? As above, the data need condensing and the text needs expanding and revision to be placed into a broader context than just this river basin.

See the above response to comment no. 4.

8. Can the paper be published?: But is this a report or a manuscript?? With major revisions

We concur and will revise appropriately when it is submitted.

PROJECT E: DEVELOPMENT OF RECOMMENDATIONS TO MAXIMIZE WATER CONVEYANCE AND MINIMIZE DEGRADATION OF WATER QUALITY IN WALKER LAKE DUE TO EROSION, SEDIMENT TRANSPORT, AND SALT DELIVERY

Historic Erosion and Sediment Delivery to Walker Lake from Lake-level Lowering: Implications for the Lower Walker River and Walker Lake under Increased Flows

<u>Response to Reviewer 1</u>

1. Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation? Aside from a few items in the results section, the story here is very clear, the conclusions are well supported by the data and results.

2. Is the paper clear, well organized and concise? There is logical organization to the paper. Its flows well through introductory material, contains pertinent background. There are some awkward paragraphs in the results when describing geomorphic change over time.

3. Are the methods appropriate, current, and described in sufficient detail? The methods are well applied. There is a nice use of combined historic information, geomorphic field work, GIS, and numerical modeling with current software.

4. If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct? The sediment transport model seems appropriate and applied within its design use. The assumptions (clear-water boundary conditions at the top, fluctuating lake elevation at the bottom) are realistic. Sediment characteristics are well described and applied in the model.

5. Are all tables and figures necessary, clearly labeled, and readily interpretable? Tables and figures are appropriate. Table 4 is a bit much, that could be an appendix, or available upon request. Maybe it would be more useful to condense the table into mean values for a station. Figure 8 doesn't provide much information, and figure 23 is almost too dark to see the feature that is the focal point. Also there should be a flow orientation on all figures.

We have kept Table 4 in manuscript, but maybe the publisher will have us put it in an appendix?

Although figure 8 may not provide that much information, we kept it in for completeness. Maybe the next editing round will suggest that we remove it.

Figure 23 is the best picture of the siphon that I have, so kept it in.

Flow is from North to South in all figures and is clearly stated in text.

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper? There could be some additional references regarding bed armoring processes, or bed re-organization over time for a flood event. These would be useful in presenting the results of sediment transport modeling.

Removed the discussion of bed armoring, so nullifies this comment.

7. Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted? The length is fine, the results could be streamlined.

8. Can the paper be published? The paper can be published with moderate revision.

Response to Reviewer 2

Abstract is good overall. I was left wondering about results such as total volume of erosion, average annual loads, etc. Can these numbers be easily included here?

Added appropriate numbers to abstract.

Introduction is very good. Here are a few notes:

-2nd paragraph under Hydrology heading justifies using Wabuska gage for proxy of flow below Weber. This is probably fine for large flow events, and total annual volumes, but daily data during low flow could be substantially different.

Added appropriate text to reflect this idea.

-3rd paragraph under Hydrology heading refers to "undisturbed" hydrology in the early 1900's. It should be noted that irrigation diversion was well-developed at that point, though not having the same effect as storage facilities, the diversions would have a cumulative downstream effect.

Added appropriate text to reflect this idea.

-The message derived from the 3rd and 4th paragraphs under the Hydrology heading is somewhat unclear. The 3rd talks about natural attenuation of flood peaks and decreasing peak magnitude downstream, and the 4th talks about presumed anthropogenic no flow periods. The message should be clear that the natural attenuation would not necessarily decrease annual volumetric contributions to Walker Lake, while human consumption would.

Added appropriate text to reflect these ideas.

Methods are clear and appropriate, here are a few questions:

-How is on the ground resolution of aerial photography determined?

Added scales of photos used, which allows readers to calculate the scales themselves, if they would like.

-Are there any estimates for how much horizontal error is associated with digitizing polygons on these photos? Does that produce compound errors in mass calculations?

We did not do a formal error calculation, but I suspect that these estimates are within a factor of two, similar to error estimates derived for shoreline erosion quantities from Lake Tahoe in Adams and Minor (2002).

-There are statements regarding issues of how to define the channel. No statement of how this was resolved.

Added appropriate text to rectify this omission.

-More detail could be provided on estimating thickness of eroded area. Was an average thickness used over the area producing a rectangular volume? If so how realistic is this geometry, what might the error be in mass calculation?

Added appropriate text to rectify this omission.

Results are clear and concise in general:

-The erosion chronology starts to get confusing at about the 5th or 6th paragraph. Timing and magnitude are harder to determine.

Added appropriate text to try to make it less confusing.

-The last 4 paragraphs of the erosion history would benefit from more discussion of the total masses transported during the timeframes in discussion. From what I understand 1.02 MT eroded between 1997 and 2005. Later, we find that 936,000 MT eroded during the 2006 runoff period alone. 1997 was a big year, why did 2006 move so much, what are implications for process?

Beefed up the discussion a bit to include quantitative estimates and implications for geomorphic process.

-Some sediment transport results seem counter-intuitive to me. The lower river seems like an area where fine grain and erodible material in the bed and banks presents an unlimited source of sand and silt to the channel. Transport under these conditions would be limited by capacity. However, a statement is made that the results show a supply limitation, does this seem realistic?

Supply-limited reaches do occur along the lower Walker River, as evidenced by ancient lake beds outcropping in the bed of the river. Therefore, tried to clarify the language a bit.

-In the 3rd paragraph under the Sediment Transport Modeling heading, a discussion of bed armoring begins. The bed is said to scour and armor, thereby reducing transport. Classic investigations of bed armoring have been carried out in gravel bed streams where large particles armor the bed, shielding smaller material at depth. Are there any studies that can be referenced for this phenomenon occurring in sand? Beside resistant clay lenses, I would imagine the potential scour depth to be considerable in this system, and the ability of sand grains to "armor" the bed to be minimal. A further doubt arises when the change in grain size is mentioned, with a finer distribution occurring through time. This implies that larger particles are removed from the bed, and smaller material begins to create an armor layer? A few good references for bed armoring, and supply vs capacity limited transport in sand systems would be helpful here.

We removed discussion of bed armoring and offered alternative explanation.

-There is no comparison of total loads estimated through modeling with those estimated through geomorphology. How do they compare? This is important. It is mentioned again in the 5th paragraph of the conclusions without stating any numbers.

We have added text that compares the estimates derived from aerial photograph analysis and from sediment transport modeling.

-The last paragraph in the results section seems to refer to an important concept in fluvial geomorphology called the "effective discharge", this could be stated clearly and referenced.

We agree and have stated the concept clearly and referenced it properly.

I hope this is beneficial to the authors, and that the article receives acceptance and publication. It is well-written, insightful, and timely in a period of increased human water use, and decreased water supply.

Evaluation of the Potential for Erosion and Sediment Transport in the Upper Walker River and Associated Impacts on Water Quality

Response to Reviewer 1

I have reviewed the report "Development of Techniques to Predict Erosion and Sediment Transport in the Upper Walker River and Associated Impacts on Water Quality" and offer you the following comments.

1. Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation? The paper is good reference and background for decision making in moving forward with plans in the Upper Walker River

2. Is the paper clear, well organized and concise? The paper is sufficiently organized but lacks some clarity with an abundance of data and results with less explanation and insightful analysis of the data. The title indicates "Development of Techniques" but the conclusions mention little of any new techniques.

The title has been modified to "Evaluation of the Potential for Erosion and Sediment Transport in the Upper Walker River and Associated Impacts on Water Quality". Completion of the project resulted in: 1) a compilation of detailed river cross section data from field surveying; 2) the development of a HEC-RAS model for the upper Walker River; and 3) a set of water quality data for various reaches of the upper Walker River.

3. Are the methods appropriate, current, and described in sufficient detail? All lab methods are well documented and very well performed.

4. If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct? The use of the HEC-RAS model lacks sufficient background support for its use (e.g., no mention of Manning's roughness coefficients and how they were selected). There is no calibration and verification of the setup, while significant use is made of the output.   Velocities and depths would vary with the roughness coefficient and no rationalization is made. Model predictions are sometimes referred to as data, and they are not.

When model is used to simulate various flow rates it is not made clear if this is steady-state or unsteady; and if unsteady what type of pulse is used to produce the peak flows mentioned. A 1-hr peak flow, 1-day or 1-week or more would produce different results.

The HEC-RAS model was run for steady-state conditions. Some text describing how the model was calibrated by adjusting values for Manning's n and a comparison of water depths measured in the field to water depths predicted by the HEC-RAS model has been added.

5. Are all tables and figures necessary, clearly labeled, and readily interpretable? Some tables include highlighting that is not specified. At least one table has scientific notation of values where decimal numbers of higher multiples would be clearer (e.g., 4.7E5 acre-ft versus 47 thousand acre-ft (taf)).

The significance of highlighted data in tables is indicated at the bottom of those tables. Scientific notation has been incorporated where appropriate.

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper? The following issues with the references are noted:

(Adams, year?) year not given and not included in references
(Parker, 2005) in text but not in references
Text says Niell (1967) but references list Neill (1968)
Bathurst (1985) in text but not in references
Dickman (1990) in text but not in references
Limerinos (1970) in text but not in references
Thein (1993) in text but not in references
(Thomas et al., 2007) in text but not in references
Brownlie, W.R. (1981) is listed twice. Need to distinguish with a and b.
Brunner, G.W. (2008) in references but not in text
Dyhouse *et al.*, 2003 in references but not in text
Hicks F.E. and Peacock T. (2005) in references but not in text
Hoggan, D.H. (1997) in references but not in text
DeVries *et al.*, 2003 in references but not in text
Murat, A.H. (2006) in references but not in text
Myers, T. (1997) in references but not in text
Rantz, S.E., (1982) in references but not in text
Sharpe et al., 2008 in references but not in text
Stacy, M.L. (2001) in references but not in text
Taylor, T. (1996) in references but not in text

The references have been updated accordingly.

7. Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted? I don't consider this a paper as much as I do a report. The report needs to contain more substantiation of the HEC-RAS calibration and verification to support any conclusions on velocities or flooding.

The text has been modified to provide more details related to the calibration of the HEC-RAS model.

8. Can the paper be published? It needs only moderate revisions to be published.

I have made numerous minor changes in text as far as grammar and word usage is concerned and will forward the printed copy with mark ups to your attention.

Response to Reviewer 2

This manuscript illustrated the work conducted on Upper Walker River to address the channel stability problem for concerned reaches. The author collected topographic data, sediment and water quality samples on the east, west branches and the combined reach of Upper Walker River, developed a HEC-RAS model to investigate the river hydraulics and conducted laboratory experiment to study the critical shear stress. Abundant data are generated from the study. The approaches are generally in the right directions though some specific treatments are questionable. The description for all tasks conducted in the project, the methods, results and conclusions are generally clear, but details were missed for some key aspects. Some approaches are apparently not consist in different sections and should be justified in detail.

Specific comments:

1. The description of the survey data is not clear in terms of quality and methodology. The horizontal resolution (points per cross section) and vertical accuracy are not provided. Did the measurement cover the under water topography (bathymetry)? Or the under water topo does not affect the flow simulation? This should be addressed.

> *The field surveying data was collected using state-of-the-art GPS surveying instrumentation. Pertinent details regarding the horizontal and vertical resolution of the coordinates collected while measuring the river cross sections has been included in the text. At each cross section, coordinates were collected at intervals of approximately 2 to 5 feet across the river channel. Coordinates of the channel bottom were collected by placing the bottom of the rover on the channel bottom. Coordinates were also collected at the edge of the water surface on both sides of the channel. This enabled the actual depths of water at each point in the channel cross section to be determined.*

2. The slopes of the channels are not illustrated, which is a key feature of stream topography.

> *Profiles of the river sections have been included in the revised version of the document.*

3. Where were sediment samples collected at each cross section? On the bank or in the bed? Under the water or besides the water? What were the sampling depths? Was there an armoring layer found?

> *Sediment samples were collected from the center of the main channel at each sampling location. The surface of the bed sediment was collected to bed depths of approximately 4 to 6 inches. There was no armoring at the selected sampling locations.*

4. Page 15: "…placing the flow sensor at 1/3 of the depth of flow from the water surface…". This is not the common place to set the sensor. It should be 0.4 depth from the bottom. Why was this setting used?

The proper methodology was used in the field when channel cross section data was collected. The text has been modified accordingly.

5. Page 17: "Adams, year?" should be corrected.

6. Page 21: What is the mechanism of PCX? What is background particle count measurements? What is the PCX reading to determine the incipient condition? More details should be provided.

The HACH PCX is a particle monitoring device that is often used to monitor the quality of filter effluent during drinking water treatment. The instrument extremely sensitive and is able to detect minute variations in concentrations of particles. A sample stream of about 100 mL/min passes through the sensor within the instrument. The blockage of light caused by particles in the sample stream is monitored. The instrument is also able to quantify the sizes of various particles in the sample stream. This instrument was used during the flume studies in order to detect the onset of incipient particle motion indicative of the critical hydrodynamic conditions. The water used during the flume studies was collected at the same time that the sediment samples were collected. Prior to the start of each flume experiment, the water was circulated within the flume in order to monitor the "background" particles present in the water. With the initiation of sediment erosion and transport, the particle counts correspondingly increased. The data obtained from the PCX is reported in terms of "normalized particle counts". It is representative of the relative concentration of particles present in the sample stream.

7. Page 23 table 2.7: reference for using d60/d10 as the uniformity coefficient

A reference has been included in the text. The uniformity coefficient is indicative of how well-sorted (poorly-graded) or poorly-sorted (well-graded) a sample of sediment is. A poorly-sorted (well-graded) sample has a flatter, broader grain size distribution curve since a larger variety of particle sizes are present. A well-sorted (poorly-graded) sample has a much steeper, narrower grain size distribution curve indicating that most are the particles are about the same size.

8. Page 26: the second sentence in the last paragraph should be revised.

The text has been modified.

9. Page 29 section 3.2: Is the method of incipient sediment size applicable for non-uniform sediment? Is the hiding and exposing effect considered? Page 30: Why not use different Shields parameter for each particle size class, which should be more accurate?

The intent of the analysis and discussion related to the incipient motion of sediment was to use a variety of accepted methods to quantify the potential of the sediments collected at various locations along the upper Walker River to be transported under a range of anticipated flow conditions. The various methods were chosen following a review of techniques presented in current literature related to sediment transport. Parker (2008) summarized the findings of Buffington and Montgomery (1997) who reviewed eight decades of incipient motion data, with a special emphasis on gravel-bed rivers. They concluded that the data generally followed the overall shape of the Shields diagram and the modified Shields diagram using the critical Shields parameter proposed by Brownlie (1981). Observations by

Neill and Yalin (1969) and Gessler (1970) indicated that values for initiation of motion of coarse materials determined using the original Shields diagram were too high. Garcia (2008) and Parker (2008) suggested that the expression proposed by Brownlie (1981) should be divided by 2 to define a lower boundary on the modified Shields diagram that is more consistent with observed data from Buffington and Montgomery (1997) for streams having d_{50} greater that 1 mm. The resulting values were found to be more relevant for engineering applications (Garcia, 2008). In a similar but smaller overview of methods for predicting incipient motion in sand bed streams, Marsh et al. (2004) also considered the Shields diagram as one of the best methods after comparing it along with three other methods (Garcia, 2008). In summary, Garcia (2008) indicated that there is sufficient evidence to conclude that the Shields diagram is quite useful for field application.

Whether one method is judged to be more or less appropriate than another, the resulting analyses consistently indicated that the sediments found in the upper Walker River would be expected to be actively transported under most of the anticipated flow conditions. This was consistent with what was observed in the field at each of the locations where sediment samples were collected. Even at relatively low flow conditions, active sediment transport was visually observed. Particles were being transported along the surface of the sediment beds.

10. Page 31: "…observed that the incipient sediment particle size suddenly decreased when the flow increased from 25 cfs to 50 cfs." The reason should be discussed.

The reason for the observed decrease in incipient sediment particle size as the flow increased has been discussed in the text. Figures showing the characteristics of the channel at 25 cfs and 50 cfs have been included to support the discussion.

11. Page 32 figure 3.6: It is a surprise that ~50 mm particles can be moved under 25 cfs flow. From table 2.7, all sampled sediment particles should be smaller than 50 mm. That means the channel is not stable even under very low flows. How could this happen?

The results obtained confirmed what was observed in the field at each of the locations where sediment samples were collected. Even at relatively low flow conditions, active sediment transport was visually observed. Particles were being transported along the surface of the sediment beds.

12. Page 35: Why was the Brownlie formula was used here rather than the constant 0.047 (page 30)? Another value of 0.03 was used in page 40. Is it for riprap? If so, provide the reference. The methodology seems inconsistent through the report. Needs justification.

The issues related to this comment have been addressed in response to Comment 9 above. The text in the relevant sections has been modified accordingly.

13. Page 42 table 3.10: Are the highlighted records the critical velocities? What is the criteria to determine the value? What is the normalized particle count?

The highlighted rows in these tables are indicative of the critical velocities corresponding to the initiation of particle motion during the flume experiments. As described in Response to Comment 7 above, the HACH PCX was a particle monitor used to detect changes in particle concentrations during the flume experiments. The data obtained from the PCX is reported in

terms of "normalized particle counts". It is representative of the relative concentration of particles present in the sample stream.

14. Page 48-50: Why was Keulegan's formula used to calculate the shear stress rather than using $\tau = \gamma RS$? S can be determined with the flume slope for uniform flow.

Determining the stability of the bed and banks of a natural alluvial channel depends on the definition of the threshold of sediment movement. Sturm (2001) discussed the threshold condition in terms of both a critical shear stress τ_{bc} and a critical velocity V_c. The critical velocities for six sediment samples from the upper Walker River were determined by performing the flume experiments. The Keulegan equation presented in Sturm (2001) was used as a means of determining a value of the critical Shields parameter τ^*_c based on the observed critical velocity V_c for each sediment sample. The Keulegan equation is given by the expression:

$$V_c = 5.75\left(\sqrt{\tau^*_c(\gamma_s - 1)gd_{50}}\right)\log\left[\frac{12.2R}{k_s}\right]$$

Once a value of the critical Shields parameter τ^*_c was determined, the critical bed shear stress τ_{bc} was calculated using the expression:

$$\tau^*_c = \frac{\tau_{bc}}{\rho g R D}$$

This enabled a unique value of the critical Shields parameter to be used for each sediment sample. This concern was raised earlier in Comment 9.

The relevant text has been modified accordingly.

15. Page 53: "In Figure 3.11, … Since all the data lay above the curve in modified Shields diagram, this indicated that all of the sediments will be actively transported under the existing flow conditions in the upper Walker River." Figure 3.11 shows the comparison of critical shear stress (shields parameter) resulted from 3 method (one experiment and two theoretical). It is not a comparison of a critical condition and an actual condition (either measured or calculated). How can it show the sediment can be transported or not? Figure 3.9 had the same issue.

The applicability of the Shields diagram as an acceptable method for predicting incipient sediment motion was addressed above in response to Comment 9. The fundamental framework of the original Shields diagram as well as the modified Shields diagram (now Figures 3.11 and 3.12 in the revised text) can be used to predict the susceptibility of a sediment to be transported. If a sediment has characteristics which fell above the solid line in these figures, then active sediment transport is anticipated.

16. I expect the flume experimental results can provide some correction on the critical shear stress prediction based on some derived regression equation. The results generated critical velocity data. But no new equation (for critical velocity as a function of sediment size and flow parameters, mainly the hydraulic radius) developed. Same as critical shear stress, critical velocity

will change with flow condition. Table 3.19 used the flume derived critical velocity directly to the field. This is not correct. If the authors want to use this method, either they can develop an empirical equation based on flume data, or they can use some existing equations, but not apply the flume results directly.

The response to this comment is related to Comment 14 above. The authors agree that the some data collected during the flume experiments was inappropriately compared directly to field data. The purpose of the flume experiments was to determine a value of the critical velocity V_c for each sediment sample. Then, the Keulegan equation was used to determine a value of the critical Shields parameter τ^*_c for each sediment sample based on the observed critical velocity V_c during the flume experiments along with the physical properties of the sediment. Then, the critical bed shear stress τ_{bc} for each sediment sample was calculated. Thus, the development of a separate empirical equation to relate the results of the flume experiments to observed field data is not necessary in this case. A similar concern was addressed in response to Comment 15 above.

The text has been modified accordingly.

17. I don't understand why the authors want to use two different methods (shear stress and velocity) to study the channel stability. They did not do any comparison between them, nor make recommendation for one over the other.

The concern raised by this comment has been addressed above in response to Comments 9 and 14.

18. The authors did not discuss the possible sediment source. If majority of movable sediment comes from bank erosion, the strategy of applying riprap on stream bed will not work, and installing settling basins will not solve the fundamental problem.

The sources of sediment in the watershed are largely the result of natural processes such as erosion during surface runoff and weathering of minerals. There is very little development within the watershed relative to its overall size. Much of the sediment is introduced into the river channel during seasonal runoff events (i.e., spring runoff) and during periodic intense thunderstorms in the summer months.

Other researchers are currently investigating the quantities of sediment yielded within various portions of the watershed.

Lining the river channel with rip rap is not considered to be an economically practical solution. Settling basins have been suggested simply as a means of capturing some of the sediment in the lower reaches of the river. Clearly, settling basins will not mitigate the source of the sediments.

19. All equations should be numbered.

All equations have been numbered.

20. Reference Parker (2005) is missing.

The references have been updated accordingly.

1. Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation? It is clear that all tasks aimed to the same goal. But some work did not show enough power to support the intension. See specific comments.

2. Is the paper clear, well organized and concise? Generally it is.

3. Are the methods appropriate, current, and described in sufficient detail? Generally the designed framework is good, but some specific methods are not correct. And some details need to be supplied. See specific comments.

4. If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct? Yes.

5. Are all tables and figures necessary, clearly labeled, and readily interpretable? Possibly.

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper? Some references are missing.

7. Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted? The length is fine.

8. Can the paper be published? The current version can not be published. I cannot make decision before reading a new version with major revisions.

PROJECT F: DEVELOPMENT OF A DECISION SUPPORT TOOL IN SUPPORT OF WATER RIGHT ACQUISITIONS IN THE WALKER RIVER BASIN

Development of a Decision Support Tool in Support of Water Right Acquisitions in the Walker River basin

We appreciate the comments provided by the two reviewers and believe that their comments have resulted in significant improvements to our final report. Below, we have provided a detailed response to each of the comments made by each reviewer:

Response to Reviewer 1

1. Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation? Yes, the report presents a cohesive story. Would be good to have an Executive Summary

This report is actually a "chapter" in a much larger report that will contain an executive summary for the entire report. Based on this comment and comment #3 made by Reviewer #2 (see below), we have added a section called Purpose and Scope beginning on page 10, after the Introduction section.

2. Is the paper clear, well organized and concise? The tense is not always consistent throughout the document.

We have made changes throughout the document with respect to the tense.

3. Are the methods appropriate, current, and described in sufficient detail? Not clear how the PRMS model was linked to the ModFlow model. The schematic shows them being linked and the title of the chapter also suggests that they are linked.

Based on this comment and comments #6 and #8 made by Reviewer #2 (see below), we have modified the text and one of the figures (please see response to comments #6 and #8 by Reviewer #2, below).

4. If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct? No statistical analysis was provided with the model output, so the evaluation of model accuracy was not permitted. Just a vision representation of the model results.

We did not propose to perform a statistical analysis of the model output in our scope of work; rather, we proposed to evaluate the model in terms of visual and objective measures. The primary purpose of the project was to develop the DST. Further analysis of the model results, improvements to the model, and scenario-based applications are planned in Phase II.

5. Are all tables and figures necessary, clearly labeled, and readily interpretable? Very good figures and tables.

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper? Page 11: Would be good to include references that support the background climate information for the area.

Unfortunately, we could not find a relevant paper to reference to climate information. Our comments in this section are based on our own interpretation of observed meteorological data and our understanding of the hydrologic processes in the study area.

7. Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted? OK

8. Can the paper be published? With minor revisions

Response to Reviewer 2

1. pg 1 title – "Decision Support Tool" is a very generic name. I think you need a better name for the software which gives a feel for what this actually does. "Water Right Acquisition Support" or something.

We decided not to modify the name of the software product since "Decision Support Tool" or "DST" is what we proposed in our scope of work and have promoted to all of the stakeholders over the life of entire duration of the project. We feel that changing the name of the product at this point would add much more confusion than leaving alone.

2..pg 2 TOC – I was confused (for a while) about all of these subsections with the same name (i.e. "Conclusions", "Purpose and Scope", etc.). Figure out how to make the subsection headings more distinctive

Titles of the subsections were modified to include the name of the model discussed within the section, e.g. PRMS Conclusions.

3. pg 8 para 1 – There is a "Purpose and Scope" sub section for each of the modeling sections describing the purpose of each model, but no "Purpose of this Document" section. Maybe the intended audience of this paper knows the purpose, but it took me awhile to figure out that this is a project status report. I think this document would be well served to have a "Purpose of this Document" paragraph at the end of the Introduction, or maybe a subsection after the Intro and before the "Site Description."

In response to this comment, we added a section called Purpose and Scope beginning on page 10, after the Introduction section that refers to the document as a whole. Purpose and Scope section titles in each of the modeling sections were removed for clarity.

4. pg 8 para 2 ln 1 – This sentence is what the whole paper is about. I went past this the first time I read this. I think you need add something more about "proposed water rights acquisitions" and "what evaluate the effectiveness" means.

See the above text of the new Purpose and Scope section in response to comment (3), which also addresses this comment.

5. pg 9 ln 1 – "University of Nevada's Desert Terminal Lakes Program" I have no idea what this is. Either list some of the specific goals or provide a Reference. Maybe list some of these out and move this up to the front of the paragraph to go with the first sentence.

This is a valid point made by the reviewer based on his or her review of this "Chapter" alone. This "Chapter" is, however, a small part of a much larger report that will include an introduction that clearly explains the Desert Terminal Lakes Program and how this work fits within the program. We don't think it would be appropriate to repeat this information in our Chapter.

6. pg 9 para 1 ln 1 – "Four models are combined to create the DST." This is confusing. In this paragraph, I see "First", "Second", and "Finally" That seems like three. Is the fourth SWL? or a repeat application of MODSIM? Also, the first level headers in the TOC indicate that there are three models.

We made the following changes. The original text "Four models are combined to create the DST." was modified to read "Three models are combined to create the DST."

7. pg 9 para 1 – I believe this is the first mention of PRMS, MODFLOW, and MODSIM. Add the citations for these models here.

Based on this comment we made the following changes.

The original text, with the modification from the previous comment (6) included,

"Three models are combined to create the DST. First, a physically based hydrologic model (PRMS) of the headwater supply areas is developed. This model is not directly linked to the others, but will be instrumental in future scenarios that may involve potential climate change. Second, groundwater flow models (MODFLOW) are developed for Smith and Mason valleys, the primary agricultural areas in the Walker River basin. The groundwater models focus on agricultural demand areas and groundwater-surface water interaction in the river corridor. Finally, a streamflow routing and reservoir operations model (MODSIM) is developed for the entire basin."

was modified to read

"Three models are combined to create the DST. First, a physically based hydrologic model (PRMS; Leavesley et al., 1983) of the headwater supply areas is developed. This model is not directly linked to the others, but will be instrumental in future scenarios that may involve potential climate change. Second, groundwater flow models (MODFLOW; Harbaugh et al., 2000) are developed for Smith and Mason valleys, the primary agricultural areas in the Walker River basin. The groundwater models focus on agricultural demand areas and groundwater-surface water interaction in the river corridor. Finally, a streamflow routing and reservoir operations model (MODSIM; Labadie and Larson, 2007) is developed for the entire basin."

8. pg 10 fig 1 – Figure 1 is confusing. Is this supposed to be a schematic diagram of water flow in the Walker River Basin? Or is it how information flows between the models? I guess it's both.

I think if you have two MODFLOW boxes you should have two PRMS boxes. Does the dashed box indicate that the whole basin is simulated by MODSIM? I don't think that is the case.

The original figure and caption
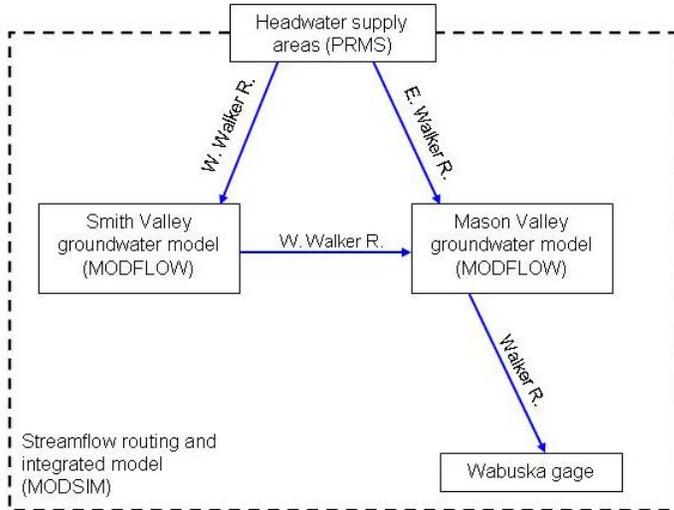
"Figure 1. Conceptualization of the DST. PRMS models the headwater supply areas. MODFLOW simulates agricultural demand areas in Smith and Mason valleys. MODSIM controls streamflow routing throughout the basin. Output from the MODFLOW models is used in the MODSIM model."

were modified as shown below.
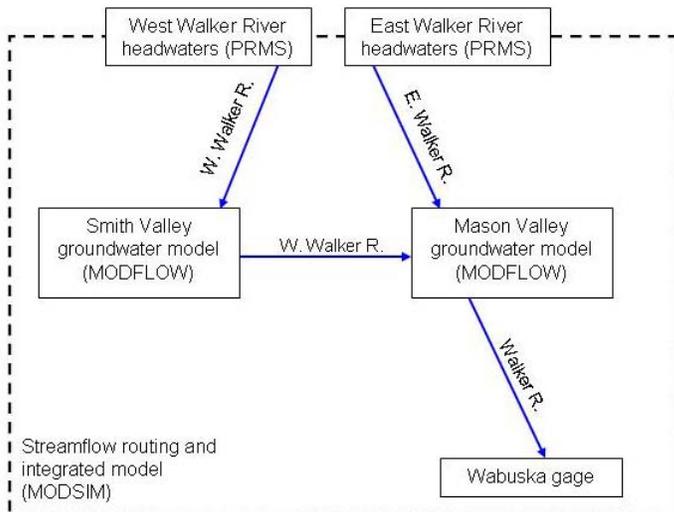
"Figure 1. Conceptualization of the DST. The exchange of information between models follows the flow of water through the basin. PRMS models the headwater supply areas. MODFLOW simulates agricultural demand areas in Smith and Mason valleys. MODSIM

controls streamflow routing and reservoir operations from the headwaters to the Wabuska gage. Output from the MODFLOW models is used in the MODSIM model."

9. pg 10 ln 1 – Where is Twin Lakes or the West Walker River gage in figure 2? Either add them to the map, delete them from the text, or indicate that they are not shown on the map.

We have made the following changes to the document.

The original text

"Twin Lakes and Bridgeport Reservoir (42,450 acre-feet, 52 million m^3, of storage) on the East Walker River, and Topaz Lake (59,400 acre-feet, 73 million m^3, of storage) on the West Walker River provide storage and control downstream flow (Sharpe et al., 2007). From the West Walker River gage at Coleville, CA (USGS gage 10296500), the river flows northeast through Antelope Valley and Smith Valley and then into southern Mason Valley."

was modified to read

"Bridgeport Reservoir (42,450 acre-feet, 52 million m^3, of storage) on the East Walker River, and Topaz Lake (59,400 acre-feet, 73 million m^3, of storage) on the West Walker River provide storage and control downstream flow (Sharpe et al., 2007). From Coleville, CA (near USGS gage 10296500, not shown in Figure 2), the river flows northeast through Antelope Valley and Smith Valley and then into southern Mason Valley."

10. pg 10 last para – I did not verify any of these climate statistics

The climate statistics on page 10 were verified.

11. pg 12 para 1 – In the paragraph that starts "The U.S. Geological Survey's (USGS) Precipitation-Runoff Modeling System (PRMS) watershed model. . ." Add numbers to parallel the description of the approach from the previous paragraph.

The following changes were made.

The original text

"The U.S. Geological Survey's (USGS) Precipitation-Runoff Modeling System (PRMS) watershed model will be used to model the headwater supply areas. ... MODSIM will be used to dynamically simulate reservoir operations and river systems within the basin. ... Agricultural demand areas will be simulated using MODFLOW. ... MODSIM will again be used to link the different models into one, integrated DST accessible to planners and managers."

was modified to read

"The U.S. Geological Survey's (USGS) Precipitation-Runoff Modeling System (PRMS) watershed model will be used to model the headwater supply areas (1). ... MODSIM will be used to dynamically simulate reservoir operations and river systems within the basin (2). ... Agricultural demand areas will be simulated using MODFLOW (3). ... MODSIM will again

be used to link the different models into one, integrated DST accessible to planners and managers (4)."

12. pg 14 ln 7 – Is this were the acronym "EWR" defined?

The acronyms were defined in Table 1; however, based on the reviewer's comment (14) we replaced subbasin acronyms in the text with the subbasin name.

13. pg 14 – Subheading "Model description". Change this to "PRMS Description"

The subheading was changed as suggested by the reviewer. See response to comment (2).

14. pg. 16 last para – There is a problem with the consistency of the use of the acronyms that you are using to name the sub basins. Sometimes these are called "West Walker headwaters" sometimes called "WWR basin." Are these names referring to different geographic areas or modeled areas? I would prefer that you not use the acronyms for the subbasins as I find them distracting.

Based on this comment from the reviewer, we removed all subbasin acronyms from the next and replaced them with the appropriate subbasin name.

15. pg. 17 fig. 4 – I did not verify the values presented in figure 4.

The values presented in Figure 4 were verified.

16. pg. 17 para 1 – Generally, PRISM works pretty well for what you are doing here, but sometimes it goes very wrong. It could be that PRISM is not very representative of the "bad PRMS basins" you plot below.

We have experienced some issues when using PRISM for this purpose in previous modeling projects, however, we have found that, in general, the use of PRISM has proven to be much more representative of the spatial and temporal distributions of precipitation and temperature in this area than any other approach we have tested. In phase II of this project, we plan to look at a variety of other issues related to snow water equivalent and the associated PRMS parameterization that may result in poor fits in the "bad" basins.

17. pg. 18 table2 – good.

We agree and made no changes to Table 2.

18. pg. 25 para 1 – PRMS and other snowmelt models have troubles in this region of the Sierra Nevada because winter daily temperatures are often near freezing. This makes it difficult to determine the form (rain or snow) of the precipitation. Small errors in HRU temperature can cause big problems in model performance.

Based on current efforts in the Truckee, Carson, and Walker basins with a similar approach that includes additional information on a variety of snow pack variables (from SNODAS) we are finding that the issue may be related to the parameterization of the snow related parameters rather than issues related to the temperature. Unfortunately, our findings are still in draft form and could not be included in this modeling effort. We do plan to incorporate

our findings into the Walker DST modeling effort in Phase II to, hopefully, improve the performance of PRMS in all of the study basins.

19. pg. 27 fig. 17 – Something appears to be wrong with the amount of precip in the EWHW and EWR subbasins (and some others). These models could be improved by checking the annual water balances and adjusting the simulated precipitation down. A table of annual water balance table for each subbasin, including volume of precip, et, measured and simulated streamflow would be very helpful in showing the strengths and limitations of these PRMS models.

We did use the annual water balance information in our calibration process for each of the study basins. Unfortunately, in this case the annual water balance information does not provide enough information to accurately determine why the PRMS models are still underperforming in some cases. Again, believe that the issues are related to the parameterization of the PRMS snow related parameters (see response to comments #16 and #18 above) and will be revisited in Phase II of this project.

20. pg. 34 para 1 – I did not verify these numbers.

The values on page 34 were verified.

21. pg. 36 para 1 – I did not verify these numbers.

The values on page 34 were verified.

22. pgs. 42-43 – I did not verify the numbers in tables 4 or 5.

The values in Tables 4 and 5 were verified.

23. pg. 47 – Get rid of the subheading "Conceptual model"

The subheading was removed from the MODFLOW section. See response to comment (2).

24. pg. 49 para 2 – Is the SWL something that DRI personnel wrote? This either needs to be cited, documented fully, or dropped from this report. Does the input to SWL come from PRMS? Or somewhere else?

The SWL is simply our term for the set of FORTRAN programs developed at DRI to implement the surface and groundwater models. The SWL codes are custom codes that were developed at DRI and currently have no documentation. We believe that the purpose and functionality of the SWL codes are well described in the report and have chosen to keep the term "SWL" in the text of the report.

25. pg. 50 – "Model development" subheading should be "MODFLOW model development.

The subheading was changed to MODFLOW model development. See response to comment (2).

26. pg. 55 – Does figure 27 need a citation?

This figure was made by DRI personnel with data available from the USGS. The paragraph referencing the figure reads: "Rating curves for width and depth as a function of river flow

are developed using USGS measurements spanning years 1947 to 2007 at Hudson, Strosnider, and Wabuska gages (Figure 27)."

27. pg. 56 para 1 – Is this paragraph describing MODFLOW or MODSIM input? If it's for MODFLOW are you recharging the cells in the HRUs?

This paragraph is providing background information on the available data for use in MODFLOW and, later, in MODSIM – there is no mention in the paragraph of either model since it is merely background on the available information.

28. pg. 62 fig 31 – Does figure 31 need a citation?

This figure was made by DRI personnel from data available in the GIS database referenced in the report. We do not believe that this figure requires a citation.

29. pg. 67-68 – I did not verify the results in tables 11 and 12.

The results in Tables 11 and 12 were verified.

30. pg. 71 para 2 – I don't think you need the acronym SNN

The acronym SNN was never used within the document, and was removed from page 71 as suggested.

31. pg. 72 – "Model evaluation" subheading should be "MODFLOW model evaluation

The subheading was changed to MODFLOW model evaluation. See response to comment (2).

32. pgs. 72-85 – These MODFLOW model evaluation sections look good, however, I did not verify any of this.

We reviewed and verified the MODFLOW model evaluation sections.

33. pg. 89 – "Conclusions" subheading should be "MODFLOW Conclusions." It's not clear what this is the conclusion of.

The subheading was changed to MODFLOW Conclusions. See response to comment (2).

34. pgs. 92-132 – This MODSIM section is beyond my technical expertise.

Understood. We did hire the developers of MODSIM to help us in the development, implementation, and review of the MODSIM applications described in this report.

35. pg. 95 ln 2 – The word "cost" needs to be defined here

Cost is a very common term used when describing the optimization of a dynamic simulation model. It is similar in concept to an objective measure used with models like PRMS during an optimization procedure – the objective measure is minimized (or maximized) through parameter value adjustment until a "best fit" is realized.

36. pg. 96 para 2 – "provided an unprecedented conjunctive surface and groundwater modeling system." Should be "provided an unprecedented conjunctive surface- and ground-water modeling system."

We agree with the reviewer's intent, but disagree with the implementation. Because we used groundwater throughout the document rather than ground-water, we made the following changes.

The original text on page 96

"Direct coupling between MODFLOW river cells and MODSIM links using a custom module provided an unprecedented conjunctive surface and groundwater modeling system."

was modified to read

"Direct coupling between MODFLOW river cells and MODSIM links using a custom module provided an unprecedented conjunctive surface water and groundwater modeling system."

37. pg. 96 para 2 ln 1 – is there a reference for "the standard MODSIM-DSS Graphical User Interface (GUI)"

The original text on page 96

"MODSIM networks can be developed manually in the standard MODSIM-DSS Graphical User Interface (GUI), or in an ArcMap extension called Geo-MODSIM (Triana and Labadie, 2007)."

was modified by adding the necessary reference

"MODSIM networks can be developed manually in the standard MODSIM-DSS Graphical User Interface (GUI) (Labadie and Larson, 2007), or in an ArcMap extension called Geo-MODSIM (Triana and Labadie, 2007)."

38. pg. 97 para 1 last ln – "GUI interface" is redundant.

The original text on page 97

"Although the base model was developed in Geo-MODSIM, subsequent model development, calibration, and simulation involved only the standard GUI interface."

was modified to read

"Although the base model was developed in Geo-MODSIM, subsequent model development, calibration, and simulation involved only the standard MODSIM GUI."

39. pg 118 para 2 – The plots look good, but aren't you setting the model output to be the measured values? I probably don't understand the significance of what you are doing here. Are you saying that the plots would not match exactly if there were shortages and because there weren't any simulated shortages, the model works? This has to be explained somehow so that

readers that are not familiar with MODSIM will understand the significance. Maybe your target audience understands this already.

Unfortunately, the calibration and implementation of the MOSIM software is a very complicated process and is generally difficult to describe to those not familiar with dynamic simulation modeling approaches. We struggled with the level of detail we should provide in the report on the calibration and implementation of the MODSIM model. On one hand we would like to make the description as simple as possible so that those unfamiliar with dynamic simulation modeling can understand the general approach, while at the same time providing enough information for those familiar with dynamic simulation modeling to be satisfied that the approach is correct. We believe that the referenced text is both appropriate and necessary for those familiar to dynamic simulation modeling and is probably difficult to understand for those not familiar. As a result, we chose not to modify the text in this case.

40. pgs. 130 and 132 – It is strange to have two "Conclusions" sections in a row.

The section named Conclusions and Model Limitations was renamed DST Summary and Limitations to clarify that it refers to the DST and report as a whole.

41. pg. 132 para 2 – "…all play a role in the Walker River system and the DST." Should be something like "…all play a role in the Walker River system and are simulated by DST."

The original text on page 132

"Climate, streamflow, upstream storage areas, irrigation practices, crop and non-agricultural ET, groundwater-surface water exchange in the river corridor, groundwater pumping and recharge, and all known existing water rights (decree, storage, and flood) all play a role in the Walker River system and the DST."

was modified to read

"Climate, streamflow, upstream storage areas, irrigation practices, crop and non-agricultural ET, groundwater-surface water exchange in the river corridor, groundwater pumping and recharge, and all known existing water rights (decree, storage, and flood) all play a role in the Walker River system and are simulated by the DST."

42. pg 133. ln 3 – Justify this statement by adding tables showing how the annual water balance is represented. See comment 19.

Please refer to our responses to comments #19, #18, and #16 above. We don't think that our statement would be justified by the suggested annual water balance information and therefore chose not to include it in the report.

43. pg 133. para 1 – "Understanding the limits of the groundwater models is..." either cut or rewrite this sentence.

The original text on page 133

"Understanding the limits of the groundwater models is an important part of their implementation. The groundwater models are limited by include the non-unique solutions,

poor representation of water levels away from the river corridor in Smith Valley, and the significance of the simulated groundwater-surface water interaction given the unknown associated errors."

was modified to read

"The groundwater models are limited by their non-unique solutions, poor representation of water levels in parts of Smith Valley, and the unknown errors associated with the simulated groundwater-surface water interaction."

44. pg. 140 – Check the use of citation: Western Regional Climate Center (WRCC), 2008a.

This citation is used in the caption for Table 6 on page 45:

Table 6. Modeled monthly ET for phreatophytes, and riparian and wetland vegetation. Rates taken from (Maurer et al., 2005; Maurer and Berger, 2006) and adjusted using monthly average precipitation in Yerington, Nevada (Mason Valley), and Smith 6N and Wellington Stations (Smith Valley) (WRCC, 2008a).pg. 140 – Missing date of report: Wilson, J.D. and R.L. Naff.

45. pg. 140 – Missing date of report: Wilson, J.D. and R.L. Naff.

The date was added to the citation on page 140, which now reads:

"Wilson, J.D. and R.L. Naff, 2004. MODFLOW-2000, the U.S. Geological Survey Modular Ground-Water Model – GMG Linear Equation Solver Package Documentation. U.S. Geological Survey Water-Resources Open-File Report 2004-1261."

PROJECT G: ECONOMIC ANALYSIS OF WATER CONSERVATION PRACTICES FOR AGRICULTURAL PRODUCERS

Economic Analysis of Water Conservation Practices for Agricultural Producers in the Walker River Basin

<u>Response to Reviewer 1</u>

1. Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation? The paper does a fair job of telling a cohesive story, although there were several sections that included apparently extraneous information. There were also some significant omissions. For example, the authors avoided addressing NV water law, quality impacts of changes in watering regimes, and the impact of incentive payments or water rights payments on cropping decisions. These appear to be critically important. The conclusions are not fully supported by the data or by references. Throughout the report, critical statements are not supported or explained in sufficient detail. This hinders the likelihood of another researcher being able to replicate the results or apply the same methodology.

> *Several sections were condensed to delete extra information. It was not the objective of this study to investigate water rights sales or water use incentive schemes, only the potential economic feasibility of lower water use crops (as compared to alfalfa). At this time all pertinent references and other statements are cited as needed. 0If another researched wished to replicate the results we would furnish our WinEPIC data base (agronomic and economic data for each crop) and they would be able to replicate the results.*

2. Is the paper clear, well organized and concise? The paper is well written, concise and given the amount of information it is also well organized.

3. Are the methods appropriate, current, and described in sufficient detail? The methods appear appropriate, but not sufficiently described. See the attached notes.

> *We have responded to the reviewers attached notes.*

4. If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct? Model summary statistics were not provided, which made it difficult to evaluate the strength of the models or validity of assumptions. Otherwise, the analyses were largely correct (except for the discussion regarding risk).

> *The WinEPIC model is not a statistical model which evaluates data, but rather generates likely outcomes regarding crop yields for up to 100 years. Hence, there are no "model" summary statists to report. All model outcomes are reported in the graphs and tables presented in the report. All agronomic and economic data assumptions were verified through the use of enterprise budgets, extension personnel, interviews with farmers, and university soil and plant scientists.)*

5. Are all tables and figures necessary, clearly labeled, and readily interpretable? The tables have a great potential to help tell a very interesting story. Unfortunately, they are difficult to read in black and white and in some cases lack appropriate labeling.

The report needs to be reproduced in color. Line markers were used to replace the colored lines, but it made the graphs more difficult to read.

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper? Several assertions of fact are not supported. Otherwise, pertinent references appear to be cited.

At this time all pertinent references and other statements are cited as needed.

7. Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted? The report is appropriate length for a technical report. The literature review should be condensed; a section should be added that addresses the sale of water rights (e.g., what price per acre foot have farmers been getting?).

The literature review was condensed. It was not the objective of this study to investigate water rights sales, only the potential economic feasibility of lower water use crops (as compared to alfalfa). Hence, we have not discussed water rights sales.

8) Can the paper be published? With major revisions

Specific comments:

1. I would have liked to see the paper estimate how much water was conserved as a result of planting the alternative crops, and/or under different assumptions about irrigation methods. The results on the crops are interesting, but the connection to water was lost shortly after the introduction.

By providing the amount of water used by each crop, producers can estimate the amount they would be able to conserve. This amount varies between producers, not all plant the same crops. Our task was not to determine the total amount of water savings, but the feasibility of alternative crops.

2. Pg. 3, 2nd para: The report describes the Walker Basin Project as a water rights purchasing program and indicates that completely halting irrigation would lead to a devastating loss to crop cover (resulting in dust bowl conditions). This suggests that adoption of alternative crops will result in residual water rights that can be sold via the project. However, on pg. 54, the authors state that Nevada law precludes partial sale of water rights. This should be addressed sooner in the report to avoid misleading the reader.

The report was created under the premise of the possibility of partial sale or lease of water rights in the future due to potential changes in Nevada water law. Although producers are allocated 4 acre feet of water per acre, during drought years most do not receive their full allocation, making this report helpful in determining if an alternative crop should be planted.

3. Pg. 4, 2nd para: Need a more full explanation of "local experts were consulted about experimental crops…" What methods were used, how many experts, was their broad consensus or disagreement?

Changed "local experts" to "local university and extension faculty".

4. Pg. 5, 1st para: It is indicated that WinEPIC has been calibrated for northern NV. Please provide a citation, and discuss the skill of the model (particularly when stating that the model can provide forecasts for 150 years).

The calibration of the model was determined by the models' creators at a workshop at Texas A&M, it was not a literature- based adjustment. The skill of the model is discussed at length under Data and Methods – Model Choice.

5. Pg. 5: consider bolding the section title "Related Literature." As-is, it looks like a typo.

Done

6. Pg. 6: the literature on water pricing (thru pg. 7) seems out of place here. The focus of the report is on alternative crops, not policy changes, and certainly not pricing of water.

Pg 6, 2nd paragraph through pg 9, 2nd paragraph has been removed from the report.

7. Pg. 7, 1st para, 2nd sentence: I would argue that the stated goal of water policy has little to do with the social costs. Instead, it is meant to bring prices closer to the long-run marginal cost (typically they are set at the SRMC) (e.g., Olmstead and Stavins 2007).

Pg 6, 2nd paragraph through pg 9, 2nd paragraph has been removed from the report.

8. Pg. 7, 1st para, 4th sentence: Please provide a citation for the statements about taxes and spot water markets.

Pg 6, 2nd paragraph through pg 9, 2nd paragraph has been removed from the report.

9. Pg. 9, last para: Please indicate how these criteria were determined. Did the expert panel define this list?

These criteria were determined by the authors. For clarification, changed "In order for an alternative crop to be economically feasible" to "In order for an alternative crop to be considered economically feasible by this study".

10. Pg. 15, 2nd para: The discussion of no-till seems misplaced, unless the results contemplate income from carbon credits or farm bill programs that is generated by adoption of no-till.

No-till is an important practice with regard to soil moisture retention, and affects the amount of water needed by the crop. Please refer to new version of report, page 13 first paragraph.

11. No-till is mentioned as being "incorporated… for all crops under consideration," but I did not see mention of no-till in the results. No-till is known to provide better long-run soil water storage for crop use, but probably does not increase water runoff to streams. If anything, no-till stabilizes production yields, but provides less water to streams. Either way, there should be some literature backing up this assertion.

This section has numerous references to the literature.

12. Pg. 17, 1st para: Please provide the page number for the quoted material. Also, move the period from "consumed." to after the parentheses.

Corrected

13. Pg. 17, 2nd para: there is an additional space in "…; Center 2006) ."

Corrected

14. Pg. 17, 3rd para, 2nd sentence: Please remove either "TAMU" or "Texas A&M University"; also, there is an unnecessary opening parenthesis before Teaxs A&M.

Corrected

15. Pg. 18, 2nd para, last sentence: please spell-out SSURGO as "Soil Survey Geographic".

Corrected

16. Pg. 19, 1st para, 2nd sentence: extraneous space following the coordinates for Smith Valley.

Corrected

17. Pg. 19, 2nd para, last sentence: Please indicate any citations and assumptions that support the alterations that were made to the crop profiles in WinEPIC.

These alterations were made by the agronomist at Blackland Research Center.

18. Pg. 20, 2nd para, 3rd sentence: Please provide additional description of how producer panels were used. How many producers per crop, etc?

The amount of producers varied by crop. Particulars were not disclosed for confidentiality reasons.

19. Pg. 21 – 22: Please provide justification (citation or personal communication) for the assumptions made on irrigation type, amount of water used, and length of rotations.

Assumptions were based on common production practices as stated in the first sentence of the paragraph.

20. Pg. 23, 2nd para: Please provide more description of how the model was validated. How much skill did the model possess? Exactly how close were the Lyon County yields to Churchill County?

Corrected

21. Pg. 25, equation 3: Please provide citations that support the use of normal and beta distributions for those crops. Also, if there are any summary statistics for the curves, please report those.

The distributions were determined by the data

22. Pg. 26, 1ˢᵗ para: Please report the alpha and beta parameters for the beta distributions. Someone wanting to replicate the results would need those.

Corrected

23. Pg. 26, equation 6: For the triangular distribution, which were the known, and which were the assumed values?

The known values were the minimum, midpoint and maximum values; the assumed values were those generated by the simulation.

24. Pg. 28: The graph is difficult to read in black & white. Consider including line markers. Ditto for all graphs in the report.

The report needs to be reproduced in color. Line markers were used to replace the colored lines, but it made the graphs more difficult to read.

25. Pg. 29: Please include the price per ton assumptions in the chart. Also, please consider comparing actual price per ton (at current or recent historic levels) to simulated break-even prices.

There are no prices per ton assumptions in this chart. Actual pricing levels are discussed for each crop in their respective sections.

26. Pg. 29: Please convert wildrye to price per ton, or explain why wildrye is only reported in price per pound.

Explanation is given under the wildrye section on page 33 of the newest version.

27. Pg. 29: Please explain why alfalfa and grapes are missing output for certain watering strategies.

Irrigation application was done in 2" increments.

28. Pg. 31: Output for onion yields appears to be very sensitive to yield curve assumptions. The authors should address whether the output has a relevant range (e.g., from 22 – 34 inches).

Yield curves were not assumed, they were generated by the WinEPIC model.

29. Pg. 33, Table 3: Please spell-out "P&I" and please provide a citation that justifies using a 7% rate.

P&I Corrected. The 7% rate was the same as the rate used in the enterprise budgets as suggested by the producer panels.

30. Pg. 33, 2ⁿᵈ para: Any difference in capital investment could be addressed by a program or policy change. Perhaps this should be addressed (at least superficially) in the discussion section.

This is beyond the scope of this report.

31. Pg. 34: Please provide a brief discussion of what assumptions you made that could significantly disrupt your results. How robust are the results?

In the previous version, page 34 discusses teff. I am unclear as to the pertinent page number for assumptions of results.

32. Pg. 40: I would like to see a section that addresses what changes in costs (perhaps due to a support program) would make less-thirsty crops economically preferred. In the last part of the 1st paragraph, the authors hint at this with a discussion of the relative costs of flood and center pivot irrigation for barley and alfalfa. [This is another way of determining what the water rights must sell for to achieve changes in crop and/or new technology adoption]

This is beyond the scope of this report.

33. Pg. 48: For wine grapes, it is unclear how the results were calculated with regard to maturing vines. Also, should an agritourism component be included?

The results were calculated using average yields over the lifecycle of the vines. This study focused on income from production.

34. Pg. 49, Figure 9: Please label the x-axis. I assume it is showing price per acre.

Corrected

35. Pg. 50, 1st para, last sentence. I disagree with the authors' characterization of how the steepness of the curves in Fig. 9 translate to one crop being preferred to another. It really depends on the level of risk aversion. Likewise, the statement "risk averse producers would rather lose $300 yearly than make a profit of $300 one year, losing $900 the next year" is not accurate. A more accurate description of a risk averse farmer would be one that would rather have $49 than a 50% chance at $100. Also, a citation is needed here.

Corrected – The last sentence has been removed.

36. Pg. 53, 3rd para: Could WinEPIC not account for quality changes with a weighting procedure?

The model does not have this capability at this time.

37. Pg. 54: The statement about NV water law (as stated) undercuts the significance of the report. I would like to see the authors address this.

The report was created under the premise of the possibility of partial sale or lease of water rights in the future due to potential changes in Nevada water law. Although producers are allocated 4 acre feet of water per acre, during drought years most do not receive their full allocation, making this report helpful in determining if an alternative crop should be planted.

38. Water rights, if sold, would supplement income. Should the sale of water rights be factored into the break-even price calculations?

There are too many unknowns regarding the price that would be received for water rights and amounts received by each individual producer.

<u>Response to Reviewer 2</u>

1.Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation? Yes, for the most part.

2. Is the paper clear, well organized and concise? The manuscript is very long and for the most part is easy to follow but is not organized in a standard research paper format, i.e., problem statement/review of literature, objectives, data and methods, results and discussion, and conclusions/ recommendations. The manuscript would have a much stronger focus and be easier to follow if it were organized like a standard research paper with appropriate subheadings in each major section. In addition, there is a lot of text that simply doesn't add to what the authors are trying to do. For example, on page 24 in the first paragraph: "Although data can be input in either English or metric units, all output data…" The reader does not need to know about this because it adds nothing to the study and thus all extraneous statements like this should be eliminated from the manuscript. Removing such language would shorten the manuscript and make the analysis much more concise.

> *The report was formatted to the requirements of the Walker Basin Project. Superfluous text corrected – Numerous paragraphs have been removed from the last version, i.e. the last paragraph of page 6 through second paragraph of page 9; these do not appear in the newest version.*

3. Are the methods appropriate, current, and described in sufficient detail? Methods used to calculate the costs of production appear to be reasonable for the most part. The authors, however, should make sure that they follow the standards out lined in the AAEA Costs and Returns handbook. I am not sure how the authors handled establishment cost for perennial crops in their simulation of net revenues. It seems to me that it would be appropriate to amortize establishment costs over the life of the investment and add the annuity value to the annual operating costs. The crop budgets don't indicate that the authors have done this.

> *Calculations were used for the formatting from UC Davis enterprise budgets. Establishment costs have been amortized over the life of the investment for perennial crops. (See UNCE special publications SP-08-06 through SP-08-14)*
> *http://www.unce.unr.edu/publications/search/*

It is a not necessary step and bad procedure to first simulate yields using the daily time step simulation model and then simulate them again in Excel using Simitar. Use the simulated yields from EPIC directly in the calculation of crop net revenues. All of the discussion of methods on pages 25 and 26 is superfluous and do not add to the analysis and may in fact add bias to the analysis in that you are making assumptions about the distribution of yields when you don't need to make the assumptions, i.e., normal or beta distributed yields.

> *The yields were not simulated again in Simitar, only the amount of variation in yields between years. It was necessary to determine the distribution of the residuals of the yields in order to create stochastic yield variables.*

I don't understand the discussion of irrigation strategies on pages 21 through page 23. Different amount of irrigation are applied to each crop but absolutely no documentation is provided to justify the set of irrigation and other production practices chosen. This certainly needs to be

documented. Then, on page 24 in the analysis section, the authors indicate that they simulate yields for each crop in increments of two inches up to 48 inches. Why discuss the different amounts of irrigation for each crop unless they are tied to a current recommendation.

As stated on page 18 of the newest version, "Irrigation amounts followed producer or research recommendations for the initial simulations". A range of irrigation levels were used to determine potential yields under all levels of available irrigation. In some cases, recommended levels were not optimum as seen with Great Basin wildrye on page 33.

4. If statistical models are used, are model assumptions, inputs, and statistical design, and analysis? See my comments in question 3

5. Are all tables and figures necessary, clearly labeled, and readily interpretable? Yes.

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by data in this paper? No. See my comments in question 3

7. Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted? See my comments in question 3.

8. Can this paper be published? With major revisions.

PROJECT H: FORMULATION AND IMPLEMENTATION OF ECONOMIC DEVELOPMENT STRATEGIES

Economic and Fiscal Impacts and Economic Development Strategies: Consequences to the Agricultural Economy in the Walker Basin

Response to Reviewer 1

General comments

I am not sure as to the general purpose of the study. If this is intended as a reference document that will be the foundation piece for other reports you can ignore most of my suggestions regarding format changes. Format and readability are less important for a reference document. If however, the document will be circulated to a wide audience I think format changes would be useful.

I would make the following recommendation regarding general format:

1. Needs an executive summary. The abstract is useful but it does not state a summary of the findings. The general declarative sentence stating the purpose of this report is not offered until page 10.

   Added executive summary

2. The table of contents needs to be revised. Chapters may not be necessary and could be renamed as sections. I think the long list of figures and tables could be removed. Also, the page listings may not be necessary for all the subsections. This assumes that the format is not mandated by an RFP.

   Table of contents revised. Chapters were renamed as sections. List of tables and figures was retained, as the authors felt it was beneficial. Some of the subsection details in the table of contents was eliminated.

3. Needs a conclusion with an overview of policy recommendations.

   Conclusions included in the executive summary

4. Add a section about the authors. In my experience these types of reports benefit from a small blurb about the authors. Users of these studies need to know the technical credibility of the authors.

   Information about the authors has been included.

5. The most useful section is Chapter 3. This is the section where the public deliberation and policy prescriptions are presented. This should be emphasized in the executive summary.

   An executive summary was added that emphasizes content from the former Chapter 3.

1. Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation? The paper does tell a comprehensive (perhaps not cohesive) story. The analysis for the various locations are clearly designated and well supported by the analysis.

Chapters one and two need to be more integrated. It is a little confusing to see analysis for Mason Valley and Smith Valley in the first chapter then see tax impacts for Lyon County and Smith County. The introductory paragraph to chapter two does delineate the regions but it may be useful to include a map or footnotes on the fiscal impacts. This may not be necessary for someone who is familiar with the region.

> *Comments have been added so that it is clear that Mason Valley and Smith Valley are parts of Lyon County, while Hawthorne and Walker Lake are in Mineral County. The report's technical competence is clearly in line with a document of this type. The methods are appropriate and properly applied.*

2. Is the paper clear, well organized and concise? It is organized to extract specific data easily but overall the format makes it a difficult read.

> *The addition of an executive summary and the reformatting of the document should mitigate the "difficult read" observation.*

3. Are the methods appropriate, current, and described in sufficient detail? The methods are appropriate and described in sufficient detail. It would be useful to describe the direct, indirect, and induced impacts in more detail. In my experience most people don't know the difference between the impacts. Also, on page 18 the sentence that states the output from "IMPLAN are much like total sales" may be misleading. IMPLAN generates an output amount of output which may constitute sales or changes in inventory. It appears that the fiscal impact sere not estimated using the IMPLAN package. It may be useful to add a line or footnote saying that.

> *Language has been added and revised to address these comments. It was very clear to the other reviewer that IMPLAN was not used for the fiscal imapct analysis. The authors felt this is quite straightforsard.*

4. If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct? The statistics presented are in line with what one would expect in such a report. The underlying assumptions of the input-output methodology are appropriate and the application of the IMPLAN model appears correct.

5. Are all tables and figures necessary, clearly labeled, and readily interpretable? The tables and figures are clearly labeled and interpretable. The maps are very useful for a project like this. It may be useful to increase the size of the legends to make them more readable. The assessed value in tables 2.3 and 2.4 do not appear to reconcile the way tables 2.5 and 2.6 reconcile.

> *The smallest legend was removed, as the information was reduncant with the tables that immediately follow the maps. The tables include different information. Changing the title of table 2.4 (now table 3.4) helps to explain the difference between the two sets of tables.*

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper? References appear appropriate.

7. Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted? The length is appropriate. Depending on how the document is to be used, the material in chapter three should be highlighted. That is the useful portion of the report.

8. Can the paper be published? I would accept this paper with moderate formatting revisions. I do not think any of the actual analysis needs to be revised.

Response to Reviewer 2

1. Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation? The paper does tell a cohesive story. Four scenarios are presented concerning possible economic and fiscal impacts on the Walker Basin. The scenarios are well-defined and clearly stated. The conclusions are supported by the data. The limitations of the study are addressed directly and are unavoidable. Overall, this is a solid report. I do have several suggestions concerning mainly the organization of the paper that might improve readability.

2. Is the paper clear, well organized and concise? Generally, the paper is well written and well organized. I have the following suggestions.

First, an introduction (in addition to the abstract) describing the nature of the problem and the analysis that will be presented would be a helpful addition to the paper. An introduction of this type would be particularly useful to someone who might read the study a decade or so from now. As currently presented, the main body of the text begins with a description of economic impact analysis with no preliminaries.

  Added executive summary

Second, Chapter 1 (Economic Impact) is far too long (60 pages) for most readers. Without much additional work, this chapter could be split into two separate chapters that would be much easier to follow. The first of the new chapters could contain the general introductory material on impact analysis and describe the four basic scenarios. The second 'new chapter' could then present the results of the impact analysis.

  Done (and renamed as sections 1 and 2)

3. Are the methods appropriate, current and described in sufficient detail? The short answer is yes. More detailed comments follow. There are essentially four methods used in the report: (a) an analysis of current agricultural production and cropping patterns in the region, (b) the economic impact of the four scenarios analysis using IMPLAN, (c) the fiscal impact analysis using the impact results and location specific tax rates, and (d) the community survey approach that formed the basis of the economic development section.

The analysis of agricultural production and cropping patterns in the region is detailed, thorough and essential for the economic impact analysis that follows. This analysis in combination with general economic and demographic information about the region is, of course, the basis for the four scenarios developed in the study. The detail clearly and convincingly indicates that the Walker Basin is not a single homogeneous region and that the economic and fiscal impacts are

likely to be different in different sub-regions. The authors have been careful to explain that water use, profitability, crop yields, and other critical variables presented are averages subject to considerable variation from farm to farm and by region.

The use of the IMPLAN software to evaluate the economic impacts of the four scenarios is an appropriate state-of-the-art approach. I use IMPLAN on a regular basis to conduct economic impact studies and the selection of this model should generate no controversy among professional economists. There are few alternatives and input-output model based multipliers contained in IMPLAN are both reasonably conservative and widely used. The explanation of the model is straight-forward and well done in the report. I have only two IMPLAN-related suggestions. First, a paragraph (no more) should be added that explains real versus nominal dollar values. The IMPLAN structural matrices are for a particular year. The most recent version is 2006 and dollar values of both inputs and outputs are in 2006 dollars. Since the water rights changes could occur over a period of several years, the fact that the results are in constant (2006?) dollars should be explained.

Explanation added.

Second, as explained (nicely) with Table 1.3, most economic impact studies attempt to estimate impacts in terms of value added, employment and labor income. Many readers of the study are likely to want to see the employment and labor income impacts. In the four basic scenarios, only value added (direct, indirect and induced) is presented in the tables (e.g., Tables 1.18 through 1.20). Why not have an additional table or two showing the employment and labor income impacts –as is done in the case of anglers and recreational use (Table 1.23)?

Added.

The method used to evaluate the fiscal impacts is to apply the appropriate tax rates to the impact results under the four scenarios. This is an appropriate method and far better than the generic tax rates built into IMPLAN. Again, I would stress that these are constant dollar estimates.

The authors felt that the fiscal impact analysis was fairly clear in indicating dollar values being based on fiscal 2007 budgets.

The economic development analysis in Chapter 3 is based primarily on surveys of local residents in a series of community meetings. This is an appropriate method to use for local economic development issues. A clear and largely unchallenged lesson from numerous local economic development studies is that the chances of success in the development arena are directly proportional to community involvement. I have only one suggestion to strengthen this approach: specifically, a discussion of the definition of economic development. This is a concept that can mean many things to different people.

A brief definition of economic development (one sentence) was added into the executive summary and the body of the report.

4. IF statistics or models are used are model assumptions, inputs, the statistical design, and analyses appropriate and correct? Please see the responses to question 3. This is not a statistical study in the usual sense of that phrase.

5. Are all tables and figures necessary, clearly labeled and readily interpretable? The tables and figures are necessary. The figures –especially the maps add clarity and ease of understanding to the material in the text. I suggested (above) a couple of additional tables containing employment and labor income impacts of the four basic scenarios. The tables are clearly labeled and relatively easy to interpret. Tables 1.18 through 1.20 probably contain too much data for most readers, but I do not have a good suggestion on how to change these.

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper? The references are pertinent. No additional references seem necessary.

7. Can the paper be published (minor, moderate or major revisions) or is it too flawed to be published even with major revisions? This paper can be published with minor revisions. I strongly recommend an introduction, splitting chapter 1 into two separate chapters, and including some discussion of real versus nominal values in the impacts section.

These comments were all addressed

PROJECT I: DEVELOPMENT OF A WATER RIGHTS GIS DATABASE

Development of a GIS Database in Support of Water Right Acquisition in the Walker Basin

<u>Response to Reviewer 1</u>

1. Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation?

1. Take the majority of information from the methods section and create metadata for each dataset collected for the project and create an appendix of metadata (which is looks like you have done already).

An excellent suggestion. As part of our report, we have developed metadata for each of the data sets described in the original Table 1 (which is now in an Appendix at the back of the report). In addition, we will be providing a USB flash drive containing the database (with FGDC metadata) to BOR when we turn in the final report.

2. Keep the report short and refer to the metadata for further discussion of methods.

*We will move Table 1 to an appendix (Appendix A) at the end of the report (now called Table A1). We will reference the metadata attached to the data sets in the **Data Acquisition and database development** section.*

3. Focus should be content of the GIS, how it was used throughout the larger project, lessons learned and recommendation for maintenance or future enhancements. For the content of the GIS describe the spatial extent, scale of datasets and uses of the data in the larger project. Much of this could be handled in the first table that lists all of the datasets. I would recommend adding a column for spatial extent and scale of the data. In addition I would add which components of the project as a whole used the data i.e. specifically which datasets provided inputs to MODSIM and MODFLOW, which datasets contributed to the EIS etc.

*Good idea. We have added three columns to Table A1 in the new appendix, one for spatial extent, one for spatial scale, and one for the actual file name of the data set on the accompanying USB flash drive so that it can be cross referenced with the table. We have added a subheading under the **Discussion** section called **Spatial data uses by project**, that summarizes which projects within the Walker Basin project utilized the specific data described in the **Data Acquisition and database development** section and listed in Table A1.*

4. Make a graphic or flow diagram that illustrates the steps that were taken to obtain the data, basic processing steps and the final result.

We feel that the major revisions we've made to the report, i.e. more detailed descriptions of the database and its purpose and content, restructuring of the original Table 1 (now a table in Appendix A, the addition of the appendix, the development of the database on an accompanying flash drive, and a summary of which projects and groups are using the various data sets, have adequately addressed some of the identified shortcomings. We feel that the

addition of flow diagrams will only repeat what is now described in the text, and will only lengthen an already long report.

5. Depending on how it ends up fitting together you might include some summary information for each study area from the GIS i.e. how many ditches, POUs etc.

We don't feel that enumerating the number of ditches, POUs, etc. in the text will provide any additional information that can't already be extracted from the attribute tables of the data layers that will now be readily available on the project flash drive that will accompany the report.

6. The data storage and distribution section could also be included or included as an appendix.

We have moved the file system figure to Appendix A and modified the discussion of the data storage and distribution issues in the Discussion section.

7. You begin to describe some of the lessons learned in the data acquisition / data development issues section but I think it could be more focused. A table or bulleted list of issues followed by the more detailed discussion of them may make it easier to follow. You could include a section on the limitations of some of the datasets.

We have added a set of bullets to the Data Acquisition/data development issues and limitations subsection that highlight some of the key issues. We have also added more detail to the discussion of these issues. We added a paragraph describing some of the limitations of the data.

8. Include recommendation to the agency for maintenance of the GIS and maybe some future enhancements. Do you have recommendations for the agency if they were to undertake this type of GIS project in another location? Where are the gaps in the available data? What datasets would be the highest priority to create in the future for supporting future analysis or enhancements to the models?

We have added a recommendation subsection to the Discussion section.

9. An alternative report

We will follow the reviewers above recommendations for a funding agency report, as that is our current reporting requirement to BOR.

2. Is the paper clear, well organized and concise?

1 .The paper was not clear, well organized or concise.

We are hoping that the above described responses, based on the reviewer's recommendations, will provide a clearer, more concise final report to BOR. We have modified and reformatted some of the sections to provide a more concise report.

3. Are the methods appropriate, current and described in sufficient detail?

1. I am concerned with the mixing of scales of data sources and no discussion about the impact on the hydrologic model. Resampling the 10 meter DEM data to 1 meter and combining it with

1 meter LIDAR data does not follow good practices. Convention is not to increase or decrease the scale of a dataset by more then 2.5 times.

We have clarified the text so the reviewer may fully understand how and why the resampling occurred and inserted a reference for hydrologic modeling using DEMs.

The reviewer is correct in their statement that analysis should not be done using a fused 1m and 10m DEM at the finer resolution; however, the only analysis we did using 1m data was to identify the Walker River main stem and this did not use any of the resampled 10m data to accomplish this task.

The resampling of the data was done to allow us to fuse the data sets together in preparation to resampling to the higher 100m pixel resolution required for analysis within the hydrologic modeling efforts. Data may be fused at any resolution, it is the analysis that may be impacted by the fusion efforts. Thus, we resampled up beyond the minimum 10 pixel resolution.

The Landsat information was used to generate different datasets and all information is scaled up to the 100m hydrologic modeling unit resolution, thus, again there is no downscaling of the Landsat information only upscaling of the final product.

It should be noted that there are multiple papers which do imagery classification at a different resolution than the DEMs which are used to discriminate further. As a matter of fact, manuscript authors have used elevation as an ancillary dataset to further refine land cover, precipitation, wildlife habitat and many other types of classifications. The assertion that all data has to be at the same pixel resolution for differing attribute information is incorrect and a quick look through PERS at many land use/land cover classification manuscripts will void any argument against this. The reviewer is correct that for a similar attribute (i.e. elevation) downscaling may and has been shown to result in "strange" data outcomes (i.e. striping). This noted many authors have stated that often this has more to do with the resampling algorithm than the actual resolution of the information. Regardless it is still an effect and we did not downscale any data that was used in a final analysis. We just downscaled to allow for fusing of data prior to upscaling.

2. I have not kept up on the Landsat interpretation literature but there may be other equations that are better then NVDI.

In previous DRI projects involving Landsat TM analyses of the Walker basin, researchers have experimented with the use of other, newer satellite-based vegetation indices such as SAVI and MSAVI. The results with these soil-adjusted vegetation indices were not as promising as with the NDVI, so a decision was made to use NDVI for this project.

3. On page 36 it would help if you describe the topography of the study area to support the assertion that the effects of topography on the NVDI values are minimal, assuming the area is flat.

We have added a description of the topography found in Mason Valley to the Landsat TM section (Mason Valley is flat).

4. It would also be helpful to have an accuracy assessment for the Landsat interpreted data.

An accurate assessment of the Landsat interpreted data would have been difficult given that the time period of the analysis was the year 2000. Accuracy assessment of sample sites within the current irrigated fields would have been difficult given the limited access to private property and the relatively short time frame of the project.

5. Page 40 – I think you conducted a zonal sum to calculate the area of irrigated lands within each HRU but it is unclear which of the zonal statistics was used. The description of this process needs to be rewritten.

*We have rewritten this section to clearly state that the ArcGIS Spatial Analyst Zonal Statistics function was used to **summarize** the number of acres of irrigated land (based on NDVI values) per HRU.*

4. If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct?

1. See comments under # 3 on resampling data and NVDI.

Addressed above and changes made to final report.

5. Are all tables and figure necessary, clearly labeled, and readily interpretable?

1. Label the rivers and lake

Done.

2. The state boundary line and the county boundary lines should be different line types.

Done.

3. What are the study area boundaries? Figure 1 – has two dashed boxes, are these the study areas? If they are the boundaries should be included on Figure 2 and 3. If they are study area boundaries are the study areas for the entire project or just the hydrologic modeling component?

The study area is the entire Walker Basin. Smith and Mason valleys were identified in Figure 1 because of their significance as prominent agricultural regions and because they are the focus of much of the GIS development work due to importance in the DST development process and other Walker Basin projects. We have identified them on Figures 2 and 3.

4. The location map should be labeled as a location map. I am finding in my own work that as strange as it may seem not everyone recognizes those as location maps.

Done

5. Figure 4 – not having the same orientation on both the surface and elevation maps makes it difficult to read. Either have two figures or have the same orientation for both the surface and the elevation. Alternatively draping the elevation over the hillshade can also be an effective method for illustrating the data. I am not sure where figure 3 and 4 are referenced in the text.

We have adjusted the orientation of the hillshade in Figure 4 to match that of the elevation map. Figure 3 is referenced on page 5; Figure 4 is referenced on page 10.

6. Recommend including hydrologic boundaries of the watersheds to the maps. If this is supporting hydrologic modeling then an indicator of the boundaries of the model should be included.

Figures indicating the hydrologic boundaries for the various DST model domains are included in the DST modeling final report; we did not duplicate them here.

7. Figure 15: The title is not clear. I am guessing that the yellow polygons are the HRU's but I am not sure how it illustrates the NDVI results.

This figure is now Figure 17. We have modified the figure caption to better explain that the yellow areas indicate the irrigated acreage found within the HRU boundaries for the Landsat scene acquired 7/27/00.

8. Table 1: I would add the scale of the original data, the extent of the data and indicate which projects used GIS data i.e. hydrologic modeling, EIS etc.

We have addressed this issue and our response is described above under Section 1.

9. Table 2: it would be nice to have the different areas illustrated on a map. Antelope Valley, East Walker etc.

We have added Figure 10, the locations of the annual diversion sections identified in Table 2 (now Table 1).

10. Table 3: unless your data is accurate to the 6th decimal place I would reduce the number of decimal places. Define what the different Crop type values are as footnote to the table.

We have reduced the number of decimal places in the table. We spelled out the crop type names (values) in the table.

11. Table 4 – in a footnote define the Type of Use code.

Done.

12. Table 5 – same comment as table 3.

Done.

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper?

1. Look for some more recent citations on NVDI analysis for identification of irrigated lands, wetlands and riparian areas.

We added several references on the use of NDVI in the assessment of irrigated lands.

2. Need to have references to support the manipulations of the elevation datasets. Look specifically for literature supporting hydrologic modeling and DEM creation.

We added a reference for hydrologic modeling using DEMs.

7. Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted?

1. The length will depend on what the focus of the report ends up being.

We have tried to keep the length of the report down by putting all of the data and metadata on a DVD to accompany the report.

8. Can the paper be published with:

1. To be published in a book or journal it would need to have an entirely different focus.

If we publish these results in a peer-reviewed journal or book, we will be integrating these results with those from the DST modeling report, condensing the material dramatically, and emphasizing the development of the spatial data specific to the model development process.

2. It can be published as a report to an agency with major revisions.

We hope that the major revisions we have made and the modifications and reformatting we have done to the document will be sufficient for the BOR report.

Response to Reviewer 2

1. Better describe why the database was built and how it would be used.

Addressed this comment in the Introduction section.

2. Grammatical changes to page 1.

Done.

3. Continued text on page 10.

Text was not continued in draft because of size of Figure 4.

4. Why list Permit data in Table 2 when all zeros?

The annual diversion data received from the Federal Water Master contained decree, storage, AND permit (flood) diversion data for the years 1996 to 2006. The example we show in the report, 2007, did not have any permit (flood) diversions because it was a relatively dry year.

5. What about domestic/municipal wells? Weren't they needed for groundwater model?

Both municipal wells and irrigation wells were included in the groundwater modeling process. We have modified the text in the document to reflect the use of both well types. NDWR did not have flow rates available for domestic wells in Mason and Smith valleys, and domestic well pumping was not thought to have a significant impact on the groundwater system in both valleys.

6. On page 35, spell out GBLW.

Acronym GBLW is identified and spelled out on page 27.

7. Why was it important to observe fluctuations in irrigated acreage for a relatively dry year, 2000?

We have added a discussion of the significance of the dry year analysis to the section Analysis of irrigated acreage in 2000.

8. Delete zeros and reduce significant digits in Table 5.

Done.

Economic and Demographic Analysis of the Walker Basin

Response to Reviewer 1

1. **Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation?** The paper explains how demographic and economic estimates are made for a subregion economy. As such, the paper does not fulfill the role of analysis that is suggested in the title. Rather the work is the estimation of stylized facts that one would need to do analysis. The interpretation is anecdotal. There are no hypotheses or theoretical frameworks guiding the paper. One is struck by the redundancy of the results and the conclusions, suggesting that a rewrite to tightening things up is called for.

The authors completely disagree that the "interpretation is anecdotal" and that "the paper does not fulfill the role of analysis". The objective of the Demographic & Economic Analysis of the Walker Basin (data acquisitioning, treatments, mapping, reporting, and interpretations) was clearly stated during the proposal, planning, development, and draft report stages. The context of the term "analysis" used in the title is the presentation of the results of this process, and, therefore, should not trigger a complete rewrite in order to meet academic definitions involving "hypotheses or theoretical frameworks".

2. **Is the paper clear, well organized and concise?** As noted above the paper drags with repetition. I would have thought with the use of GIS methods that some descriptive graphs might be useful in describing what the author(s) want to say. Rather than repetitively presenting facts in sentences, table would go a long way in making this work easier to read and understand. There are a number of style issues that would improve the text–every where one sees "in order to" the simple "to" works better. An editor would be of help here. As such, this are areas that needs addressing so as to help the reader. The text is a good draft from the writer(s) perspective, but there is awkwardness and extensive repetition for the reader.

The reviewer was provided with a preliminary draft that completely lacked graphs, tables, and maps. Since the preliminary draft was provided to the reviewer, graphs have been imbedded within the narrative and 20 pages of maps and tables have been added. A review of the "style issues" has been performed and adjustments made.

The development and discussion of subregion data to further the understanding of demographics and economics in Walker Basin and its communities was the stated goal throughout the project. Translating the resulting data into narratives was just as torturous,

we are sure, as the reviewer having to read the narrative, but the intent was not to entertain. Explanation of the GIS methods is a good topic, but not integral to the intended results and would have lengthened the already extensive paper.

3. Are the methods appropriate, current, and described in sufficient detail? The paper falls into the class of work that covers facts of use–what many call stylized facts. Obviously, the approach falls short of a full discussion on water, either the work needs to be formulated on a narrower focus on demographic and economic facts for undertaking an analysis of water issues, using the Walker Basin, as an example or linkage to water added, thereby adding to the focus and direction of the paper.

RESPONSE: The stated objective of Demographic & Economic Analysis of the Walker Basin was to benchmark the various demographic and economic indicators in the Basin and communities within the Basin without their correlation to water use. There are several tasks within the overall Walker Basin Report that addresses the issues of water in the Basin. Tying the demographic and economic activity to water use is an excellent proposal and will be discussed for future research if needed.

The reviewer completely misses the mark regarding the methods used to develop the analysis. One cannot simply obtain the information used in the analysis from websites or local governments. Because of the multiple sub-county and unincorporated areas in the Basin that span four counties and two states, the core information is extremely difficult to obtain and develop into meaningful data. The methods used and documented allowed the development of current demographic and economic information for communities in the Basin that have never been analyzed in such detail. The "stylized facts" (the narrative) are a result of difficult and complex processes to cull the information from various databases and package into reports.

4. If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct? The uses descriptive statistics; there are no models. As a result, the analysis stays at a fundamental level. I would not want to suggest that an elaborate modeling structure is called for, that is not where the paper is going. This focus depends on the framework of the editor and publishers.

5. Are all tables and figures necessary, clearly labeled, and readily interpretable? I suggest that tables are called for– it would be a help to the reader. Also, with tables and GIS graphs and figures, the author(s) should not repetitively keep the text, resulting in further redundancy.

The reviewer was provided with a preliminary draft that completely lacked graphs, tables, and maps. Since the preliminary draft was provided to the reviewer, graphs have been imbedded within the narrative and 20 pages of maps and tables have been added.

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper? No bibliography is attached, though references are made to data sources in the text.

7. Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted? I recommend condensing. The results and the conclusions are highly repetitive.

RESPONSE: We agree that the discussion of the various demographic and economic indicators is a boring read, but a necessary process to report on the complete set of attributes. We fail to see where we repeat the conclusions for a specific attribute. Moreover, this reviewer's request to condense conflicts with the requests from the second reviewer stated below.

8. Can the paper be published? With major revisions

<u>Response to Reviewer 2</u>

1. Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation? The paper provides a concise overview of current and historical demographic and economic trends for the "Walker Basin" region. It was highly useful to show these trends at both the regional level and at the county-local level. Useful comparisons to state-wide activity also provided additional insight into how these demographic and economic trends within the "Walker Basin" region compare to state-wide behavior.

For the most part, the conclusions that the authors arrive to are supported by the data presented. The interpretation is clear and concise, especially when compared to state-wide trends. However, state-wide comparison was not used in all sections. Additional comparison with state-wide trends may provide additional insight and support for the author's conclusions and interpretations. Additional comparison to national demographic and economic trends might also provide further support for the conclusions made by the authors.

The draft was reviewed for areas where comparisons to statewide trends can be added. In some cases statewide comparisons were added. We resist the request for comparisons to nationwide trends to keep the document concise and not overly worded, as suggested by the first reviewer.

2. Is the paper clear, well organized and concise? The authors have written a fairly clear, well organized and concise paper. Demographic and economic trends for the "Walker Basin" region are presented for the following: population; age, race and sex; occupation and education; income; housing units; housing values; firms, employment and payroll; taxable sales; crop yields and value; residential construction activity; and proposed commercial activity.

When the authors present data for the various categories listed above, quantitative and statistical analysis is presented in a way that is clearly understandable and directly related to estimating and illustrating current and historical demographic and economic trends for the "Walker Basin" region. When data is presented on sub-areas of the "Walker Basin" region, the authors use a consistent and concise approach which makes comprehension easy.

In-terms of organization of the paper itself, the "Conclusion" section should be moved to the beginning of the paper between the "Methods and Approach" section and the "Results" section. I would also recommend that the "Conclusion" section be renamed to something like "Summary of Findings" or just "Findings". This reorganization may help make the paper easier to understand as well as more "reader-friendly" – especially for policy makers that are not

interested in reading through the entire document before getting to the author's own conclusions and results.

   The "conclusion" section was moved above the "results" section and renamed to "summary of findings", as suggested.

3. Are the methods appropriate, current, and described in sufficient detail? Given that the authors are only presenting demographic and economic trend analysis, there is no need for overly-complicated statistical and/or econometric analysis. The level of quantitative and statistical analysis presented is sufficient to support the conclusions and interpretations made by the authors. The methods used are appropriate and are very common across similar studies and the authors have described these methods and approaches in sufficient enough detail.

   Thank you for supporting our position against the first reviewer's take on the methods used.

4. If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct? As mentioned previously in my response to Question 3, the authors are merely presenting demographic and economic current and historical trend analysis for the "Walker Basin" region. This level of analysis does not require complex model building found in higher-level statistical and/or econometric analysis. The approach used by the authors within the study is appropriate and correct for the type of analysis being used.

However, the authors could expand upon their demographic and economic trend analysis by presenting results of a location-quotient and/or input-output analysis to show potential growth industries across the "Walker Basin" region. Location-quotient and input-output analysis is common in this type of analysis and would be most useful in the "Firms & Employment" section located on Page 10. The use of location-quotient and input-output analysis would help strengthen the conclusions and interpretations already made by the authors. If the authors choose to add location-quotient and/or input-output analysis, state-wide and nation-wide comparisons would be useful.

Another useful point of comparison would be to compare the various trends presented throughout the paper to the largest population centers in Nevada, including the Reno-Sparks-Washoe County area and the Las Vegas metropolitan area. Although much of this comparison is already indirectly captured by presenting state-wide comparison trends, comparing current and historical demographic and economic trends in the "Walker Basin" region to similar data sets for Nevada's two largest urbanized centers would help make clearer the differences between rural and urban communities. On several occasions, the authors assert the conclusion that the rural nature of the "Walker Basin" region helps define and explain some of the trends. It would be useful to have an "urban comparison" in order to support the conclusion that the rural nature of the "Walker Basin" region helps to define current and historical demographic and economic trends.

   The location-quotients are an excellent suggestion that will be explored for future research. A input-output analysis was conducted within the "Economic & Fiscal Impacts and Economic Development Strategies" section of the Walker Basin Report.

5. Are all tables and figures necessary, clearly labeled, and readily interpretable? There are no tables or figures in the study. The authors have chosen to use a narrative to present all of their

data. This might be a potential draw back of the study as much of the data in the narrative presentation may be easier to understand if presented in table and/or figure form.

In the "Results" section of the paper, much of the data presented could have been presented in table form. I would recommend that the authors consider developing simple tables to more clearly present the study's quantitative findings. For example, the following table could be developed for the "Population" sub-section:

Population for "Walker Basin" Region

Area	Total Population 2007	Percentage of Total 2007	Persons per Square Mile
"Walker Basin" Region	18,999	100.0%	5
Mason Valley	8,583	45.0%	47
Mineral County (Hawthorne, Schurz, Walker River)	4,128	22.0%	
Ect.	Ect.	Ect.	Ect.

Although I have not finished this table, similar tables could have been developed for the entire "Results" section without any change to the narrative component. The narrative component is important in helping provide insight into various trends and in helping to provide explanations in the trends. However, similar tables to the one example I have presented above would provide the reader with a more "user-friendly" way to view the data and how various sub-region demographic and economic trends compare to region-wide, state-wide, and even national-wide trends. Sub-region vs. region vs. state vs. national trend comparisons are made easier through the use of data tables.

Bar charts and line charts would also be useful, especially in the "Residential Construction Activity" sub-section of the study to show year-to-year changes in construction activity. Similar sub-region vs. region vs. state vs. national trend comparisons in residential construction activity could also easily be communicated using bar charts, line charts, and tables. The narrative is useful in helping explain these trends but is less useful in helping to simply present the data itself.

I would also recommend that the authors include maps when appropriate. The authors refer (several times) to various mapping techniques – especially for crop yield production and value. The inclusion of maps provides helpful visual references to the reader and would add to the clarity of the narrative presentation already provided in the study.

The reviewer was provided with a preliminary draft that completely lacked graphs, tables, and maps. Since the preliminary draft was provided to the reviewer, graphs have been imbedded within the narrative and 20 pages of maps and tables have been added.

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper? The authors have provided significant and appropriate citations and references for all data collected and analyzed throughout the study. Superior citation and referencing was made in the "Methods & Approach" section. No changes are necessary.

7. Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted?The length of the report is appropriate. The authors have successfully been able to communicate a large amount of data and material in a concise manner without either understating or overstating the conclusions.

As already mentioned, I recommend that the "Conclusion" section be moved to the top of the report between the "Methods and Approach" section and the "Results" section. This should not alter the length in any way.

The addition of possible charts, tables, figures, and/or maps, would lengthen the report as it currently is but should not lengthen it too much.

The "conclusion" section was moved as suggested and a significant amount of graphs, tables, and maps were added.

8. Can the paper be published? The document, as it is currently, could be published as is without any revisions, changes, and/or alterations. Even without the suggested changes to the "Conclusion" section that I have suggested, or even without the addition of strategically placed charts, tables, figures, and/or maps, the document, as is, is perfectly acceptable and meets contemporary standards for high-quality scholarly and practitioner work.

GENERAL COMMENTS:

After reviewing the report, "Demographic & Economic Analysis of the Walker Basin", I congratulate the authors on a job well done. The authors have clearly used exhaustive means to detail the various demographic and economic characteristics of the "Walker Basin" region. I was particularly impressed with the ability of the authors to provide such detailed quantitative analysis on a wide array of various socio-demographic and economic characteristics and provide those characteristics and trends over a significant number of years.

The report on various demographic and economic trends for the "Walker Basin" region will undoubtedly serve as a critical first step in developing concise and consistent economic development policy for the entire region.

PROJECT J. WILD HORSE AND BURROW MARKETING STUDY PURSUANT TO H.R. 2419, P.L. 109-103, SECTION 208

BLM Wild Horse and Burro Policy: Auction Design and Horse Park Feasibility Study

<u>Response to Reviewer 1</u>

The following review does not provide an exhaustive list of comments. Somecomments overlap between sections. The following comments provided are fromboth a policy maker and researcher perspective. Given the seriousness of the flaws of the experimental design used to address the issue before the researchers, I find that this research is not publishable either as part of a comprehensive feasibility study or as an unrelated research article.

1. Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation? The body of the work between the introduction and conclusion does nothing to link the two.

> *The purpose of the paper was to present studies that potentially could improve wild horse adoption by the federal government, estimate characteristics of wild horses to enhance adoption, and develop an approach to introduce risk in estimating returns from horse adoptions. These three objectives successfully completed provide information that could enhance wild horse adoption that is currently unavailable.*

• Given the title of the paper, the three extensions of previous work and the conclusions, I am not sure what story the authors are trying to tell. There seems to be a problem statement embedded in the introduction related to the reduction in adoptions and [that] a "…research team…investigated several aspects of wild horse adoption, with a particular focus on how auctions have been used to distribute the animals.". This problem statement is again noted in the conclusions "Placing animals…raising revenue…" The main conclusion of the study states that "This study has investigated auctions which might increase adoptions of wild horses…while simultaneously increasing revenues from adoptions…

> *The primary objective of this paper is to present results of three investigations of wild horse adoptions which could enhance adoption rates. The paper also provides a range of potential revenues from a targeted auction of wild horses. This provides readers with not only a most likely revenue scenario which is really a 50% chance of revenues but revenues from a worst and best case scenario. The reader is given a range of possible revenues which provides the reader with a risk assessment of revenue generation from adoption of wild horses.*

• The main conclusion is not supported by the data in any fashion. No meaningful analysis of the actual auction data was conducted. Actual auction designs are neither defined nor controlled for in the experimental design. Therefore, the policy maker has not learned if what she is doing is flawed, and if so, what she can do about it.

> *This comment raises several points which we will address in order. First the reviewer indicates that the main conclusion is not supported by the data in any fashion. We view this statement as false. We have provided extensive evidence from our experimental auctions that*

suggests contrary to existing theory, the optimal way to auction multiple heterogeneous goods to risk adverse bidders is via sequential rather than right to choose auctions.

Next the reviewer correctly notes that no meaningful analysis of the actual auction data was conducted. While we would have preferred to incorporate the actual auction data into our analysis, the data provided by BLM is insufficient to carry out any meaningful analysis – at least with regards to the "on site" auctions. Importantly, the data provided by BLM for the "on site" data does not contain any information regarding the number of bidders that participated in the auctions. Lacking such information, we are unaware of any empirical approach that could be used to back out the underlying distribution of values for the auctioned horses. Although one could attempt to use a reduced form approach that examines the correlation between a particular auction format and revenues, we would not know what to make of such information in the absence of data on the number of bidders as both variables influence observed bids. Thus, even if one found a correlation between auction formats and revenues in the BLM data, it is impossible to determine whether this reflects differences in bidding behavior directly related to changes in the auction format or differences in the number of bids submitted in the corresponding auction – particularly if the number of bidders who participate is correlated with auction formats.

Finally, the reviewer claims that actual auction designs are neither defined nor controlled for in the experimental design. We could not disagree with this comment more and believe that this and many similar statements made throughout this review demonstrate a fundamental flaw in the reviewer's understanding of auction theory. It is true that we employ a sealed-bid, second price auction in our experiment and that this is only one of several formats used by the BLM to organize horse sales. Yet under very standard conditions, these various auction formats are outcome equivalent. In fact, all one needs to show equivalence of the sealed-bid, second price auction and an ascending English auction is that bidder's values are independent (IPV) draws from a common underlying distribution. Importantly, this IPV assumption is the foundation upon which the theory for right-to-choose auctions is built.

• Authors state "Since we are interested in learning about…RTC in a setting similar to that of the BLM auction…". However, the design of the experiments is in no way comparable to BLM auctions. Definitions of BLM auctions are not provided.

Again, we could not disagree with this reviewer more strongly. From the perspective of theory, there are a number of key features of an auction market that dictate whether RTC auctions should outperform equivalent good-by-good auctions. The first is that the auctioneer wants to sell heterogeneous goods. The second is that the goods are not perfect substitutes, i.e., we are not selling two of the exact same good. Third, it must be the case that buyers have a potential heterogeneous preference ordering over the goods – i.e., you and I may prefer different goods. And finally, although it is not necessary, much of the RTC literature focuses on cases where the underlying distribution of values for the different goods is similar. That is, we want goods that are of similar value.

Previous work by several of the PIs has shown that these latter three conditions hold for wild horses. Importantly, this work shows that individuals view different horses as substitutes but have a distinct ordering over these types and recognize that such values differ across the

population of potential horse buyers. Hence, we are confident that the BLM horse auctions satisfy the key features of the models upon which we build our experiment. Further, the goods that we selected to use in our auction also satisfy these properties. Hence we see a direct analog between our setting and BLM horse auctions.

• The apparent focus of the research is on three extensions to other peoples' work (i), (ii) and (iii) which has little to do with the 'mandate' of the project:

Unclear as to what the reviewer is referring. The experimental field auctions are novel and directly related to the substance of the project. The live auction data has not been previously analyzed. The revenue simulation builds on earlier work conducted in the department.

• (i) Preferences are evidenced by prices paid for adoption and are split out by horse characteristics and auction type. Horse characteristic groupings are not justified. In total, no discussion over seller preferences for auction type was addressed.

Live auction data: The characteristics were in the original dataset as well as discussion of aggregation and seller preferences.

• (ii) and (iii) Will be discussed later.

2) Is the paper clear, well organized and concise?

• I was forced to dig through the experiment instructions to complete my understanding of the mechanism design and aspects of the experimental auctions.

• I was forced to call BLM officials for a description of their auctions.

This research was to investigate alternative auctioning procedures that could be adopted by BLM. This was not a study of current BLM auction practices.

3) Are the methods appropriate, current, and described in sufficient detail?

• I would agree that using experimental markets to better understand heterogeneous preferences impacts on revenue generation; given appropriate auction mechanism designs is an appropriate method.

• The mechanism design of the experiment does not follow the sole auction theory paper cited, Burguet (2007). Most notably, Burguet's bidding agents are unit demand constrained, the items for sale are heterogeneous and the auction mechanism is an oral ascending second-price auction and information is private. In the experimental design, bidders are not constrained in purchases, the items for sale are not even in the same product market, and the auction mechanism is a sealed-bid second-price auction. Revenue equivalence of the oral ascending and the authors sealed bid auction are questionable given the discrepancies between the two auction mechanisms, products offered, etc. As such, it was not surprising that the results were 'contrary' to the theory.

We again could not disagree with the reviewer more. In both Burguet's (2007) paper and in earlier working paper versions of this article, he discusses how the results of his model readily generalize to situations where bidders are not unit demand constrained. Moreover, although Burguet focuses much of his discussion on the sale of distinct condos with a given

complex, this does not mean that his theory only holds if the goods sold are from the same product market. The key in Burguet's, or any similar model, is that the goods being sold are not perfect substitutes. That is, each bidder must value one good more than the others and know that there is some probability with which other bidders also most prefer (highly value) this same good. This is clearly the case in our experiment and is easily borne out if one examines the data from our sequential auctions (for which the equilibrium bidding strategy is to bid your value). For all three goods in these auctions, the distribution of bids from those agents who indicate that the item was their most preferred stochastically dominates that generated from agents who indicate that the respective item was their second most preferred which stochastically dominates that generated from agents who indicate that the respective item was their least preferred.

• To address preferences and auction mechanism design, I would begin reviewing the sequential auction literature. In this line of literature it has been found that the order of heterogeneous items offered for sale can significantly impact seller revenue. Given the three *distinct* products auctioned, the order the items were sold must be controlled for in the experiment. If the auction design gets back on track of sequential sales of one heterogeneous product (horses), this line of literature may provide some enlightening experimental work.

The order the goods were sold in the sequential treatments was randomized so this issue should not have an impact on a comparison between the two auction types.

• Special attention needs to be made about the information provided sellers in horse auctions and maintained in the experiment. For the exception of horse owners who desire a 'wild horse' in their back yard which is a private value auction, all other horse owners face a common value auction. They must purchase the item before they know its true value (and costs). Therefore, I would focus on the sequential common value auction literature.

Bidders have preferences for types that are observable in advance.

• I noticed some confusion by the authors as far as the difference between common values and correlated values. When 'uninformed' bidders rely on signals from the bidding of 'informed' bidders in order to determine their own valuation, I would look to the literature which focuses on 'correlated values'. In common value auctions, bidders know the distribution of signals. As such, bidders incorporate drop out bid information of rivals to form a conservative reservation bid designed to address the adverse selection of holding the highest private signal. Bydoing so they bidder mitigates the 'winner's curse'.

We agree that correlated values are potentially important in this setting.

4) If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct?

• The authors note that they are conducting a 2X2 factorial experiment (table 5). However the statistical models reported do not maintain the design of the experiment (table 8). For instance, the random effects model controls for auction type, but not information.

The second draft is revised to include the informational sessions which were underway at the time of the first draft. We do not find a treatment effect and therefore pool the data. The substance of the findings is unaffected.

• Sequential auctions in general and the experimental auctions also have a time series component, apparently not addressed in the analysis.

The time-series aspect is collinear with pref in the RTC auction and is therefore addressed in the regression. We find no evidence of an effect over time for the performance of SEQ treatments. As noted previously we randomize the good sequence in the SEQ treatment.

• The authors point out that there may be learning across auction sessions, and is not explicitly controlled for in the statistical procedures. The lagged revenue variable is not justifiably incorporated into all bidders reaction functions as only the winner realizes the surplus. Also, given that three *distinct* products are sold, I am not sure how anyone could learn anything from the previous bid, unless it was rival budget constraints or preferences of rivals for unrelated goods.

We find that the lagged revenue variable has an impact on subsequent bids, despite the reviewer's uncertainty of the cause. We also find this behavior interesting and deserving of further study.

• Authors' definition of measure of risk proxy, IQ proxy and supporting literature for said definitions are absent.

Added to revised version

• (iii) When using someone else's output table, a detailed description of the model and output is still warranted. For instance, are the estimated coefficients latent variable impacts or are they actually the marginal impacts on probabilities? Significance of each variable? The assumption that the horse characteristics in the model are independent, particularly size and age, gentle and quiet is not justifiable. Also, the issue of endogeneity of expense was not addressed, which I assume came from auction data where expense is the dependent variable. However, expense was not defined, so I have nothing to go on.

The model description for the table is in the text and the appendix. Expenses are also defined in the text.

5) Are all tables and figures necessary, clearly labeled, and readily interpretable?

• Table 2 would benefit by including the frequency of each category. Also, it would be nice to have some justification for the delineation between categories. Also, I am curious to know what the 'other' category is related to the sex and type of animals.

These points are addressed in the text as with the regression results.

• Given the comments I had regarding actual auction design, Table 3 would benefit from breaking out the trained and untrained categories to auction type.

Table 3 comments are addressed in the paper.

• Some of the interpretation problems the authors found with 'gelding' is that this category is not broken out by training, age and or burro vs. horse.

References have been added as requested and needed in the text. This response belongs below…

6) Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper?

• Literature review is basically non-existent.

References have been added as needed in the text.

• There are likely no direct theoretical references as the experimental design is such a hybrid.

7) Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted?

• Justifying experimental procedures to addresses policy issues is not supported by the fact that Smith won a Nobel. The other justifications are appropriate.

The reference to Smith does not state what the reviewer claims.

• (iii) I am not convinced that the projected revenue section adds anything to the paper.

We disagree with the reviewer. Policy makers who have interest in a possible wild horse and burro national interpretative center would be interested in possible revenues. However a most likely revenues scenario is a 50% solution. It is only under average conditions. What would be of interest to policy makers is the potential range in revenues so they can have an idea of potential revenues under the worst and best conditions. Knowing the range on revenues provides risk averse decision maker information to make policy decisions.

8) Can the paper be published with:

As a policy maker, this research tells me nothing and runs the risk of policy makers not believing in the value of experimental economics. As a researcher, the methods are flawed, hypotheses unclear and supported evidence significantly lacking. As such, I conclude that this article is *too flawed to be published even with major revisions.* The authors need to clearly identify the problem, stay on track and not run experiments for experimentation's sake. In general, the paper seems to be a status report rather than a final product as on more than one occasion 'more work [experiments] is needed' without explanation about what that work [experiments] would entail. From what I learned with a few phone conversations, there are numerous auction formats contained in the field auction data already collected. At no time are the two 'generalized' actual auction types described in the paper, nor how buyer preferences play a role in the observed price differentials between the actual formats. For instance, the Federal Government uses several auction mechanisms, 'onsite' auctions are English, sealed-bid and silent while 'internet' are strictly a silent auction. Finally, I learned that many of the horses are simply adopted by paying the $125 adoption fee, which is the seller's reservation price with no competitive bid taken. This makes me wonder about the 'Live Auction Data' presented.

As noted in previous replies, we believe the reviewer's discussion of the experiments reflect important misunderstandings.

The author's should not overlook the 'gold' mine of natural experiments over various auction designs within their natural data. I would suggest analyzing the natural data first. Issues such as adoptees being capacity constrained by virtue of qualifications for adoption and no clear title upon purchase may undermine the value of any auction design. After fully understanding the natural data, I would then design alternative auction mechanisms to test if there is any revenue improvement. The issue of adoption per se is a marketing issue. For instance, to increase adoption rates, the BLM would benefit from targeting a consumer market. I suspect most consumers want to ride and 'bond' with their horse. Your data already suggest that consumers are willing to pay more for broke and gentle horses, which are apparently in short supply. A feasibility study of training horses may be in order, if it hasn't already.

As noted in previous replies, we believe the reviewer's discussion of the experiments reflect important misunderstandings.Responses to Reviewer 1

Response to Reviewer 2

General comments

Page 4. Without knowing anything about the horse auction market, the bids make perfect sense to me. I would expect the internet bids to be slightly smaller, since they do not have the ability to see the horses they are bidding on up-close (an opportunity I assume live bidders have). Further, the higher variance for live bids makes sense, since presumably the bidders are skillful, and will place much higher bids for "better" horses, and lower bids for "inferior" horses. (Quotes used since I have no idea what makes for a good horse other than what I read in your paper.)

This is a helpful discussion.

Page 10: Font size changed.

All font size in the paper is of the same size.

Page 10: I don't think Kahneman won for being an experimentalist. His contributions were to issues related to experiments, but he was awarded the prize more for his work in economics and psychology.

Reference to Kahneman has been modified.

Page 12: Were bids placed for all 3 items simultaneously in the SEQ auction?

Auction design questions/issues:

How could you tell which was the preferred item in the sequential auction? Was this just what people told you? Or, were you going by the highest bids? If you were going by the highest bids – did you vary the order that you auctioned off the three products? That could make a difference – as one might expect slightly lower bids.

The order the goods were sold in the sequential treatment was randomized across sessions. Preferences were elicited in survey conducted after the auction was completed.

What does the total bid represent in the sequential auction (table 7)? Is that the mean across all three products? Is that the sum of the bids for the 3 products?

These are the total revenues, averaged over each auction session.

Is the sum of the bids for the three products theoretically equivalent to bidding on each product individually? I would assume it is not. For instance, if I thought the IPOD was worth $80, and the other two items were worth $20, if the IPOD was available during all three rounds of bidding, the sum of my bids would be $240. However, in the sequential, it would only be $120.

Conditional on the good remaining available, bids should not diminish over the phases.

Can you list the sample size in your tables? (E.g., for table six, just put (N=?) after "preferences")

The counts in table 6 do represent the sample size. We have modified to make more clear.

Conclusion:

This is pretty skimpy. Can you discuss your findings and/or make policy recommendation? Saying you investigated auctions.

The conclusion section was expanded as desired.

In addition to any general comments please address the following questions.

1. Does the paper tell a cohesive story? Are the conclusions supported by the data and their interpretation? The story is not too cohesive. It has some interesting results, but I really have no idea what is the better method the end. I think the authors should tell a better story (is one of these auctions preferable or why?).

The paper conclusions were expanded and the text was changed to make parts of the paper more cohesive.

2. Is the paper clear, well organized and concise? As far as being clear, I think it would help to have a "in this paper, we do XYZ" sentence. Through the introduction, there seems to be about a sentence and a half on what you are doing in the paper (investigating aspects of horse adoption…). More details might help the reader follow your paper more clearly.

In the beginning of the paper, the text was changed to provide an overview of the objectives of the paper. This should add cohesion to the paper.

3. Are the methods appropriate, current, and described in sufficient detail? I had some confusion over the auction methods and results that I noted in my general comments.

Responded to the general comments

4. If statistics or models are used, are model assumptions, inputs, the statistical design, and analyses appropriate and correct? They seem appropriate.

5. Are all tables and figures necessary, clearly labeled, and readily interpretable? Yes

6. Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this paper? Yes

7. Is the length appropriate? Should any parts of the paper be expanded, condensed, combined, or deleted? Length is appropriate.

8. Can the paper be published? With major revisions (to conclusion, introduction, and auction design)