

## **APPENDIX 2: Responses to Bureau of Reclamation Reviews**

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

Project B, “Alternative Agriculture and Vegetation Management in the Walker Basin” and Project C, “Plant, Soil and Water Interactions” presented preliminary data in their respective reports, which were therefore not submitted for peer review.

There were no additional reviewer comments or suggestions for 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.”

## **Project A: Contemporary Limnology of Walker Lake, Nevada**

*Pg. 4 (PDF Page 16). A detailed baseline dataset cannot be derived from a 1-2 yr study. The hydrology of temperate terminal lakes is highly variable and both years of this study were fairly low runoff. The long-term (30 yr) dataset from Mono Lake shows just how variable the productivity of a terminal lake may be. Furthermore, defining a specific condition as baseline has important policy and legal implications (e.g. Why not consider Cooper and Koch's work of the 1970s baseline conditions?). Long-term monitoring of a suite of limnological parameters at Walker Lake is an important part of restoration efforts and studies in the 70s, 90s, and current NDOW monitoring are integral to this.*

We struck the term “baseline” from our text. However, it is worth mentioning here that the database included historic data from all other sources that we were able to collect (therefore not just representing the 1-2 DRI/UNR study, as suggested by reviewer).

*Pg. 4 (PDF Page 16). Caption should include time of year. Also elevation is better shown as a continuous line plot.*

Lake levels in Figure 1 represent the annual mean of monthly measurements. This information has been added to the caption and the chart has been changed to a continuous line plot, as recommended.

*Pg. 5 (PDF Page 17). It is not true that prior to anthropogenic desiccation, TDS was near the upper limits for LCT and tui chub.*

Revised sentence to remove reference to upper limits of acceptable salinity and pH for freshwater fish such as trout and chubs, as recommended by reviewer.

*Pg. 5 (PDF Page 17). Although Beutel assumes the hypolimnion "historically" did not go anoxic, he provides no evidence of this except by comparison to Pyramid. Many productive temperate lakes, saline and fresh, experience hypolimnetic anoxia. It is clear from Cooper's 70s studies that Walker was experiencing hypolimnetic anoxia nearly 40 years ago and it is not known how much earlier and at what lake volume this began to occur. The "health of the fisheries" was much higher even during Cooper's study period with anoxic hypolimnions. The work "historically" should be carefully quantified throughout these reports to prevent misleading the reader.*

Revised text to indicate that Beutel et al. have suggested from their studies that historically the lake did not become anaerobic.

*Pg. 5 (PDF Page 17). "stronger summer stratification" needs a reference. Also, "increased solute load" needs a reference. Is there data that shows "solute load", which usually refers to inputs from streams, has increased.*

Removed text on stronger summer stratification etc.

*Pg. 6 (PDF Page 18). There are a great many basic research questions that could be pursued at Walker Lake and we can assume that Walker Lake harbors unique and interesting microbial communities. However, the basic microbial processes are well-known (even for salt lakes) and the authors have not provided compelling arguments as to how basic research on microbial*

*processes in Walker Lake will provide useful management information or even monitoring data to assess the "health" of the lake.*

This comment on the microbial work is the usual argument about basic research: how can it contribute to developing useful management information? Since the lower trophic and microbial/microalgal community structure of Walker Lake was previously uncharacterized, some preliminary work was needed. Subsequently, as developed in the updated narrative of this chapter, there are substantial reasons to believe that microbial processes are relevant to understanding lake ecological functions and predicting change, which is important for developing good management strategies.

*Pg. 8 (PDF Page 20). These six stations are a subset of our 10 stations which are being monitored monthly. It duplicates a portion of ongoing work that was already being conducted by NDEP, NDOW, and WLFIT. Note they only sampled twice in 2008. It is difficult to see how their program provided higher spatial and temporal resolution.*

Added suggested text to indicate that the five DRI/UNR monitoring sites were a subset of those maintained by NDEP, NDOW and WLFIT.

*Pg. 11 (PDF Page 23). As there are so many authors cited here, there are many potential method or variations of methods being employed. I would like the methods to be clearly specified.*

Methods were cited in the text, and authors can be contacted for further information.

*Pg. 13 (PDF Page 25). A compelling rationale for this research and its relevance to management and policy has not been established. None of the results presented here change this assessment and incorporation of this work into long-term monitoring would seem highly inappropriate based on the needs of the restoration efforts. This is not to say that it isn't interesting 'basic' science.*

There seems to be principled opposition to the microbiological study done as part of this report. We disagree. This was essential baseline research into the role that microorganisms play in controlling the lake's redox and nutrient chemistry. Evidence was found for multiple effects, several of which could have direct impacts on fish (e.g. the detection of an anaerobic process for sulfide removal). As the lake level continues to decline, our window for developing an understanding of how major biogeochemical cycles currently operate is slipping away. It would be regrettable if we had no information concerning how the major nutrient and biogeochemical cycles of this lake functioned prior to further change or the loss of its major fisheries. The study was not funded to develop management strategies based on these findings, rather simply to determine whether microorganisms could be relevant to biogeochemical processes in Walker L (and the answer was yes).

*Pg. 18 (PDF Page 30). While no long-term met data exist at the lake, USGS did have a met station in the lake during a year-long evaporation study. As the calibration of the model in*

*several cases was done with only a year's data it would seem highly appropriate to use the USGS data. At least a comparison should be made between their data and the data collected at Hawthorne.*

We completely agree that such a comparison would be very valuable. We indeed requested and received this data from the USGS during model development. Because the met data did not overlap with the present limnological study, and because the data was only available for one year (with several gaps due to instrument malfunctions) we decided not to use the data in the original model setup. However, returning to this data to further evaluate model performance would be an excellent next step if additional resources become available.

We would like to make it abundantly clear to the reviewer that resources allocated to model development were a fraction of the overall project and in our opinion were at about 10% of the level that necessary to thoroughly test, validate, and implement the model for decision making. The text on which the reviewer made is comments was unfortunately slightly outdated, and we have made efforts throughout to clarify that this is a preliminary effort and we consider it to be a "good start" at this point in its development.

*Pg. 18 (PDF Page 30). I don't think doubling the shear coefficient is a minor adjustment. DYRESM was designed to have fixed parameters. I do not know if doubling this parameter (see below) is within the range that the designer's intended. This should be discussed further*

I struck the word "minor" from the sentence, "By making minor adjustments...."

*Pg. 18 (PDF Page 30). "parameters" usually mean the model coefficients. "State variables" or just "variables" refers to the modeled and observed data against which the model is calibrated.*

As suggested, changed word from "parameters" to "variables."

*Pg. 19 (PDF Page 31). As the main "health" criteria of the lake should be based on the LCT population it is not clear that the model has the capability to perform "impact assessments". There is no validation for any of the phytoplankton, zooplankton, and fish components of CAEDYM.*

Reviewer correctly indicates there is no validation for the phytoplankton, zooplankton or fish components of CAEDYM. Funding was sufficient to develop the hydrodynamic DYRESM portion more fully than the ecological CAEDYM portion of the coupled DYRESM-CAEDYM model. [Thus our recommendation for continued model development during Phase 2 of the Walker Basin Project.]

*Pg. 22 (PDF Page 34). As this is strong counter-evidence to the hypothesized increased internal loading and decreasing "health" associated with "successional processes", it is remarkable that the authors do not comment further.*

It may be "remarkable" that we did not comment on the significance of increased clarity seen in the long-term data, but we did not feel there was enough information available to speculate as to the potential mechanisms. We have revised Figure 5 to better show this relationship and added a regression line for

the trend. We also indicate that this trend could just be the result of a greater frequency of measurements in recent years.

*Pg. 24 (PDF Page 36). The pairs of TDS and EC shown here display quite high errors for measurements taken in the lab. Higher TDS -- lower EC, not possible. It is not clear how TDS was measured.*

I don't understand reviewer comment here (we don't have any TDS values greater than the EC values), with TDS determined at the EPA and State certified DRI Water Analysis Laboratory (EPA 160.1, SM 2540C).

*Pg. 34 (PDF Page 46). Nothing in this section provides compelling support that it could provide useful information to management or restoration monitoring. I say this even though I find this work scientifically interesting and have myself been part of a large team of microbiologists studying similar processes and communities in Mono Lake. However, there we were funded by NSF to conduct basic research into microbial processes.*

Again, the reviewer appears to believe that if information does not have direct management implications, it should not be included. We disagree, and feel that the microbiology results presented from Walker L. are relevant on a fundamental basis, and may prove to be relevant to management in the future.

*Pg. 40 (PDF Page 52). A number of water budgets have been done for Walker Lake based on partially gaged runoff, precipitation, and lake elevation changes. The DYRESM run employed the 11,000 ac-ft groundwater estimated by Thomas (1995). At what depth was this assumed to be input to the lake? The surface elevation agreement between observed and predicted during 2007 implies that DYRESM is accurately modeling evaporation. What was the calculated evaporation during this year? How does that compare to USGS's estimates and the prior estimates used in the previous water budgets? A much better validation would be to simulate multiple years. It would be a simple matter to simulate 10 or 20 years for which the same met data and river discharge data are available. This is particularly important if you are going to make predictions over 5-yr periods.*

Because no specific data were available on depth of river input to the lake, we distributed this input along the vertical profile.

We agree that a long-term comparison of 5 years or more would be very valuable and should be conducted in the future. The biggest limitation here is the spotty meteorological data in the region. We considered using a weather generator or data from the North American Regional Reanalysis to perform such a simulation, but resources did not allow that in the first stage of the study. We did rely heavily on the USGS ET report as a "reality check" for the model results, but we did not perform any type of direct comparison because their study was limited to one year, and it was not a year we have simulated, yet. Our ET rates varied from about 80 mm in January to about 200 mm in July.

*Pg. 42 (PDF Page 54). In general, I do not believe the model has been adequately validated to place any confidence in its use (see comments in other chapter). Much more effort should have been devoted to validation. For instance, can the model capture any of the observed variation in*

*the 2003-2007 dissolved oxygen profiles. There was significant variation over this period. There should be some objective criteria developed to examine model performance. One of the most basic is that it explains more of the seasonal and inter-year variation than the simple seasonal multi-year means of dissolved oxygen concentrations. 2003-2007 provide a good data set, but the 90s and 70s would also provide additional validation.*

Sure, as mentioned above, we completely agree that the model requires additional validation before it can be used with confidence to inform management. Again, this was a very modestly funded task and we consider the model to be a good start on a tool that can inform management.

*Pg. 42 (PDF Page 54). It appears that under the model, the high-flow condition results in an increase in the hypolimnion which would be worse for the Lake. This concept may benefit from additional discussion.*

The reviewer suggests that high-flow condition in the model shows an increase in the hypolimnion, which would be detrimental to the lake. This is not apparent to us. The elevation of the reduced temperature region increases slightly under the high flow condition, but its thickness as a proportion of the total lake depth decreases slightly. The region of depressed DO concentrations decreases. These trends need further investigation, first to assess the model's performance in representing mixing and stratification mechanisms and then second to evaluate the implications for the lake.

*Pg. 47 (PDF Page 59). The use of pH, nutrients, carbon, chla, and zooplankton for model calibration is not well-documented, if at all in this or other chapters.*

As to the first comment on this page, these lake parameters were not used for model calibration but rather for model parameterization. For example, pH and nutrient concentrations are specified as initial conditions and boundary conditions, as discussed in this chapter and the model addendum. Chl-a and zooplankton data were used to set certain model characteristics, such as which predefined model groups to use (as described in the model addendum), and the initial conditions for these groups. These parameters should be used to further calibrate and validate the model in the future. However, this was not possible during this stage of model development for two reasons. First, as mentioned previously, limited resources did not allow us to develop the ecological component of the model to an advanced stage. Second, due to the compressed time scale of this project, most of the ecological data were not provided to us until the very tail-end of the project period.

*Pg. 47 (PDF Page 59). This is true of all lakes and is well-known.*

The second comment simply notes that some of our general observations on microbially-driven processes is true of lakes in general. We agree, which is why we recommend these should be further studied at Walker, since there are important differences in the rates and microbial taxa involved.

*Pg. 47 (PDF Page 59). What is proposed here is a great deal of work to develop a complex model of detailed microbial processes of carbon and nitrogen cycling. This does not seem appropriate to the management issues at hand.*

As to the third comment on this page, our recommendation is that greater effort is needed to represent phytoplankton blooms, since they are both important and transitory, so may not be captured by the regular monthly sampling program. We still feel that this is a reasonable observation.

*Pg. 48 (PDF Page 60). The lake turns over in autumn and sampling during any of late autumn through late winter provide suitable information on lake-wide conditions. Current NDOW monitoring is scheduled monthly from Jan-November. This is more than adequate.*

We have revised the paragraph on sampling during the period of maximum mixing to represent our intended message that sampling does not need to occur each month during this period, but at a regularly scheduled time each year when the mixing is likely to be deepest (perhaps January).

*Pg. 48 (PDF Page 60). "Despite" makes no sense here. Mono Lake at 98,000 mg/l salinity is still holomictic in winter and stratifies in summer.*

No change to first sentence of conclusion is needed, as the reviewer simply notes that Mono Lake remains holomictic at a higher salinity than Walker Lake.

*Pg. 50 (PDF Page 62). How understanding microbial processes contribute to developing sound management strategies is unclear.*

Added the following text to make management implications more clear (referencing second comment on this page).

“Since microbial and microalgal biogeochemistry controls many of the factors that define ecosystem function and potential, ranging from availability of limiting nutrients (N), to toxin production (H<sub>2</sub>S, NH<sub>3</sub>) and trophic status, it is evident that understanding these processes could be essential for developing sound management strategies and predicting effects as the lake conditions change.”

*Pg. 50 (PDF Page 62). There is NO evidence provided that the Walker Lake ecological model (the coupled DYRESM/CAEDYM) can be used to optimize water deliveries in terms of lake benefits. The most likely factors involved in optimizing water deliveries will deal with LCT in-stream movements and possibly tui chub recruitment. Beyond that, there is no evidence that anything other than the total volume of water deliveries is important.*

Added the phrase, “... with continued development ....” to improve clarity.

We don't feel the need here to argue that improved understanding of lake processes and the development of tools that allow us to predict the response of such processes to changed boundary conditions (including volume, timing and quality of water inflow) will lead to improved management decisions. However, there's no doubt this tool needs further refinement before it can fulfill that role. The model was selected because of its flexibility and ability to incorporate a wide range of lake characteristics -- including fish. It also could provide projected

environmental conditions to drive more complicated food web or ecological community models. Once again, we see this tool as a good start and it could be used to inform water delivery and acquisition decisions in the future.

*Pg 64 (PDF Page 76). I think it a waste of time to include Walker-related documents in an ACCESS relationship database. There are any number of much more efficient and widely used bibliographic software programs that are much better suited for this purpose. Very few people will use this feature. Resources would be much better utilized to develop a Walker Lake bibliography in EndNote, Reference Manager, Zotero or another software package.*

Reviewer suggests that EndNote or similar bibliographic software would be better suited for compiling Walker related documents than the Walker Database (developed in MS Access). This may be true, but we were working with Access and added the bibliography as an extra; we were not funded to develop a bibliographic database.

Walker Lake and River Database Chapter. Revised

## **Project A: Ecological Model for Walker Lake, Nevada**

A common theme throughout all reviews of the Walker Lake model has been that the model has several shortcomings and was not thoroughly tested. We agree. The reality of the situation is that this task was originally cut from the project, and then added back in on a very meager budget. Working nights and weekends, we produced a model that we consider to be a very good start and it seems to be capturing the lakes physical processes fairly well. A more sophisticated model would capture the processes better. Additional resources would have allowed us to test it more thoroughly. Also, standard model performance metrics such as RMS, Nash-Sutcliffe, etc. do not lend themselves well to applications with both temporal and spatial variability. For example, there's not a single gaging station (or better yet, multiple stations) for which we can compare a modeled and observed parameter throughout the simulation – except for lake depth. Instead, sporadic observations are available to which we can compare results and use professional judgment to ascertain model performance. Sharing every instance of these comparisons is not feasible in this situation. Further, due to the compressed time-scale of this project, much of the observed data was not available to us until very late in the project. However, we agree that a more thorough and systematic model evaluation should be completed before the model should be used to evaluate future water management scenarios with any certainty. We do concede, however, that our original document painted an overly rosy perspective of the model. We have added extensive caveats to the text in subsequent drafts. However, the number and versions of drafts have caused some confusion as they have been circulated to various reviewers.

*Page 74: I would argue that none of these objectives were met. Much more realistic objectives should have been set.*

Objectives were updated based on re-scoped and re-budgeted model.

*Page 74: How was the model customized? A clear description should be provided. A complete list of parameter noting which ones were changed and where they were obtained should be included. This is normal practice in deploying a model.*

Text was added to describe parameterization.

*Page 75: There is little confidence that the model can forecast ecological responses to specific Walker River flow scenarios. This has clearly not been established.*

Emphasis to preliminary nature of the model was added throughout.

*Page 78: WLM is not previously defined except as a station ID in a table.*

Defined.

*Page 78: previous?*

Former?

*Page 82: Parameters describing the physical mixing processes in DYRESM were designed to calibration free. Doubling the shear production efficiency does not seem to be a minor modification. This may illustrate an inability of DYRESM to model internal mixing processes, the inadequacy of the 1-D approximation for Walker Lake, inadequate wind field data, or something else. A full explanation of this parameter and how it has been varied in other studies using DYRESM is warranted. CWR should be consulted on this modification.*

Accounting for shear production efficiency in a single coefficient is a limitation of a one-dimensional model. It is normal practice to “tweak” these coefficients in order to achieve a pattern of vertical mixing that is consistent with observed data. Adjustment of a default parameter of this nature by a factor of two is not unusual. Yeates and Imberger (2003) describe the complexities of internal mixing and the limitations of parameterizing these processes in order to reach feasible solutions. The values used in other DYRESM applications are not typically stated.

*Page 82: Displaying a single temperature profile is completely inadequate to convincingly demonstrate the adequacy of the physical mixing processes in DYRESM. An objective measure of performance must be developed and analyzed for both seasonal and varying conditions observed in different years. Furthermore, there is great danger in relying on this simple "chi-by-eye" procedure and assuming this validates the model. Press et al, recognized experts in the field, say it best:*

*"The important message we want to deliver is that fitting of parameters is not the end-all of parameter estimation. To be genuinely useful, a fitting procedure should provide (i) parameters, (ii) error estimates on the parameters, and (iii) a statistical measure of goodness-of-fit. When the third item suggests that the model is an unlikely match to the data, then items (i) and (ii) are probably worthless. Unfortunately, many practitioners of parameter estimation never proceed beyond item (i). They deem a fit acceptable if a graph of data and model "looks good." This approach is known as chi-by-eye. Luckily, its practitioners get what they deserve."*

Emphasis to preliminary nature of the model was added throughout. Interesting the reviewer did not suggest an appropriate goodness-of-fit measure. Of course,

traditional measures such as RMS and Nash-Sutcliffe are difficult to apply in this context and the budget did not allow for a systematic calibration effort. Further, observing how well the model is capturing phenomena such as stratification and de-stratification with sporadic observed data is nearly impossible with standard metrics. However, a fuller investigation into model performance should be conducted if future resources allow.

*Page 82: This is not a measure of the ability to describe physical processes in the lake. It is a measure of its calculation of evaporation. The model's estimate of evaporation for different years and conditions should be explicitly presented and compared to other estimates.*

Funny, we thought evaporation was a physical process.

*Page 83: Same comment as for temperature applies. Also note that the agreement is rather poor and that a different date is shown than that for temperature. It is absolutely necessary to show the entire seasonal predictions and observed data from multiple years to have any confidence in the model. Until that is done, no prediction or scenario runs are even warranted.*

Showing entire seasonal temperature profiles is not feasible in a summary report. As stated in the report, these were “typical” results.

*Page 86: Objective measures of performance are needed.*

Such as? Again, a detailed model analysis was not feasible with the given resources and objective measures, or metrics, are not easily defined.

*Page 87: Please explain the mechanism of this counter-intuitive result. Heat exchange due to differences in air temperature occur in the very surface layers. Increased heating of the uppermost surface layer would normally increase vertical thermal stratification and decreased the depth of the mixed layer. The opposite pertains to cooler air temperatures.*

Not necessarily. The depth of the mixed layer obviously depends on a host of complex interacting processes. This is, however, an interesting observation that deserves a closer look. Unfortunately, teasing out how the model is arriving at specific surface layer depths is not straightforward and cannot be conducted at this point in the project.

*Page 92: Here the authors note that the model is entirely insensitive to order of magnitude changes in nutrient content of the lake-bed sediments. Yet, in an earlier section they contend that it is important to determine the nutrient characteristics of the sediments more accurately. Like many of their recommendations this seems based more on personal preferences than objective reasoning. However, I would add that I do not think the sensitivity analysis included an appropriate metric of performance and I also think multi-year simulations would be more appropriate for analyzing the sensitivity of sediment characteristics.*

Good point on the multi-year simulations. However, in spite of the model’s “apparent” insensitivity to a range of boundary conditions, we currently have no information on lake sediments. As described in the report, the boundary was thus estimated based on sediments from other systems. This complete lack of data would make this a data gap by any metric that you choose.

*Page 92: As above, the authors note that the model predictions are insensitive to large variations (order of magnitude) in nutrient loads from river inflows, yet they state it is important to greatly increase monitoring of river nutrient loads.*

As the reviewer correctly notes in the comment above, extended simulations would likely have shown a response to changed nutrient loads and this should be looked at in the future. As with lake sediments, this data is almost non-existent and thus should be augmented for more realistic estimates of nutrient loads. However, the extent of such monitoring should be weighed with other project needs.

*Page 95: Without an objective measure or presentation of this data, the reader is left in the dark and has to accept all this on faith. A table is certainly appropriate and a list of all parameters used is imperative! Note the vertical mixing coefficient was doubled to get an approximate temperature fit to the Aug 28, 2007 profile.*

The only parameters that were changed from default conditions were specifically noted in the text. The model depends on dozens of parameters and a table would be redundant. The vertical mixing coefficient was changed as noted throughout all simulations, not just for the one profile that was included in the report (see notes above).

*Page 95: I do not believe the model has been adequately analyzed and validated to have any confidence in scenario analysis. Also, note that the most important "health" metrics of the lake (LCT, tui chub, nuisance algal blooms) are not even considered in the validation. What are the phytoplankton, zooplankton and fish predictions of the model? How do these compare to observed values? Does the model have any utility in assessing these indicators of lake health?*

The model was selected because of its flexibility and ability to incorporate a wide range of lake characteristics -- including fish. It also could provide projected environmental conditions to drive more complicated food web or ecological community models. Including such complexity was way beyond the resources available for this task. The model could be further developed to inform such questions. However, forecasting algal blooms and fish populations is a very complex task with a high degree of uncertainty.

## **Project A: Walker Lake: Hypolimnetic Oxygen Deficit Assessment and Associated Limnological Factors**

We thank the reviewers for their comments and input related to the Walker Lake Hypolimnetic Oxygen Deficit Assessment and Associated Limnological Factors section of the Walker Basin Task 6 Final Report. In the responses that follow, we address each reviewer's specific comments related to this section of the report and detail responses to the comments that require changes to this section of the document. However, other groups need to provide information for other sections and general comments, and we presume that these concerns are being addressed by other authors.

*Page 108; Note NDEP has been collecting water chemistry on a much more regular basis. Also much of this work unnecessarily duplicated ongoing monitoring by NDOW which began in 2002 and has continued through present, and a more intensive design implemented by the WLFIT that began in 2007.*

Noted. Some duplication is beneficial for the added value of projects. Full disclosure of available data and open channels of communication regarding plans allow for more focused studies.

*Page 116; It is interesting that the areal HOD has decreased over this period. This implies that the productivity is declining. This deserves special comment.*

The total oxygen content of the lake's hypolimnion has declined. Therefore, if the total oxygen content of the lake's hypolimnion is consumed, this could lead to a decline in HOD but not necessarily be accompanied by a decline in primary productivity. Therefore, one cannot assume that the scaling is due to a decline in productivity at this time.

*Page 117; It is unlikely that the internal loading scales directly to surface area as this ignores the well known transport of organic matter to the deeper portion of the lake resuspension and settling. Simple morphometric ratios must be used with caution.*

We agree that the ratios must be used with caution, but feel this is an appropriate use of these ratios.

*Page 119; It is almost certain that the relative contribution of the littoral zone is increasing with decreasing depth. This is a well known function for almost all lakes. More integration with the existing limnological literature would be helpful here.*

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.

*Page 126; Although seasonal de-oxygenation of the hypolimnion is almost assured well into the future, it is not true that all data indicate they will increase or that Walker Lake is in the midst of a successional phenomena towards hypereutrophy. How does increasing Secchi depths and decreasing areal HOD support these statements.*

We would like to point out that the driving force behind the positive feedback loop discussed is the abundance of available phosphorus, which is discussed in the previous paragraph. This is a general statement that is meant to convey that as long as the positive internal loading is maintained, the phosphate will remain an issue and cyanobacterial blooms will be expected to continue. It is indeed interesting that Secchi depths did reach these values in early and late 2007. However, minimum Secchi depths of less than  $< 2$  m are still consistent with hypereutrophy. 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.

## References

Lopes, T. J., and J. L. Smith. 2007. Bathymetry of Walker Lake, West-Central Nevada: U.S. Geological Survey Scientific Investigations Report 2007-5012.

## Project A: The Contemporary Ecology and Food Web Energetics of Walker Lake

- 1) All editing comments were noted and changed in the manuscript.
- 2) The authors have made numerous requests since November to receive reports and documentation from the reviewers that support existing efforts or strains of fishes introduced into the lake. The BOR liason has not received responses from the reviewers for these reports. Thus, no additional changes were made to the final document but can be incorporated later after the authors are allowed to determine if they are appropriate for citation.
- 3) Specific comments and responses are provided below.

*Page 132 The 12.4 g/l in Beutel refers to 1996. It is misleading to cite this when talking about 2005. Please cite dates along with any salinity measurements.*

Agreed and the text has been changed to reflect increasing salinity

*Page 133: Some or all of these numbers are wrong, but I can't tell without a date being given. In general dates should accompany statements like these as the lake has changed markedly.*

Agreed and the changes have been made accordingly.

*P134: What about the phytobenthos? Filamentous algae is extensive in the lake and epilithic diatoms are also present and important as food resources to grazers.*

Agreed however we did not measure benthic periphyton or plant production and thus this was not introduced as a topic in the paper.

*P137: In reference to the equation, Isn't it the weight in grams x 10<sup>5</sup> divided by length cubed? Not the wt raised to the 5th power.*

This has been corrected but it should be noted that we used the same equation as described above but presented a different mathematical version. In any case, we have correct the equation and just provided the original reference for this equation that is standard in fisheries textbooks.

*P139: Please explicitly state how data were corrected for baseline variation.*

This has been corrected.

*P140: Many of the figures in this Part of task 1 appear multiple times though various "chapters" (or reports). It is not clear why it was organized this way and it is somewhat confusing.*

The figures are repeated throughout the chapters to give context to each chapter. Since this chapter was dealing with contemporary food web structure and the

lower food web is very important for fisheries production it was included here as well as the other chapters.

*The spatial variability portion of this chapter appears to be work done by L. Newton in 2003-2004. Please document the use of N. crassa instead of N. spumigena or N. spumigene var. crassa. I understand that this genera is undergoing revision, but it should be clear to the reader that all the studies from the 70s, 90s and current are talking about the same species.*

This has been corrected.

*P 146: Is data in this figure from the 2003-04 study or from the 2007 study?*

We are more explicit in the text in regards to these numbers in the paragraph.

*Numbers shown here do not agree with the text for both Moina and Leptodiamtomus. And the numbers for all species do not match Figure 8.*

This has been corrected.

*P149: "Benthic invertebrates are represented by a single combined measure of C & N isotopes and this almost certainly misrepresents trophic positions among the group of inverts in the lake that are known to represent predators, grazers, and detritovores, and mixed consumers."*

The reviewer is correct however does not understand the utility of the isotopes. Benthic invertebrate signatures typically do not represent discrete trophic levels either 1) due to their differential metabolism for processing isotopes or 2) invertebrate taxonomists inadvertently misidentify the true feeding nature of these organisms which likely receive their energy from a variety of sources. Until this debate is resolved the authors continue to utilize and interpret the isotope data with the best available scientific literature available and have followed methods similar to other studies.

*Potentially the most interesting finding is the fact that LCT are almost exactly a trophic level (15N of 3.1) higher than chub, and chub exactly one trophic level higher (3.2) than benthic inverts. This suggests LCT are dependent on chub. This is at odds with recent stomach analyses. What is the explanation for this apparent discrepancy?*

This is not at odds with the stomach data. The utility of the isotope data is that it integrates signals over time and avoids the traditional issues with stomach collections where the fish has digested stomach matter and it is misidentified. Our understanding is that LCT are not necessarily caught during all seasons in the lake and the sample size for stomachs analysis may be low. Thus, we believe the isotope data is more accurate than the stomach data reflects.

*The conclusion that chub are dependent on benthic inverts should be tempered by the fact that no YOY chub were analyzed and it is generally thought that the younger age classes would be most dependent on zooplankton. This was well-documented in Koch et al (1979). If it is no longer true than this is a significant change in the plankton dynamics.*

While no YOY chub were analyzed, age class 2+ or greater were analyzed. Based on the authors experiences at Pyramid and Eagle lake LCT utilize non YOY chub

as forage and will eat non YOY chubs. This information combined with the isotopic nitrogen signal suggests the chubs are feed on a mix of chub and plankton.

## **Project A: Major-ion and trace-element chemistry of Walker River and Walker Lake, Nevada.**

All comments made by the late 2009 reviewer were redundant with earlier comments. No changes were made in the final report.

### **Project A: Walker River Periphyton**

*Page 27: Previous sentence talked about the east fork and Mason Valley - what part of the river are you talking about here- only the lower portion?*

Edited text to; “Standing stocks of algal biomass were present at levels often considered to signify eutrophic conditions (greater than 5 to 15  $\mu\text{g chl } a/\text{cm}^2$ ) in the sites along the East Walker and into Mason Valley. Overall, the river had high abundances of siltation-tolerant diatom taxa, with the most notable abundances (exceeding 60%) at the lower sites.”

*Page 29: Interesting. It might be worth noting that diversion out takes throughout the Walker River Basin can act as fish barriers, decreasing the utility of fish as an indicator species. In addition to fish barriers, localized stocking can skew fish data.*

We feel that this topic is better left to discussion in the fish data.

*Page 30: Management for what? Is this referring to future water management based on water acquisitions? Land management?*

Many management applications could be evaluated using periphyton. We feel that a list of specific river management schemes could confuse the reader or infer artificial limits to the use of periphyton dynamics.

*Page 30: Is this baseline evaluation clearly and succinctly written up somewhere? This will be a very helpful tool to measure benefits of water acquisitions, impacts of restoration projects, etc.*

Does the reviewer feel this should be done in the introduction?

*Page 31: Are these different locations than the previous Chapter or just different nomenclature? Also GPS locations would be helpful.*

We updated the map and all of the graphs to use the same site nomenclature throughout the document. Specifically related to the site map, we added UTM coordinates to the border of the map so site locations could be determined and updated the site abbreviations.

*Page 32: Does not match map. FLSTN or FLST - FLST is used throughout the document. It would be helpful to date the photos.*

We updated the nomenclature in this chapter to the latest revision.

*Page 33: Confusing. SHRZ is on the main stem not the West Fork. A point graph with each sampling location labeled may be more helpful. This causes problems throughout the discussion. It would be clearer if sampling points on the main stem were called such. Or just clearly explain this figure - could not find a description of figure 3 in the text.*

We agree with the reviewer and updated all of the graphs with a similar layout to include a different shade and symbol for the main stem, rather than plotting both forks on the same point for the main stem.

*Page 33: Should this habitat be briefly explained or is it common knowledge?*

Text was edited to clarify; "Sampling of the richest targeted habitat (RTH), i.e. the in-stream habitat type that supports the taxonomically richest assemblage of organisms within a sampling reach at each sampling location, helped in identifying differences in periphyton communities in relation to water quality."

*Page 35: Was FSW collected from each sampling location?*

Text was edited to clarify; "Filtered stream water (FSW), collected the day of sampling at each site and filtered through a 47-mm Whatman GF/F filter with approximately 180 mm Hg vacuum, was used as the transporting medium (solvent) for the periphyton that was scraped from the cobble."

*Page 39: Why the last decade? USGS measurements go back significantly further and samples were only collected in '07 and '08.*

A recent historical context was helpful in understanding how the sampling years compare with recent flow regimes.

*Page 42: Explain that the last two years have been drought conditions.*

Edited text to include; "Due to drought conditions the discharge during this study (2007 to 2008) was typically 40 to 60 percent lower all along the river than the means for the last decade."

*Page 46: This paragraph is unclear. Variation in water and land management make it very difficult to compare the Truckee and Walker systems. Is this taken into account anywhere in the text?*

See response to second to last comment from page 76. The text was also edited as follows; "The ratio of TN to TP (Figure A.7.9) ranged between 5 and 25 (mol:mol), averaged 12.64, and tended to decrease from upstream to downstream. The ratio did not appear to display as evident or as strong a gradient as has been documented in the Truckee River (Green and Fritsen 2006). However, it should be noted that the sampling sites in the Walker basin did not extend to the higher elevations in the Sierra Nevada as did that particular study of nutrient balance within the Truckee River."

*Page 49: Is this related to work on the Weber Reservoir and flow releases?*

We do not feel there is adequate information to show a relationship to the aforementioned activities.

*Page 51: The whole river system or only the EF was in eutrophic conditions?*

Edited text for clarification; “In combination with the high nutrient concentrations measured in the East Walker at the EWB site, it is apparent that the stream system was in a eutrophic condition at select sites. Such eutrophic conditions may lead to large oxygen fluctuations and high export or loading of organic matter to downstream locations, especially during summer (Dodds and Gudder 1992). Despite the high biomass (Figure A.7.11) and nutrient concentrations in some locations, portions of the river exhibited more meso- or even oligotrophic characteristics. For instance, the West Walker that exhibits both low biomass (Figures A.7.11 and A.7.12) and low nutrient concentrations (and more unrestricted/regulated water discharges) appeared to be in a condition that could be considered oligotrophic (based on TP being less than 0.8  $\mu\text{M}$  and benthic chl *a* being less than 2  $\mu\text{g chl } a/\text{cm}^2$ ; Dodds *et al.* 1998).”

*Page 51: Because flows associated with irrigation and agricultural use are higher in the summer?*

Through the downstream movement of formerly attached algae that becomes detached as water temperatures reach levels beyond tolerance.

*Page 59: The SHRZ site was likely being impacted by tribal following associated with construction on Weber Dam, thus altering the timing of flows in the lower reach.*

Text was edited for clarity.

*Page 59: Associated with low water years?*

We do not have adequate information to make this association.

*Page 61: Again, it is important to point out the Truckee and Walker are managed differently and have very different anthropogenic influences.*

See second to the last response which is a response to a comment on page 76 and also the response to the comment from page 46.

*Page 68: What defines "good" water quality?*

Added clarification in text; “A high proportion of these genera usually correlate with “good” water quality (i.e. low concentrations of phosphorus, nitrogen, and chloride) (Wang *et al.* 2005).”

*Page 72: Are certain agricultural processes increasing the nutrient supply?*

Yes.

*Page 72: The proper name is the "Mason Valley Wildlife Management Area (MVWMA)."*

Text updated with current revision of nomenclature.

*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.

*Page 75: Can target periphyton populations be established off these data?*

Alone, these data could not be used for this purpose, but could after being integrated with a program such as EMAP protocols.

*Page 76: This chapter still needs a good editing. Lots of minor typos and sentence structure issues. This chapter seems to have a fair amount of detailed information, but the discussion/conclusion is missing some important points.*

*-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).

*-The Walker River Paiute Tribe has been following 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.

*-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 acquisitions 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 (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.

*-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 two 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.*

*-The graphs used throughout the text (beginning on Fig. 3) could be potentially confusing. Just make sure it is clear to the reader what the graph is showing - I did not see Fig 3 discussed in text.*

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.

## **Project A: Relationships between Aquatic Environments and Walker River Benthic Macroinvertebrate Communities, Nevada and California**

A paragraph was added to generally describe how humans have altered physico-chemical characteristics of the Walker River, and that information provided by this study provides insight into the influence of some of these alterations on benthic macroinvertebrate communities. This study could only examine a limited number of human influences, but it provides guidance for future work and the some of the best ecological information ever compiled on an eastern Sierra river system.

*Page 78: Previous sentence discussed downstream sites. This sentence is unclear. Is it still referring to downstream sites?*

Sentence was not changed. The 'previous sentence' describes environmental conditions observed in Mason Valley and below, and the following sentence describes changes that were observed along the stream gradient toward the Sierra Nevada.

*Page 79: Unclear sentence.*

Sentence revised as suggested.

*Page 79: Which values?*

Sentence revised to include 'tolerance values'.

*Page 82: lowering lake level?*

Sentence revised as suggested.

*Page 83: Why is this under methods?*

Section deleted, but information moved into the Introduction.

*Page 83: There are some minor storage reservoirs in the headwaters.*

Sentence revised as suggested.

*Page 83: This may be more closely related to peak flows associated with calling for irrigation water. Because water is being stored in the reservoirs.*

Sentence revised to note the influence of irrigation storage and releases on river discharge. But, in reference to runoff occurring during May and June, the reviewer observed that this may be attributed to irrigator calls for water. This may be true, but peak springtime runoff following the natural hydrograph also occurs during this period. The same is true for the period November through February. Yes, storage occurs during this period but this is also the typical low flow period following the natural hydrograph. A statement was inserted referring to flows being affected by water management.

*Page 109: This is the first time in all the chapters that there has been any discussion of agricultural land use potentially influencing results. Similar to the other chapters the lack of any sort of significant discussion to current land and water management activities is disappointing. There must be some recommendations and conclusions based on these results.....or even future BMI values that would indicate improved river conditions.*

Reviewer comments not addressed. While interests of the review are an important goal of ecological work in the river, but we believe that data collected over two years of drought provide for a relatively weak assessment of the effects of human activity on river life. We hope that future studies will include years with average and above average annual precipitation so that a gradient of differences between

these conditions a drought can be analyzed. Management recommendations were not included because these were never a proposed part of this study.

**Project A: Spatial and temporal variability in elemental composition and stoichiometry of benthic macroinvertebrate communities in the Walker River, Nevada and California.**

*Page 127: Diversions are primarily for agricultural use.*

Corrected as suggested.

*Page 128. Awkward.*

Sentence restructured.

*Page 128. Awkward.*

Sentence restructured.

*Page 129: Does this paper discuss impacts of agricultural influences on C, N, and P levels? Yes. Agricultural influences on C, N and P levels due to both fertilizer runoff into the river as well as reduction in flow due to diversions.*

*Page 129: specifically?*

Corrected as suggested.

*Page 129: They (?) being the headwaters?*

This sentence no longer exists in the chapter as this has been covered in previous chapters.

*Page 129: at the confluence.*

Same as response to Comment 6.

*Page 132: Is this influenced by irrigation and agriculture?*

It is mainly due to higher flow in winter and smaller flow mixed with agricultural runoff containing fertilizers in spring and summer. The sentence was revised to reflect this information.

*Page 132: Is this directly correlated to certain agricultural practices? It is not clear if this is directly correlated to certain agricultural practices.*

We did not look at details of agricultural practices.

*Page 144: Cite historical information.*

Citation inserted.

*Page 145: This is the first time that fertilizers, livestock, non-point source solution have been mentioned in any detail.....isn't this impacting results in all the other studies.*

This information is discussed in Chapter A.8, river environments and benthic macroinvertebrates.

## **Project A: Fishes of Walker River: Present composition and basic ecology**

*Page 153: This leaves out key information regarding fish species in the river. There is no mention of the stocking programs by NDOW, which influence species composition. There is no mention of the various barriers to fish movement.*

These comments were addressed by discussing barriers to fish migration in the river. It was not our intention in this project to discuss the stock and catch of fishes in the river. Thus, this information was not included in the chapter. This information can be obtained from the Walker Basin Working group, the USFWS and NDOW who stock the river with game fish.

*Page 153: If all samples were collected at the same location, they should all have the same name. Two of the 6 papers have different sampling location nomenclature.*

Report changed to correct this.

*Page 153: Opening sentence needs to discuss sampling methodology, seems very choppy and confusing.*

This has been corrected by providing a broader introductory paragraph.

## **Project D: Science, Politics, and Water Policy: Resolving Conflict in the Walker River Basin**

### **GENERAL**

As noted in more detail below, this report contains many undocumented quotes, claims, and/or assertions attributed to both named and anonymous sources; each should be specifically referenced, re-stated as the author's opinion, or dropped. The analysis in Chapters 1 and 4 focuses primarily on the research aspects of the Walker Basin Project (involving a commitment of not more than 20% of available funds), and far too little on its more ambitious, and controversial, water acquisition component (involving a commitment of not less than 80% of available funds). Additional discussion of the evolving acquisition effort would be valuable, which as of this writing includes 11 separate option and purchase agreements between the University and willing sellers with a composite negotiated value (subject to many contingencies) of more than \$90 million; completion of a Draft Environmental Impact Statement (EIS) on the acquisition program by the U.S. Bureau of Reclamation, which includes discussion of their decision to complete a Final EIS but not a Record of Decision on the program; and legislation pending (see next comment) that will, if it becomes law, substantially re-structure and build upon the Project's fee acquisition and research efforts to-date, end the University's direct involvement with the acquisition program, and launch a 3-year

demonstration water leasing program (and additional fee acquisitions) as part of a more comprehensive effort. Finally, as the above comments make clear, the most important parts of this report are already out-of-date. No doubt it was always going to be difficult to produce an historical account of the Walker Basin Project so soon after its inception, however in light of recent and pending events – above all the anticipated enactment of into law of federal legislation (as well as companion actions already taken by the University of Nevada’s Board of Regents) that would lead to the University’s assignment of its acquisition-related rights, interests, and obligations to the National Fish and Wildlife Foundation before the end of 2009 – it would seem most appropriate to revise and update this report to focus on what can now be seen as “phase 1” of a longer-term restoration effort.

I wish to thank the reviewer for taking time out of what must be a very busy schedule to review this project. The comments and recommendations are appreciated. I address the reviewer’s concerns below.

The reviewer notes that the manuscript is outdated where acquisitions are concerned. I agree. Had the manuscript been reviewed closer to the time of submission (March 2009), it would have been up to date to that point in time. Given that nearly nine months have passed since that time, several important events have occurred which should be included in the manuscript. The reviewer specifically notes that 11 options to purchase water have been negotiated at a value of approximately \$90 million; a draft EIS on the acquisition program has been completed by the US Bureau of Reclamation; and legislative efforts are pending that, if they become law, would (a) substantially restructure and build upon the project’s fee acquisition and research efforts to-date; (b) end the university’s direct involvement with the acquisitions program; (c) launch a three-year demonstration water leasing program (and additional fee acquisitions); and (d) lead to the university’s assignment of its acquisition-related rights, interests and obligations to the national fish and wildlife federation before the end of 2009. These points are all well-made and all are now mentioned in chapter four. I also provide more information about the current status of these items. Jim Richardson, who is part of the acquisitions effort, notes as well that it is possible that most or even none of these options will ever be exercised, and that if they are, that process will take at least four or five years. There is also the possibility that the leasing program itself will render some the purchase options moot—and the possibility, as well, of technological advancements that will do the same thing (i.e., desalinization technology, which is currently under development by Amy Childress and Scott Tyler at University of Nevada, Reno). I will reference an article that appeared on this in the Nevada News “Suarez develops new solar distillation pond methods,” by Mike Wolterbeek.

The reviewer’s second main general point is that this report “contains many undocumented quotes, claims and/or assertions [that are] attributed to both named and anonymous sources.” This comment puzzles me. If a claim or quote is “attributed,” then how does that translate into “undocumented”? I did, however, delete the material that was referenced to anonymous. That having been said, I

went through the manuscript carefully to see where more documentation needed to occur. I found a few and put in a citation, as appropriate. I should note here, however, that an editor and reviewer of these chapters, whom I hired on my own, noted that the manuscript was “over referenced”. For additional citation issues, I relied on the points the reviewer made in this regard in the “specific comments” section. All of the “specific comments” are discussed in the following pages.

Finally, the reviewer notes that chapters one and four focus “primarily on the research aspects of the Walker Basin Project (involving a commitment of not more than 20% of available funds), and far too little on its more ambitious, and controversial, water acquisition component (involving a commitment of not less than 80% of available funds).” First of all, the fact that 80% of the funds were dedicated to purchases is irrelevant. I do not plan to devote anywhere near 80% of the book to acquisitions. Further, the options are private transactions that should not be made public at this time (even if I had access to detailed information about them, which I do not). All that can be said is that 11 options exist. The book is about the bigger picture, of which the acquisitions program is but a small part. Second, chapter one devotes only one paragraph to a discussion of Walker research. Chapter four is dedicated entirely to Walker research—and it was designed to do just that.

It might be useful here to note the outline of the book. Chapter one provides a general background for the project, including a discussion of the negotiated settlement of water issues relating to the Newlands project and the Carson and Truckee Rivers. I include that discussion as a transition into the Walker Project because the strategies undertaken to resolve the walker issues are very different from those undertaken for the Carson/Truckee.

The reviewer in several places asks that I provide more detail on the negotiated settlement. I wrote an entire book about that topic and am only providing a brief overview in this manuscript. The idea is not to delve into the specifics, but to set up a contrast between that and Walker.

Chapter two covers western settlement in general and settlement of Nevada in particular. It includes a discussion of early explorers and settlers, reclamation, Anglo/Indian contact, and various other influences on Nevada (gold and silver, the Mormons, etc.). It ends on Nevada becoming a state. This is all by way of historical background.

Chapter three, when finished, will provide background on the development of agriculture and ranching in the state of Nevada, including the development of Mason and Smith valleys and the creation of the walker river irrigation district—it will end by covering the disputes over water in those valleys, and the legislation that was passed that resulted in efforts to save Walker Lake, which in turn led to the Walker Basin Project itself.

Chapter four focuses specifically on the Walker Basin Project.

Chapter five will cover whatever has happened with the project and the acquisitions process at the time that the chapter is written. It was and is my understanding that the timeline for this book stretches through 2011.

A final chapter will be devoted to a summary and conclusions. It will include, likely, a discussion of the increasing use of “civic science” (which is what my field calls research like that conducted for the Walker Basin Project).

Re the reviewer’s specific comments

*P3, 1st paragraph: if defined as the “land of interior drainage” the Great Basin includes most of Nevada (not just “most of northern Nevada”) and at best very small portion of western Wyoming (?)*

This description is taken directly from an expert. I do not understand the reviewer’s problem with this description. It appears the reviewer is not clear either, as he ends his comment with a question mark.

*P3, 2nd paragraph: the last sentence -- “To make matters worse, all of the rivers in Nevada are over-allocated” -- seems out of place, value-laden, and ill-defined (at least at this point)*

Agreed. Deleted offending sentence.

*P3, 3rd paragraph: if defined as the “land of interior drainage” (see 1st paragraph above) then the headwaters of the Truckee, Carson, and Walker Rivers in California do not lie outside the Great Basin*

Deleted “outside the Great Basin.”

*P3, last sentence: “since completion of Derby Dam in 1905, more than half (and sometimes all) of the flows of the Truckee River have been diverted to Lahontan Reservoir” – this statistic seems dated, and at a minimum should include reference to the specific period of record to which it applies; ditto the use of that water “to support irrigated agriculture” (only) at the end of that sentence on P4*

Provided more detail. Specifically: the Truckee River historically terminated in Pyramid Lake, but since completion of derby dam in 1905, an average of 250,000 acres-feet-year (a-f-y) was diverted, through 1968, from the Truckee river to Churchill county, where it has been used, along with Carson river water, to support irrigated agriculture in the area (Horton, 1996:1-7). According to Joe Gremban, president of the Sierra Pacific Power Company in the late 1980s, the diversion took, at time, all of the water in the Truckee River at Derby Dam, leaving nothing to flow to Pyramid Lake.

*P4, 1st full paragraph: again, dated and without particulars (and more generally, much has happened over the past 2 decades to affect the diversion and use of waters of the Truckee and Carson Rivers into and below Lahontan Reservoir)*

I do not see how this is “dated.” A “significant” portion of the Walker River is captured and stored for use in Smith and Mason valleys to support agriculture. How much depends on a number of factors: how much water is requested by

individual farmers, how much water is stored in the reservoirs, whether it is a wet or dry year, etc. I used the word significant, because it is significant enough to have seriously impacted Walker Lake. This information is taken directly from an expert's take on it. I do not dispute this expert. Similar descriptions are reported by others.

*P4, 2nd full paragraph: Smith and Mason Valleys in Nevada*

Agreed. It should be Mason and Smith Valleys in Lyon County, Nevada. I added Nevada.

*P4, same paragraph: "the volume of each has been greatly diminished" – as above, a few specific details would be helpful*

Again, this is language taken from an expert. The volume of each fork of Walker is "greatly diminished." How much diminished they are, again, depends on a number of variables.

*P4, last paragraph: tribal governments are an important 4th level, esp. in the context of this Paper*

Reviewer wants me to mention specifically the tribes as one of the competitive users of Walker River water. I do not mention the others specifically (that comes later) either. I refer to a generic set of users. It is not appropriate at this place to specify all of the competing users for walker river water.

*P5, first full paragraph: the first major extractive use of water in the West was for mining; prior to that Native peoples relied on the same waters in situ for much of their livelihoods, sustenance, and cultural identity*

Agreed. Added that prior to Anglo use of these waters, the Native Americans depended on that water for sustenance and as a source of cultural identity.

*P5, last paragraph: does "the three river systems" mean the Truckee, Carson, and Walker? (This becomes clear only later in the paragraph, which begins with discussion of Lake Tahoe only)*

Cannot find reference to "three river systems" in this paragraph. Did find it on page eight and included mention of the three in parentheses.

*P6, 2nd to last paragraph: Appendix A is referenced, however there is no appendix*

Appendix A is indeed referenced. The reviewer notes that it was not included. Either you did not give it to him with the other chapters or I did not give it to you. I will make sure to include both appendices when I return the revised chapters to you.

*P6, last paragraph: The statement “There were just too many issues left unresolved by the compact for it to be ratified” should, at a minimum, be preceded by a qualifier (e.g., “As discussed in the next section, ...”)*

Added “as discussed below,” per reviewer’s suggestion.

*P7, 2nd paragraph: It is not at all clear that, as quoted, Article I (Purposes) of the Compact has any particular “bias” built into it*

Clarified how the appropriation doctrine has bias built into it, which was reflected in the 1968 compact in its description of the purposes of that compact.

*P7, 3rd paragraph: “The west is the fastest growing and the most urbanized region in the United States” – most urbanized?? – at a minimum, footnote 7 should be expanded to include references that help to define and support these statements*

Yes, the West is the fastest growing region in the United States according to not only this source, but many others. Changed “most urbanized” to “is becoming increasingly urbanized.”

*P7, 3rd paragraph: suggest “...greater value being placed on the western environment in general and on water dependent environmental resources in particular.”*

Substituted his suggested language.

*P7, 4th paragraph: this paragraph jumps very quickly from questions about the use of water for irrigated agriculture (with little discussion of irrigated agriculture’s dominant role in the development and use of western water) to the recognition of non-consumptive uses of water as beneficial – how did this happen??*

Added “which had consumed the lion’s share of western water supplies for decades.”

*P8: P8, 1st paragraph: why did Pyramid Lake matter to Interior? (i.e., no discussion of the Pyramid Lake Paiute Tribe, it’s ancestral interests in the Lake, the fact that it’s modern, Reservation entirely surrounds the Lake, Interior’s crucial if sometimes conflicted role as a Tribal trustee, etc.)*

Added that Pyramid Lake had by that time been receiving recognition as a national treasure that should be protected; it was also home to two endangered species (the Pyramid Lake cutthroat trout and the cui-ui).

*PP 8-9: while interesting, the several pages devoted to the Native American rights movement and the early environmental movement seems both incomplete and out-of-place here*

Agreed that the discussion of the Indian rights movement should not be in here as it adds little value and is a distraction. I deleted it.

*P9, last paragraph: “Two of northern Nevada’s lakes...” -- most of this paragraph includes background information that should have come much earlier*

I disagree. The discussion of the environmental problems faced by these lakes as a consequence of the Newlands project follows a general discussion of the importance of the environmental movement in changing western water policy. I went from the general to the specific example. I saw no opportunity to get to this level of detail earlier in the chapter. Left as is.

*P9, 2nd paragraph: Stillwater National Wildlife Refuge is mentioned here for the first time but not well described – where is it? How had it been “greatly affected by the Newlands Project?” etc.*

I developed and inserted language that does both.

*P10, 1st paragraph: “by 1966 the level of Pyramid Lake had dropped by 80 feet” – from what date?*

1882. Added.

*P10, 2nd paragraph: “by 1966 the Walker Lake level dropped by 108 feet; as of 2007 it had dropped by 145 feet” – again, from what date? (also the first two sentences in this paragraph should go at the end of the prior paragraph; everything thereafter is really focused on the Truckee-Carson system)*

There is not enough room in this chapter for this discussion. Indeed, as previously noted, I have written a book on the negotiated settlement; two complete chapters are devoted to these details. These are not needed here, in my view. Instead, I have included a summary of the provisions of that settlement.

*P10, 3rd paragraph: Senator Reid’s efforts were clearly very important, however there is no discussion as to why the Walker Basin was “dropped” from the efforts that led to enactment of PL 101-618 (this comes later, on p11, but should be moved up or at least referenced in advance), nor mention of the importance of the 1989 Preliminary Settlement Agreement between the Pyramid Lake Paiute Tribe and Westpac Utilities (i.e., the “nucleus” for a new, legislated version of the Truckee-Carson portion of the compact), nor of the role of local and national environmental/conservation groups in developing an innovative approach for protecting and restoring the Stillwater NWR and other downstream wetlands concurrent with protecting and restoring Pyramid Lake (i.e., voluntary water acquisitions, which gained important support and prominence at the national level, and which in many ways set the stage for what is now being pursued in the Walker Basin)*

Ditto a recommended inclusion of all of the elements (the Preliminary Settlement Agreement and TROA, among others) that led to the passage of Public Law. 101-618. That is the topic of a different book. I am giving an overview of the provisions of that settlement, and briefly describing how it came into being, by way of contrast to how the Walker Project came about.

*P11, line 1: the Settlement Act ended decades of litigation among most of the parties involved in the underlying disputes (to their great relief, especially the federal government?)*

Done.

*P11, 1st paragraph: there are several references in this paragraph (and one in the following) to things that “Senator Reid believed” – is this all based on the author’s interview with the Senator as finally referenced at the end of the 2nd paragraph (i.e., Reid, 2008)? That interview (or the proper source) should be cited each time.*

Line one: changed “among the parties” to “among most of the parties;” deleted the part about “to their great relief.” First paragraph: inserted reference/citation to Senator Reid.

*P11, 1st paragraph, line 12: “In addition, the federal government had a major presence in Churchill County” (and maybe explain how/why Churchill County matters in the Truckee-Carson context, and briefly what that major federal presence included, esp. in contrast to the situation in the Walker Basin)*

I deleted reference to federal presence in Churchill County.

*P11, last paragraph and P12, 1st paragraph: these both seem completely out of place*

I deleted extraneous paragraphs, as recommended.

*P12, 2nd paragraph: suggest a new heading here (e.g., On to the Walker?) – also as noted above, the water acquisition program at Stillwater (and now similar efforts elsewhere) has helped to set the stage for what is currently being pursued in terms of a large-scale, federally-funded effort to acquire water from willing sellers in order to protect and restore at-risk aquatic resources in the Walker River Basin – clearly the concurrent focus on (and funding for) associated research at the University of Nevada is an important and perhaps unique part of this effort, however at this point it remains to be seen whether and to what extent that research will result in a “science driven” acquisition program or whether the program will, perhaps more accurately, be informed by the results of that research over time (as well as shaped by politics and many other factors as it evolves)*

I inserted a new heading (On to the Walker River), as recommended. Inserted requested language in footnote 21

*P12, 3rd paragraph: given litigation ongoing in the Truckee-Carson system it does not seem appropriate to say that the water wars have “essentially ended” without at least a little more explanation; and as above, it would be best to link Senator Reid’s goals (even if presumptively correct) to specific statements or other citable sources*

I deleted references to water wars, per recommendation.

*P12, last paragraph: having finally turned back to the Walker, the paper now heads back to an examination of “major influences that shaped the creation and development of the state of Nevada”? (This definitely feels like filler; and this reviewer will now jump directly to Chapter 4, which seems more like the heart of the matter...)*

Reviewer notes a problem with chapter two and does not therefore review it. The contents of chapter two were part of the proposal. Lay readers (and that will be the eventual target audience) need to understand the development of the state of Nevada vis a vis its water resources and other influences in order to understand present-day water politics. Chapter two also sets up chapter three, which in turn

sets up chapter four. Some of the stakeholders seemed keen on having a history of Nevada and the development of Mason and Smith Valleys covered here. I did review chapter two again, checking for the kinds of problems the reviewer identified in the first chapter, and made the appropriate (in my view) changes.

## Chapter Four

*P31, paragraph 1: can't tell from this description where the two forks cross into NV*

Included sentences noting where the east and west forks of walker cross the state line into Nevada.

*P31, 2nd paragraph: is Lyon Co still "one of the fastest growing counties in the nation"? And in the last sentence, the Tribe and Mineral County are also interested in the preservation of Walker Lake*

Yes, Lyon County is still one of the fastest growing counties in the nation.

*P31, 3rd paragraph and P32, 1st paragraph: maybe good to break this up? Suggest "support" rather than "contain" a freshwater fishery; suggest putting "normal" (water years) in quotes, there really is no such thing; also upstream barriers (including major storage reservoirs, diversion dams, lack of passage and screening facilities, etc.) have contributed to the demise of the Walker Lake ecosystem and its fishery; current TDS levels are greater than 17,000 mg/l (vs. 13,000 mg/l here); important to note that laboratory studies show 16,000 mg/l as a 100% mortality threshold for LCT, however the Lake contains microenvironments that are spring-fed, etc. so those are all that's buying time at this point (and pretty sure tui chub, the LCT's principal food source, are also struggling to survive)*

Changed "contains" to "supports," changed normal to "normal," noted the upstream barriers, changed 13,000 to 17,000, noted that the lake contains microenvironments that are spring-fed, which helps buy time for the trout and the lake. I had already noted the problems the tui chub were having.

*P32, 2nd paragraph: this should refer back to Chapter 1, no need to repeat things here*

Deleted paragraph, as recommended.

*P32, 3rd paragraph: federal presence involves more than "advocacy" on behalf of the Walker River (and Yerington) Paiute Tribes, it's a federal trust obligation involving DOI through BIA and DOJ at least; also the Army Depot in Hawthorne is a factor, as are BLM-owned lands surrounding much of Walker Lake and Forest Service-owned lands upstream; and pretty sure the "interest in maintaining trout fisheries with non-indigenous hatchery stocks" is a state, not federal, interest; the figure of 110,850 acres represents the basin wide total for lands with surface water rights (decree plus storage), not lands in production (i.e., comparable to the 80,000 acres of water righted land reported to lie within WRID boundaries, including Smith Valley, Mason Valley, and the East Walker in NV)*

I added to the description of the federal government's presence in Walker Basin, as recommended. I also noted in a footnote that the number reported represented the basin-wide total for lands with surface water rights.

*P33, 3rd full paragraph: this report on the evolution of PL 107-171 paints only a partial view, and suffers (as above) from numerous undocumented claims (“This stipulation was intended to address opposition articulated by agricultural interests...; According to an anonymous source...; Senator Reid was determined...”)*

Deleted reference to Reid’s thinking. Deleted material that came from an anonymous source.

*P33, last line: PL 108-7 did not appropriate any funds, it simply allocated funds previously appropriated under section 2507 of PL 107-171 (this may seem like a small point but when it comes to federal outlays it can make a big difference)*

Changed appropriated to allocated.

*P34, 1st paragraph: Section 207 of PL 108-7 did not lift the original prohibition under PL 107-171 against purchase or lease of water rights, and so did not “pave the way for a water rights acquisitions [sic] program in the Walker Basin” (that did not occur until the enactment of PL 109-103 in Nov 2005)*

Deleted sentence about paving the way for a water rights acquisition program, per recommendation.

*P34, 2nd and 3rd paragraphs: these details are mostly extraneous to what eventually occurred under PL 109-103, with one exception: the “outsourced public education and 6 outreach initiative” led to the first public proposal to acquire water rights from a specific potential willing seller, who eventually became the first person to sign an option and purchase agreement as part of the University’s Walker Basin Project*

Deleted extraneous paragraphs and noted that the outsourced public education initiative led to the first proposal to acquire water rights from a specific willing seller, who eventually became the first person to sign an option and purchase agreement as part of the university’s Walker Basin Project.

*P34, 4th paragraph: this should have a new section header (i.e., no longer a precursor to PL 109-103); technically, this Act was the Energy and Water Development Appropriations Act of 2006, and the directive was to provide not more than \$70m “to the University of Nevada” (i.e., not just UNR)*

Added a section header, per recommendation. Reviewer insists that the funds were appropriated to NSHE, not UNR. He goes on in the next recommendation to indicate that the funds were allocated to the University of Nevada under the direction of NSHE. He seems to be confusing the University of Nevada (which he notes is the language used in the legislation) with NSHE. Final note here: all of the legislation refers to UNR, not NSHE. I double checked and have that legislation on my computer.

*P35: rather than simply listing what the legislation provides it would be helpful to explain what each of these programs involves and how, if at all, the various funding authorizations are, or could be, tied together*

Noted that Reclamation essentially entered into a master agreement to obligate the \$70 and that funds would be released based on individual task orders. Changed section 28 to 208. I also removed the sentence that says that the horse and burro program was added at the request of Senator Ensign. Instead I said that it was added at the request of some of locals and the Nevada congressional delegation.

*P35, 3rd paragraph: section 208, not section 28; also the “collaborative effort” involves the University of Nevada, Reno (UNR) and the Desert Research Institute (DRI) under the direction of the Nevada System of Higher Education (collectively, the University of Nevada, per the legislation); also good to explain the significance of/reasons for vesting oversight with a “Walker Basin Working Group” vs. an “Executive Steering Committee” as noted at the end of footnote 69*

DRI and UNR do not “collectively” form the University of Nevada. The legislation refers to the University of Nevada. The decision to vest oversight with a Walker Basin Working Group was made by at the system level, working with the PR people at UNR and DRI. I do not see the need to explain it to the reader. There was an Executive Steering Committee at the system level—and this is discussed in the document, separately from the Working Group.

*P35, 4th paragraph and P36, 1st paragraph: here, and in most of what follows, the focus seems to be on the authorization of funding for research while saying very little about the authorization of funding “to acquire land, water appurtenant to land, and related interests in the Walker River Basin, Nevada” – the latter, of course, is by far the most controversial when it comes to the University’s “deep, historic ties to Nevada agricultural interests,” and most at odds with the previous prohibition on the purchase or lease of water rights moreover, while it may well be the case that “the policy goal of this appropriation [as interpreted by the University?] is to deliver water to Walker Lake,” the legislation does not say that directly...so what is the back story that allows the reader to understand this assertion? How and why was the decision made to pursue a “virtual” research center and a “water only” options program, when it appears that the legislation anticipated a physical research center and pursuit of acquisitions involving land, water, and related interests that would be “most beneficial” not only to “environmental restoration” but to the establishment and operation of such a center? (These interpretations may or may not be accurate or complete, however these and other formational elements of the Walker Basin Project should at the very least be discussed.)*

Reviewer suggests that the policy goal of the legislation is not necessarily to deliver water to the lake, but that was merely UNR’s or NSHR’s interpretation. I disagree. So would Mary Conelly and Senator Reid. Mary even has buttons that says as much. Also wants me to focus more on the acquisitions program. That is not the purpose of this chapter. That program is briefly described, as are the other components of the Walker Basin Project. More detail will be provided later in the manuscript, in a section that will deal exclusively with this topic. This is not the appropriate place for an in-depth discussion of that topic.

*P36, 2nd paragraph: this paragraph should be deleted -- it is a very long reach to suggest that the federal government (beyond the provision of federal earmark funding) is seriously interested in resolving this conflict, particularly as federal agencies step away from taking responsibility for the evolving acquisition program (e.g., the Bureau of Reclamation's recent decision not to issue a Record of Decision for the water acquisition program EIS) -- the rest of the paragraph seems, at best, aspirational and at worst, pre-mature and self-serving*

Deleted, per reviewer's recommendation.

*P36-41 (under Walker Basin Project): suggest breaking this out into (a) the research program and associated outreach efforts (which is the primary focus of what follows); and (b) the acquisition program (which is only mentioned in four places – i.e., that 80 percent of the funds were budgeted by the University for such purposes (p36); that an anticipated timeline for the acquisition process was developed as part of the initial planning process (p36); that Western Development and Storage was selected to coordinate the acquisition process (p37); followed by a somewhat-tortured one-paragraph description of the acquisition process (p40), which as of July 2009 included 11 recorded option and purchase agreements at a composite negotiated value (subject to appraisal, confirmation of title, and other due diligence) of more than \$90 million*

I have done that. He also suggested adding to the discussion of acquisitions, specifically noting the changes that have occurred since Mmarch 2008. I have done that.

*P26, 2nd paragraph under Walker Basin Project: again, what are “the policy directives found in the legislation and appropriations”?*

This language appears earlier in the document and clearly spells out the policy directives: “Section 208 directs the Secretary of Interior (under the provisions of section 2507 of the Farm Bill of 2002) to provide not more than \$70 million to the University of Nevada, Reno to accomplish the following goals:

(A) to acquire from willing sellers land, water appurtenant to the land, and related interests in the Walker River Basin, Nevada; and (B) to establish and administer an agricultural and natural resources center, the mission of which shall be to undertake research, restoration, and educational activities in the Walker River Basin relating to—(i) innovative agricultural water conservation; (ii) cooperative programs for environmental restoration; (iii) fish and wildlife habitat restoration; and (iv) wild horse and burro research and adoption marketing [sec. 208 (a)].

*P36, last paragraph: how is it known that “a Wild Horse and Burro Marketing Study was included...[a]t the request of Senator John Ensign”? Also, like Appendix A, Appendix B is referenced but not included*

Deleted reference to Senator Ensign, per recommendation.

*P37, last paragraph: the various “public claims” should be referenced*

Public claims (comments and opposition) are referenced now.

*P37, footnote 73: same footnote as #69 (except for reference to its previous life as an Executive Steering Committee at end)*

Deleted, as it appeared earlier and was redundant.

*P38, 2nd paragraph: “Such criticism began to wane”?? Perhaps so, for a brief period, but that does not reflect the tenor of most comments received during public hearings on the Draft EIS conducted by the Bureau of Reclamation during the summer of 2009, nor the tone of most of the articles authored by Mr. Sanford for the Mason Valley News throughout the course of 2009 (though this analysis seems to end with articles and interviews conducted in March 2008 and, as noted, focuses primarily on the research end of the Walker Basin Project through about that time)*

I stand by my assertion that criticism of the researchers and the project began to wane as the community began to interact with the researchers and a sense of trust began to develop. I cited several articles that indicated the same thing. I did add a footnote that noted that opposition was loud and clear at the public hearing Reclamation held on the draft EIS in the summer of 2009, per reviewer’s request.

*P39, first paragraph: the points made here relating back to PL 101-618, including footnote 74, need further explanation and discussion (and, as elsewhere, reference to actual conversations, new articles, whatever)*

Deleted reference to negotiated settlement.

*P39, 2nd paragraph: What project? What study outline? What policy goal established by Congress? Where has the water flow model been used?*

See earlier description of the project and policy directives, above. I do not see why the reviewer keeps referring to this when it has been so clearly spelled out, with direct quotes from the legislation itself. The models referred to in this document, at the time of this writing, were being developed and tested. I did not say any of these had been “used” for the project.

*P39, 3rd paragraph: questions concerning the future use and management of lands from which appurtenant water rights are acquired and transferred are likely to be very important to the long-term success of the acquisition program and deserve far more attention than the brief discussion here*

This will be covered in greater detail in a subsequent revision of this manuscript, when more information about that program is available and when options are converted to acquisitions.

*P39, 4th paragraph: what are the University’s “longstanding commitments” in the Walker River Basin?*

I dealt with this by deleting the reference to “longstanding commitments” and including the following language instead (in chapter four): The federal government’s physical presence in the Walker Basin is threefold. The U.S. Geological Survey (USGS) has several gaging stations by which it measures instream flows in the Walker River. There is an Army Depot in Hawthorne.

And the Bureau of Land Management (BLM) owns the land surrounding much of Walker Lake. It has a legal presence as well, because of its trust obligations to the Walker Lake Paiute Tribe, on whose behalf the federal government advocates.

*P40, 1st paragraph: this paragraph begins with reference to incentives for agricultural water conservation under Nevada water law (which part? what incentives?) and ends with a statement, now stale, that “all of these research projects [relating to agricultural water conservation?] are well underway, with report deadlines scheduled for December 2008” – what is the point of this discussion?*

This statement was not stale at the time of submission. It will, of course, be updated in the final version, as will any other parts of this manuscript that needs updating.

*P40, 2nd paragraph: how can the acquisition portion of the Walker Basin Project be “science driven” (see comment for P12, 2nd paragraph, above) while acquisitions are being pursued “in parallel with the research project”? The Nevada State Engineer (rather than State of Nevada water engineer), and eventually the federal District Court (for water rights adjudicated under the Walker River Decree), must consider and either reject or approve, with or without conditions, applications submitted in proper form to change the place, manner, and/or purpose of use of water rights now appurtenant to lands in Nevada following acquisition of title (or with the cooperation of the owner) – the U.S. Bureau of Reclamation has no role in that process, rather it oversees (as lead federal agency) the NEPA (EIS) process (which has nothing to do with the Environmental Protection Agency), except that Reclamation has recently decided that, because it has no programmatic discretion or control over the expenditure of funds, it will complete a Final EIS but will not issue a Record of Decision for the proposed project (i.e., what is described in the Draft EIS as the University’s Acquisition Program) – finally, as noted, federal legislation pending (as of early October 2009), as well as companion actions already undertaken by the University of Nevada Board of Regents, would allow the National Fish and Wildlife Foundation to accept assignment of the University’s rights, interests, and obligations under the Walker Basin Project, and would provide additional funding to the Foundation to develop and implement a more comprehensive restoration program going forward*

I did a search and found no such phrase (science-driven). Yes, additional things need to happen and some have since (the time of this writing) happened. These will be included in the revision.

*P40, 3rd paragraph: most of this discussion seems like background information that could/should be presented earlier; quotes are also included but not referenced, as elsewhere throughout this document; and it is unclear how the Walker Tribe’s claims to Walker River water would be sanctioned by P.L. 101-618, which of course pertained to the waters of the Truckee and Carson Rivers (so, at a minimum, additional explanation is needed)*

I found no such discussion in the manuscript. I must have dealt with this concern when I first responded to the reviewer’s comments, but did not make note of it in my reply.

*P41, 1st paragraph: this entire paragraph discusses potential settlement legislation “slated for introduction in late 2009” but that is not how things have evolved (and that alone would 9 make for an interesting story, including how and whether more recent/current events might help to shape a comprehensive settlement going forward)*

Appears to be convoluted and redundant; deleted.

*P41, Conclusion: again, what are “the public policy goals stated in the legislation?” It is not the case that “purchase and title transfer [will be] contingent upon it being shown...that water can be delivered to Walker Lake” – that may well be a subject for examination by the Nevada State Engineer and/or the federal District Court as part of the water rights change approval process, but it has nothing to do with most of the options now pending that the University has negotiated with willing sellers; and yes, the project – and this description of its birth and evolution – are definitely in “midstream” at best (or, as suggested in the General comments above, at the end of the first phase of a longer term effort)*

Conclusion: he strongly believes the purpose of the legislation and the project is not necessarily to deliver water to the lake. I respectfully disagree. So would Conelly, Reid and Dickens.

Thanks to the reader for helpful comments and suggestions.

## **PROJECT F: Development of a Decision Support Tool in Support of Water Right Acquisitions in the Walker River Basin**

*Page 13: PRISM over-estimates average annual precipitation in the Walker River basin by about 20 percent. See Lopes and Medina (2007). How does this affect model results?*

We agree that PRISM estimates of the precipitation in the Walker River basin (as well as many other areas) may tend to be greater than those observed at some ground-based observation points. We do not actually use the PRISM estimates as input to our models, rather we use the PRISM long-term average monthly values over each 4km grid as a means to distribute actual daily observations to other locations in the watersheds that don't have actual ground-based observations. This is a common hydrologic modeling approach that has been shown through a variety of other studies to be more accurate than the development of regressions among point observations sites for spatial distribution of precipitation.

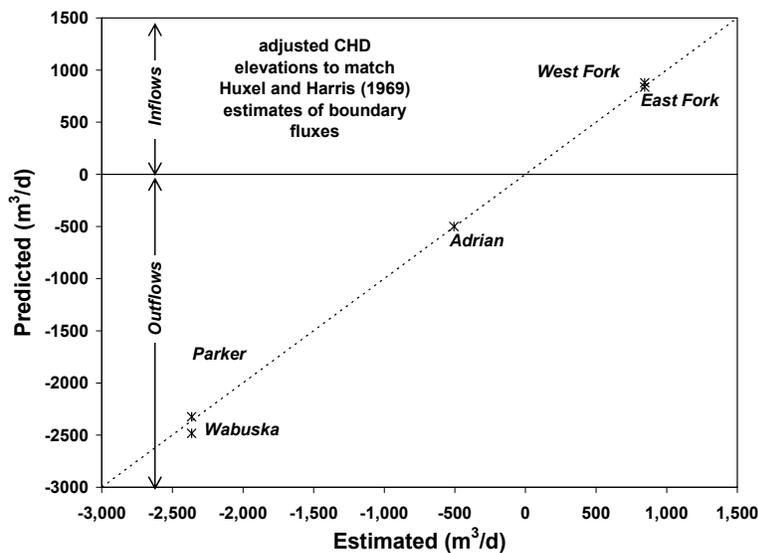
*Page 18: The model estimates spring runoff but didn't estimate any runoff for the 1997 flood and missed peaks for other events. Why?*

The models do estimate the 1997 peak runoff well for a few of the watersheds, however the single peak value is not well simulated for most. This is most likely due to issues related to the distribution of temperature from the observed locations to the individual modeling response units. The methodology for distributing temperature is identical to the method used to distribute precipitation; the PRISM long-term average monthly values over each 4km grid are used to distribute actual daily observations to other locations in the watersheds that don't have actual ground-based observations. Unfortunately, this relationship does not always work

well during extreme rain on snow events (i.e., a different spatial distribution of temperature may be occurring during these events). As a result, we are often forced to settle for poor model performance during these extreme events in order to simulate the remaining time periods reasonably well.

*Page 28: Revised estimates of basin outflow through Walker and Parker Gaps total 800 af/yr, 600 af/yr less than Huxel and Harris (1969; Lopes and Allander, 2009b, p. 32). Subsurface outflow also appears to be through the Wabuska lineament and, based on water levels in the area, is as much or greater than subsurface flow through the gaps. How do these revisions affect the model?*

Inter-basin groundwater flows beneath the river channel or through geologic gaps in the surrounding bedrock (i.e., Adrian, Wabuska, Parker Gap and East Gap) were modeled using MODFLOW's GHB package. The GHB elevations were adjusted to obtain observed boundary fluxes primarily from Huxel and Harris (1969) using MODFLOW's GBOB observation package with annual net inflow and outflow volumes provided in the report. The GBOB calibration, however, was done under steady state conditions in the 1960s. The result of the calibration is shown in the figure below. No observed estimates were ever given for the East Gap and so the East Gap was not used in the GBOB calibration.

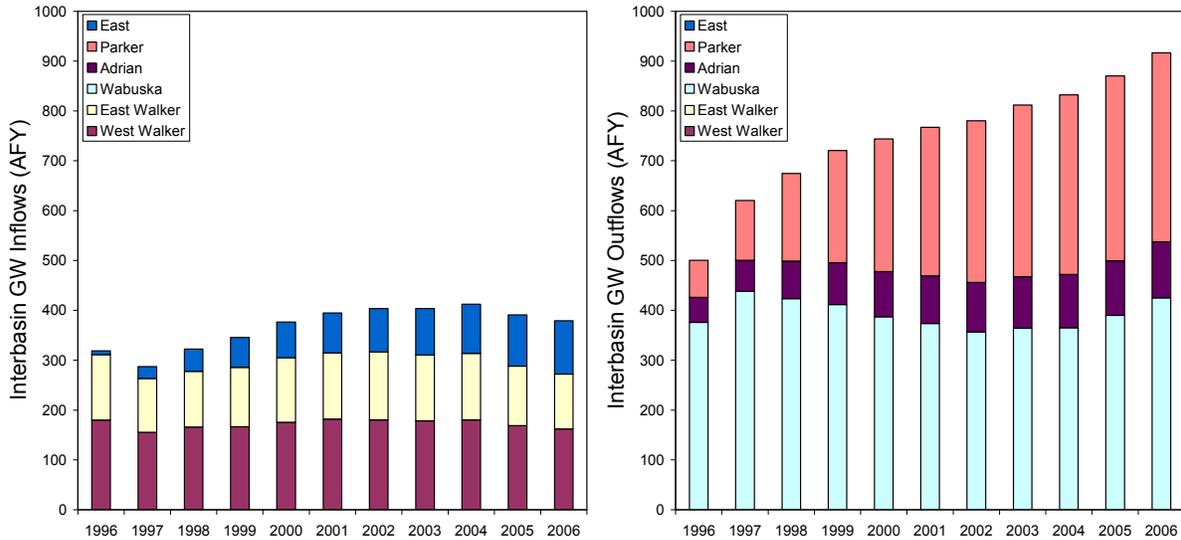


**Figure:** Predicted and observed interbasin groundwater flow for the 1960s steady state model. Quantities are in cubic meters per day NOT acre feet per year (AFY). For comparison computed fluxes in AFY are: Wabuska = -688, Parker = -735, Adrian = -148, West Walker = 248, East Walker = 260. Total In (without East Gap) = 508, Total Out (without East Gap) = 1571.

Since no flux was given for East Gap, water table elevations (estimates) from Huxel and Harris (1969) were used to establish the GHB elevation for this region.

Given the reviewers comments, we have rerun the transient model and tracked all interbasin groundwater flows during the entire transient run. See the figures below for annual input and output in AFY. A few points were discovered in this more

complete analysis. First, East Gap is contributing water and this may not be correct. Second, Parker losses grow over time and may not be in equilibrium. Finally, all flows are reduced compared to the steady state calibration. These issues will need to be investigated and addressed in the second phase of the project.



*Page 41: The SVGM was modeled as one layer. However, there are flowing wells in Smith Valley around the river and Artesia Lake so there must be confining layers. Well logs for the basin describe thick clay layers in the basin (Lopes and Allander, 2009a, p. 52). Is it appropriate to model this valley as one layer?*

Given the available information to describe the hydraulic properties of the aquifer, we felt it was appropriate to take a parsimonious approach. Therefore, we utilized one model layer, which thereby assumes no vertical flow. In reality there are likely numerous clay lenses, etc, that cause complex flow patterns within the subsurface, but these were of less interest to us as compared to developing an accurate water balance for the system. It is highly unlikely that a multiple layer model would yield substantially different results in terms of simulated return flows to the river.

*Page 47: The first sentence reads like regressions were done by Gallagher, not this study. Where are regression results? Others might want to use them.*

The existing sentence in the report:

“Regressions of streamflow and groundwater withdrawals (Gallagher, 2004) are developed for Smith Valley using Hoye gage data (1994 to 2003), and for Mason Valley using the combined Hudson and Strosnider gage data (1995 to 2002).”

was modified to read:

“Regressions are developed in Smith Valley using Hoye gage data (1994 to 2003) and groundwater withdrawals (Gallagher, 2004), and in Mason Valley using the combined Hudson and Strosnider gage data (1995 to 2002) and groundwater withdrawals (Gallagher, 2004).”

The regression equations were not included in the report but will be available by the authors on request.

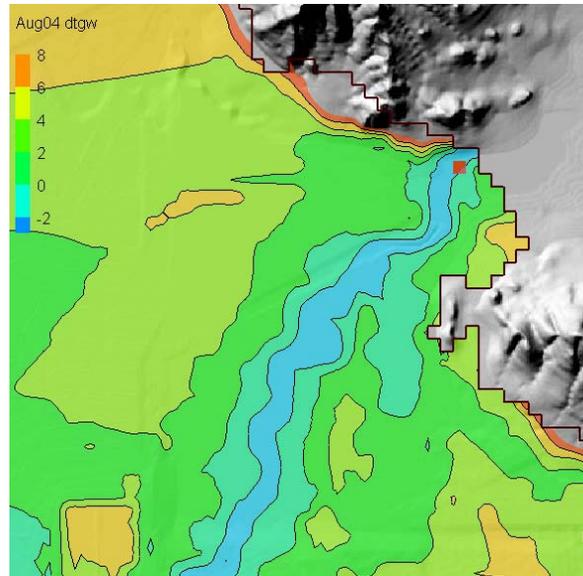
*Page 51: The Maxey-Eakin method was mis-applied. M-E method does not specify where recharge occurs; it is only a basin-wide estimate.*

We will look at more recent recharge estimates (i.e. USGS) during the second modeling phase. In reality the small amount of natural recharge does make much difference, except maybe in Smith Valley.

*Page 53: Where is East Gap? This is the first and only use of term in entire report. There could be outflow through Wabuska lineament, how would this affect the model? Water 10 ft below land surface at Wabuska seems too deep. This reach is gaining at times. Water 20 ft below land also seems too deep for Adrian, it's a discharge area for Mason V. Why is water at Hudson 10 ft deep for Mason but 8 ft for Smith?*

East Gap is marked in the map showing gages and gaps. It is to the south of Parker and constitutes a small alluvial gap along the eastern boundary of the modeled domain. GHB elevations at each location were calibrated to get estimated fluxes, except for the East Gap in which GHB elevations were taken from head maps in Huxel and Harris (1969). The Mason and Smith groundwater models were developed as separate entities and better correspondence between the separate models will be done in the second modeling phase.

Below is a map of modeled depth to groundwater for August 2004 in the Wabuska gage region. Positive depths are below ground surface and negative depths are above ground surface. Ground surface for the river is defined as the minimum elevation in the 100m-cell grid, while non-stream cells use an average elevation in the grid. The assumption is that water flows in the lowest elevation. As seen in this map, water elevations are about 2 m above land surface at Wabuska. Fluxes to and from the river are determined by the gradient between stream depths (as determined by the rating curves defining flow and depth) and groundwater. At the monthly scale, Wabuska is modeled as generally losing water.



*Page 53: What is 20% infiltration along major ditches based on? This seems high. If there is 57,500 to 70,000 acre-ft/yr of irrigation recharge (p. 28), that would mean diversions are about 290,000 to 350,000 acre-ft/yr.*

The local water district estimates that major delivery ditches lose 20 percent of the surface diversion to the groundwater via leakage. Calibration and sensitivity analysis were used to refine this preliminary estimate and found the model relatively insensitive to values of leakage below 30%. Therefore the initial estimate of 20% was maintained.

Ditch leakage is computed as 20% of the surface diversion for a given month along a given ditch with totals for the entire basin on the order of 16,000 AFY to 38,000 AFY. Groundwater recharge from excess irrigation water ranges from 35,700 AFY to 66,400 AFY. Combining ditch leakage with irrigation recharge produces annual volumes of 60,400 AFY to 99,400 AFY returned to the groundwater system. This net recharge is reasonable when compared to groundwater recharge volumes set forth by Huxel and Harris (1969) at 70,000 AFY. The Huxel and Harris (1969) recharge estimate occurred during early groundwater development when rates of withdrawal were low compared to modern-day volumes.

*Page 64: Total gw inflow seems high, up to 30,000 af/yr higher than previous high estimate. Groundwater levels have been going down in valley since 1960 so pumpage exceeds average inflow.*

The simulation period covers periods of both high and low flow periods. Total groundwater flow is highly dependent on river flow because of the linkages to irrigation and groundwater pumping. Previous studies in which water balance estimates were generated did not cover such a large range in river flows. In conclusion, we don't feel that the groundwater inflow estimate is necessarily high, but during phase II of the study we will be further refining these estimates.

*Page 64 Estimated recharge and riparian ET from surface-water budget is within this range, see Lopes and Allander (2009b).*

It is nice to know that our results fit well within the context of the recent work done by the USGS. Our report was completed in 2008 and the referenced document (Lopes and Allander (2009) was not available at that time. It would be easy for us to add a sentence to our report now to indicate that our results fit well with the referenced document, however, we would like some additional time to review the document and understand the results before citing the work. In addition, if we cited the work, others may assume we had access to other information the report and wonder why we did not make other comparisons, etc. As a result, we have chosen to not cite the recent work in our report but are grateful that the reviewer has identified the work and recognized the similarities in the results.

*Page 71: last paragraph. Gage 10293000 is downstream from Bridgeport Reservoir, not upstream. It's the east fork from Bridgeport to Mason Valley, not west.*

The current paragraph:

“The MODSIM model extent begins upstream of Bridgeport Reservoir, CA at USGS gage 10293000 on the East Walker River, and at Coleville, CA at USGS gage 10296500 on the West Walker River and continues downstream to Wabuska, NV, at USGS gage 10301500 (Figure 41). Agricultural demands in Antelope Valley, Smith Valley, Mason Valley, and on the West Walker River from Bridgeport to Mason Valley are represented in the model. A monthly time step is used for the model and all volumes are calculated in acre-feet. The model is calibrated over the period 1996 to 2006 and simulations cover the same period.”

was modified to read:

“The MODSIM model extent begins just below Bridgeport Reservoir, CA at USGS gage 10293000 on the East Walker River, and at Coleville, CA at USGS gage 10296500 on the West Walker River and continues downstream to Wabuska, NV, at USGS gage 10301500 (Figure 41). Agricultural demands in Antelope Valley, Smith Valley, Mason Valley, and on the East Walker River from Bridgeport to Mason Valley are represented in the model. A monthly time step is used for the model and all volumes are calculated in acre-feet. The model is calibrated over the period 1996 to 2006 and simulations cover the same period.”

*Page 73: How was equation 4 determined? Some explanation is needed.*

The fundamental operation of the MODSIM modeling software is based on the assignment and minimization of costs (or penalties for failing to match selected time series or targets). In order to simulate continuous streamflow in terms of water rights with associated priorities, MODSIM requires the conversion of water right priorities into a cost. The standard equation used in MODSIM (see referenced user manual) is given in equation 4. We have performed a variety of experiments to investigate the significance of this equation and have determined that the results in the Walker are not sensitive to the equation structure at all. In

fact, we could have just ranked the water right priorities from high to low using whole numbers and would have obtained the same results.

## **Project G: Economic Analysis of Water Conservation Practices for Agricultural Producers in the Walker River Basin**

*An important and challenging research project. It addresses a critical subject for the Acquisition Program. Data are analyzed in several ways that greatly contribute to the understanding of these crops, their production potential relative to applied water, their feasibility, and their risk profiles. It is clear that a tremendous amount of agronomic and statistical knowledge was required to conduct the study so comprehensively, using sophisticated tools. The project achieves what appears to be its stated objective “to determine the viability of these crops for both the region and the market.” (p.5)*

*The Abstract, Introduction, and Conclusions sections are very well written. The conclusions are well substantiated by the research results.*

*The following critiques of the paper are offered:*

*The paper seems mis-titled. The phrase “water conservation practices” may include crop switching but also implies a broad set of technological, behavioral, and managerial practices for growing existing crops with less water. A better title would be “Economic Analysis of Alternative Water Conserving Crops for the Walker River Basin” or simply “Economic Analysis of Alternative Crops for the Walker River Basin.”*

The title has been changed from the original project title to one recommended by the reviewer.

*The Analysis subsection of the Data and Methods section is very technical; it would be useful for most readers if it was explained more clearly.*

This section has been edited somewhat to delete repetitive information and run-on sentences. The technical portion is appropriate for such a research report such as this.

*A concern of the paper is the manner in which prices are handled.*

*The rationale for selecting the prices used for break-even yields and comparisons of net returns is not stated in the Yield Analysis section. More importantly, although the sets from which the relevant prices were chosen are made clear in a later section, neither a methodology for making the selections nor explicit justifications for the choices are provided. It appears that judgment by the researchers or by producer panels has been employed, rather than a systematic method. If that is the case, even if very good judgments were made, it calls into doubt the ability of the research results to be replicated scientifically. Clarification in the text should be added to discuss how methodology was developed and justification of choices used.*

All prices were taken from the enterprise budgets, which are five-year averages, when historical data were available. If historical data was not available, the prices

were chosen by producer panel. The text has been updated to discuss this and the enterprise budgets are now cited and also listed in the references.

*Similarly, no systematic method appears to have been used for determining the points of triangular price distributions that were created for most of the crops in the study. The approach seems inconsistent from one crop to the next. These inconsistencies are troubling because they raise doubt on the reliability of the risk analysis results that provide the foundation for the study's conclusions. Perhaps an explanation of the method used for determining the points of triangular price distributions would help clarify this issue.*

Historical data and that from the enterprise budgets were used. We have added citations for data and how or why used.

*A related issue is that formal citations are omitted for the sources of the specific prices.*

Sources or pricing have been added to the text.

*The time value of money is handled in a manner that is not explained. Because costs were adjusted to 2009 values by a producer cost inflation factor, it appears that net returns were intended to be expressed in 2009 dollars. However, the way in which price distributions are developed for almost all of the crops is not consistent with such an approach – prices from various years were used. If there are valid reasons to neglect making revenues and costs temporally consistent, the reasons should be made clear.*

The idea was to simulate through 2009, and in order to do so we adjusted cost of production by an inflation factor. There was no intention to express net returns in 2009 dollars.

*There are two other methodological issues that arose regarding the risk analysis:*

*For three of the crops, the yield analysis results from WinEPIC were adjusted to account for marketability. It appears that these adjustments were not carried forward to the risk analysis, but if so this should be made clearer.*

Yes, the marketable yields were used for the risk analysis. This is now mentioned in the analysis section.

*The risk analysis makes use of WinEPIC yield output for Dithod soil in Yerington. The reader would benefit from the authors' assessment of the extent to which this limits the applicability of the study results to other soils and to Smith Valley.*

Dithod soil was chosen due to its prevalence in Mason Valley, and Mason Valley (Yerington) was chosen as it is the largest agricultural producing area of the Basin. This is now mentioned in the text. This was done to show the risk and variability in the crops for illustration purposes. This analysis could have been done in another area or by all soils, but we do not believe it would have added much to the discussion. Additionally, we used yield response functions (adjusted for marketability) for each crop to build an analysis spreadsheet called WATER-ACIS for individual producers to use to analyze the options for their weather

station and soil type. This spreadsheet and user guide is publicly available at:  
<http://www.cabnr.unr.edu/curtis/Extension/Extension.html>.

*The attached word document contains comments covering the above critiques as well as more minor comments and suggested edits.*

Comments and edits have been adjusted in the text.

## **Project H: Formulation and Implementation of Economic Development Strategies**

*A very informative study. The stated purpose of the study “was to look at possible economic impacts in each of the sub-regions, and in particular to look at a several potential outcomes under various scenarios in areas targeted for acquisition of water and water rights, and then to identify potential economic development opportunities that might help mitigate any potential negative impacts.” This objective was achieved.*

*The study’s recommendations for economic development strategies, as summarized in the paper’s Executive Summary, seem well thought out, reasonable, and appropriate. The same is true for the recommendation on p.47 regarding funding and technical assistance for crop switching. It should also be included in the Executive Summary.*

*This review examined the summary level data pertaining to scenarios, economic impact examples, and the fiscal analysis. It did not examine the details of the analysis, such as those pertaining to individual crops and water right values. Based upon this level of review, the analysis and conclusions appear sound and reasonable. The only result that is of concern was the statement in the subsection on fiscal impacts to Lyon Co. that reads, “...total employment will likely be net unchanged...” (p.58). This over-simplification can be corrected easily with rewriting of a sentence or two. More minor comments (e.g. needs for clarification) and editing suggestions are contained in the accompanying Word file.*

*The Table of Contents is missing a couple of important subsections s following the one titled “Conclusions – Consequences to the agricultural economy of Mason Valley and Smith Valley.”*

A subsection heading was added – see below

*Page 3: Table of Contents should show where the Hawthorne area impacts are discussed (p.49)*

Added line in table of contents for Hawthorne area impact analysis (p.49)

*Page 7: Removed reference to acquiring water rights because acquisitions will involve leasing of water, not just outright purchase of water rights.*

Removed these references to water rights and used the term water instead.

However, later in the document there is reference to both water rights acquisition and leasing, so only made these changes in the paragraph indicated.

*Page 10: FYI, this may not be a good assumption. There are formidable water quality barriers that Homestretch may not overcome. Reclamation's EIS analysis did not assume the Homestretch option would be implemented in their analysis. Suggest maybe revising Example 3 to reflect a worst case scenario for impacts to ag (i.e. 100% ag water acquisition), and possibly both of the other examples, too.*

The assumption was based in part on a potential for treating the water to remove/reduce "contaminants", and some language was added to provide more about this assumption. Language was also added to address what the worst case economic impact might be if this were not possible.

*Page 11: Explain why not include market development for Great Basin wild rye? This would involve further market research and investigation of potential barriers to market growth, and potential promotion of the crop for appropriate uses such as revegetation.*

Teff and two-row malt barley provide the possibility of value-added processing and market development, while the Great Basin Wild Rye would not involve value-added processing – only market development. While this might help in converting farmers to growing Great Basin Wild Rye, it would not result in the sort of job creation that the value-added processing (teff and two-row malt barley) accomplishes.

*Page 12: Homestretch already produces power from the geothermal source water. I interpret the original reference to geothermal water to mean post-power production effluent and the edit is intended to make this more clear.*

Suggestion implemented.

*Page 12: Homestretch's main constituent of concern to NDEP and the Tribe is fluoride.*

Suggestion implemented.

*Page 17: Explain why East Walker area not included?*

The Walker River Chronology published by the Nevada Division of Water Resources ([http://water.nv.gov/Water\\_Planning/walker/walker1.cfm](http://water.nv.gov/Water_Planning/walker/walker1.cfm)) clearly shows in Table 2 (p.6) that there is no major agricultural area in the basin on the East Walker River in Nevada. The significant agricultural land on the East Walker is in California above Bridgeport.

*Page 41: Footnote b needs to be modified to reflect current table numbering. The Net Econ Impact box for Scenario 4 needs attention, too.*

Table 17 footnote b was changed, as were footnotes to tables 11, 12, 13, 15 and 16.

*Page 47: This excellent recommendation needs to be included in the Executive Summary.*

This comment, in a slightly modified version, was incorporated into the Executive Summary (p. 10).

*Page 49: Text added noting the similarities of Figure 7 to graphs of discharge at Wabuska and Walker lake levels. Alternatively, consider inserting an appropriately scaled graph similar to Figure 6 in Adams and Chen's paper and letting the reader see for him/herself.*

Change made.

*Page 52: Please provide perspective. Minimal compared to what? 30 jobs is approximately equivalent to 1/8 of Mineral Co. unemployment.*

“Lost jobs” do not automatically become “jobs regained” after more water begins flowing into Walker Lake. Assuming the project is successful in delivering an additional 50,000 AF annually to the Wabuska Gauge, we have no indication as to how long it may take to restore the fishery to the point where the approximately 30 lost jobs might be recovered. A modifier was added to this sentence.

*Page 52: The wording in the last two sentences has been changed so as not to imply that existing reservoir management practices necessarily will change. It is thought by those working on the Acquisition Program that upstream reservoir operation is not going to change. Water will be released as before, probably during the ag season, and will just go farther downstream.*

Suggestion implemented.

*Page 54: Is this the way that NV counties refer to property taxes? Not sure what this means. Clarify?*

This is the way that fiscal analysts who do fiscal studies for Nevada Counties refer to property taxes.

*Page 57: Including school districts and cities in the county?*

The sales tax numbers only reflect money coming to the county, not other entities such as school district, cities, etc. This was because the region of concern is not within any other cities or entities that receive sales tax. As far as the school district is concerned, the nature of the school funding formula means that they are unaffected by sales tax fluctuations which are made up by state revenues.

*Page 57: This is equivalent to a combined local sales tax rate of approximately 3.8%. How does this reconcile with Table 26?*

The sales taxes received average 3.8% because they reflect several different taxes (BCCRT, SCCRT, option, etc) which are shared in different proportions with different overlapping entities.

*Page 58: Any explanation for why Mineral Co. sales tax receipts are up when Lyon Co's are way down?*

Explanation added.

*Page 59: This overstates the case per Table 21.*

Sentence changed to rectify.

*Page 59: How about “these workers and their families” instead of “these people,” since the prior reference was to “temporary populations”?*

Language changed, but most of the workers do not bring their family members, per discussions with John Snyder and with other farmers.

*Page 59: Do you have a citation for this?*

The Snyders and Peri Brothers, the two largest onion growers who employ the greatest number of migrant workers, both provide housing for the migrant workers.

*Page 59: Why? Please elaborate. I could imagine that a concentration of newly unemployed migrant workers and their families might need social services from local govt just when those services are already strained.*

According to John Snyder, the migrant workers come specifically for the work they are hired to do and leave when the work is done. Sniders have been hiring the workers from the same community (and from the same families) in Mexico for decades, and this is the pattern.

## **Project I: Development of a GIS Database in Support of Water Right Acquisitions in the Walker Basin**

The review comments and edits were very helpful, and we appreciate the time the USGS took to review the project report.

Major Comments summarized in USGS Memorandum:

*The Landsat section needs to include path, row, and scene data. It is unclear from the report what scenes were used and how they were manipulated, with the exception of the NDVI development.*

Path, row, and descriptive scene data for the eight Landsat scenes used have been added to the Landsat Thematic Mapper (TM) satellite imagery section.

*Throughout the various sections of the report, the authors tend to wander and describe how the data was used. This should be limited to the data use section and/or to the individual project reports. For example, how the Decision Support Tool Project (DST) used or requested data is mentioned in several sections. As a reader, this was distracting and diverted attention away from the report objective of describing the data and the data base development.*

References to how the data were used by various other projects has been minimized in each of the data description sections. Full data use descriptions have been placed in the data use section of the report.

*General comments and edits as marked in the body of the report*

Comments and edits marked in the report were addressed as deemed appropriate. Most of the edits were incorporated and many of the comments were addressed in the appropriate sections.