TO: Governance Committee (GC)
FROM: Executive Director's Office (EDO)
RE: Peer Review of Lower Platte River Stage Change Study
DATE: June 5, 2012

GC Decision

The EDO requests a formal GC decision regarding the Lower Platte River Stage Change Study Report and the associated Peer Review. Three options are presented for GC consideration:

1) Accept the Peer Review and the Stage Change Study as final without revisions, as per the Technical Advisory Committee (TAC) Motion (described below); or

2) Revise the Stage Change Study as per the EDO/Contractor responses to each peer review comment and then consider the Peer Review and Stage Change Study as final; or

3) Revise the Stage Change Study as per the EDO/Contractor responses to each peer review comment and then re-submit the revised report for additional peer review as per recommendations contained in the Minority Opinion to the TAC Motion (described below).

Options #2 and #3 would require budget shifts in the PRRIP FY 2012 Budget because no funds are approved or allocated for re-hiring the Stage Change Study contractor team to make revisions. The contractor team is now estimating the cost for revising the Stage Change Study according to the peer review comments and an estimated cost may be available for discussion during the GC meeting. There is approved and available funding in PRRIP FY 2012 Budget Line Item PD-3 ("AMP & IMRP Peer Review") for an additional peer review of the Stage Change Study as described in Option #3 if so directed by the GC. The process of re-initiating agreements with the five peer review panelists, having Atkins coordinate the review, conducting the review, evaluating the new peer review comments, and determining next steps would take considerable time, likely pushing conclusion of the process into 2013.

Technical Advisory Committee (TAC) Action & Motion

Peer review of the PRRIP Lower Platte River Stage Change Study was conducted in 2011 according to a Program-approved Scope of Work (**Exhibit A**). Results of the peer review were received in October 2011 (**Exhibit B**). The TAC first discussed the peer review results and the EDO/Contractor responses to each peer review comment (**Exhibit C**) on November 30, 2011 and again on April 18, 2012. At the April 18 meeting, the TAC approved the following motion:

The Technical Advisory Committee moves to recommend the Governance Committee accept the Stage Change Study Peer Review and the Stage Change Study as final without revisions.

That motion was not approved unanimously by the TAC. The U. S. Fish and Wildlife Service submitted the following Minority Opinion to the Motion the EDO on June 4, 2012:

Lower Platte River Stage Change Study (LPRSCS) Peer Review – TAC Minority Opinion

A motion was made and seconded in the Technical Advisory Committee (TAC) which recommended Governance Committee (GC) approval of the Lower Platte River Stage Change Study and the peer review report as final documents without revision. The TAC representative for the U.S. Fish and Wildlife Service (Service) voted in opposition to the motion. The Service supports the peer review comments and the peer review report. However, the Service suggests that the LPRSCS report would need to be edited to

adequately address peer review comments. Three of the five peer reviewers of the study provided the recommendation to accept the stage change study with revisions. The peer review comments imply that revisions were important to the scientific process, and study shortcomings were sufficient to preclude general acceptance of the study. The Executive Director Office provided TAC members with the LPRSCS author's responses to the peer review comments during a November, 2011 meeting. The author's responses to the peer review comments included 43 instances where editorial changes were needed and dozens of instances where major revisions may be needed to address peer review comments. Selected peer review summaries included in this document further characterize the revisions needed for peer review acceptance. The TAC motion to accept the LPRSCS report precludes editorial and major revisions to the report.

The Service did not support the TAC motion because the motion does not reflect standards practiced by the scientific community. Any manuscript submitted to a scientific journal would be required to address peer review comments prior to acceptance by the journal for publication. This same standard should be applied to program research. In absence of revisions, one peer reviewer stated that the study should be characterized as qualitatively correct which implies that the current study may be quantitatively incorrect (i.e., major revisions needed).

The Service TAC members provide the following recommendations for finalizing the LPRSCS report:
1. Allow the LPRSCS authors to revise the report to address peer review comments.
2. Allow for a second round of peer review with reviewers that recommended "accept report with revisions".
3. The revised LPRSCS report and the peer review report should be provided to peer reviewers for the second round of reviews.
4. Peer reviewers will provide the recommendation to: accept report, accept report with revisions, or deem report unacceptable.
5. If a peer reviewer recommends accepting the report with revisions, then peer reviewer must specify what modifications/deletions to the report manuscript would be needed for acceptance (review comments should avoid requests for additional studies or modifications to methods).

Selected Peer Review Summaries:

Dr. David Gaeuman

Is the Stage Change Study sufficient to determine if First Increment Program water activities can be detected (statistically significant beyond the error of the gauging equipment) from base flow conditions?
No. A better evaluation of gaging errors is needed, as described in my comments above. I would also suggest that the idea of detectability be better defined. It seems that for a small water augmentation to be detected, one would have to know what the discharge would have been without the augmentation. How would the work? And what is the time scale over which the detection should occur? Detecting a small change on a particular day is a different matter than detecting a sustained small change over a month or a year.

Statistical design and analyses: Are they appropriate and correct? Can the reader readily discern which measurements or observations are independent of which other measurements or observations? Are replicates correctly identified? Are significance statements justified?
There is little in the way of formal statistics in this study. An instance in which error margins on gage records may be misinterpreted is pointed out in my comments above.

Recommendation **–** If this were a draft to be revised I'd recommend major revision. But it seems to be a final report, so my recommendation is to accept its general conclusions as being qualitatively correct.

Dr. Christopher S. Guy

Statistical design and analyses: Are they appropriate and correct? Can the reader readily discern which measurements or observations are independent of which other measurements or observations? Are replicates correctly identified? Are significance statements justified?

This is the major shortcoming of the study. That is, I believe the measurements for most analyses are not independent (i.e., true replicates). I would encourage the authors to clarify their experimental units and replicates and explain how they are relevant to the inference space described in the RFP.

Are the findings of the stage change study and the conclusions reached in the report supported by the data and analysis?

In general, I believe the conclusions are supported by the data, although the conclusions are not clearly articulated. I am concerned that most of the analyses and measures of variation represent pseudo-replication. This relates to my comments in the first question. I believe the best way to determine the effects of Program water activities on physical parameters that are thought to be of significance to pallid sturgeon would be to conduct the Stage Change Study in multiple reaches (i.e., the reaches are the experimental unit). Although one could argue that reaches are not independent, I surmise that it better represents available habitat for pallid sturgeon and the influence of Program water activities on that habitat. The most important aspect of having multiple reaches is that one will have a better understanding of the uncertainty of Program related water activities on pallid sturgeon habitat.

Dr. Dennis R. Helsel

Does the Stage Change Study adequately address the overall objective of the RFP, which is "…to develop information needed to evaluate the effects of Program water management activities, including new activities covered by state or federal depletion plans, on water stage and how those stage changes affect physical parameters in the reach of the lower Platte River from the Elkhorn River confluence to the Missouri River confluence?"

The method for extrapolation of missing record to the Loup River at Columbus is flawed, and so the resulting errors on the analysis are unknown.

Are the physical parameters and measured data considered in the study (flow quantity, depth, velocity, temperature, turbidity, sediment, and sandbars and bedforms at selected sites throughout the study reach) adequate and scientifically defensible for the purposes of the study?

The data themselves are presumably scientifically defensible. They are fairly routine parameters with established protocols for collection. The amount of data is adequate. Analysis of the data is not adequate, if the purpose is to determine whether proposed flow augmentation and withdrawals for storage will significantly affect those parameters.

If "yes" to Question #4 above, is the Stage Change Study sufficient to detect if First Increment Program water activities have an impact (statistically significant beyond the error of the gauging equipment) on stage, velocity, temperature, turbidity, substrate, or channel morphology?

No. Determination of differences in water quality parameters using Analysis Of Variance is flawed because the serial correlation in the data was not accounted for. The current analysis is not sufficient to determine whether there are significant impacts for these parameters.

Are the findings of the stage change study and the conclusions reached in the report supported by the data and analysis?

The Study's conclusions in regards to flow are supported by the data and analysis. The conclusions in regards to water quality parameters are not. The conclusions in regards to effects on habitat are beyond my area of expertise, but appear to be the most thoroughly supported portion due to the modeling work.

PLATTE RIVER RECOVERY IMPLEMENTATION PROGRAM

EXHIBIT A

STAGE CHANGE STUDY PEER REVIEW SCOPE OF WORK

PLATTE RIVER RECOVERY IMPLEMENTATION PROGRAM
Scope of Work
Lower Platte River Stage Change Study Peer Review

Purpose of Peer Review

The Lower Platte River Stage Change Study was completed in early 2010 by a contractor team led by HDR pursuant to the Platte River Recovery Implementation Program ("Program" or "PRRIP") RFP (Request for Proposals) dated 12/10/2007 (Attachment 1). The purpose of this peer review is to provide independent review of the stage change study to determine if it satisfies the objective(s) of the RFP and withstands scientific and technical scrutiny.

The purpose of the stage change study is to serve as a tool to assist the Governance Committee (GC) in determining the effect of "Program related flow effects", if any, over time on lower Platte River stage and associated parameters thought to be of significance to pallid sturgeon. The stage change study was not intended to define lower Platte River pallid sturgeon "habitat", evaluate the quantity or quality of pallid sturgeon habitat in the lower Platte River, or document or evaluate use of habitat by pallid sturgeon in the lower Platte River.

For the purposes of the stage change study, the spatial scale of the lower Platte is the "associated habitat" for pallid sturgeon. As defined by the Program, the associated habitat is the reach of the lower Platte River from its confluence with the Elkhorn River downstream to its confluence with the Missouri River (mouth of the Platte River).

Scope of Work

Each Peer Review Panel member will be tasked with reviewing the Stage Change Study from their particular area of expertise following the PRRIP Peer Review Guidelines for Reports & Studies (Attachment 2). Peer reviewers will be asked to submit all comments, questions, and other communication in writing to ensure an appropriate record is built, and all communication with peer reviewers will be conducted via e-mail. Peer Review Panel members will be provided with the following information:

- Lower Platte River Stage Change Study Peer Review Scope of Work (PRRIP)
- Final Lower Platte River Stage Change Study, including all appendices and figures (HDR)
- Final Stage Change Study Protocol Development Report (HDR)
- Final PRRIP Stage Change Study RFP (PRRIP)
- PRRIP Peer Review Guidelines for Reports & Studies (PRRIP)
- Additional information as requested by Peer Review Panel members – if a document(s) is requested by one member, it will be transmitted to all members simultaneously

Specific Questions

Review of the Stage Change Study should address the following specific questions:

1) Does the Stage Change Study adequately address the **overall objective** of the RFP, which is *"…to develop information needed to evaluate the effects of Program water management activities, including new activities covered by state or federal depletion plans, on water stage and how those stage changes affect physical parameters in the reach of the lower Platte River from the Elkhorn River confluence to the Missouri River confluence"*?

2) Are the physical parameters and measured data considered in the study (flow quantity, depth, velocity, temperature, turbidity, sediment, and sandbars and bedforms at selected sites throughout the study reach) adequate and scientifically defensible for the purposes of the study?

3) Are the habitat classifications considered in the study (slackwater, flat, riffle, run, isolated pool, and plunge) adequate and scientifically defensible for the purposes of the study?

4) Is the Stage Change Study sufficient to determine if First Increment Program water activities can be detected (statistically significant beyond the error of the gauging equipment) from base flow conditions?

5) If "yes" to Question #4 above, is the Stage Change Study sufficient to detect if First Increment Program water activities have an impact (statistically significant beyond the error of the gauging equipment) on stage, velocity, temperature, turbidity, substrate, or channel morphology?

6) Are the findings of the stage change study and the conclusions reached in the report supported by the data and analysis?

If the answer to any of the questions above is "no", please suggest possible remedies to data collection methodologies, analysis, or other study tasks.

General Comments

Review of the Stage Change Study should also address more general comments and questions as outlined in the PRRIP Peer Review Guidelines for Reports & Studies. Please refer to Attachment 2 for information regarding these guidelines.

Peer Review Panel

The stage change study will be the first Program document peer reviewed in 2011. Potential reviewers will be screened and recommended by PBS&J. The GC will ultimately approve the members of the Peer Review Panel, but certain areas of expertise are considered essential for representation on this panel:

- Pallid sturgeon ecology (prefer experience with fish habitat modeling)
- Riverine physical processes/geomorphology
- River engineering and hydraulic modeling
- Hydrology and hydrologic analysis
- Ecological statistics

Budget Implications

Each Peer Review Panel member receive a stipend of **$5,000** for a total of **$25,000** (5 panel members X $5,000/each). Stipends will be paid from the PRRIP FY 2011 Budget Line Item *PD-3: AMP & IMRP Peer Review*.

ATTACHMENT #1

LOWER PLATTE RIVER STAGE CHANGE STUDY RFP

PLATTE RIVER RECOVERY IMPLEMENTATION PROGRAM REQUEST FOR PROPOSALS

SUBJECT: Lower Platte River Stage Change Study
REQUEST DATE: December 10, 2007
CLOSING DATE: January 18, 2008
POINT OF CONTACT: Chad Smith – Executive Director's Office
Platte River Recovery Implementation Program
6512 Crooked Creek Drive
Lincoln, Nebraska 68516
(402) 261-3185
smithc@headwaterscorp.com

RECITALS

The Governance Committee of the Platte River Recovery Implementation Program (Program) submits this Request for Proposals (RFP) to solicit proposals from contractors to develop and implement a protocol for a lower Platte River (Nebraska) stage change study. The protocol will be used to define the final scope and budget for the stage change study, but proposals submitted in response to this RFP need to provide enough detail on the overall project to convey an understanding of the stage change study. The results of the study will serve as a tool for the Governance Committee to assist in determining the effects of flow changes over time on river stage and associated physical parameters thought to be of significance to pallid sturgeon (*Scaphirhynchus albus*).

In responding to this RFP, the Governance Committee requests the following information:

1) **Scope of work** for completing this project. Prospective contractor should address the tasks outlined herein.

2) **Detailed schedule** for completing each task in the preliminary scope. The following are the critical dates for the Governance Committee's preferred schedule for the project:

February 15, 2008	Protocol draft for Governance Committee review
March 31, 2008	Final Protocol/ Notice to Proceed with Protocol Implementation
September 30, 2008	Draft of First Progress Report on field work activities
December 31, 2008	Draft of Second Progress Report on field work activities
July 30, 2009	Complete field portions of study as defined in the Scope of Work
September 30, 2009	Submit draft report and other materials for review
December 31, 2009	Final Report

Prospective contractors should address their capability to comply with the above schedule. If it is deemed that the above critical dates should be revised, prospective contractors should offer alternative schedules describing the logic and reasons for the alternative.

3) **Conflicts of Interest Statement** addressing whether or not any potential conflict of interest exists between this project and other past or on-going projects, including any projects currently being conducted for the Program.

4) **Detailed cost not to exceed proposal** to complete the project, separated into protocol development and protocol implementation. The proposal should identify costs and hours allocated for each task in the scope of work and the total cost for the study. Hourly rates and reimbursable expenses for the proposing firm/individual and any sub-contractors must be attached to the detailed price proposal. The contract will be awarded on a Cost Not to Exceed basis. The initial contract will be for protocol development. Governance Committee approval is needed before the contractor is authorized to begin protocol implementation.

5) **List of relevant project experience** within the past five (5) years, including name, location and brief description of the projects; name, address and phone number of the contracting officer for the client; and identification of key participants and their tasks on previous projects who would also be working on this study.

6) **Resumes** of key participants and subcontractors proposed for this study. The resumes should address experience on projects similar to this stage change study. Types of expertise that may be appropriate include familiarity with pallid sturgeon biology and the key physical parameters, river hydraulics and hydrology, the lower Platte River, and river monitoring and research techniques.

7) **Description of Insurance** shall be provided with the proposal. Proof of insurance will be required before a contract is issued. Minimum insurance requirements will include $1,000,000 general liability per occurrence. To the extent authorized by law, the contractor shall indemnify, save, and hold harmless the Nebraska Community Foundation; the states of Colorado, Wyoming, and Nebraska; the Department of the Interior; members of the Governance Committee; and the Program Executive Director's Office, their employees, employers, and agents; against any and all claims, damages, liability, and court awards including costs, expenses, and attorney fees incurred as a result of any act or omission by the contractor or its employees, agents, subcontractors, or assignees pursuant to the terms of this project.

8) **A pre-bid meeting** of interested parties will be held to address questions associated with this Request for Proposals at a time and location that will be set by the Program's Executive Director's Office.

Please submit one electronic copy of your proposal in PDF format by January 18, 2008 to Chad Smith at *smithc@headwaterscorp.com*.

Terms and Conditions: The selected contractor will be retained by:

Nebraska Community Foundation
650 J Street, Suite 305
PO Box 83107
Lincoln, NE 68501

Terms and conditions will be negotiated as mutually agreeable. It is understood that the right is reserved by the Governance Committee to accept any proposal that, in its judgment, is the best proposal, and to waive any irregularities in any proposal.

Proposal Costs: Proposal costs incurred in response to this RFP will be the responsibility of the bidder. Neither Nebraska Community Foundation nor the Governance Committee will be liable for any costs incurred by the bidder in the completion and submission of the proposal.

Point of Contact: Questions regarding this RFP that could impact budget estimates or scope of services should be e-mailed to Chad Smith at *smithc@headwaterscorp.com*. Questions and responses will be provided by e-mail to all bidders.

SCOPE OF WORK FOR CONTRACT SERVICES

INTRODUCTION

The Platte River Recovery Implementation Program (Program) was initiated on January 1, 2007 between Nebraska, Wyoming, and Colorado and the Department of the Interior to address endangered species issues in the central and lower Platte River basin. The species considered in the Program, referred to as "target species", are the whooping crane, piping plover, interior least tern, and pallid sturgeon.

A Governance Committee has been established that reviews, directs, and provides oversight for activities undertaken during the Program. The Governance Committee is comprised of one representative from each of the three states, three water user representatives, two representatives from environmental groups, and two members representing federal agencies. The Governance Committee has named Dr. Jerry Kenny to serve as the Program Executive Director. Chad Smith, representing the Program Executive Director's Office, will be the primary contact for prospective contractors responding to this RFP.

NEEDS AND SCOPE

The overall objective of the study is to develop information needed to evaluate the effects of Program water management activities, including new activities covered by state or federal depletion plans, on water stage and how those stage changes affect physical parameters in the reach of the lower Platte River from the Elkhorn River confluence to the Missouri River confluence. **The physical parameters to be considered include flow quantity, depth, velocity, temperature, turbidity, sediment, and sandbars and bedforms at selected sites throughout the study reach, and over the range of discharges which are important in determining these parameters.**

In accordance with the Program's Adaptive Management Plan (AMP), the study should provide sufficient data to evaluate the effect of changes in river stage over a range of flows on a micro, meso, and macro scale. The following example is provided to help define these terms and provide a framework for the range of flows to be considered and the interval measurements that need to be made:

River Gage:	Louisville, NE (Station ID 06805500)
Range of River Flows:	5,000 cfs to 39,000 cfs (bankfull flows)
Precision Level:	90% confidence
Possible Measurement Interval:	Every 1,000 cfs (roughly 0.1 foot of stage change)

In responding to this RFP, potential contractors should provide information on the needed methods to obtain this data, the appropriate discharge/stage measurement intervals necessary for achieving the desired level of precision, and the efficacy of applying these methods over a larger range of flows. In addition, potential contractors can use guidance provided by the Program's Final Environmental Impact Statement (EIS) and the Final Biological Opinion (BO) to better understand the types of flow changes of concern and the related impact on the identified physical parameters. Copies of the relevant sections of the Final EIS and Final BO can be downloaded from the Program Web site (www.PlatteRiverProgram.org) or obtained from Chad Smith.

Given this framework, the study should provide information sufficient to estimate changes in the physical parameters identified above, across the identified range of flows and the three scales of measurement intervals, that occur during the study period and as can be determined from historic information. The intent should be to draw inferences to the types of process changes that would occur in the system as a result of river stage changes.

a. Information will be sufficient to determine if Program water activities can be statistically identified (significant beyond the error of the gauging equipment) from base flow conditions (AMP Hypothesis X-Y Graph PS-2).

b. Information will be sufficient to detect if Program water activities have a statistically significant impact on stage, velocity, temperature, turbidity, substrate, or channel morphology (AMP Hypothesis X-Y Graphs PS-3, PS-4, PS-6, PS-9).

This includes an emphasis on floodplain connectivity and the inundation of otherwise terrestrial habitat (not out of the high banks), and how both of these factors vary with flow.

Proposals should include the scope, timeline, and budget for developing a detailed protocol for estimating the effects of stage change on the identified physical parameters. The protocol will be reviewed by a selected Program sub-group before being finalized. The final protocol will be in sufficient detail to identify all aspects of data collection, analysis, reporting, and deliverables for the overall project. The final protocol and detailed budget estimate for actual implementation of the protocol will be provided to the appropriate Program sub-group and/or Advisory Committee for review. Approval of the protocol and budget by the Governance Committee is needed prior to the contractor proceeding with protocol implementation.

AVAILABLE INFORMATION

In addition to the Program Document and its AMP (Attachment 3), several additional sources of information are available to assist potential contractors in responding to this RFP. Many of these documents can be accessed either from the Program Web site (www.PlatteRiverProgram.org) or by contacting the originating party or Chad Smith.

1) In the late 1980's, the Nebraska Game and Parks Commission (NGPC) recorded transect information on sections of the lower Platte River for use with Instream Flow Incremental Methodology analysis. While this information may no longer be current, it allows historical comparisons in some stretches of river.

2) Multiple cross-sectional transect data collected by Mussetter, Inc. as part of a NGPC evaluation of the Sarpy County/Clear Creek Levee project.

3) Cumulative Impact Study for the Lower Platte River Corridor Alliance that may have some overlap with this study. The information includes a digitized series of aerial photographs of the lower Platte reach, and a GIS database covering a decadal time-step.

4) Several reports from Drs. Ed Peters and Jim Parham on pallid sturgeon use of the lower Platte River: one submitted to NGPC for a Federal Aid to Sport Fish Restoration Grant; one submitted to the Pallid Sturgeon/Sturgeon Chub Task Force; and one in press as a NGPC Technical Report.

5) Relevant sections of the Program's Final EIS and Final BO.

6) Completed and ongoing, studies conducted by the U.S. Geological Survey and other partners related to pallid sturgeon use of the Missouri River.

7) The National Research Council's report titled "Endangered and Threatened Species of the Platte River".

DELIVERABLES

The first project deliverable will be a draft protocol (see above discussion). The protocol will be reviewed and revised, as needed, before being finalized. Once approved, the protocol will be implemented as agreed upon by the contractor and the Governance Committee. Future deliverables will be clearly identified as one of the items in the protocol. It is anticipated that progress reports will be provided along with a final report. Other deliverables will include any raw data, models, and other documents or materials collected and/or developed as a part of the study. Data will be reported in accordance with guidelines outlined in the Program's AMP and the Program's Database Management System.

ATTACHMENT #2

PRRIP PEER REVIEW GUIDELINES FOR REPORTS & STUDIES

PLATTE RIVER RECOVERY IMPLEMENTATION PROGRAM
Excerpt from PRRIP Peer Review Guidelines – Reports & Studies

Instructions to Peer Reviewers

Thank you for agreeing to review this product. The following is a summary of expectations for peer-review and the topics that we wish each peer reviewer to address.

A. INDEPENDENCE OF A PEER REVIEW

Peer-review must provide an unbiased opinion of the scientific quality of a product (proposal, report, data, map, etc.) by individuals who are independent from the authors and external to them and their institution. A review must be independent of various types of conflicts of interest with the author(s) and with the product under review. The Platte River Recovery Implementation Program (Program) places considerable reliance on the objectivity, integrity, and professionalism of each peer reviewer to provide technical opinion of each product without bias or conflict of interest.

Please review each question about your bias or independence. Your peer-review will be anonymous to the author unless you choose to share it. Your review will be held in the file for the Program as documentation of the peer-review process for this product.

YOUR CONSIDERATIONS SHOULD INCLUDE THE FOLLOWING FACTORS THAT COULD LEAD TO BIAS OR CONFLICT OF INTEREST:

- Financial interest in the product or the author(s);
- Familial relationship with the author(s);
- Bias, for personal reasons, for or against the author(s) or institutions of this product;
- Professional connection (current or former: student or advisor, supervisor or supervised, employer, etc.) to the author(s) or the institution of this product;
- Organizational affiliation (same agency, department, organization, business, etc.);
- Impacts of lobbying or political pressure exerted by persons looking for a particular result or more work in the area of this product;

IF YOU FEEL THAT YOU CANNOT PROVIDE AN UNBIASED REVIEW, PLEASE DO NOT REVIEW THIS PRODUCT AND IMMEDIATELY RETURN THE DOCUMENT TO THE PROGRAM'S EXECUTIVE DIRECTOR.

CONFIDENTIALITY – The enclosed manuscript is a privileged communication. Please do not show it to anyone or discuss it, except to solicit assistance with a technical point. Your review and your recommendation should also be considered confidential.

TIMELINESS – In fairness to the author(s) and to the needs of the Program, please return your review within __ days. If it seems likely that you will be unable to meet this deadline, please return the manuscript immediately or contact the Executive Director.

CONFLICTS OF INTEREST – Please review "Independence of a Peer-Review" above. If you feel you might have any difficulty writing an objective review, please return the paper immediately, un-reviewed. If your previous or present connection with the author(s) or an author's institution might be construed as creating a conflict of interest, but no actual conflict exists, please discuss this issue in the cover letter that accompanies your review.

YOUR REVIEW SHOULD ADDRESS THE FOLLOWING:
What is the major contribution of this document? What are its major strengths and weaknesses, and its suitability for publication and/or use by the Program? Are conclusions based on sound scientific methods and reasoning? Please include both general and specific comments bearing on these questions and emphasize your most significant points.

General Comments:
1. Scientific soundness
2. Organization and clarity
3. Conciseness
4. Degree to which conclusions are supported by the data
5. Cohesiveness of conclusions

Specific Comments:
Please support your general comments with specific evidence and literature. You may write directly on the manuscript, but please summarize your handwritten remarks separately. Comment on any of the following matters that significantly affected your opinion of the manuscript:

1. Presentation: Is a tightly reasoned argument evident throughout? Does the manuscript wander from the central purpose?

2. Methods: Are they appropriate? Current? Described clearly and with sufficient detail so that someone else could repeat the work?

3. Data presentation: When results are stated in the text of the manuscript, can you easily verify them by examining tables and figures? Are any of the results counterintuitive? Are all tables and figures clearly labeled? Well planned? Too complex? Necessary?

4. Statistical design and analyses: Are they appropriate and correct? Can the reader readily discern which measurements or observations are independent of which other measurements or observations? Are replicates correctly identified? Are significance statements justified?

5. Conclusions: Has the author(s) drawn conclusions from insufficient evidence? Are the interpretations of the data logical, reasonable, and based on the application of relevant and generally accepted scientific principles? Has the author(s) overlooked alternative hypotheses?

6. Errors: Point out any errors in technique, fact, calculation, interpretation, or style.

7. Citations: Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in the manuscript?

FAIRNESS AND OBJECTIVITY

If the research reported in this paper is flawed, criticize the science, not the scientist. Harsh words in a review will cause the reader to doubt your objectivity; as a result, your criticisms will be rejected, even if they are correct!

Comments should show that:

1) You have read the entire manuscript carefully.
2) Your criticisms are objective and correct, and are not merely differences of opinion, and are intended to assist the author in improving the manuscript.
3) You are qualified to provide an expert opinion about the research reported in this manuscript.

ANONYMITY

You may sign your review if you wish. If you choose to remain anonymous, avoid comments to the authors that may serve as clues to your identity, and do not use paper that bears the watermark of your institution.

RATING:

Please score each aspect of this manuscript using the following rating system: 1=excellent, 2=very good, 3=good, 4=fair, 5=poor.

	Rating
Scientific soundness	___
Degree to which conclusions are supported by the data	___
Organization and clarity	___
Cohesiveness of conclusions	___
Conciseness	___
Importance to objectives of the Program (For use by internal review panel only)	___

RECOMMENDATION	**(check one)**
Accept	___
Accept after revision	___
Unacceptable	___

PLATTE RIVER RECOVERY IMPLEMENTATION PROGRAM

EXHIBIT B

STAGE CHANGE STUDY PEER REVIEW RESULTS

Platte River Recovery Implementation Program
Lower Platte River Stage Change Study Peer Review

SUMMARY REPORT

I. Introduction

The Lower Platte Stage Change Study (Stage Change Study) was peer reviewed by five (5) panel members in September 2011 as requested by the Platte River Recovery Implementation Program (PRRIP). Each reviewer was tasked with reviewing the Stage Change Study from their particular area of expertise and to submit comments (both answering specific questions and submitting their own comments/inquiries) in writing to the Atkins North America (Atkins), who facilitated the peer review. Areas of expertise for the Stage Change Study included: (1) pallid sturgeon ecology; (2) riverine physical processes/geomorphology; (3) river engineering and hydraulic modelling; (4) hydrology and hydrologic analysis; and (5) ecological statistics. Peer reviewers for the Stage Change Study, including their affiliations and area of expertise, are listed in the table below.

Name	Affiliation	Area of Expertise
Christopher Guy	U.S. Geological Survey	Pallid Sturgeon Ecology
David Gaeuman	U.S. Bureau of Reclamation	Riverine Processes & Geomorphology
Larry Weber	University of Iowa	River Engineering/Hydraulics
Lee Wilson	Lee Wilson & Associates	Hydrology/Hydrologic Analysis
Dennis Helsel	Practical Stats	Ecological Statistics

II. Summary Report

Reviewers were asked to do the following tasks as part of the Stage Change Study Peer Review:

- Task 1 - Review the Stage Change Study from their area of expertise;
- Task 2 - Address the set of questions related to the Stage Change Study (as per the Scope of Work [SOW]);
- Task 3 - Provide general comments on scientific soundness, organization and clarity, conciseness, degree to which conclusions are supported by data, and cohesiveness of conclusions;
- Task 4 - Provide specific comments (as per the SOW) addressing presentation, methods, data presentation, statistical design and analyses, conclusions, errors, and citations (pee reviewers were to comment on these facets of the Stage Change Study if they significantly affected the peer reviewer's opinion); and
- Task 5 - Rate the Stage Change Study using the rating system provided in the SOW. See *Table 1* in *Section IV* below.
- Task 6 - Provide a recommendation (Accept, Accept with Revision, or Unacceptable) as it applies to the Stage Change Study.

This summary report provides an overview of the comments received from Task 3 (general comments), 5 (ratings) and 6 (recommendations) listed above. Comments received for Tasks 1, 2 and 4 are included in the *Lower Platte River Stage Change Study Peer Review Comment-Response Table* (*Attachment 1*). All comments have been inserted into a comment-response table as requested by the PRRIP so they can be easily referenced and tracked. Copies of the reviews are compiled in *Attachment 2*.

III. General Comments and Ratings

Reviewers were asked to provide comments on the Stage Change Study with respect to the following general categories[1]: (1) scientific soundness; (2) organization and clarity; (3) conciseness; (4) degree to which conclusions are supported by the data; and (5) cohesiveness of conclusions. Reviewers were to consider the major strengths and weakness of the document, its suitability for publication and/or use by the PRRIP, and its soundness in terms of both methods and scientific reasoning. A summary of responses for each category is included in subsequent sections. If specific examples or comments are cited, the reviewer's last name appears in parentheses following it.

Scientific Soundness

Reviewers indicated the scientific soundness of the Stage Change Study is *Good* (average rating of 2.8 = good; see ratings in *Table 1* in *Section IV*). Ratings ranged from 2 (very good) to 4 (fair). Most reviewers felt the technical aspects were generally good, excluding a few technical issues that were identified by specific comments. Of note were the following issues with scientific soundness.

1. Much of the study was based on analyses from unpublished FWS reports – results hinge on these results and some statement from the FWS should be included that verifies the analyses, spreadsheets etc., to ensure they are valid. The FWS reports do not discuss the methods that produced the conclusions or whatever product is being cited....the implication is the report is being accepted as truth (Helsel).
2. There is concern that most of the analyses and measures of variation represent pseudo-replication. A better way to determine the effects of PRRIP water activities on physical parameters that are thought to have significance to pallid sturgeon would be to conduct stage change studies in multiple reaches. It is a better way to represent available habitat for pallid sturgeon and the influence of PRRIP water activities on habitat (Guy).

Degree to Which Conclusions are Supported by Data

Reviewers indicated the degree to which conclusions are supported by data in the Stage Change Study is *Good/Very Good* (average rating of 2.6 = very good/good). There was a wide range of responses, from 1 (excellent) to 4 (fair) and thus perhaps an average rating is not the best means of evaluating this category. Three of the five reviewers felt the conclusions were well supported, particularly within their area of expertise (Gaeuman, Guy, and Weber). Although he believed the conclusions in the Stage Change Study are supported by the data, one reviewer suggested that the robustness of the data and the conclusions could be enhanced by a better experimental design (Guy). The remaining two reviewers felt the conclusions were not particularly well supported. One of the reviewers felt the water quality conclusions were not well supported (Helsel). The other reviewer felt that it was very difficult to determine how well supported the conclusions were without direct access to copies of the datasets, spreadsheets and models (Wilson).

Organization and Clarity

Reviewers indicated the organization and clarity of the Stage Change Study is *Good* (average rating of 3 = good). Ratings ranged from 1 (excellent) to 4 (fair). In terms of the document as a whole, reviewers felt it was relatively well organized and clear but could use standardization in terms of primary, secondary and tertiary headings, the addition of an executive summary, introductory

[1] Some reviewers rated "Importance to Objectives of the Program" even though the PRRIP document indicated that this category was for internal panel use only. Atkins assumed (as did several of the reviewers) that the internal panel was the PRRIP Governance Committee. Since some panelists rated it while others did not, ratings will not be included for this category. If clarification is needed, please provide it for use in future peer reviews.

section with background for context, and conclusions section, clarification to table and figure headings, and additional background information for clarity.

Cohesiveness of Conclusions

Reviewers indicated the cohesiveness of the conclusions in the Stage Change Study are *Good/Very Good* (average rating of 2.5 = good/very good). Ratings ranged from 2 (very good) to 4 (fair). One reviewer did not provide a rating for this category (it is marked as non-applicable [N/A] in *Table 1*). The rating may have been based on how willing the reviewer was to search for the conclusions within the Stage Change Study document. For example, one reviewer thought the conclusions were cohesive (rating of 2) but noted he had to search for them within the Discussion Section because they were interwoven (Weber). A conclusion section would have been helpful. Another reviewer suggested the addition of a conclusion section (Helsel) for ease of understanding. One reviewer even suggested that "much has been left unsaid in this study...and a stranger to this process might not be able to properly judge the end results (Wilson).

Comment [EBH1]: Atkins' deleted the rating associated with this comment. Upon discussion with Lee Wilson, the rating and the comment were not linked.

Lee rated the document based on what he was provided with (report, appendices, etc.). He made additional comments on how the report could be improved if it were revised.

Atkins mistakenly linked the ratings to the comments and that wasn't necessarily the intention of the reviewer.

Conciseness

Overall, reviewers indicated the conciseness of the Stage Change Study is *Very Good/Excellent* (average rating of 1.8 = very good/excellent). Most reviewers felt the document was well written and presented an appropriate amount of information in terms of breadth and depth.

IV. Ratings

Table 1 summarizes the ratings for each of the categories discussed in *Section III* (Task 5 in *Section II*). The ratings are organized by reviewer and an average rating is included as well. In most cases, average ratings tend to be a good representation of the overall sentiment of the reviewers. Exceptions are noted in *Section III* above.

	Reviewer	Gaeuman	Guy	Helsel	Weber	Wilson**	Average
Categories	Scientific soundness	4	3	3	2	2	**2.8**
	Degree to which conclusions are supported by the data	3	3	4	2	1	**2.6**
	Organization and clarity	4	4	4	1	2	**3**
	Cohesiveness of conclusions	N/A	2	4	2	2	**2.5**
	Conciseness	3	2	2	1	1	**1.8**

Table 1: Ratings given per each reviewer following the rating system: 1=excellent, 2=very good, 3=good, 4=fair, 5=poor.

****during the rating process, Lee Wilson inverted the rating system – he classified 5 = excellent and 1 = poor. Atkins was able to identify this reversal given that Lee's comments were counter to his ratings. Table 1 corrects for this. Atkins will verify this with Lee once he returns stateside in mid-October 2011.

Comment [EBH2]: Atkins verified that Lee Wilson did invert his ratings. Table 1 is correct as included in this report.

V. Recommendations

Reviewers were also asked to make a recommendation with respect to the document. They were given the following choices: (1) accept it; (2) accept it with revisions; or (3) deem it unacceptable. Before the recommendations can even be considered, it is important to note the confusion associated with this task. First, peer reviewers were unclear as to whether the Stage Change Study was a draft or final document – could it be revised? In some cases, the recommendation hinged on whether the reviewer felt it was feasible to make a specific recommendation given it may not be something that could be changed. Additionally, there may have been confusion amongst reviewers

as to how the Stage Change Study was going to be used in the future – would it be published? Was it going to be used by the PRRIP and if so, how? Perhaps it would be useful to provide a one paragraph summary to peer reviewers (as they begin their peer review) that provides context for the study being reviewed and how it will be used by the PRRIP.

Given this, Weber and Wilson recommended the Stage Change Study be accepted. Gaeuman, Guy and Helsel recommended it be accepted with revisions (assuming it can be revised). In the case of Gaueman, he suggested a major revision but given its status as a final report, he would accept the general conclusion as being "qualitatively" correct.

ATTACHMENT 1

Lower Platte River Stage Change Study
Comment-Response Table

Lower Platte River Stage Change Peer Review

Comment-Response Table

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
1	Gaeuman	Fluvial Geomorphology	SOW Question 1		Yes.	
2	Gaeuman	Fluvial Geomorphology	SOW Question 2		Yes. However, bedforms played a very minor role in this study. It's not clear how they were incorporated into the quantification of sturgeon habitat availability.	
3	Gaeuman	Fluvial Geomorphology	SOW Question 3		Yes, but I do not claim to be an expert in that subject.	
4	Gaeuman	Fluvial Geomorphology	SOW Question 4		No. A better evaluation of gaging errors is needed, as described in my comments above. I would also suggest that the idea of detectability be better defined. It seems that for a small water augmentation to be detected, one would have to know what the discharge would have been without the augmentation. How would the work? And what is the time scale over which the detection should occur? Detecting a small change on a particular day is a different matter than detecting a sustained small change over a month or a year.	
5	Gaeuman	Fluvial Geomorphology	SOW Question 5		N/A	
6	Gaeuman	Fluvial Geomorphology	SOW Question 6		yes	
7	Gaeuman	Fluvial Geomorphology	General		The scope of this study outlined in the RFP targets two related, but distinct, objectives: determining what measurable effect, if any, Program water delivered at upstream locations will have on discharge in the Platte River downstream from its confluence with the Elkhorn River, and quantifying how changes in discharge might translate to changes in hydraulic parameters and physical habitat characteristics in that stream segment.	
8	Gaeuman	Fluvial Geomorphology	General		The authors of the study approach these two objectives quite differently. With respect to how discharge affects habitat, the authors present an analysis based on numerical modeling of flow under existing geomorphic conditions. Although this modeling analysis neglects the potential for future flows to modify the current stream configuration and produce longer-term changes in habitat availability, it does address the question posed in the RFP. The question, the approach used to address it, and therefore the review of the analysis, is straight-forward. My review of that portion of the report is presented first.	
9	Gaeuman	Fluvial Geomorphology	General		For the question regarding the effect upstream Program water on downstream discharge, however, the authors opted to rely heavily on some earlier Fish and Wildlife Service analyses, which were incorporated in the report as Appendix A and Appendix B. In doing so, they implicitly endorse those reports and accept some level of responsibility for any problems with the methods and explanations presented in them. I found those reports quite difficult to interpret, so I'll save my comments on that portion of the Stage Change Study for last.	
10	Gaeuman	Fluvial Geomorphology	General		I note here that I have not attempted to systematically copy edit this report because, according to the title, this is a Final version. I take that to mean that typographic errors, unclear statements, and so on will not be corrected as might happen if this were a Draft version. Instead, my comments focus on the broader-scale "Specific Questions" identified in Review scope of Work and the "Specific Comments," "Rating," and "Recommendation" identified in the PRRIP Peer Review Guidelines. The questions from the Scope of Work and the Peer Review Guidelines are addressed explicitly following my free-form comments on the Hydraulics and Geomorphology section and the Hydrology section.	
11	Gaeuman	Fluvial Geomorphology	Hydraulics and Geomorphology	General	The approaches used to address the question posed in the RFP are appropriate. The general approach of modeling hydraulic parameters and using model output to classify habitat types is good. It could perhaps be improved by incorporating bedform types into the classification system, in addition to depth and velocity. Bedforms can have a large effect on flow velocities and turbulent structures near the bed, and so are likely very important components of physical habitat. The section on describing and predicting bedforms is good, but it's not clear whether or how that information was used to inform the final conclusions of the study.	

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
12	Gaeuman	Fluvial Geomorphology	General		The contractor appears to have an adequate understanding of the modeling tasks to produce credible results. However, the modeling analysis seems to include some mistakes and misinterpretations that might have the potential to affect the Study's conclusions and recommendations. Two problems with the model itself are worth highlighting: the 2d model domain lacks lead in and lead out sections and is generally too short (see comment 19), and the quantity of topographic data appears to be very small compared to the resolution of the model mesh (see comment 20). Both of these issues substantially degrade the accuracy of the model and the confidence that can be placed in its output. Two additional issues regarding the interpretation of the model results are worth mentioning: The sensitivity analysis regarding how model errors affect habitat classification may be flawed (see comment 31), and percentages in each habitat type are based on submerged area rather than total area (see comment 38). That said, I doubt that correcting these problems would materially change the Study's conclusions concerning how incremental changes in discharge alter habitat availability.	
13	Gaeuman	Fluvial Geomorphology		Page 9	"A hydraulic and geomorphologic analysis…" not sure what part of this is a geomorphologic analysis. It's mostly limited to hydraulic modeling.	
14	Gaeuman	Fluvial Geomorphology		Page 9, last paragraph	"…trend over this period." Which period?	
15	Gaeuman	Fluvial Geomorphology		Page 10, 2nd paragraph	refers to a 10-year model run. What does that mean?	
16	Gaeuman	Fluvial Geomorphology		Page 10, 3rd paragraph	Not sure what's meant by the different model versions incorporating cross sections from different dates. The preceding sentence is about water surface elevations at the cross sections. Were different cross sections (geometry) used in the two model versions, or just different water surface elevations for validation?	
17	Gaeuman	Fluvial Geomorphology	Table 7		Table headings are unclear and awkward. I'm not sure what an average maximum or average minimum is. Are these the extreme instantaneous values for a given day averaged over X number of days? Is "average mean" the average of X number of daily mean values, or the average of something else? The text on page 10 that references Table 7 doesn't help with this.	
18	Gaeuman	Fluvial Geomorphology		Page 11	The discussion of the models of different dates is poorly organized and confusing. It would help if the point of all this were explained at the outset. Much later in the text, in the section about bedforms I believe, it becomes apparent that the point is to account for differences in roughness due to differences in bedform regime at different flow levels.	
19	Gaeuman	Fluvial Geomorphology		Page 12, 4th paragraph	Figures 19-20: The model mesh is 1,700 ft long. From the figures, it's seen that this corresponds to about 1 channel width. This is far too short of a model reach. First, it is a very small sample in term of area from which to generalize about the river segment. But more importantly, every point within the model is a short distance from the model boundaries. It is standard practice to extend the model mesh at least a few channel widths upstream and downstream of the reach of interest. That allows some space and time for any errors or imperfections in the boundary conditions to dissipate.	
20	Gaeuman	Fluvial Geomorphology		Page 12, last paragraph	refers to "detailed topographic and bathymetric data" used in the model. There is no indication in this report that detailed topographic data was collected. The onlydiscussion along those lines concerns collection of a relatively small number of cross sections. The 2d mesh is said to have a mesh resolution of 10 feet. This density is irrelevant unless the topo data mapped to the mesh is of similar resolution, as might be obtained with an intensive sonar survey using an array of transducers or a multi-beam. There is no indication that this was the case. The value of the fine mesh is, to a large extent, nullified if the topography was interpolated from cross sections.	
21	Gaeuman	Fluvial Geomorphology		Page 12, last paragraph	It's not explained where the n values of 0.023 and 0.027 in the 2d model came from. Were these transferred from the 1d calibration in some way?	
22	Gaeuman	Fluvial Geomorphology		Page 13, 4th paragraph	Figures 24-26: It is stated that the match between measured and modeled water surface elevation and water velocities is "good." This seems to be an overstatement. Plus or minus 0.5 ft in elevation does not seem especially good to me, and velocity errors seem to range up to around 50% (Figure 26).	
23	Gaeuman	Fluvial Geomorphology		Pages 14-15	Nice overview on bedforms.	
24	Gaeuman	Fluvial Geomorphology		Page 16, 2nd paragraph	S'* is introduced, but not defined until it come up again on page 17. Same for SG in the equation given for d*.	
25	Gaeuman	Fluvial Geomorphology		Page 16, last paragraph	I think this should be the relation between the average shear stresses (as indicated in equation 1), rather than velocity.	
26	Gaeuman	Fluvial Geomorphology		Page 17	Some of the notation seems odd. ' is used in the definition of S'*, but is not defined (equation 1 introduces '0 and , but not '). Should it be just ? The shields parameter is denoted F* -- why not use * or like most everyone else? (SG-1) is often denoted by R, and SG itself is usually /s. I've usually seen transport stage denoted with T rather than S.	

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
27	Gaeuman	Fluvial Geomorphology		Page 17, 4th paragraph	the VBA script is said to solve for the “necessary values…” It’s difficult to be sure what is being done here. I infer that is specified on the basis of model output, and equation 1 is solved for ’0, but that’s not clear from the text.	
28	Gaeuman	Fluvial Geomorphology		Page 17, last paragraph	Discussion switches abruptly from bedform types to how much of the site is subaerially exposed. What’s the connection?	
29	Gaeuman	Fluvial Geomorphology		Pages 18-19	habitat evaluation: This seems like a good approach. Why are there no pools in this classification? Are especially deep scours and holes not relevant for sturgeon, or perhaps these environments are not present in the Platte?	
30	Gaeuman	Fluvial Geomorphology		Page 20	top: re-states that the model is well calibrated. See comment 22.	
31	Gaeuman	Fluvial Geomorphology		Page 20	numbered item 1: velocity units are given as ft.	
32	Gaeuman	Fluvial Geomorphology		Page 20	numbered item 2: Was the simulated error applied to each node independently? Or to put it another way, would adjacent nodes be assigned uncorrelated errors? That would clearly be incorrect – for example, if a given node had a large positive error in depth, all nearby nodes (and maybe every node in the model) would probably also have positive errors. Assigning each node an error that is independent of all the other errors would cause the random errors to cancel, and probably result in very little net change in the proportion of particular habitat types.	
33	Gaeuman	Fluvial Geomorphology		Page 21	The text says that Table 11 shows variation among transects and among sample episodes, but it doesn’t show that. Is a “sample episode” a day?	
34	Gaeuman	Fluvial Geomorphology	Table 12	Page 22	The table suggests that conductivity and turbidity behave in the same way with respect to different “phases” (what’s the independent variable here, discharge maybe?). Meanwhile, Figure 42 shows that they behave in opposite ways. What point is being made with these statistics anyway?	
35	Gaeuman	Fluvial Geomorphology		Page 22, 3rd paragraph	What is meant by “bottom velocity?” This must refer to some height above the bed.	
36	Gaeuman	Fluvial Geomorphology		Page 22, 3rd paragraph	The explanation for why run and plunge habitat is considered most suitable is not very convincing. Where are the sturgeon actually found? Do the cited publications refer to run and plunge habitats?	
37	Gaeuman	Fluvial Geomorphology		Page 23, 1st paragraph	The gaging error magnitudes defined in the hydrology sections are applied here. I suspect that the interpretation of gage errors may have a problem – see comment 46.	
38	Gaeuman	Fluvial Geomorphology		Page 23-24	The actual changes in the availability of various habitat types may change more with discharge than is indicated. It appears that the percentages given for habitat types are the percents of the total submerged area. It would be more meaningful to report this in terms of actual area or as a percentage of the model domain area because the extent of the submerged area changes with discharge.	
39	Gaeuman	Fluvial Geomorphology	Hydraulics and Geomorphology	General	The hydrology studies presented in the two USFWS reports and incorporated into the Stage Change Study leave much to be desired in terms of both technical credibility and the clarity of the presentation. Some of the problems with the original reports are noted in the specific comments below. The authors of the Stage Change Study apparently reproduced the analyses described in the USFWS reports. That would require sorting out the details regarding what those analyses involved. Having done that, I would expect the authors of the Stage Change Study to provide a better description of what they did than simply referencing and copying text from the Appendices.	
40	Gaeuman	Fluvial Geomorphology	Hydraulics and Geomorphology	General	The flow losses due to evaporation, transpiration, and seepage estimated in these reports are, in my opinion, unreliable. The reported total loss figures become more credible if they are considered to be generic losses, not attributable to any particular sink. Nonetheless, I agree with general conclusion that small discharge augmentations upstream of Grand Island of the magnitude discussed will not be very noticeable at Louisville. This is not so much related to gaging uncertainty (which I think is overestimated in the reports), but is instead due to the fact that the augmentation volumes discussed are small compared to everything else that is going on. Changes in flow on the order of 100 cfs would be difficult to distinguish even if the gages were perfectly accurate, because the changes can be swamped by much larger flow fluctuations caused by a variety of other factors.	
41	Gaeuman	Fluvial Geomorphology		Page 2, end of 2nd paragraph	States that the selected flows are considered appropriate for modeling, but doesn’t explain why. Does anything about pallid sturgeon habitat enter into this determination?	

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
42	Gaeuman	Fluvial Geomorphology	Table 2 and associated text		Meaning of the headings indicating time periods are unclear. These look like periods of record for the gages, but are not. Time periods listed for the Loup near Columbus include times when there are no gage records. It takes careful picking through the text to figure out how to interpret these dates. I'm unsure of what is meant by "period of analysis." This could refer to the period from which flow records were drawn to quantify the hydrologic characteristics of the gage site, which could then be extrapolated to other years, or it could mean that consideration of the gage site was entirely confined to that time period.	
43	Gaeuman	Fluvial Geomorphology		Page 3, 3rd paragraph	This paragraph is very hard to follow. It does not clearly identify what is being estimated – language like "the USFWS analysis" and "these flows" do not identify the gages and dates for which flows were being reconstructed.	
44	Gaeuman	Fluvial Geomorphology		Page 3, last paragraph	A new gage can apparently supply better information about powerhouse return flows, but was not used. This information could have at least been used to check on the accuracy of the method in the USFWS analysis.	
45	Gaeuman	Fluvial Geomorphology		Pages 4-5	The Study basically just sends the reader to Appendices A and B. There appears to have been little or no critical review of the USFWS reports by the Study authors.	
46	Gaeuman	Fluvial Geomorphology		Page 5, last paragraph	This interpretation of gage accuracy seems overly simplistic. It is stated that the USGS considers 95% of the gage readings to be within 10% of the actual discharge. This report follows the USFWS reports in translating that into error bounds of plus or minus 10%. Assuming the errors are independent random variables, the actual error bound should be related to the number of samples used to generate an estimate. For example, the USGS error estimate could be interpreted as suggesting that the individual errors have a standard deviation of around 5% (because close to 95% of a normally-distributed population is within 2 standard deviations of the mean). Whether the standard deviation is 5% or something else, the standard error of the estimate is equal to the standard deviation divided by the square root of the sample size. If the estimate is monthly mean flow, the sample size is about 30. These numbers suggest that the error bound for the monthly mean might be around 2% at the 95% confidence level. I am not a statistician, and the details of this example may not be exactly correct. For example, the errors on sequential days are probably correlated to some degree. The point is simply that the 10% error bounds assumed in the reports need to be re-examined.	
47	Gaeuman	Fluvial Geomorphology		Page 7	In repeating the USFWS reports, the Study incorporates an abundance of errors, confusing explanations, and obscure objectives. Page 7 discusses what happens to an incremental increase in flow at Grand Island by the time it reaches Louisville. The discharge increments considered seem arbitrary. It would be most helpful if the Study would explain why these particular increments are relevant, and more generally, what "Program water" or "First Increment water" is. After consulting the Biological Opinion, the Adaptive Management Plan, the Record of Decision, the Platte River Recovery Implementation Program Final Environmental Impact Statement, and the Platte River Recovery Implementation Program, I've determined that First Increment water refers to 130,000 to 150,000 acre-feet of water annually, perhaps in the form of baseflow discharge targets or (undefined?) pulse flows. Spread evenly across the full year, that volume of water is equivalent to about 200 cfs, which is in the range of increases being evaluated. I speculate that the documents I've consulted are ambiguous about Program water because it has not yet been fully determined how that water is to be used. If so, the hydrologic analyses in the Study seem to be putting the cart before the horse. They seem to ask: if the upstream flow is bumped by X, could it be detected downstream, and would it materially improve habitat? Would it not make more sense to go about it other way around? That is, to ask: How much of an increase in flow is needed in the lower river to materially improve habitat there, and how much discharge needs to be added to upstream flows to hit that downstream target? Perhaps this is how the question is being approach, but it's hard to tell from what's written.	
48	Gaeuman	Fluvial Geomorphology		Page 7, 5th paragraph	The paragraph begins and ends describing evaporation trends, but refers to total volume lost in the middle. It's unclear whether this means total volume lost through evaporation, or total volume lost including seepage losses. It's also unclear whether evaporation here includes transpiration. More generally, the analysis contained here and in the USFWS reports is often muddled in this regard. Terms like evaporation and ET do not seem to be used in a consistent manner throughout. However, the distinction may be an unnecessary complication, given the methods used to estimate these losses. See comments on that later.	
49	Gaeuman	Fluvial Geomorphology		Page 8	The section on hydrograph translation is difficult to interpret. It could be greatly improved by telling the reader more specifically what the EA flow was. Four paragraphs into the section it is noted that "the peak of the EA flow at Duncan is estimated to be approximately 2000 cfs above base flows." From this, a reader might infer that something like 2000 cfs was released from somewhere upstream or otherwise generated somehow. Is there some reason that what was done and where it was done can't be clearly stated?	
50	Gaeuman	Fluvial Geomorphology	Appendix A	Page 1	The report discusses evaporation and seepage losses. Are there no diversions or pumps to consider?	

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
51	Gaeuman	Fluvial Geomorphology	Appendix A	Page 2	The Figure 1 referenced here is missing. The same or a similar figure 1 is missing from Appendix B as well. The missing figures seem to be maps showing where all these gages, reaches, and tributaries are.	
52	Gaeuman	Fluvial Geomorphology	Appendix A	Page 5	Estimated lag times are very crude. All are integer days, and variations in lag time with discharge are not considered. This component of the analysis deserves more attention than it was given.	
53	Gaeuman	Fluvial Geomorphology	Appendix A	Page 5	Figure 2 referenced here is missing.	
54	Gaeuman	Fluvial Geomorphology	Appendix A	Page 6-7	It would make sense to look at channel width during the time of year when evaporation losses are greatest. Seasonal trends in channel widths were considered indirectly through the application of “liberal” and “conservative” widths. Seasonal differences in width could be addressed more directly.	
55	Gaeuman	Fluvial Geomorphology	Appendix A	Page 8	The use of pan evaporation rates to estimate river evaporation rates is a big leap. I suspect that the temperature of the pan is quite different than the temperature of the river. The pan coefficient might be intended to account for that, but no explanation or justification for the factor of 0.7 is given. The adjustment factors used for ET losses also lack explanation. These things need to be explained.	
56	Gaeuman	Fluvial Geomorphology	Appendix A	Page 9-10	Seepage losses are calculated as the difference between the net inputs to a reach (inflows minus E/ET losses) and the outflow from the reach. This raises the question of why the analysis even bothers to estimate E/ET, because its magnitude is irrelevant to the result. If the estimate of E/ET was arbitrarily increased by 20 cfs, for example, the corresponding estimate of seepage loss would come out 20 cfs lower. The total loss, however, would remain the same regardless of what value was used for E/ET. It would be simpler and equally useful to simply define “losses” as the difference between inflows and outflows without regard to whether they are E/ET or seepage.	
57	Gaeuman	Fluvial Geomorphology	Appendix A	Page 10	States that “Percent ungaged gains were not calculated, as this quantity is not relevant to this analysis.” I’m not sure how to interpret this statement, but I do not agree that gains are irrelevant. It’s also unclear whether “gain” refers to ungaged tributary input only, or to all gains (such as groundwater inflows and return flows from diversions).	
58	Gaeuman	Fluvial Geomorphology	Appendix A	Page 11	Seepage loss estimates are called “conservative.” It would be clearer to say the reported losses underestimate the actual losses. It would also be good to say something about the magnitude of underestimation.	
59	Gaeuman	Fluvial Geomorphology	Appendix A	Bottom of Page 12	“Total estimated daily evaporation + ET losses” are given in units of cfs, that is, rate units instead of volume. And again on page 14. The figures referenced in this text give the losses in percent of flow.	
60	Gaeuman	Fluvial Geomorphology	Appendix A	Page 18, 1st paragraph	This paragraph is unnecessarily confusing. The example discusses a reach, a subreach, a stream gage, and added Program water with no explanation of the geographic relationship between these elements. That difficulty would be partly relieved if Figure 1 wasn’t missing from the report. It is stated that flow is 1000 cfs at Duncan on a particular day. It then refers to the “historic Platte River inflow,” which, from the arithmetic that follows, appears to refer to the 1000 cfs at Duncan. Then, 200 cfs of Program water is introduced, although it’s not clear how or where. Again, from the arithmetic, it seems that the Program water is also an inflow at the top of the reach, so that the flow at Duncan is actually 1200 cfs, not 1000 cfs. The presentation of the arithmetic is also overly complicated. It could be presented as three simple operations: determine the volume of inflows (including distance weighted gains), calculate the proportion of the inflows that are lost to E/ET (equal to losses/inflows), and multiply the Program water volume by that proportion.	
61	Gaeuman	Fluvial Geomorphology	Appendix A,	Page 19, 5th paragraph	The sensitivity analysis for open water width needs more explanation. It seems to me that, according to how the total losses are calculated, changing the open water width would have zero effect on total losses because E/ET is subtracted from inflows before computing seepage losses. Could it be that the authors of this report applied 2 different estimates of E/ET to the same analysis? That is, did they subtract the original estimate of E/ET from inflows, then calculate seepage losses, then use those seepage losses with new, larger estimates of E/ET to arrive at new total losses? That would clearly be incorrect.	
62	Gaeuman	Fluvial Geomorphology	Appendix A	Figures 9 and 10	Why do these graphs present different results than the similar graphs in Appendix C of the other USFWS report included as Appendix B (Page 17 in Appendix B)? Graph titles and axes labels are the same in both appendices, but the plotting positions differ.	
63	Gaeuman	Fluvial Geomorphology	Appendix A	Page 23, 1st paragraph	States that there are no major diversions below Grand Island. What about numerous small diversions? Has that been evaluated?	
64	Gaeuman	Fluvial Geomorphology	Appendix B	Page 5, 6th paragraph	Mentions a Tri-County supply canal system. I didn’t see that mentioned anywhere else. I wonder where that is, and if it is, or should be, considered in the analysis presented in Appendix A.	
65	Gaeuman	Fluvial Geomorphology	Appendix B, Table 2		Uncertainty is assumed to be 10% of the measured flow. See comment 46.	
66	Gaeuman	Fluvial Geomorphology	Appendix B, Table 3		I’m wondering why the effect of First Increment Program activities is to cause negative changes in flow in some months. Here would be a good place to provide some explanation as to what First Increment Program activities include.	

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
67	Gaeuman	Fluvial Geomorphology	Appendix B	Page 16	These travel times could be used to improve the Appendix A analysis.	
68	Gaeuman	Fluvial Geomorphology	Appendix D	Page 18 of Appendix B and text on pages 9-10	Would be appropriate to define what the "OPSTUDY Model" is.	
69	Guy	Fisheries Ecology and Aquatic resource Management	SOW Question 1		The Stage Change Study does address the overall objective of the RFP for a specific area in the Platte River. I believe that the study could have been more robust by extending the spatial extent of the study. The objective clearly states '…from the Elkhorn River confluence to the Missouri River confluence,' but the study was conducted on a reach from the Nebraska highway 50 bridge to the Chicago Rock Island and Pacific Railroad pedestrian bridge. I would agree that this reach is likely representative of much of the lower Platte River and is an area where pallid sturgeon have been located (Peters and Parham 2004); however, the Platte River at the confluence with the Missouri River is likely quite different and should have been included. The confluence is central to these analyses because much of the use of the Platte River by pallid sturgeon occurs near the confluence (Peters and Parham 2004). Had the investigators conducted measurements in at least two reaches (i.e., the current reach and one at the confluence), preferably more than two reaches (i.e., also include a reach near the Elkhorn River confluence), the precision, understanding of uncertainty, and inference space would have been greater with respect to Program water management activities. Further, the confluence reach is unique given that discharge in the Missouri River can influence the habitat dynamics in the Platte River which in turn will affect the results of Program water management activities, most likely different than the reach near Louisville, Nebraska. This criticism is especially relevant to the 2D modeling exercise which provides the most useful information for pallid sturgeon conservation. Understanding the effects of Program water management activities for additional reaches in the Platte River is instrumental if the Governance Committee is going to use this information to determine the effects of discharge on physical parameters thought to be important to pallid sturgeon. The effects of stage changes on physical parameters appears to be well studied for the reach near Louisville, Nebraska and should provide information needed to evaluate Program water management activities in that area. With that said, it would be beneficial if the investigators made it more clear regarding the discharges under which empirical data were collected, it is difficult to determine as currently written.	
70	Guy	Fisheries Ecology and Aquatic resource Management	SOW Question 2		The selected physical parameters seem reasonable given the current state of knowledge regarding pallid sturgeon ecology. However, it is unclear what aspects of the pallid sturgeon life-history are targeted by Program water management activities. Providing habitat for adults is likely quite different than providing habitat for larvae. I realize this was not part of the scope of research for the investigators, but should be considered by the Governance Committee. This will help refine the effects of Program water management activities and how they relate to specific aspects in the conceptual models. Defining the life-history aspects of interest will also make the physical parameters more scientifically defensible. It is becoming clearer that habitat diversity and complexity are important to riverine fishes. Thus, combining metrics into a richness or diversity value and evaluating those data as a composite with varying Program water management activities might be more ecologically relevant than studying each parameter separately.	
71	Guy	Fisheries Ecology and Aquatic resource Management	SOW Question 3		The selected habitat classifications seem reasonable given the current state of knowledge regarding pallid sturgeon ecology. It may be implicit in some of the habitat classifications, but a more detailed analysis of the thalweg dynamics would have been informative (e.g., thalweg depth and migration under varying discharges). I believe understanding the dynamics of the thalweg given varying Program water management activities would be highly beneficial given that several studies indicate that pallid sturgeon are typically found in or near the thalweg. I recognize that the investigators are aware of the importance of this habitat type because they allude to it when they discuss run and plunge habitat. Again, it is important that the life-history aspect of interest is well defined because habitat use likely changes with ontogeny. As stated above, combining habitat classifications into metrics that describe the richness or diversity of habitat may be more ecologically meaningful.	
72	Guy	Fisheries Ecology and Aquatic resource Management	SOW Question 4		Yes, given the error associated with the Louisville gage and the results from the 100, 500, and 1,000 cfs additional Program water at Grand Island reaching Louisville as summarized in Figures 3, 4, and 4a. However, the amount detected varies temporally.	
73	Guy	Fisheries Ecology and Aquatic resource Management	SOW Question 5		Yes, relative to stage and velocity, but not temperature, turbidity, substrate, or channel morphology because those are not measured by the gauging equipment. It is clear in the results that there is temporal variation in water quality metrics and that the variation can be detected given the sample sizes, but it is not clear how the variation in water quality metrics relate to Program water activities.	

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
74	Guy	Fisheries Ecology and Aquatic resource Management	SOW Question 6		In general, I believe the conclusions are supported by the data, although the conclusions are not clearly articulated. I am concerned that most of the analyses and measures of variation represent pseudo-replication. This relates to my comments in the first question. I believe the best way to determine the effects of Program water activities on physical parameters that are thought to be of significance to pallid sturgeon would be to conduct the Stage Change Study in multiple reaches (i.e., the reaches are the experimental unit). Although one could argue that reaches are not independent, I surmise that it better represents available habitat for pallid sturgeon and the influence of Program water activities on that habitat. The most important aspect of having multiple reaches is that one will have a better understanding of the uncertainty of Program related water activities on pallid sturgeon habitat.	
75	Guy	Fisheries Ecology and Aquatic resource Management		Page 1, 2nd paragraph	"*bed topography at low to intermediate flows* " Why not bed topography at high flow?	
76	Guy	Fisheries Ecology and Aquatic resource Management		Page 1, 3rd paragraph	"*Within the Study Reach, depth, velocity, turbidity, water temperature, dissolved oxygen, and conductivity measurements, as well as bed topography, were obtained* …" Why not sediment transport or large woody debris?	
77	Guy	Fisheries Ecology and Aquatic resource Management		Page 5, 4th paragraph	"*Water Quality Measures* " These are commonly measured, but why? What are your hypotheses related to these or how do they relate to a conceptual model	
78	Guy	Fisheries Ecology and Aquatic resource Management		Page 5, 4th paragraph	"Data collected from each phase of sampling were then used to conduct a power analysis to determine whether sample sizes were adequate…" This is true at one site, but wouldn't it be better to measure these at multiple reaches and treat those as the experimental unit?	
79	Guy	Fisheries Ecology and Aquatic resource Management		Page 7, 3rd paragraph	*"The results, assuming 100, 500, and 1,000 cfs of additional Program water at Grand Island, are summarized in Figures 3, 4, and 4a, respectively"* Very informative.	
80	Guy	Fisheries Ecology and Aquatic resource Management		Page 7, 6th paragraph	"*Comparison with USFWS Analysis* " Was this part of the original RFP?	
81	Guy	Fisheries Ecology and Aquatic resource Management		Page 11, 3rd paragraph	"These comparisons indicate that the low-flow channel or channels tended to deepen during the high spring flow events and tended to become shallower in response to periods of low flow..." I find this very informative given pallid sturgeon tend to use the main channel, i.e., thalweg. We have found that pallid sturgeon avoid shallow, small tributaries.	
82	Guy	Fisheries Ecology and Aquatic resource Management	Figure 23	Page 13	Why so few samples at high discharge? Also, does the variation in the number of samples collected influence the results?	
83	Guy	Fisheries Ecology and Aquatic resource Management		Page 13, 4th paragraph	"in conjunction with the topographic data on which the hydraulic model is based (Figure 24)." Some statistics on the regression would help reduce this subjective statement. Why is one of the data points missing from this figure? It is the outlier in Figure 25. Am I missing something?	
84	Guy	Fisheries Ecology and Aquatic resource Management	Figure 26	Page 13	Seems like a lot of scatter, should you explain the variation?	
85	Guy	Fisheries Ecology and Aquatic resource Management		Page 16, 2nd paragraph	This paragraph and the following two paragraphs are difficult to read.	
86	Guy	Fisheries Ecology and Aquatic resource Management		Page 16, 2nd paragraph	"(d* = D50{(SG-1)g/v2}1/3)) " I think the parentheses are off a bit.	

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
87	Guy	Fisheries Ecology and Aquatic resource Management		Page 17, 2nd paragraph	"*is the sediment transport strength defined as (τ'/τcr-1)*" -I don't think this is defined?	
88	Guy	Fisheries Ecology and Aquatic resource Management		Page 17, 3rd paragraph	"*Based on six grab samples of the surface bed material*" -Is six good enough? Why six?	
89	Guy	Fisheries Ecology and Aquatic resource Management		Page 18, 1st paragraph	"Evaluation of the areas occupied by dunes indicates that the median predicted dune height increases from 0.45 feet (~5.4 inches) at 3,700 cfs to 0.81 feet (~10 inches)…" These data are very interesting. Especially from a fish ecology aspect because we believe fish use these as velocity refuge. Any measures of variation with these data?	
90	Guy	Fisheries Ecology and Aquatic resource Management		Page 18, Figure 34	Excellent figure!	
91	Guy	Fisheries Ecology and Aquatic resource Management		Page 18, 2nd paragraph	"Plunge areas represent a complex habitat that is characterized by not only a rapid change of depth, but also its spatial location relative to bars and banklines within the detailed study reach…" This information and the bullets below are a bit difficult to follow.	
92	Guy	Fisheries Ecology and Aquatic resource Management		Page 19, 1st paragraph	"*Slackwater, Riffles, and Runs* ." Why caps now?	
93	Guy	Fisheries Ecology and Aquatic resource Management		Page 19, Figure 36	Excellent figure.	
94	Guy	Fisheries Ecology and Aquatic resource Management		Page 19, 2nd paragraph	"The procedure used to develop the uncertainty bands in Figures 38a-d are described in the next section..." This is good, but make it clear what uncertainty you are measuring. I don't think this is uncertainty related to Program water activities, which is the central question.	
95	Guy	Fisheries Ecology and Aquatic resource Management		Page 20, Figure 44a	Very useful information.	
96	Guy	Fisheries Ecology and Aquatic resource Management	Table 11	Page 21	Measures of variation?	
97	Guy	Fisheries Ecology and Aquatic resource Management		Page 22, 3rd paragraph	"*...it can be concluded that changes in habitat areas as a result of 100 or 500 cfs environmental releases would have a negligible influence on pallid sturgeon habitat in the lower Platte River.*" I agree. Nice work.	
98	Guy	Fisheries Ecology and Aquatic resource Management		Page 23, 1st paragraph	"Finally, the increase in discharge does not move the conductivity, turbidity, temperature, or dissolved oxygen outside the typical range preferred by pallid sturgeon (Figures 42 and 43)." Not sure we know what typical is for pallid. Can you reword to avoid 'typical' and 'preferred?'	
99	Guy	Fisheries Ecology and Aquatic resource Management		Page 23, 3rd paragraph	"*Based on this stage change study, the % habitat in the lower Platte River experiences a relatively high rate of change for flows ranging between 4,000 cfs to 6,000 cfs.*" Not true for all habitats see Figures 44 and 45.	
100	Guy	Fisheries Ecology and Aquatic resource Management		Page 24, 1st paragraph	"*The Flat classification would have been increased from approximately 30% (± 7%) to 40% (± 8%) of the habitat area…" Do you mean ±9?*	

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
101	Guy	Fisheries Ecology and Aquatic resource Management		Page 24, 1st paragraph	"*The decrease in discharge does not move the conductivity, turbidity, temperature, or dissolved oxygen outside the typical range preferred by pallid sturgeon (Figures 42 and 43)* ." see comment #24	
102	Guy	Fisheries Ecology and Aquatic resource Management		Page 24, 3rd paragraph	"*Spring is likely the most critical period so that should be protected as best possible* ." What does this mean? I don't think we can say this with much confidence.	
103	Guy	Fisheries Ecology and Aquatic resource Management		Page 25, 4th paragraph	*"Therefore, the results from this Study should be used as one part of a larger perspective on available habitat rather than an absolute factor in driving conclusions and decisions related to population dynamics."* Yes, nice work!	
104	Helsel	Environmental Statistics	SOW Question 1		The Study adequately addresses the relative magnitude of stage change due to management activities in relation to existing flows and habitat of the pallid sturgeon. It does not discuss the proposed changes in light of existing appropriations and any current legal constraints on flow in the Platte River. In other words, if these diversions were implemented would they impact the water rights of existing rights owners? The method for extrapolation of miSSing record to the Loup River at Columbus is flawed, and so the resulting errors on the analysis are unknown.	
105	Helsel	Environmental Statistics	SOW Question 2		The data themselves are presumably scientifically defensible. They are fairly routine parameters with established protocols for collection. The amount of data is adequate. Analysis ofthe data is not adequate, if the purpose is to determine whether proposed flow augmentation and withdrawals for storage will significantly affect those parameters.	
106	Helsel	Environmental Statistics	SOW Question 3		This is not my area of expertise.	
107	Helsel	Environmental Statistics	SOW Question 4		Yes. Given that equipment and gauging error is listed as 10% (presumably +5% and ·5%0, the Study determined that flow changes such as those on page 24, going from 5,040 cfs to 3,290 cfs, are expected to be much greater than 5% (the direction is known), and so will be detectable as different from base flow conditions.	
108	Helsel	Environmental Statistics	SOW Question 5		No. Determination ofdifferences in water quality parameters using Analysis of Variance is flawed because the serial correlation in the data was not accounted for. The current analysis is not sufficient to determine whether there are significant impacts for these parameters.	
109	Helsel	Environmental Statistics	SOW Question 6		The Study's conclusions in regards to flow are supported by the data and analysis. The conclusions in regards to water quality parameters are not. The conclusions in regards to effects on habitat. are beyond my area of expertise, but appear to be the most thoroughly supported portion due to the modeling work.	
110	Helsel	Environmental Statistics	General		One fundamental problem with the Study is that many analyses were based on two apparently unpublished reports by the USFWS (2002 a and b). Results hinge so much on these draft reports that some statement from the Service should be included that verifies that the analyses, spreadsheets, etc. in these reports are valid, and that they received peer review and were considered accurate, even though the reports were never published. Or if this is not the case, a statement to the effect that the analyses were never peer reviewed or verified. Citations in this Study to those two reports usually do not discuss the methods that produced the conclusions, or speadsheets, or whatever product is being cited. The citations imply that what was reported is accepted as truth. What were the quality of these methods? Are there any plans for reviewing, verifying and publishing these 10-year old reports?reports that some statement from the Service should be included that verifies that the analyses, spreadsheets, etc. in these reports are valid, and that they received peer review and were considered accurate, even though the reports were never published. Or if this is not the case, a statement to the effect that the analyses were never peer reviewed or verified. Citations in this Study to those two reports usually do not discuss the methods that produced the conclusions, or speadsheets, or whatever product is	
111	Helsel	Environmental Statistics		Page 3	An example of the dependence on these two reports is the method used for extrapolation from one gage to another using regression. This procedure has for years been known to dampen variability in flows, as regression predicts mean values. So the predicted daily flows for 30 years at the Loup River at Columbus (1978-2008) relied upon in this report will not be as variable, high or low, as would have been the actual record ifit had been measured. Other methods for extrapolation (one is often called MOVE or LOC) are preferred when the probability ofhitting a high or low flow is at issue, which it is here. These probabilities of high and low events will be underestimated, as regression by design predicts values towards the center. Given that the referenced report was never taken beyond draft, methods in that report including this one may be less than 'industry standard'.	

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
112	Helsel	Environmental Statistics		Page 4	Please make the method for estimating missing evaporation data more clear. Were simply long-term monthly averages used? That is what is implied in the text. Or were monthly temperatures for the period to be estimated incorporated as well, so an unusually hot June for example had higher evaporation than the long-term average for June?	
113	Helsel	Environmental Statistics		Page 4	Isn't the statement that "the effect of flow changes in the central Platte River for the magnitude currently envisioned under the Platte River Program are not likely to be detectable at Louisville, Nebraska" (USFWS, 2002b)" one ofthe questions that this Study is to answer? Why then cite the answer, from a draft report at that, here, with implied great authority? No background or insight into the method the USFWS used to make this conclusion is presented here. I'd suggest you delete this statement until later after you have presented your analysis ofthis question. From my reading of the analysis, the Study finds that the flow changes will certainly be detectable at Louisville, decreasing II ...the flow at Louisville from 5,040 cfs to 3,290 cfs" (from page 24). So if not deleting the statement, make sure it is clear that this report finds a different result.	
114	Helsel	Environmental Statistics		Page 5	Data are not "illustrated" in a table such as Table 5. They are "listed". If they should be illustrated, draw a figure. Tables don't illustrate anything.	
115	Helsel	Environmental Statistics		Page 5	What is the objective of determining whether "water quality data can differentiate between flow conditions"? This implies that the flow data cannot differentiate, and that water quality might be needed to do this. Or do you mean "water quality is different at different flow conditions"? The latter is focused on water quality, rather than on using it to say something about flow. Clarify the objective for why this analysis is being undertaken.	
116	Helsel	Environmental Statistics		Page 5	Your title "Accuracy Assessment of USGS Stream Gage Measurements" is misleading. You aren't doing an assessment of the accuracy of their methods. No data were collected to do so. You are just using their own accuracy assessment to compute the magnitude of 10 percent of observed flows. You should rename this section. Then you compute tables of differences in uncertainty estimates (Tables 4 and 6) without stating what these are good for, or how they came about. Was the method used in the USFWS report different from yours, and therefore the differences? If so, what were the two methods and why do you think they differ? Or are these the same methods just applied to different time intervals, and no change in the physical system has occurred? If this is true, then discuss how this helps you and how the difference in flows between 1975-1994 and 1995-2008 produce the observed differences listed in Tables 4 and 6	
117	Helsel	Environmental Statistics		Page 8	I have no idea what "Program staff also provided some preliminary information evaluating the pulse flow event to the Grand Island gage" means. Please reword or delete if not important.	
118	Helsel	Environmental Statistics		Page 9	So your conclusions here are that a release of 13K AF upstream is not really discernable by the time it travels downstream to Louisville. What are the implications of this for your later findings, given that the later findings seem to disagree with this?	
119	Helsel	Environmental Statistics	Modeling		You found that you have well-calibrated models, and that the Platte acts like most other rivers in scouring the bed during high flows, increasing channel depth. You have a handle on the types of bedforms and bars likely present at differing flow regimes. This was translated into models of the amount of habitat available for different flow regimes. You evaluate uncertainty in habitat computations based on differences between measured and modeled flows. However this underestimates the true error; as errors for calibration data are always smaller than verification data not used to calibrate the model. A verification step of some sort, possibly a cross-validation procedure, should be used to quantify uncertainties instead. Yours are very likely too small.	
120	Helsel	Environmental Statistics		Page 21	These daily values are not independent. Analysis of variance (as well as other standard statistical tests) assume independence of observations, that there is no sequential correlation. There certainly is for day to day measures of temperature and water depth, and probably for the other parameters as well. The result is that sample sizes are incorrect, that 46 observations for September 2008 for example may have the equivalent information of 20 independent observations. Therefore the test should be run using n=20 rather than 46, and the differences between months may with reduced sample sizes actually not be significant. Because this was not considered, these tests do not prove that differences actually have occurred between months. The tests should be run by correcting for serial correlation, which can be done with more complex software, or by more simply computing the 'effective sample size' that is a function of the magnitude of correlation between observations in the time series.correlation. There certainly is for day to day measures of temperature and water depth, and probably for the other parameters as well. The result is that sample sizes are incorrect, that 46 observations for September 2008 for example may have the equivalent information of 20 independent observations. Therefore the test should be run using n=20 rather than 46, and the differences between months may with reduced sample sizes actually not be significant. Because this was not considered, these tests do not prove that differences actually have occurred between months. The tests should be run by correcting for serial correlation, which can be done with more complex software, or by more simply computing the 'effective sample size' that is a function of the magnitude of correlation between observations in the time series.	

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121	Helsel	Environmental Statistics		Page 21	Serial correlation similarly invalidates standard power calculations. No detail on how power was calculated is given here. Standard ANOVA power calculations assume both independence and a normal distribution, and turbidity and depth data are probably not normally distributed (the others may be based on working with similar data). Much more detail should be given here on the procedure of the power calculations.	
122	Helsel	Environmental Statistics		Page 22	Even more importantly, the questions that the power analysis and ANOVA are addressing should be explicitly stated. What is the value in these analyses? State why you are performing them.	
123	Helsel	Environmental Statistics		Page 22	Figures 42 and 43 are stated as being composed of only the May 2009 data. Yet on page 23 they are used to compare to conditions at other additional times. This isn't valid, certainly for temperature. In addition, the data should be tagged and color coded by rising and falling stages of the hydro graph. Part of the large variation for similar discharges is due to differences between water quality when the storm is rising versus falling. Turbidity can certainly be expected to be very different for the same discharge depending on which limb of the hydro graph it occurs on.	
124	Helsel	Environmental Statistics		Page 23	The meaning of the statement" the magnitude ofthe change in discharge is subject to the same uncertainty as the overall flow" is unclear. Be more specific or delete this.	
125	Helsel	Environmental Statistics		Page 23	The statement" the increase in discharge does not move the conductivity, turbidity, temperature, or dissolved oxygen outside the typical range preferred by pallid sturgeon (Figures 42 and' 43)" is too broad and sweeping of a statement considering that the figures are based on data only from one month, and you've already stated that based on an ANOVA the levels of these parameters differ between months. Graphs of the relationship between these parameters and discharge should be based on data from all four months of interest where diversions are expected (note that May is not one of those months and so is incorrectly used for the data in these graphs), while considering variation due to rising vs falling hydrograph and to temperature effects. In short, you cannot use the current graphs to make the conclusion you are heading toward.	
126	Helsel	Environmental Statistics		Page 24	a typo? The Run classification would be reduced from 45% to 34%, a decrease of 1 %??? Plus, you report different values in Appx G. Please clarify.	
127	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	SOW Question 1		The report does adequately address the overall objective as stated. The report is logically organized and compete, however, it would be helpful to include a background section early in the report that describes the type of flow conditions being considered to place the study in context.	
128	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	SOW Question 2		Yes, the physical parameters are adequate and scientifically defensible. Clearly, the need for improved scientific understanding of selection and utilization of specific, local flow conditions (both hydrodynamics and water quality) and habitat-scale flow patterns that pallid sturgeon prefer is still needed, but outside of the scope of this project. The report does a very good job of describing available data and current understanding and utilizing this information to reach the conclusions.	
129	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	SOW Question 3		Yes, the habitat classifications are adequate and scientifically defensible. In addition, to the uncertainty analysis and quantification of habitat areas by type, it would be helpful to include a broader discussion about the space-time utilization of individuals that may be residing or moving through the area. For instance, “what is known about adjacencies or distributions of habitat types”, this may be important for habitat utilization and may be impacted by stage change. From the information it did not appear that distribution or adjacency would change, but would be good to include this in the discussion.	
130	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	SOW Question 4		Yes, the report clearly addresses the detectability of the stage change from Program Water activities. It would be helpful, within the discussion section to refer to the stage discharge curves for the reach.	
131	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	SOW Question 5		Yes, the report addresses the impact of the stage change on the river parameters listed. It would be helpful to list other parameters that may be important, such as flow shear lines, and eddy structures, however, less is know about these features than the parameters given. With that said, some acknowledgement that the parameters considered may not be the only flow features that determine habitat function and utilization would be useful. The second to last paragraph of the report provides some comments towards this, but could be expanded.	
132	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	SOW Question 6		Yes, the findings of the study and conclusions reached are supported by data and sound engineering and scientific analysis. It would be beneficial to include an executive summary of the report and a clear conclusions / summary section in the report	

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133	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	General		Scientific Soundness – The methods and approaches were based on sound engineering and science. Unfortunately, although there is literature and past studies that describe general habitat preferences and utilization, there is little available information from a first-principles understanding of specific habitat needs for the species of interest. This short-coming is, however, common in most aquatic restoration and management programs. The project report uses sound, available engineering and science to address this inherent uncertainty in its habitat evaluation. Although further studies and fundamental research could improve this understanding, it is clearly outside of the scope of this project.	
134	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	General		Organization and Clarity – The report logically presents the engineering analysis of the hydrologic conditions of the study reach; data collection programs; hydraulic model construction, calibration and utilization; geomorphic assumptions and analysis, flow habitat assumptions and habitat discrimination technique; and conclusions. Uncertainties of methods, models and approaches are adequately described throughout the report.	
135	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	General		Conciseness – The report is well written and presents an appropriate amount (both depth and breadth) of information. The report also, includes relevant information in the appendices and adequately sites previous and related published work.	
136	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	General		Degree to which the conclusions are supported by the data – The report provides a logical progression from hydrologic conditions of the study reach through final conclusions, including the uncertainty of information utilized in the decision process.	
137	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	General		Cohesiveness of conclusions – The formulation of the conclusions is based on sound engineering and science. The conclusions/summary statements should have been explicitly organized in a closing, Conclusion or Summary section in the report rather than simply woven into the Discussion section.	
138	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling			In the discussion of minimum and maximum flow selection, a flow recurrence /exceedance plot would be helpful to place the selected flows in context, rather than referring to figure 2. Also the period of record should be stated for this analysis in the Study Flows section.	
139	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	Figure 2		x-axis of figure 2 should use the first day of the month for each major grid line and label	
140	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling			A better location map would be helpful to locate the study reach within the state and along the Platte River Stream network.	
141	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling			It would be helpful to explicitly state that the 2D SRH model is a fixed bed model andthis geometry is used throughout for all simulations. How this impacts the local flow conditions for higher flows should be addressed.	
142	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling			Figures 24, 25 and 26 are useful data plots, however, it would be helpful to see the distribution of the difference between model and field data on a spatial image of the study area. This would be helpful to understand the performance of the model, but likely does not negatively impact the use of the model results.	

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
143	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling		Page 24, first paragraph after table 13	….45% (+8%) of the habitat area to approximately 34% (+8%) of the habitat area, a decrease of 1%. The “1%” should be “11%”.	
144	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	Discussion		In addition to the text description, it would be helpful to tabulate the changes to habitat classification in the discussion section. This to compare across conditions of interest, and to show the impact of the management actions.	
145	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	SOW Question 1		Yes, subject to comments	
146	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	SOW Question 2		Yes, to the extent that they can actually be meaningfully evaluated by the methods used.	
147	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	SOW Question 3		This is a good example of a subject that can’t be evaluated if one considers the report in isolation, because habitats get minimal attention in this report.	
148	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	SOW Question 4		Yes and No. Yes the study answered the question; no, program activities (as to flow) cannot be detected. Effects of other activities (sediment mobilization for example) were not assessed.	
149	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	SOW Question 5		N/A	
150	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	SOW Question 6		Yes, especially given the conclusion is “did not find”.	
151	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		I consider the core elements of the study to be technically sound and useful. With some exceptions noted below, the work satisfied the scientific and technical scrutiny that was within my expertise to apply, and within the peer review budget to investigate. The study report appears to satisfy the objectives of the RFP.	
152	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		In my experience, a role of peer review is to focus on potential weaknesses or limitations in a study. Thus the critical nature of my comments should not be taken to suggest the study is seriously flawed, but rather as my effort to provide constructive input to future work. In the specific comments, I observe the following aspects of the study that I thought might be in most need of improvement or of further evaluation.	
153	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		For purposes of organization and clarity, it would be beneficial to provide an introduction that puts the study in context. See specific comments on p. 1.	
154	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		I suggest reconsidering the methodology and results of the loss analysis. See specific comments on p. 2.	
155	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		The effects of flow modification by hydropower appear to be potentially profound and need further evaluation. See specific comments on p. 8.	
156	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		The apparent rigor of certain of the analyses does not fully capture the uncertainty in the bottom line results. See specific comments on p. 20.	

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
157	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Scientific soundness* . The technical aspects of the document were generally good, with possible exceptions noted under Specific Comments.	
158	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		Organization and clarity. The Specific Comments (especially regarding Pages 1 and 9) identify ways the organization and clarity of the report could have been improved by providing additional background discussion. That being said, within what was actually presented, the report was well organized and well written.	
159	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Conciseness* . Good.	
160	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Degree to which conclusions are supported by the data* . Hard to say without copies of the data sets, spreadsheets, and models.	
161	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Cohesiveness of conclusions* . Ok within the context of the report. But there is so much unsaid, that a stranger to the process might not be able to properly judge the end results.	
162	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Presentation: Is a tightly reasoned argument evident throughout? Does the manuscript wander from the central purpose?* The true central purpose is never stated. Within the organization as presented, the report does a good job of walking through the methods, data and results without any wandering.	
163	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Methods: Are they appropriate? Current? Described clearly and with sufficient detail so that someone else could repeat the work?* Except for the evaluation of losses, the methods are appropriate and current. The level of detail in methods is good. I don't know enough about the models to know if one could repeat the work, but I suspect it would be necessary to get the actual model I/O files to do so.	
164	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Data presentation: When results are stated in the text of the manuscript, can you easily verify them by examining tables and figures? Are any of the results counterintuitive? Are all tables and figures clearly labeled? Well planned? Too complex? Necessary?* Good marks on all of this.	
165	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Statistical design and analyses: Are they appropriate and correct? Can the reader readily discern which measurements or observations are independent of which other measurements or observations? Are replicates correctly identified? Are significance statements justified?* A lot of attention is paid to statistical determinations, but there is a fair amount more that could and probably should have been said. See comments on P. 20.	
166	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Conclusions: Has the author(s) drawn conclusions from insufficient evidence? Are the interpretations of the data logical, reasonable, and based on the application of relevant and generally accepted scientific principles? Has the author(s) overlooked alternative hypotheses?* I found the overall results acceptable, since they agreed with what was fairly evident even without the study, that no significant relationships can be quantitatively established.	
167	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Errors: Point out any errors in technique, fact, calculation, interpretation, or style.* My review was not in depth, but I found nothing of concern except for the loss analysis (see comments on P. 2).	
168	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Citations: Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in the manuscript?* It's a good reference list.	

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
169	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 1	In my first paragraph of general comments, I said the study appears to satisfy the objectives of the RFP. I used the word "appears" because neither the RFP nor report does a good job of placing the study objectives into context, i.e. explaining to what ultimate purpose the work was being done. To understand the work, I relied on the Biological Opinion and the limited discussion in the Protocol. I don't fault the authors for this necessarily, as it isn't clear from the RFP that they were tasked to provide context in the report. Nonetheless, the lack of context made reading and evaluating the report much more difficult than it should have been (at least for me). The standard organization for a scientific paper includes an introduction that presents the background knowledge necessary for the reader to understand the findings of the paper. This is especially important when, as here, there is no executive summary to bring everything together. In this case the following would have been useful in providing the reader with important background knowledge: 1) A brief synopsis of the nexus between stage and sturgeon as it is now understood. Note that the fact that this paper is about pallid sturgeon isn't even mentioned until halfway through the report (p. 14). 2) One or more hypotheses about how the Program could impact that nexus (including a "non-detect" hypothesis). This would disclose the current thinking about why the study reach is important to sturgeon, and why we are interested in predicting impacts to depth, velocity, bedforms, topography and the like. 3) A clear and succinct statement of the methodological approach to evaluating the hypotheses. This might be a flow chart indicating that first we have to route Program flows to the reach; then model their impact on the parameters of interest; which means very complex hydraulic models and interpretations relating especially to bedforms; and finally translate that to impacts to sturgeon habitats. It may seem obvious, but that doesn't mean the report shouldn't be clear about what is being done. job of placing the study objectives into context, i.e. explaining to what ultimate purpose the work was being done. To understand the work, I relied on the Biological Opinion and the limited discussion in the Protocol. I don't fault the authors for this necessarily, as it isn't clear from the RFP that they were tasked to provide context in the report.	
170	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Pages 1 and 2	Figure 1 would benefit from an inset location map.	
171	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 2	The loss analysis is an update of a FWS study provided in Appendix A. It is difficult to fully evaluate the method without a copy of the spreadsheet. Nonetheless, I was very surprised about the results, and wonder if the Program is approaching this important issue correctly. I did not review Appendix A in sufficient detail to know for sure that my concerns are valid, so please consider this discussion accordingly. My two primary concerns are as follows. Some of the loss rates reported are much higher than I have seen, even in arid western rivers. If it has not been done, I strongly recommend each element of the loss be independently verified. For example, analytical methods using groundwater head data can be used to independently estimate seepage losses. It appears that the method calculates Program losses in proportion to flows. An alternative (and in my experience more appropriate) approach is to calculate them on an incremental basis. If the current procedure has not been affirmatively deemed more appropriate than an incremental approach, the incremental method should be To illustrate my concern, consider the result of the accounting done by the Bureau of Reclamation for the loss of water imported into the Rio Grande Basin (this loss rate is important for quantification of endangered species impacts as well as available water supplies). Based on quantification conducted by the Rio Grande Compact Commission, a loss rate has been calculated for the reach from Heron Reservoir (near the Colorado border on a tributary of the Rio Chama) to Albuquerque (a distance roughly comparable to Grand Island-Louisville). The loss rate applies to the flow added to natural flow by imported water. There are elements of the rate calculation that are not entirely apples-apples to that made for the Lower Platte, but these would have a modest effect at most. The Rio Grande loss rate is 2%. Given this result, it is difficult for me to understand loss rats as high as 90% in eastern Nebraska. The subject of losses above Grand Island is not considered, but it would be of interest to know the Louisville flow as compared to an upstream reservoir release The following comment is not related to the above, but to the reference to selection of "appropriate" flows on page 2. Appropriate how? With no discussion of matters such as sturgeon habitat, the reader cannot know. It is also confusing to indicate that a flow of 39,000 cfs is of "primary interest", without explaining why it was then appropriate to use 8,000 cfs as the high end of flows selected.	

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
172	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 3	I did not understand how the study made use of two different periods of record for extended analysis.	
173	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 4	The new spreadsheet analysis probably should be provided in an Appendix.	
174	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 5	The power analysis probably should be provided in an Appendix.	
175	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 6	The focus on gage uncertainty may cause readers to overlook the uncertainty in the USFWS spreadsheet which estimates impacts of Program flows.	
176	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 7	In addition to the plots in Figures 3, 4 and 4a, it would be interesting to see the data plotted as flow duration curves.	
177	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 8	This page presents Figure 5 and makes note of the “obvious” intraday flow variation. The discussion focuses on how to smooth that out so the pulse can be translated from Grand Island to Louisville, which is certainly appropriate. However there is no discussion whatsoever about the fact that the hydropower effect causes a 1 foot diurnal change in stage, which is far greater than the transformed impact of the pulse. The implied premise of the study is that stage impacts habitat, through effects on velocity, depth and bedforms. If so, how is it that the effects of such a large and rapid stage change are not considered at all? Had the study found that Program releases did impact habit in the study reach, that conclusion would have been called into question because the interday flow variation was not considered and could be such that it swamped out any Program impact.	
178	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 9	Another aspect of context that wasn’t effectively presented was the cause-effect relationship being studied. The stated objective puts “stage” as the focal point, whereas after reading the report, I perceive the operational objective was to evaluate the impact of flow (cfs) as it directly impacts water depth and velocity, and the consequent effects on sediment, bedforms and habitat. Stage as such seemed not to be that much of a consideration, or a particularly good surrogate, especially in terms of assessing velocity and its consequences. The lack of hypotheses was surprising given the nature of the Adaptive Management Plan.	
179	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 10	Given that stage is the focus of the study, are two water surface data points sufficient for the cross-sections?	
180	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 11	It would be useful to have an assessment of the change in roughness with flow, and especially whether it is reasonable to interpolate values.	
181	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 12	I did not follow the explanation of the very low n values for the 2D model.	
182	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 15	The entire bedform discussion would benefit from illustrations.	
183	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 19	I found Figure 36 hard to interpret.	

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
184	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 20	The use of a Monte Carlo analysis to assess uncertainty gives an impression of statistical rigor to the results. Certain other aspects of the work give a similar impression. However if one starts at the very beginning of the work, i.e. an increment of flow at Grand Island (with unstated uncertainty), and carries it through to the end, many other issues become apparent – the loss estimates, hydrograph translation, error bars on model inputs (median grain size is a good example), and more. This cascade of uncertainties would have undermined the results had a positive relationship been found. As the bottom line of the report did not assert any relationships had been statistically demonstrated, these issues are perhaps not critical. Still, I would have liked to see (in the discussion section) a recap of all the assumptions, limitations and uncertainties in the work.	
185	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 22	Of interest given prior discussion, the models are (correctly) said to evaluate depth and velocity, not “stage change”. One question not posed previously: why is the release being evaluated so small?	
186	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 25	Perhaps emphasize that lack of statistical significance does not equal lack of effect. In fact, qualitatively one can say that a release probably does have at least marginal benefit (this is a bit more affirmative than “no additional stress”).	

ATTACHMENT 2

Lower Platte River Stage Change Study
Peer Review Submissions

Reviewer #1

Dr. David Gaeuman

Expertise: Fluvial Geomorphology

Review of:
Lower Platte River Stage Change Study Final Protocol Implementation Report

The scope of this study outlined in the RFP targets two related, but distinct, objectives: determining what measurable effect, if any, Program water delivered at upstream locations will have on discharge in the Platte River downstream from its confluence with the Elkhorn River, and quantifying how changes in discharge might translate to changes in hydraulic parameters and physical habitat characteristics in that stream segment.

The authors of the study approach these two objectives quite differently. With respect to how discharge affects habitat, the authors present an analysis based on numerical modeling of flow under existing geomorphic conditions. Although this modeling analysis neglects the potential for future flows to modify the current stream configuration and produce longer-term changes in habitat availability, it does address the question posed in the RFP. The question, the approach used to address it, and therefore the review of the analysis, is straight-forward. My review of that portion of the report is presented first.

For the question regarding the effect upstream Program water on downstream discharge, however, the authors opted to rely heavily on some earlier Fish and Wildlife Service analyses, which were incorporated in the report as Appendix A and Appendix B. In doing so, they implicitly endorse those reports and accept some level of responsibility for any problems with the methods and explanations presented in them. I found those reports quite difficult to interpret, so I'll save my comments on that portion of the Stage Change Study for last.

I note here that I have not attempted to systematically copy edit this report because, according to the title, this is a Final version. I take that to mean that typographic errors, unclear statements, and so on will not be corrected as might happen if this were a Draft version. Instead, my comments focus on the broader-scale "Specific Questions" identified in Review scope of Work and the "Specific Comments," "Rating," and "Recommendation" identified in the PRRIP Peer Review Guidelines. The questions from the Scope of Work and the Peer Review Guidelines are addressed explicitly following my free-form comments on the Hydraulics and Geomorphology section and the Hydrology section.

Hydraulics and Geomorphology

General Comments and Recommendation on Hydraulics and Geomorphology Section

The approaches used to address the question posed in the RFP are appropriate. The general approach of modeling hydraulic parameters and using model output to classify habitat types is good. It could perhaps be improved by incorporating bedform types into the classification system, in addition to depth and velocity. Bedforms can have a large effect on flow velocities and turbulent structures near the bed, and so are likely very important components of physical habitat. The section on describing and predicting bedforms is good, but it's not clear whether or how that information was used to inform the final conclusions of the study.

The contractor appears to have an adequate understanding of the modeling tasks to produce credible results. However, the modeling analysis seems to include some mistakes and

misinterpretations that might have the potential to affect the Study's conclusions and recommendations. Two problems with the model itself are worth highlighting: the 2d model domain lacks lead in and lead out sections and is generally too short (see comment 7), and the quantity of topographic data appears to be very small compared to the resolution of the model mesh (see comment 8). Both of these issues substantially degrade the accuracy of the model and the confidence that can be placed in its output. Two additional issues regarding the interpretation of the model results are worth mentioning: The sensitivity analysis regarding how model errors affect habitat classification may be flawed (see comment 20), and percentages in each habitat type are based on submerged area rather than total area (see comment 26). That said, I doubt that correcting these problems would materially change the Study's conclusions concerning how incremental changes in discharge alter habitat availability.

Specific Comments on Hydraulics and Geomorphology Section

1. Page 9: "A hydraulic and geomorphologic analysis…" not sure what part of this is a geomorphologic analysis. It's mostly limited to hydraulic modeling.

2. Page 9, last paragraph: "…trend over this period." Which period?

3. Page 10, 2nd paragraph refers to a 10-year model run. What does that mean?

4. Page 10, 3rd paragraph: Not sure what's meant by the different model versions incorporating cross sections from different dates. The preceding sentence is about water surface elevations at the cross sections. Were different cross sections (geometry) used in the two model versions, or just different water surface elevations for validation?

5. Table 7: Table headings are unclear and awkward. I'm not sure what an average maximum or average minimum is. Are these the extreme instantaneous values for a given day averaged over X number of days? Is "average mean" the average of X number of daily mean values, or the average of something else? The text on page 10 that references Table 7 doesn't help with this.

6. Page 11: The discussion of the models of different dates is poorly organized and confusing. It would help if the point of all this were explained at the outset. Much later in the text, in the section about bedforms I believe, it becomes apparent that the point is to account for differences in roughness due to differences in bedform regime at different flow levels.

7. Page 12, 4th paragraph, Figures 19-20: The model mesh is 1,700 ft long. From the figures, it's seen that this corresponds to about 1 channel width. This is far too short of a model reach. First, it is a very small sample in term of area from which to generalize about the river segment. But more importantly, every point within the model is a short distance from the model boundaries. It is standard practice to extend the model mesh at least a few channel widths upstream and downstream of the reach of interest. That allows some space and time for any errors or imperfections in the boundary conditions to dissipate.

8. Page 12, last paragraph refers to "detailed topographic and bathymetric data" used in the model. There is no indication in this report that detailed topographic data was collected. The only

discussion along those lines concerns collection of a relatively small number of cross sections. The 2d mesh is said to have a mesh resolution of 10 feet. This density is irrelevant unless the topo data mapped to the mesh is of similar resolution, as might be obtained with an intensive sonar survey using an array of transducers or a multi-beam. There is no indication that this was the case. The value of the fine mesh is, to a large extent, nullified if the topography was interpolated from cross sections.

9. Page 12, last paragraph: It's not explained where the n values of 0.023 and 0.027 in the 2d model came from. Were these transferred from the 1d calibration in some way?

10. Page 13, 4th paragraph, Figures 24-26: It is stated that the match between measured and modeled water surface elevation and water velocities is "good." This seems to be an overstatement. Plus or minus 0.5 ft in elevation does not seem especially good to me, and velocity errors seem to range up to around 50% (Figure 26).

11. Pages 14-15: Nice overview on bedforms.

12. Page 16, 2nd paragraph: S'* is introduced, but not defined until it come up again on page 17. Same for SG in the equation given for d*.

13. Page 16, last paragraph: I think this should be the relation between the average shear stresses (as indicated in equation 1), rather than velocity.

14. Page 17: Some of the notation seems odd. τ' is used in the definition of S'*, but is not defined (equation 1 introduces τ'_0 and τ, but not τ'). Should it be just τ? The shields parameter is denoted F* -- why not use τ* or θ like most everyone else? (SG-1) is often denoted by *R*, and SG itself is usually ρ/ρ_s. I've usually seen transport stage denoted with T rather than S.

15. Page 17, 4th paragraph: the VBA script is said to solve for the "necessary values…" It's difficult to be sure what is being done here. I infer that τ is specified on the basis of model output, and equation 1 is solved for τ'_0, but that's not clear from the text.

16. Page 17, last paragraph: Discussion switches abruptly from bedform types to how much of the site is subaerially exposed. What's the connection?

17. Pages 18-19, habitat evaluation: This seems like a good approach. Why are there no pools in this classification? Are especially deep scours and holes not relevant for sturgeon, or perhaps these environments are not present in the Platte?

18. Page 20, top: re-states that the model is well calibrated. See comment 10.

19. Page 20, numbered item 1: velocity units are given as ft.

20. Page 20, numbered item 2: Was the simulated error applied to each node independently? Or to put it another way, would adjacent nodes be assigned uncorrelated errors? That would clearly be incorrect – for example, if a given node had a large positive error in depth, all nearby nodes

(and maybe every node in the model) would probably also have positive errors. Assigning each node an error that is independent of all the other errors would cause the random errors to cancel, and probably result in very little net change in the proportion of particular habitat types.

21. Page 21: The text says that Table 11 shows variation among transects and among sample episodes, but it doesn't show that. Is a "sample episode" a day?

22. Table 12 and top of page 22: The table suggests that conductivity and turbidity behave in the same way with respect to different "phases" (what's the independent variable here, discharge maybe?). Meanwhile, Figure 42 shows that they behave in opposite ways. What point is being made with these statistics anyway?

23. Page 22, 3rd paragraph: What is meant by "bottom velocity?" This must refer to some height above the bed.

24. Page 22, 3rd paragraph: The explanation for why run and plunge habitat is considered most suitable is not very convincing. Where are the sturgeon actually found? Do the cited publications refer to run and plunge habitats?

25. Page 23, 1st paragraph: The gaging error magnitudes defined in the hydrology sections are applied here. I suspect that the interpretation of gage errors may have a problem – see comment 32.

26. Page 23-24: The actual changes in the availability of various habitat types may change more with discharge than is indicated. It appears that the percentages given for habitat types are the percents of the total submerged area. It would be more meaningful to report this in terms of actual area or as a percentage of the model domain area because the extent of the submerged area changes with discharge.

Hydrology

General Comments and Recommendation on the Hydrology Section

The hydrology studies presented in the two USFWS reports and incorporated into the Stage Change Study leave much to be desired in terms of both technical credibility and the clarity of the presentation. Some of the problems with the original reports are noted in the specific comments below. The authors of the Stage Change Study apparently reproduced the analyses described in the USFWS reports. That would require sorting out the details regarding what those analyses involved. Having done that, I would expect the authors of the Stage Change Study to provide a better description of what they did than simply referencing and copying text from the Appendices.

The flow losses due to evaporation, transpiration, and seepage estimated in these reports are, in my opinion, unreliable. The reported total loss figures become more credible if they are considered to be generic losses, not attributable to any particular sink. Nonetheless, I agree with general conclusion that small discharge augmentations upstream of Grand Island of the magnitude discussed will not be very noticeable at Louisville. This is not so much related to

gaging uncertainty (which I think is overestimated in the reports), but is instead due to the fact that the augmentation volumes discussed are small compared to everything else that is going on. Changes in flow on the order of 100 cfs would be difficult to distinguish even if the gages were perfectly accurate, because the changes can be swamped by much larger flow fluctuations caused by a variety of other factors.

Specific Comments on the Hydrology Section

27. Page 2, end of second paragraph: States that the selected flows are considered appropriate for modeling, but doesn't explain why. Does anything about pallid sturgeon habitat enter into this determination?

28. Table 2 and associated text: Meaning of the headings indicating time periods are unclear. These look like periods of record for the gages, but are not. Time periods listed for the Loup near Columbus include times when there are no gage records. It takes careful picking through the text to figure out how to interpret these dates. I'm unsure of what is meant by "period of analysis." This could refer to the period from which flow records were drawn to quantify the hydrologic characteristics of the gage site, which could then be extrapolated to other years, or it could mean that consideration of the gage site was entirely confined to that time period.

29. Page 3, 3rd paragraph: This paragraph is very hard to follow. It does not clearly identify what is being estimated – language like "the USFWS analysis" and "these flows" do not identify the gages and dates for which flows were being reconstructed.

30. Page 3, last paragraph: A new gage can apparently supply better information about powerhouse return flows, but was not used. This information could have at least been used to check on the accuracy of the method in the USFWS analysis.

31. Pages 4-5: The Study basically just sends the reader to Appendices A and B. There appears to have been little or no critical review of the USFWS reports by the Study authors.

32. Page 5, last paragraph: This interpretation of gage accuracy seems overly simplistic. It is stated that the USGS considers 95% of the gage readings to be within 10% of the actual discharge. This report follows the USFWS reports in translating that into error bounds of plus or minus 10%. Assuming the errors are independent random variables, the actual error bound should be related to the number of samples used to generate an estimate. For example, the USGS error estimate could be interpreted as suggesting that the individual errors have a standard deviation of around 5% (because close to 95% of a normally-distributed population is within 2 standard deviations of the mean). Whether the standard deviation is 5% or something else, the standard error of the estimate is equal to the standard deviation divided by the square root of the sample size. If the estimate is monthly mean flow, the sample size is about 30. These numbers suggest that the error bound for the monthly mean might be around 2% at the 95% confidence level. I am not a statistician, and the details of this example may not be exactly correct. For example, the errors on sequential days are probably correlated to some degree. The point is simply that the 10% error bounds assumed in the reports need to be re-examined.

33. Page 7: In repeating the USFWS reports, the Study incorporates an abundance of errors, confusing explanations, and obscure objectives. Page 7 discusses what happens to an incremental increase in flow at Grand Island by the time it reaches Louisville. The discharge increments considered seem arbitrary. It would be most helpful if the Study would explain why these particular increments are relevant, and more generally, what "Program water" or "First Increment water" is.

After consulting the Biological Opinion, the Adaptive Management Plan, the Record of Decision, the Platte River Recovery Implementation Program Final Environmental Impact Statement, and the Platte River Recovery Implementation Program, I've determined that First Increment water refers to 130,000 to 150,000 acre-feet of water annually, perhaps in the form of baseflow discharge targets or (undefined?) pulse flows. Spread evenly across the full year, that volume of water is equivalent to about 200 cfs, which is in the range of increases being evaluated.

I speculate that the documents I've consulted are ambiguous about Program water because it has not yet been fully determined how that water is to be used. If so, the hydrologic analyses in the Study seem to be putting the cart before the horse. They seem to ask: if the upstream flow is bumped by X, could it be detected downstream, and would it materially improve habitat? Would it not make more sense to go about it other way around? That is, to ask: How much of an increase in flow is needed in the lower river to materially improve habitat there, and how much discharge needs to be added to upstream flows to hit that downstream target? Perhaps this is how the question is being approach, but it's hard to tell from what's written.

34. Page 7, 5th paragraph: The paragraph begins and ends describing evaporation trends, but refers to total volume lost in the middle. It's unclear whether this means total volume lost through evaporation, or total volume lost including seepage losses. It's also unclear whether evaporation here includes transpiration.

More generally, the analysis contained here and in the USFWS reports is often muddled in this regard. Terms like evaporation and ET do not seem to be used in a consistent manner throughout. However, the distinction may be an unnecessary complication, given the methods used to estimate these losses. See comments on that later.

35. Page 8: The section on hydrograph translation is difficult to interpret. It could be greatly improved by telling the reader more specifically what the EA flow was. Four paragraphs into the section it is noted that "the peak of the EA flow at Duncan is estimated to be approximately 2000 cfs above base flows." From this, a reader might infer that something like 2000 cfs was released from somewhere upstream or otherwise generated somehow. Is there some reason that what was done and where it was done can't be clearly stated?

36. Appendix A, page 1: The report discusses evaporation and seepage losses. Are there no diversions or pumps to consider?

36. Appendix A, page 2: The Figure 1 referenced here is missing. The same or a similar figure 1 is missing from Appendix B as well. The missing figures seem to be maps showing where all these gages, reaches, and tributaries are.

37. Appendix A, Page 5: Estimated lag times are very crude. All are integer days, and variations in lag time with discharge are not considered. This component of the analysis deserves more attention than it was given.

37. Appendix A, Page 5: Figure 2 referenced here is missing.

38. Appendix A, Page 6-7: It would make sense to look at channel width during the time of year when evaporation losses are greatest. Seasonal trends in channel widths were considered indirectly through the application of "liberal" and "conservative" widths. Seasonal differences in width could be addressed more directly.

39: Appendix A, Page 8: The use of pan evaporation rates to estimate river evaporation rates is a big leap. I suspect that the temperature of the pan is quite different than the temperature of the river. The pan coefficient might be intended to account for that, but no explanation or justification for the factor of 0.7 is given. The adjustment factors used for ET losses also lack explanation. These things need to be explained.

40: Appendix A, Page 9-10: Seepage losses are calculated as the difference between the net inputs to a reach (inflows minus E/ET losses) and the outflow from the reach. This raises the question of why the analysis even bothers to estimate E/ET, because its magnitude is irrelevant to the result. If the estimate of E/ET was arbitrarily increased by 20 cfs, for example, the corresponding estimate of seepage loss would come out 20 cfs lower. The total loss, however, would remain the same regardless of what value was used for E/ET. It would be simpler and equally useful to simply define "losses" as the difference between inflows and outflows without regard to whether they are E/ET or seepage.

41: Appendix A, Page 10: States that "Percent ungaged gains were not calculated, as this quantity is not relevant to this analysis." I'm not sure how to interpret this statement, but I do not agree that gains are irrelevant. It's also unclear whether "gain" refers to ungaged tributary input only, or to all gains (such as groundwater inflows and return flows from diversions).

42: Appendix A, Page 11: Seepage loss estimates are called "conservative." It would be clearer to say the reported losses underestimate the actual losses. It would also be good to say something about the magnitude of underestimation.

43. Appendix A, bottom of Page 12: "Total estimated daily evaporation + ET losses" are given in units of cfs, that is, rate units instead of volume. And again on page 14. The figures referenced in this text give the losses in percent of flow.

44. Appendix A, Page 18, 1st paragraph: This paragraph is unnecessarily confusing. The example discusses a reach, a subreach, a stream gage, and added Program water with no explanation of the geographic relationship between these elements. That difficulty would be partly relieved if Figure 1 wasn't missing from the report. It is stated that flow is 1000 cfs at Duncan on a particular day. It then refers to the "historic Platte River inflow," which, from the arithmetic that follows, appears to refer to the 1000 cfs at Duncan. Then, 200 cfs of Program water is introduced, although it's not clear how or where. Again, from the arithmetic, it seems that the

Program water is also an inflow at the top of the reach, so that the flow at Duncan is actually 1200 cfs, not 1000 cfs. The presentation of the arithmetic is also overly complicated. It could be presented as three simple operations: determine the volume of inflows (including distance weighted gains), calculate the proportion of the inflows that are lost to E/ET (equal to losses/inflows), and multiply the Program water volume by that proportion.

45. Appendix A, Page 19, 5th paragraph: The sensitivity analysis for open water width needs more explanation. It seems to me that, according to how the total losses are calculated, changing the open water width would have zero effect on total losses because E/ET is subtracted from inflows before computing seepage losses. Could it be that the authors of this report applied 2 different estimates of E/ET to the same analysis? That is, did they subtract the original estimate of E/ET from inflows, then calculate seepage losses, then use those seepage losses with new, larger estimates of E/ET to arrive at new total losses? That would clearly be incorrect.

46. Appendix A, Figures 9 and 10: Why do these graphs present different results than the similar graphs in Appendix C of the other USFWS report included as Appendix B (Page 17 in Appendix B)? Graph titles and axes labels are the same in both appendices, but the plotting positions differ.

47. Appendix A, Page 23, 1st paragraph: States that there are no major diversions below Grand Island. What about numerous small diversions? Has that been evaluated?

48. Appendix B, Page 5, 6th paragraph: Mentions a Tri-County supply canal system. I didn't see that mentioned anywhere else. I wonder where that is, and if it is, or should be, considered in the analysis presented in Appendix A.

49. Appendix B, Table 2: Uncertainty is assumed to be 10% of the measured flow. See comment 32.

50. Appendix B, Table 3: I'm wondering why the effect of First Increment Program activities is to cause negative changes in flow in some months. Here would be a good place to provide some explanation as to what First Increment Program activities include.

51. Appendix B (page 16) of Appendix B: These travel times could be used to improve the Appendix A analysis.

52. Appendix D (page 18) of Appendix B and text on pages 9-10: Would be appropriate to define what the "OPSTUDY Model" is.

Reply to Specific Questions in the Review Scope of Work

1) Does the Stage Change Study adequately address the **overall objective** of the RFP, which is *"…to develop information needed to evaluate the effects of Program water management activities, including new activities covered by state or federal depletion plans, on water stage and how those stage changes affect physical parameters in the reach of the lower Platte River from the Elkhorn River confluence to the Missouri River confluence"*?

Yes

2) Are the physical parameters and measured data considered in the study (flow quantity, depth, velocity, temperature, turbidity, sediment, and sandbars and bedforms at selected sites throughout the study reach) adequate and scientifically defensible for the purposes of the study?

Yes. However, bedforms played a very minor role in this study. It's not clear how they were incorporated into the quantification of sturgeon habitat availability.

3) Are the habitat classifications considered in the study (slackwater, flat, riffle, run, isolated pool, and plunge) adequate and scientifically defensible for the purposes of the study?

Yes, but I do not claim to be an expert in that subject.

4) Is the Stage Change Study sufficient to determine if First Increment Program water activities can be detected (statistically significant beyond the error of the gauging equipment) from base flow conditions?

No. A better evaluation of gaging errors is needed, as described in my comments above. I would also suggest that the idea of detectability be better defined. It seems that for a small water augmentation to be detected, one would have to know what the discharge would have been without the augmentation. How would the work? And what is the time scale over which the detection should occur? Detecting a small change on a particular day is a different matter than detecting a sustained small change over a month or a year.

5) If "yes" to Question #4 above, is the Stage Change Study sufficient to detect if First Increment Program water activities have an impact (statistically significant beyond the error of the gauging equipment) on stage, velocity, temperature, turbidity, substrate, or channel morphology?

6) Are the findings of the stage change study and the conclusions reached in the report supported by the data and analysis?

Yes.

Reply to Specific Questions in the PRRIP Peer Review Guidelines

1. Presentation: Is a tightly reasoned argument evident throughout? Does the manuscript wander from the central purpose?

The manuscript stays on task well. It addresses the questions posed in the RFP.

2. Methods: Are they appropriate? Current? Described clearly and with sufficient detail so that someone else could repeat the work?

General methods are appropriate, but the description of methods in the hydrology section is poorly organized and difficult to follow. Methods in both the hydrology and hydraulic sections are deficient in certain details, as is described in my comments above.

3. Data presentation: When results are stated in the text of the manuscript, can you easily verify them by examining tables and figures? Are any of the results counterintuitive? Are all tables and figures clearly labeled? Well planned? Too complex? Necessary?

Many of the tables contain headings that are difficult to decipher, especially in the Hydrology section. Instances of this are pointed out above.

4. Statistical design and analyses: Are they appropriate and correct? Can the reader readily discern which measurements or observations are independent of which other measurements or observations? Are replicates correctly identified? Are significance statements justified?

There is little in the way of formal statistics in this study. An instance in which error margins on gage records may be misinterpreted is pointed out in my comments above.

5. Conclusions: Has the author(s) drawn conclusions from insufficient evidence? Are the interpretations of the data logical, reasonable, and based on the application of relevant and generally accepted scientific principles? Has the author(s) overlooked alternative hypotheses?

The general conclusions of the study are reasonable.

6. Errors: Point out any errors in technique, fact, calculation, interpretation, or style.

I have done that in my comments above.

7. Citations: Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in the manuscript?

The citations given seem reasonable, but additional supporting discussion and references is needed in some parts of the study. For example, the reasoning and sources used to choose values for evaporation and transpiration coefficients are not given. See detailed comments above.

Rating

Scientific soundness – 4
Degree to which conclusions are supported by the data – 3
Organization and clarity – 3 (hydraulics) and 5 (hydrology)
Conciseness – 3

Recommendation – If this were a draft to be revised I'd recommend major revision. But it seems to be a final report, so my recommendation is to accept its general conclusions as being qualitatively correct.

Reviewer #2

Dr. Christopher S. Guy

Expertise: Fisheries Ecology and Aquatic Resource Management

Lower Platte Stage Change Study Peer Review Questions

1) Does the Stage Change Study adequately address the **overall objective** of the RFP, which is "…to develop information needed to evaluate the effects of Program water management activities, including new activities covered by state or federal depletion plans, on water stage and how those stage changes affect physical parameters in the reach of the lower Platte River from the Elkhorn River confluence to the Missouri River confluence?"

   The Stage Change Study does address the overall objective of the RFP for a specific area in the Platte River. I believe that the study could have been more robust by extending the spatial extent of the study. The objective clearly states '…from the Elkhorn River confluence to the Missouri River confluence,' but the study was conducted on a reach from the Nebraska highway 50 bridge to the Chicago Rock Island and Pacific Railroad pedestrian bridge. I would agree that this reach is likely representative of much of the lower Platte River and is an area where pallid sturgeon have been located (Peters and Parham 2004); however, the Platte River at the confluence with the Missouri River is likely quite different and should have been included. The confluence is central to these analyses because much of the use of the Platte River by pallid sturgeon occurs near the confluence (Peters and Parham 2004). Had the investigators conducted measurements in at least two reaches (i.e., the current reach and one at the confluence), preferably more than two reaches (i.e., also include a reach near the Elkhorn River confluence), the precision, understanding of uncertainty, and inference space would have been greater with respect to Program water management activities. Further, the confluence reach is unique given that discharge in the Missouri River can influence the habitat dynamics in the Platte River which in turn will affect the results of Program water management activities, most likely different than the reach near Louisville, Nebraska. This criticism is especially relevant to the 2D modeling exercise which provides the most useful information for pallid sturgeon conservation. Understanding the effects of Program water management activities for additional reaches in the Platte River is instrumental if the Governance Committee is going to use this information to determine the effects of discharge on physical parameters thought to be important to pallid sturgeon.

   The effects of stage changes on physical parameters appears to be well studied for the reach near Louisville, Nebraska and should provide information needed to evaluate Program water management activities in that area. With that said, it would be beneficial if the investigators made it more clear regarding the discharges under which empirical data were collected, it is difficult to determine as currently written.

2) Are the physical parameters and measured data considered in the study (flow quantity, depth, velocity, temperature, turbidity, sediment, and sandbars and bedforms at selected sites throughout the study reach) adequate and scientifically defensible for the purposes of the study?

The selected physical parameters seem reasonable given the current state of knowledge regarding pallid sturgeon ecology. However, it is unclear what aspects of the pallid sturgeon life-history are targeted by Program water management activities. Providing habitat for adults is likely quite different than providing habitat for larvae. I realize this was not part of the scope of research for the investigators, but should be considered by the Governance Committee. This will help refine the effects of Program water management activities and how they relate to specific aspects in the conceptual models. Defining the life-history aspects of interest will also make the physical parameters more scientifically defensible. It is becoming clearer that habitat diversity and complexity are important to riverine fishes. Thus, combining metrics into a richness or diversity value and evaluating those data as a composite with varying Program water management activities might be more ecologically relevant than studying each parameter separately.

3) Are the habitat classifications considered in the study (slackwater, flat, riffle, run, isolated pool, and plunge) adequate and scientifically defensible for the purposes of the study?

   The selected habitat classifications seem reasonable given the current state of knowledge regarding pallid sturgeon ecology. It may be implicit in some of the habitat classifications, but a more detailed analysis of the thalweg dynamics would have been informative (e.g., thalweg depth and migration under varying discharges). I believe understanding the dynamics of the thalweg given varying Program water management activities would be highly beneficial given that several studies indicate that pallid sturgeon are typically found in or near the thalweg. I recognize that the investigators are aware of the importance of this habitat type because they allude to it when they discuss run and plunge habitat. Again, it is important that the life-history aspect of interest is well defined because habitat use likely changes with ontogeny. As stated above, combining habitat classifications into metrics that describe the richness or diversity of habitat may be more ecologically meaningful.

4) Is the Stage Change Study sufficient to determine if First Increment Program water activities can be detected (statistically significant beyond the error of the gauging equipment) from base flow conditions?

   Yes, given the error associated with the Louisville gage and the results from the 100, 500, and 1,000 cfs additional Program water at Grand Island reaching Louisville as summarized in Figures 3, 4, and 4a. However, the amount detected varies temporally.

5) If "yes" to Question #4 above, is the Stage Change Study sufficient to detect if First Increment Program water activities have an impact (statistically significant beyond the error of the gauging equipment) on stage, velocity, temperature, turbidity, substrate, or channel morphology?

   Yes, relative to stage and velocity, but not temperature, turbidity, substrate, or channel morphology because those are not measured by the gauging equipment. It is clear in

the results that there is temporal variation in water quality metrics and that the variation can be detected given the sample sizes, but it is not clear how the variation in water quality metrics relate to Program water activities.

6) Are the findings of the stage change study and the conclusions reached in the report supported by the data and analysis?

   In general, I believe the conclusions are supported by the data, although the conclusions are not clearly articulated. I am concerned that most of the analyses and measures of variation represent pseudo-replication. This relates to my comments in the first question. I believe the best way to determine the effects of Program water activities on physical parameters that are thought to be of significance to pallid sturgeon would be to conduct the Stage Change Study in multiple reaches (i.e., the reaches are the experimental unit). Although one could argue that reaches are not independent, I surmise that it better represents available habitat for pallid sturgeon and the influence of Program water activities on that habitat. The most important aspect of having multiple reaches is that one will have a better understanding of the uncertainty of Program related water activities on pallid sturgeon habitat.

If the answer to any of the questions above is "no", please suggest possible remedies to data collection methodologies, analysis, or other study tasks.

General Comments:
1. Scientific soundness

See comments above regarding replication.

2. Organization and clarity

I believe the report could be more clearly organized. One thing that would help is standardization with primary, secondary, and tertiary headings. Executive summary and conclusion sections would also be helpful.

3. Conciseness

The report is concise.

4. Degree to which conclusions are supported by the data

Again, see comments above. Overall, I believe the conclusions are supported by the data, but the robustness of the data and conclusions could be enhanced by a better experimental design.

5. Cohesiveness of conclusions

Specific Comments:
Please support your general comments with specific evidence and literature. You may write directly on the manuscript, but please summarize your handwritten remarks separately. Comment on any of the following matters that significantly affected your opinion of the manuscript:
1. Presentation: Is a tightly reasoned argument evident throughout? Does the manuscript wander from the central purpose?

I believe the authors could do a better job of organizing the methods, results, and discussion by question being addressed.

2. Methods: Are they appropriate? Current? Described clearly and with sufficient detail so that someone else could repeat the work?

See above.

3. Data presentation: When results are stated in the text of the manuscript, can you easily verify them by examining tables and figures? Are any of the results counterintuitive? Are all tables and figures clearly labeled? Well planned? Too complex? Necessary?

Data presentation is excellent and can verify the results with the tables and figures. Some of the figure captions could be expanded to provide more substantive information.

4. Statistical design and analyses: Are they appropriate and correct? Can the reader readily discern which measurements or observations are independent of which other measurements or observations? Are replicates correctly identified? Are significance statements justified?

See above. This is the major shortcoming of the study. That is, I believe the measurements for most analyses are not independent (i.e., true replicates). I would encourage the authors to clarify their

experimental units and replicates and explain how they are relevant to the inference space described in the RFP.

5. Conclusions: Has the author(s) drawn conclusions from insufficient evidence? Are the interpretations of the data logical, reasonable, and based on the application of relevant and generally accepted scientific principles? Has the author(s) overlooked alternative hypotheses?

See above.

6. Errors: Point out any errors in technique, fact, calculation, interpretation, or style.

See above.

7. Citations: Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in the manuscript?

RATING:
Please score each aspect of this manuscript using the following rating system: 1=excellent, 2=very good, 3=good, 4=fair, 5=poor.

Rating
Scientific soundness _3__
Degree to which conclusions are supported by the data _3__
Organization and clarity _4__
Cohesiveness of conclusions _3__
Conciseness __2_
Importance to objectives of the Program _2__
(For use by internal review panel only)
RECOMMENDATION (check one)
Accept ___
Accept after revision __x_
Unacceptable ___

Reviewer #3

Dr. Dennis R. Helsel

Expertise: Environmental Statistics

TECHNICAL REVIEW OF
" Lower Platte River Stage Change Study Final Protocol Implementation Report, Version 1.0", dated December 2009

A. Lower Platte Stage Change Study Peer Review Questions

1) Does the Stage Change Study adequately address the overall objective of the RFP, which is "…to develop information needed to evaluate the effects of Program water management activities, including new activities covered by state or federal depletion plans, on water stage and how those stage changes affect physical parameters in the reach of the lower Platte River from the Elkhorn River confluence to the Missouri River confluence?"

   The Study adequately addresses the relative magnitude of stage change due to management activities in relation to existing flows and habitat of the pallid sturgeon. It does not discuss the proposed changes in light of existing appropriations and any current legal constraints on flow in the Platte River. In other words, if these diversions were implemented would they impact the water rights of existing rights owners? The method for extrapolation of missing record to the Loup River at Columbus is flawed, and so the resulting errors on the analysis are unknown.

2) Are the physical parameters and measured data considered in the study (flow quantity, depth, velocity, temperature, turbidity, sediment, and sandbars and bedforms at selected sites throughout the study reach) adequate and scientifically defensible for the purposes of the study?

   The data themselves are presumably scientifically defensible. They are fairly routine parameters with established protocols for collection. The amount of data is adequate. Analysis of the data is not adequate, if the purpose is to determine whether proposed flow augmentation and withdrawals for storage will significantly affect those parameters.

3) Are the habitat classifications considered in the study (slackwater, flat, riffle, run, isolated pool, and plunge) adequate and scientifically defensible for the purposes of the study?

   This is not my area of expertise.

4) Is the Stage Change Study sufficient to determine if First Increment Program water activities can be detected (statistically significant beyond the error of the gauging equipment) from base flow conditions?

   Yes. Given that equipment and gauging error is listed as 10% (presumably +5% and -5%0, the Study determined that flow changes such as those on page 24, going from 5,040 cfs to 3,290 cfs, are expected to be much greater than 5% (the direction is known), and so will be detectable as different from base flow conditions.

5) If "yes" to Question #4 above, is the Stage Change Study sufficient to detect if First Increment Program water activities have an impact (statistically significant beyond the error of the gauging equipment) on stage, velocity, temperature, turbidity, substrate, or channel morphology?

   No. Determination of differences in water quality parameters using Analysis of Variance is flawed because the serial correlation in the data was not accounted for. The current analysis is not sufficient to determine whether there are significant impacts for these parameters.

6) Are the findings of the stage change study and the conclusions reached in the report supported by the data and analysis?

   The Study's conclusions in regards to flow are supported by the data and analysis. The conclusions in regards to water quality parameters are not. The conclusions in regards to effects on habitat are beyond my area of expertise, but appear to be the most thoroughly supported portion due to the modeling work.

B. Specific Comments, by page

One fundamental problem with the Study is that many analyses were based on two apparently unpublished reports by the USFWS (2002 a and b). Results hinge so much on these draft reports that some statement from the Service should be included that verifies that the analyses, spreadsheets, etc. in these reports are valid, and that they received peer review and were considered accurate, even though the reports were never published. Or if this is not the case, a statement to the effect that the analyses were never peer reviewed or verified. Citations in this Study to those two reports usually do not discuss the methods that produced the conclusions, or speadsheets, or whatever product is

being cited. The citations imply that what was reported is accepted as truth. What were the quality of these methods? Are there any plans for reviewing, verifying and publishing these 10-year old reports?

Page 3. An example of the dependence on these two reports is the method used for extrapolation from one gage to another using regression. This procedure has for years been known to dampen variability in flows, as regression predicts mean values. So the predicted daily flows for 30 years at the Loup River at Columbus (1978-2008) relied upon in this report will not be as variable, high or low, as would have been the actual record if it had been measured. Other methods for extrapolation (one is often called MOVE or LOC) are preferred when the probability of hitting a high or low flow is at issue, which it is here. These probabilities of high and low events will be underestimated, as regression by design predicts values towards the center. Given that the referenced report was never taken beyond draft, methods in that report including this one may be less than 'industry standard'.

Page 4. Please make the method for estimating missing evaporation data more clear. Were simply long-term monthly averages used? That is what is implied in the text. Or were monthly temperatures for the period to be estimated incorporated as well, so an unusually hot June for example had higher evaporation than the long-term average for June?

Page 4. Isn't the statement that "the effect of flow changes in the central Platte River for the magnitude currently envisioned under the Platte River Program are not likely to be detectable at Louisville, Nebraska" (USFWS, 2002b)" one of the questions that this Study is to answer? Why then cite the answer, from a draft report at that, here, with implied great authority? No background or insight into the method the USFWS used to make this conclusion is presented here. I'd suggest you delete this statement until later after you have presented your analysis of this question. From my reading of the analysis, the Study finds that the flow changes will certainly be detectable at Louisville, decreasing "…the flow at Louisville from 5,040 cfs to 3,290 cfs" (from page 24). So if not deleting the statement, make sure it is clear that this report finds a different result.

page 5. Data are not "illustrated" in a table such as Table 5. They are "listed". If they should be illustrated, draw a figure. Tables don't illustrate anything.

Page 5. What is the objective of determining whether "water quality data can differentiate between flow conditions"? This implies that the flow data cannot differentiate, and that water quality might be needed to do this. Or do you mean "water quality is different at different flow conditions"? The latter is focused on water quality, rather than on using it to say something about flow. Clarify the objective for why this analysis is being undertaken.

Page 5. Your title "Accuracy Assessment of USGS Stream Gage Measurements" is misleading. You aren't doing an assessment of the accuracy of their methods. No data were collected to do so. You are just using their own accuracy assessment to compute the magnitude of 10 percent of observed flows. You should rename this section. Then you compute tables of differences in uncertainty estimates (Tables 4 and 6) without stating what these are good for, or how they came about. Was the method used in the USFWS report different from yours, and therefore the differences? If so, what were the two methods and why do you think they differ? Or are these the same methods just applied to different time intervals, and no change in the physical system has occurred? If this is true, then discuss how this helps you and how the difference in flows between 1975-1994 and 1995-2008 produce the observed differences listed in Tables 4 and 6

Page 8. I have no idea what "Program staff also provided some preliminary information evaluating the pulse flow event to the Grand Island gage" means. Please reword or delete if not important.

Page 9. So your conclusions here are that a release of 13K AF upstream is not really discernable by the time it travels downstream to Louisville. What are the implications of this for your later findings, given that the later findings seem to disagree with this?

Modeling section. You found that you have well-calibrated models, and that the Platte acts like most other rivers in scouring the bed during high flows, increasing channel depth. You have a handle on the types of bedforms and bars likely present at differing flow regimes. This was translated into models of the amount of habitat available for different flow regimes. You evaluate uncertainty in habitat computations based on differences between measured and modeled flows. However this underestimates the true error, as errors for calibration data are always smaller than verification data not used to calibrate the model. A verification step of some sort, possibly a cross-validation procedure, should be used to quantify uncertainties instead. Yours are very likely too small.

Page 21. These daily values are not independent. Analysis of variance (as well as other standard statistical tests) assume independence of observations, that there is no sequential correlation. There certainly is for day to day measures of temperature and water depth, and probably for the other parameters as well. The result is that sample sizes are incorrect, that 46 observations for September 2008 for example may have the equivalent information of 20 independent observations. Therefore the test should be run using n=20 rather than 46, and the differences between months may with reduced sample sizes actually not be significant. Because this was not considered, these tests do not prove that differences actually have occurred between months. The tests should be run by correcting for serial correlation, which can be done with more complex software,

or by more simply computing the 'effective sample size' that is a function of the magnitude of correlation between observations in the time series.

page 21. Serial correlation similarly invalidates standard power calculations. No detail on how power was calculated is given here. Standard ANOVA power calculations assume both independence and a normal distribution, and turbidity and depth data are probably not normally distributed (the others may be based on working with similar data). Much more detail should be given here on the procedure of the power calculations.

Page 22. Even more importantly, the questions that the power analysis and ANOVA are addressing should be explicitly stated. What is the value in these analyses? State why you are performing them.

Page 22. Figures 42 and 43 are stated as being composed of only the May 2009 data. Yet on page 23 they are used to compare to conditions at other additional times. This isn't valid, certainly for temperature. In addition, the data should be tagged and color coded by rising and falling stages of the hydrograph. Part of the large variation for similar discharges is due to differences between water quality when the storm is rising versus falling. Turbidity can certainly be expected to be very different for the same discharge depending on which limb of the hydrograph it occurs on.

Page 23. The meaning of the statement " the magnitude of the change in discharge is subject to the same uncertainty as the overall flow" is unclear. Be more specific or delete this.

Page 23. The statement " the increase in discharge does not move the conductivity, turbidity, temperature, or dissolved oxygen outside the typical range preferred by pallid sturgeon (Figures 42 and 43)" is too broad and sweeping of a statement considering that the figures are based on data only from one month, and you've already stated that based on an ANOVA the levels of these parameters differ between months. Graphs of the relationship between these parameters and discharge should be based on data from all four months of interest where diversions are expected (note that May is not one of those months and so is incorrectly used for the data in these graphs), while considering variation due to rising vs falling hydrograph and to temperature effects. In short, you cannot use the current graphs to make the conclusion you are heading toward.

page 24, a typo? The Run classification would be reduced from 45% to 34%, a decrease of 1%??? Plus, you report different values in Appx G. Please clarify.

C. Rating

Please score each aspect of this manuscript using the following rating system: 1=excellent, 2=very good, 3=good, 4=fair, 5=poor.

	Rating	
Scientific soundness	___3____	sections vary
Degree to which conclusions are supported by the data	___4___	
Organization and clarity	___4___	
Cohesiveness of conclusions	___4___	
Conciseness	___2___	
Importance to objectives of the Program	___3___	

RECOMMENDATION	Check One
Accept	_____
Accept after revision	___X____
Unacceptable	_____

Reviewer #4

Dr. Larry J. Weber

Expertise: River Hydraulics and Mechanics, River Restoration, and Computational Modeling

Larry J. Weber

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September 16, 2011

Eliza Hines
Senior Scientist, Integrated Water Resources
ATKINS
701 San Marco Blvd Suite #1201
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Contract: Platte River Stage Change Peer Review

Dear Ms. Hines,

I have completed my peer review of the Platte River Stage Change study as defined in the scope of work document transmitted to me 16 August 2011. In particular, I have reviewed all of the documents provided including the original project RFP, the Protocol Development Report, the Final Implementation Report, and all appendices and associated documents. My review report includes answers to the *Peer Review Questions* and responses to the *Guidelines for Peer Reviewers*. Although my comments will include all technical aspects of the report, my primary expertise in the context of this work relate to hydraulic modeling and river hydrodynamics.

Peer Review Questions

1) Does the Stage Change Study adequately address the **overall objective** of the RFP, which is "...to develop information needed to evaluate the effects of Program water management activities, including new activities covered by state or federal depletion plans, on water stage and how those stage changes affect physical parameters in the reach of the lower Platte River from the Elkhorn River confluence to the Missouri River confluence?"

   The report does adequately address the overall objective as stated. The report is logically organized and compete, however, it would be helpful to include a background section early in the report that describes the type of flow conditions being considered to place the study in context.

2) Are the physical parameters and measured data considered in the study (flow quantity, depth, velocity, temperature, turbidity, sediment, and sandbars and bedforms at selected sites throughout the study reach) adequate and scientifically defensible for the purposes of the study?

   Yes, the physical parameters are adequate and scientifically defensible. Clearly, the need for improved scientific understanding of selection and utilization of specific, local flow conditions (both hydrodynamics and water quality) and habitat-scale flow patterns that

pallid sturgeon prefer is still needed, but outside of the scope of this project. The report does a very good job of describing available data and current understanding and utilizing this information to reach the conclusions.

3) Are the habitat classifications considered in the study (slackwater, flat, riffle, run, isolated pool, and plunge) adequate and scientifically defensible for the purposes of the study?

   Yes, the habitat classifications are adequate and scientifically defensible. In addition, to the uncertainty analysis and quantification of habitat areas by type, it would be helpful to include a broader discussion about the space-time utilization of individuals that may be residing or moving through the area. For instance, "what is known about adjacencies or distributions of habitat types", this may be important for habitat utilization and may be impacted by stage change. From the information it did not appear that distribution or adjacency would change, but would be good to include this in the discussion.

4) Is the Stage Change Study sufficient to determine if First Increment Program water activities can be detected (statistically significant beyond the error of the gauging equipment) from base flow conditions?

   Yes, the report clearly addresses the detectability of the stage change from Program Water activities. It would be helpful, within the discussion section to refer to the stage discharge curves for the reach.

5) If "yes" to Question #4 above, is the Stage Change Study sufficient to detect if First Increment Program water activities have an impact (statistically significant beyond the error of the gauging equipment) on stage, velocity, temperature, turbidity, substrate, or channel morphology?

   Yes, the report addresses the impact of the stage change on the river parameters listed. It would be helpful to list other parameters that may be important, such as flow shear lines, and eddy structures, however, less is know about these features than the parameters given. With that said, some acknowledgement that the parameters considered may not be the only flow features that determine habitat function and utilization would be useful. The second to last paragraph of the report provides some comments towards this, but could be expanded.

6) Are the findings of the stage change study and the conclusions reached in the report supported by the data and analysis?

   Yes, the findings of the study and conclusions reached are supported by data and sound engineering and scientific analysis. It would be beneficial to include an executive summary of the report and a clear conclusions / summary section in the report

General Comments

1) Scientific Soundness – The methods and approaches were based on sound engineering and science. Unfortunately, although there is literature and past studies that describe

general habitat preferences and utilization, there is little available information from a first-principles understanding of specific habitat needs for the species of interest. This short-coming is, however, common in most aquatic restoration and management programs. The project report uses sound, available engineering and science to address this inherent uncertainty in its habitat evaluation. Although further studies and fundamental research could improve this understanding, it is clearly outside of the scope of this project.

2) Organization and Clarity – The report logically presents the engineering analysis of the hydrologic conditions of the study reach; data collection programs; hydraulic model construction, calibration and utilization; geomorphic assumptions and analysis, flow habitat assumptions and habitat discrimination technique; and conclusions. Uncertainties of methods, models and approaches are adequately described throughout the report.

3) Conciseness – The report is well written and presents an appropriate amount (both depth and breadth) of information. The report also, includes relevant information in the appendices and adequately sites previous and related published work.

4) Degree to which the conclusions are supported by the data – The report provides a logical progression from hydrologic conditions of the study reach through final conclusions, including the uncertainty of information utilized in the decision process.

5) Cohesiveness of conclusions – The formulation of the conclusions is based on sound engineering and science. The conclusions/summary statements should have been explicitly organized in a closing, *Conclusion* or *Summary* section in the report rather than simply woven into the *Discussion* section.

Specific Comments

1) In the discussion of minimum and maximum flow selection, a flow recurrence / exceedance plot would be helpful to place the selected flows in context, rather than referring to figure 2. Also the period of record should be stated for this analysis in the Study Flows section.
2) x-axis of figure 2 should use the first day of the month for each major grid line and label
3) A better location map would be helpful to locate the study reach within the state and along the Platte River Stream network.
4) It would be helpful to explicitly state that the 2D SRH model is a fixed bed model and this geometry is used throughout for all simulations. How this impacts the local flow conditions for higher flows should be addressed.
5) Figures 24, 25 and 26 are useful data plots, however, it would be helpful to see the distribution of the difference between model and field data on a spatial image of the study area. This would be helpful to understand the performance of the model, but likely does not negatively impact the use of the model results.
6) Page 24, first paragraph after table 13. ….45% ($\pm$8%) of the habitat area to approximately 34% ($\pm$8%) of the habitat area, a decrease of 1%. The "1%" should be "11%".

7) Discussion section. In addition to the text description, it would be helpful to tabulate the changes to habitat classification in the discussion section. This to compare across conditions of interest, and to show the impact of the management actions.

Rating (1=excellent, 5=poor)

Scientific soundness	2
Degree to which conclusions are supported by the data	2
Organization and clarity	1
Cohesiveness of conclusions	2
Conciseness	1

Comment: Overall this is a very good study report, providing insight and comprehensive summarization of multiple data sets. My decision not to use ratings of '1' is primarily a result of the inability to basic first-principles understanding and analysis, which is currently unavailable for this complex project. I have no hesitation in recommending acceptance of the report

Recommendation

Based on my review of the materials provided, it is my recommendation to accept the Final Protocol Implementation Report and its conclusions.

Please do not hesitate to contact me if I can be of any further assistance.

Sincerely,

Larry J. Weber

Reviewer #5

Dr. Lee Wilson

Expertise: Hydrology, Environmental Impact Assessment, Geomorphology

In accordance with my contract, I have conducted a peer review of the Lower Platte River Stage Change Study. The review is organized according to my understanding of the peer review guidelines, as follows.

1. General comments.
2. Specific comments.
3. Response to questions.
4. Ratings.

I will be on travel until mid-October, after which I will be available to answer any questions on this submittal.

I appreciate being selected to be part of the peer review team, and in that way to contribute to the Platte River Recovery Implementation Program.

1. General comments

I consider the core elements of the study to be technically sound and useful. With some exceptions noted below, the work satisfied the scientific and technical scrutiny that was within my expertise to apply, and within the peer review budget to investigate. The study report appears to satisfy the objectives of the RFP.

In my experience, a role of peer review is to focus on potential weaknesses or limitations in a study. Thus the critical nature of my comments should not be taken to suggest the study is seriously flawed, but rather as my effort to provide constructive input to future work. In the specific comments, I observe the following aspects of the study that I thought might be in most need of improvement or of further evaluation.

- For purposes of organization and clarity, it would be beneficial to provide an introduction that puts the study in context. See specific comments on p. 1.
- I suggest reconsidering the methodology and results of the loss analysis. See specific comments on p. 2.
- The effects of flow modification by hydropower appear to be potentially profound and need further evaluation. See specific comments on p. 8.
- The apparent rigor of certain of the analyses does not fully capture the uncertainty in the bottom line results. See specific comments on p. 20.

The following are responses to particular considerations posed in the peer review guidelines ("guidelines"), under the heading of general comments.

- *Scientific soundness*. The technical aspects of the document were generally good, with possible exceptions noted under Specific Comments.
- *Organization and clarity*. The Specific Comments (especially regarding Pages 1 and 9) identify ways the organization and clarity of the report could have been improved by providing additional background discussion. That being said, within what was actually presented, the report was well organized and well written.
- *Conciseness*. Good.
- *Degree to which conclusions are supported by the data*. Hard to say without copies of the data sets, spreadsheets, and models.
- *Cohesiveness of conclusions*. Ok within the context of the report. But there is so much unsaid, that a stranger to the process might not be able to properly judge the end results.

2. Specific comments

My specific comments are provided in two parts. First, I respond to considerations set out in the guidelines. Then I go through the document and present comments that are specific to particular pages. For Pages 1, 2, 3, 9, and 20 these include expanded discussions of the bullet points presented in my general comments above.

1. *Presentation: Is a tightly reasoned argument evident throughout? Does the manuscript wander from the central purpose*? The true central purpose is never stated. Within the organization as presented, the report does a good job of walking through the methods, data and results without any wandering.

2. *Methods: Are they appropriate? Current? Described clearly and with sufficient detail so that someone else could repeat the work*? Except for the evaluation of losses, the methods are appropriate and current. The level of detail in methods is good. I don't know enough about the models to know if one could repeat the work, but I suspect it would be necessary to get the actual model I/O files to do so.

3. *Data presentation: When results are stated in the text of the manuscript, can you easily verify them by examining tables and figures? Are any of the results counterintuitive? Are all tables and figures clearly labeled? Well planned? Too complex? Necessary?* Good marks on all of this.

4. *Statistical design and analyses: Are they appropriate and correct? Can the reader readily discern which measurements or observations are independent of which other measurements or observations? Are replicates correctly identified? Are significance statements justified?* A lot of attention is paid to statistical determinations, but there is a fair amount more that could and probably should have been said. See comments on P. 20.

5. Conclusions: Has the author(s) drawn conclusions from insufficient evidence? Are the interpretations of the data logical, reasonable, and based on the application of relevant and generally accepted scientific principles? Has the author(s) overlooked alternative hypotheses? I found the overall results acceptable, since they agreed with what was fairly evident even without the study, that no significant relationships can be quantitatively established.

6. *Errors: Point out any errors in technique, fact, calculation, interpretation, or style*. My review was not in depth, but I found nothing of concern except for the loss analysis (see comments on P. 2).

7. *Citations: Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in the manuscript*? It's a good reference list.

Page 1. In my first paragraph of general comments, I said the study appears to satisfy the objectives of the RFP. I used the word "appears" because neither the RFP nor report does a good job of placing the study objectives into context, i.e. explaining to what ultimate purpose the work was being done. To understand the work, I relied on the Biological Opinion and the limited discussion in the Protocol. I don't fault the authors for this necessarily, as it isn't clear from the RFP that they were tasked to provide context in the report.

Nonetheless, the lack of context made reading and evaluating the report much more difficult than it should have been (at least for me). The standard organization for a scientific paper includes an introduction that presents the background knowledge necessary for the reader to understand the findings of the paper. This is especially important when, as here, there is no executive summary to bring everything together.

In this case the following would have been useful in providing the reader with important background knowledge.

- A brief synopsis of the nexus between stage and sturgeon as it is now understood. Note that the fact that this paper is about pallid sturgeon isn't even mentioned until halfway through the report (p. 14).
- One or more hypotheses about how the Program could impact that nexus (including a "non-detect" hypothesis). This would disclose the current thinking about why the study reach is important to sturgeon, and why we are interested in predicting impacts to depth, velocity, bedforms, topography and the like.
- A clear and succinct statement of the methodological approach to evaluating the hypotheses. This might be a flow chart indicating that first we have to route Program flows to the reach; then model their impact on the parameters of interest; which means very complex hydraulic models and interpretations relating especially to bedforms; and finally translate that to impacts to sturgeon habitats. It may seem obvious, but that doesn't mean the report shouldn't be clear about what is being done.

In between pages 1 and 2. Figure 1 would benefit from an inset location map.

Page 2. The loss analysis is an update of a FWS study provided in Appendix A. It is difficult to fully evaluate the method without a copy of the spreadsheet. Nonetheless, I was very surprised about the results, and wonder if the Program is approaching this important issue correctly. I did not review Appendix A in sufficient detail to know for sure that my concerns are valid, so please consider this discussion accordingly.

My two primary concerns are as follows.

- Some of the loss rates reported are much higher than I have seen, even in arid western rivers. If it has not been done, I strongly recommend each element of the loss be independently verified. For example, analytical methods using groundwater head data can be used to independently estimate seepage losses.
- It appears that the method calculates Program losses in proportion to flows. An alternative (and in my experience more appropriate) approach is to calculate them on an incremental basis. If the current procedure has not been affirmatively deemed more appropriate than an incremental approach, the incremental method should be

To illustrate my concern, consider the result of the accounting done by the Bureau of Reclamation for the loss of water imported into the Rio Grande Basin (this loss rate is important for quantification of endangered species impacts as well as available water supplies). Based on quantification conducted by the Rio Grande Compact Commission, a loss rate has been calculated for the reach from Heron Reservoir (near the Colorado border on a tributary of the Rio Chama) to Albuquerque (a distance roughly comparable to Grand Island-Louisville). The loss rate applies to the flow added to natural flow by imported water. There are elements of the rate calculation that are not entirely apples-apples to that made for the Lower Platte, but these would have a modest effect at most. The Rio Grande loss rate is 2%. Given this result, it is difficult for me to understand loss rats as high as 90% in eastern Nebraska.

The subject of losses above Grand Island is not considered, but it would be of interest to know the Louisville flow as compared to an upstream reservoir release

The following comment is not related to the above, but to the reference to selection of "appropriate" flows on page 2. Appropriate how? With no discussion of matters such as sturgeon habitat, the reader cannot know. It is also confusing to indicate that a flow of 39,000 cfs is of "primary interest", without explaining why it was then appropriate to use 8,000 cfs as the high end of flows selected.

Page 3. I did not understand how the study made use of two different periods of record for extended analysis.

Page 4. The new spreadsheet analysis probably should be provided in an Appendix.

Page 5. The power analysis probably should be provided in an Appendix.

Page 6. The focus on gage uncertainty may cause readers to overlook the uncertainty in the USFWS spreadsheet which estimates impacts of Program flows.

Page 7. In addition to the plots in Figures 3, 4 and 4a, it would be interesting to see the data plotted as flow duration curves.

Page 8. This page presents Figure 5 and makes note of the "obvious" intraday flow variation. The discussion focuses on how to smooth that out so the pulse can be translated from Grand Island to Louisville, which is certainly appropriate. However there is no discussion whatsoever about the fact that the hydropower effect causes a 1 foot diurnal change in stage, which is far greater than the transformed impact of the pulse.

The implied premise of the study is that stage impacts habitat, through effects on velocity, depth and bedforms. If so, how is it that the effects of such a large and rapid stage change are not considered at all? Had the study found that Program releases did impact habit in the study reach, that conclusion would have been called into question because the interday flow variation was not considered and could be such that it swamped out any Program impact.

Page 9. Another aspect of context that wasn't effectively presented was the cause-effect relationship being studied. The stated objective puts "stage" as the focal point, whereas after reading the report, I perceive the operational objective was to evaluate the impact of flow (cfs) as it directly impacts water depth and velocity, and the consequent effects on sediment, bedforms and habitat. Stage as such seemed not to be that much of a consideration, or a particularly good surrogate, especially in terms of assessing velocity and its consequences. The lack of hypotheses was surprising given the nature of the Adaptive Management Plan.

Page 10. Given that stage is the focus of the study, are two water surface data points sufficient for the cross-sections?

Page 11. It would be useful to have an assessment of the change in roughness with flow, and especially whether it is reasonable to interpolate values.

Page 12. I did not follow the explanation of the very low n values for the 2D model.

Page 15. The entire bedform discussion would benefit from illustrations.

Page 19. I found Figure 36 hard to interpret.

Page 20. The use of a Monte Carlo analysis to assess uncertainty gives an impression of statistical rigor to the results. Certain other aspects of the work give a similar impression. However if one starts at the very beginning of the work, i.e. an increment of flow at Grand Island (with unstated uncertainty), and carries it through to the end, many other issues become apparent – the loss estimates, hydrograph translation, error bars on model inputs (median grain size is a good example), and more. This cascade of uncertainties would have undermined the results had a positive relationship been found. As the bottom line of the report did not assert any relationships had been statistically demonstrated, these issues are perhaps not critical. Still, I would have liked to see (in the discussion section) a recap of all the assumptions, limitations and uncertainties in the work.

Page 22. Of interest given prior discussion, the models are (correctly) said to evaluate depth and velocity, not "stage change". One question not posed previously: why is the release being evaluated so small?

Page 25. Perhaps emphasize that lack of statistical significance does not equal lack of effect. In fact, qualitatively one can say that a release probably does have at least marginal benefit (this is a bit more affirmative than "no additional stress").

3. Response to questions

1) *Does the Stage Change Study adequately address the overall objective of the RFP, which is "…to develop information needed to evaluate the effects of Program water management activities, including new activities covered by state or federal depletion plans, on water stage and how those stage changes affect physical parameters in the reach of the lower Platte River from the Elkhorn River confluence to the Missouri River confluence?"* Yes, subject to comments above.

2) *Are the physical parameters and measured data considered in the study (flow quantity, depth, velocity, temperature, turbidity, sediment, and sandbars and bedforms at selected sites throughout the study reach) adequate and scientifically defensible for the purposes of the study? Yes, to the extent that they can actually be meaningfully evaluated by the methods used.*

3) *Are the habitat classifications considered in the study (slackwater, flat, riffle, run, isolated pool, and plunge) adequate and scientifically defensible for the purposes of the study? This is a good example of a subject that can't be evaluated if one considers the report in isolation, because habitats get minimal attention in this report.*

4) *Is the Stage Change Study sufficient to determine if First Increment Program water activities can be detected (statistically significant beyond the error of the gauging equipment) from base flow conditions?* Yes and No. Yes the study answered the question; no, program activities (as to flow)

cannot be detected. Effects of other activities (sediment mobilization for example) were not assessed.

5) *If "yes" to Question #4 above, is the Stage Change Study sufficient to detect if First Increment Program water activities have an impact (statistically significant beyond the error of the gauging equipment) on stage, velocity, temperature, turbidity, substrate, or channel morphology*? No.

6) *Are the findings of the stage change study and the conclusions reached in the report supported by the data and analysis*? Yes, especially given the conclusion is "did not find".

4. Rating

RATING:
Please score each aspect of this manuscript using the following rating system: 1=excellent, 2=very good, 3=good, 4=fair, 5=poor.

Scientific soundness: 4
Degree to which conclusions are supported by the data: 5
Organization and clarity: 4
Cohesiveness of conclusions: 4
Conciseness : 5
Importance to objectives of the Program: 3

PLATTE RIVER RECOVERY IMPLEMENTATION PROGRAM

EXHIBIT C

EDO/CONTRACTOR RESPONSES TO PEER REVIEW COMMENTS

Lower Platte River Stage Change Peer Review
Comment-Response Table

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
1	Gaeuman	Fluvial Geomorphology	SOW Question 1		Yes.	Comment noted.
2	Gaeuman	Fluvial Geomorphology	SOW Question 2		Yes. However, bedforms played a very minor role in this study. It's not clear how they were incorporated into the quantification of sturgeon habitat availability.	Comment noted.
3	Gaeuman	Fluvial Geomorphology	SOW Question 3		Yes, but I do not claim to be an expert in that subject.	Comment noted.
4	Gaeuman	Fluvial Geomorphology	SOW Question 4		No. A better evaluation of gaging errors is needed, as described in my comments above. I would also suggest that the idea of detectability be better defined. It seems that for a small water augmentation to be detected, one would have to know what the discharge would have been without the augmentation. How would the work? And what is the time scale over which the detection should occur? Detecting a small change on a particular day is a different matter than detecting a sustained small change over a month or a year.	If the Program elects to issue a final revised report, will add clarifying text similar to that in USFWS 2002 when discuss Table 5. Something like: "In other words, a change in daily flow of at least several hundred cfs would be needed under median flow conditions in any month for the Program-related change to be detectable (i.e., exceed gage uncertainty inherent in flow measurement). Program-related flow changes would need to be greater than about 450 cfs under median flow conditions from Jul through Sep to be detectable. Program-relted flow changes would need to be greater than about 150 cfs under low flow conditions (i.e., 90% exceedance) from Jul through Sep to be detectable. Based on an approximate travel time of 4 days from Grand Island to Louisville, Program-related flow changes will be assessed on an average daily flow basis. This will also average out the diurnal fluctuations at the Louisville Gage associated with releases from the Columbus Powerhouse, and facilitate isolation of effects of Program-related flows."
5	Gaeuman	Fluvial Geomorphology	SOW Question 5		N/A	Comment noted.
6	Gaeuman	Fluvial Geomorphology	SOW Question 6		yes	Comment noted.
7	Gaeuman	Fluvial Geomorphology	General		The scope of this study outlined in the RFP targets two related, but distinct, objectives: determining what measurable effect, if any, Program water delivered at upstream locations will have on discharge in the Platte River downstream from its confluence with the Elkhorn River, and quantifying how changes in discharge might translate to changes in hydraulic parameters and physical habitat characteristics in that stream segment.	Comment noted.
8	Gaeuman	Fluvial Geomorphology	General		The authors of the study approach these two objectives quite differently. With respect to how discharge affects habitat, the authors present an analysis based on numerical modeling of flow under existing geomorphic conditions. Although this modeling analysis neglects the potential for future flows to modify the current stream configuration and produce longer-term changes in habitat availability, it does address the question posed in the RFP. The question, the approach used to address it, and therefore the review of the analysis, is straight-forward. My review of that portion of the report is presented first.	Comment noted.
9	Gaeuman	Fluvial Geomorphology	General		For the question regarding the effect upstream Program water on downstream discharge, however, the authors opted to rely heavily on some earlier Fish and Wildlife Service analyses, which were incorporated in the report as Appendix A and Appendix B. In doing so, they implicitly endorse those reports and accept some level of responsibility for any problems with the methods and explanations presented in them. I found those reports quite difficult to interpret, so I'll save my comments on that portion of the Stage Change Study for last.	Comment noted.
10	Gaeuman	Fluvial Geomorphology	General		I note here that I have not attempted to systematically copy edit this report because, according to the title, this is a Final version. I take that to mean that typographic errors, unclear statements, and so on will not be corrected as might happen if this were a Draft version. Instead, my comments focus on the broader-scale "Specific Questions" identified in Review scope of Work and the "Specific Comments," "Rating," and "Recommendation" identified in the PRRIP Peer Review Guidelines. The questions from the Scope of Work and the Peer Review Guidelines are addressed explicitly following my free-form comments on the Hydraulics and Geomorphology section and the Hydrology section.	Comment noted.
11	Gaeuman	Fluvial Geomorphology	Hydraulics and Geomorphology	General	The approaches used to address the question posed in the RFP are appropriate. The general approach of modeling hydraulic parameters and using model output to classify habitat types is good. It could perhaps be improved by incorporating bedform types into the classification system, in addition to depth and velocity. Bedforms can have a large effect on flow velocities and turbulent structures near the bed, and so are likely very important components of physical habitat. The section on describing and predicting bedforms is good, but it's not clear whether or how that information was used to inform the final conclusions of the study.	Comment noted.
12	Gaeuman	Fluvial Geomorphology	General		The contractor appears to have an adequate understanding of the modeling tasks to produce credible results. However, the modeling analysis seems to include some mistakes and misinterpretations that might have the potential to affect the Study's conclusions and recommendations. Two problems with the model itself are worth highlighting: the 2d model domain lacks lead in and lead out sections and is generally too short (see comment 19), and the quantity of topographic data appears to be very small compared to the resolution of the model mesh (see comment 20). Both of these issues substantially degrade the accuracy of the model and the confidence that can be placed in its output. Two additional issues regarding the interpretation of the model results are worth mentioning: The sensitivity analysis regarding how model errors affect habitat classification may be flawed (see comment 31), and percentages in each habitat type are based on submerged area rather than total area (see comment 38). That said, I doubt that correcting these problems would materially change the Study's conclusions concerning how incremental changes in discharge alter habitat availability.	See responses to Comments 19, 20, 32, and 38.
13	Gaeuman	Fluvial Geomorphology		Page 9	"A hydraulic and geomorphologic analysis…" not sure what part of this is a geomorphologic analysis. It's mostly limited to hydraulic modeling.	The micro-scale bedform analysis portion of this is the geomorphologic portion of the analyses. Habitat classification was based on bedforms such as dunes, ripples, and upper regime bedforms.
14	Gaeuman	Fluvial Geomorphology		Page 9, last paragraph	"…trend over this period." Which period?	The 20 year period from the mid-1970s through 2001 period (i.e., same period as the available cross-section data stated in previous paragraph). Stated in the previous paragraph, and also later in this paragraph.
15	Gaeuman	Fluvial Geomorphology		Page 10, 2nd paragraph	refers to a 10-year model run. What does that mean?	The model run was a steady state run using the 10-year recurrence interval discharge. Sentence will be reworded if the Program elects to issue a revised final report.
16	Gaeuman	Fluvial Geomorphology		Page 10, 3rd paragraph	Not sure what's meant by the different model versions incorporating cross sections from different dates. The preceding sentence is about water surface elevations at the cross sections. Were different cross sections (geometry) used in the two model versions, or just different water surface elevations for validation?	Both. Surveyed geometry and WSE were used in the updated model to make it more applicable for recent topography and at lower flows relevant to flows considered for this study. 2nd sentence of this paragraph explains that surveyed cross sections replaced USACE-OD model sections.
17	Gaeuman	Fluvial Geomorphology	Table 7		Table headings are unclear and awkward. I'm not sure what an average maximum or average minimum is. Are these the extreme instantaneous values for a given day averaged over X number of days? Is "average mean" the average of X number of daily mean values, or the average of something else? The text on page 10 that references Table 7 doesn't help with this.	Editorial comment. Will be edited if Program elects to issue a revised final report.
18	Gaeuman	Fluvial Geomorphology		Page 11	The discussion of the models of different dates is poorly organized and confusing. It would help if the point of all this were explained at the outset. Much later in the text, in the section about bedforms I believe, it becomes apparent that the point is to account for differences in roughness due to differences in bedform regime at different flow levels.	Editorial comment. Will be edited if Program elects to issue a revised final report.

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
19	Gaeuman	Fluvial Geomorphology		Page 12, 4th paragraph	Figures 19-20: The model mesh is 1,700 ft long. From the figures, it's seen that this corresponds to about 1 channel width. This is far too short of a model reach. First, it is a very small sample in term of area from which to generalize about the river segment. But more importantly, every point within the model is a short distance from the model boundaries. It is standard practice to extend the model mesh at least a few channel widths upstream and downstream of the reach of interest. That allows some space and time for any errors or imperfections in the boundary conditions to dissipate.	This is a relatively short reach. However, it has the characteristcs of the remainder of the reach of concern, including the variability seen up and downstream. The issue of "lead-in" and "tail-out" is only valid to the extent that the boundary conditions contain error. In our case, the downstream stage is assumed to be known from the 1D model. The upstream flow alignment and distribution may contain some error; however, considerable effort was made to insure that the flow distribution across the upstream boundary reasonable for all flows, and the boundary was established so that the flow direction was a perpendicular as possible to the boundary. Extending the model up- and downstream would require significantly more topographic data than we were able to collect within the time and budgetary constraints of the project. It is our opinion that any error introduced at the upstream boundary is relatively minor and does not propogate significantly into the remainder of the model domain.
20	Gaeuman	Fluvial Geomorphology		Page 12, last paragraph	refers to "detailed topographic and bathymetric data" used in the model. There is no indication in this report that detailed topographic data was collected. The onlydiscussion along those lines concerns collection of a relatively small number of cross sections. The 2d mesh is said to have a mesh resolution of 10 feet. This density is irrelevant unless the topo data mapped to the mesh is of similar resolution, as might be obtained with an intensive sonar survey using an array of transducers or a multi-beam. There is no indication that this was the case. The value of the fine mesh is, to a large extent, nullified if the topography was interpolated from cross sections.	6,638 topographic points were collected within the 49 acre 2D model domain This equates to one point every ~320 ft^2 or an average spacing of ~18'
21	Gaeuman	Fluvial Geomorphology		Page 12, last paragraph	It's not explained where the n values of 0.023 and 0.027 in the 2d model came from. Were these transferred from the 1d calibration in some way?	They were final calibrated n-values for the 2D model.
22	Gaeuman	Fluvial Geomorphology		Page 13, 4th paragraph	Figures 24-26: It is stated that the match between measured and modeled water surface elevation and water velocities is "good." This seems to be an overstatement. Plus or minus 0.5 ft in elevation does not seem especially good to me, and velocity errors seem to range up to around 50% (Figure 26).	+/-0.5' is pretty good for a river of this size. In fact, there's probably that much local variability in the WSEL when there are bedforms, etc.
23	Gaeuman	Fluvial Geomorphology		Pages 14-15	Nice overview on bedforms.	Comment noted.
24	Gaeuman	Fluvial Geomorphology		Page 16, 2nd paragraph	S'* is introduced, but not defined until it come up again on page 17. Same for SG in the equation given for d*.	If Program elects to issue a revised final report, the following clarification will be added. Definitions for S'* and SG under Eqn (2) on p. 17 will be added in 2nd paragraph on p. 16.
25	Gaeuman	Fluvial Geomorphology		Page 16, last paragraph	I think this should be the relation between the average shear stresses (as indicated in equation 1), rather than velocity.	Comment is correct. If Program elects to issue a revised final report, the sentence will be modified to indicate the relation is between shear stress (not velocity).
26	Gaeuman	Fluvial Geomorphology		Page 17	Some of the notation seems odd. ' is used in the definition of S'*, but is not defined (equation 1 introduces '0 and , but not '). Should it be just ? The shields parameter is denoted F* -- why not use * or like most everyone else? (SG-1) is often denoted by R, and SG itself is usually /s. I've usually seen transport stage denoted with T rather than S.	Notation in this report was essentially the same that Bennett (1995) used. Two differences should be noted: (1) SG was used for specific gravity rather than s, and (2) D50 was used rather than d50 for consistency with other related Program documents. Bennett used the same notation for the equation in the 3rd line after Eqtn (2), but it should clearly be tau0' in the context used here. If Program elects to issue a revised final report, tau0' will replace tau'.
27	Gaeuman	Fluvial Geomorphology		Page 17, 4th paragraph	the VBA script is said to solve for the "necessary values…" It's difficult to be sure what is being done here. I infer that is specified on the basis of model output, and equation 1 is solved for '0, but that's not clear from the text.	Some detail left out in the interest of readability, and will not be added to the report. However, the following clarification is provided here as a response. Total shear stress is based on 2D model predicted values. The VB program was then use to iteratively solve Equations (1) and (2) with an assumed starting value for shear stress due to grain resistance (T'o). First Equation 1 is solved for beform height using the assumed starting value for shear stress due to grain resistance. That calculated bedform height value is then used in Equation (2) to solve for sediment transport strength and subsequently shear stress due to grain resistance. This new value for shear stress due to grain resistance then replaces the originally assumed value in Equation (1), and Equations (1) and (2) are iteratively solved in this process until shear stress due to grain resistance used in Equation (1) matches the calculated shear stress due to grain resistance from Equation (2).
28	Gaeuman	Fluvial Geomorphology		Page 17, last paragraph	Discussion switches abruptly from bedform types to how much of the site is subaerially exposed. What's the connection?	Subaerially exposed (i.e., dry) areas are important because they are definitively not habitat for pallid sturgeon regarless of bedform type, and this is the difference between Figures 32 and 33. This discussion continues into 1st paragraph on p. 18 discussing the mix of bedforms for the remainder of the domain that is submerged.
29	Gaeuman	Fluvial Geomorphology		Pages 18-19	habitat evaluation: This seems like a good approach. Why are there no pools in this classification? Are especially deep scours and holes not relevant for sturgeon, or perhaps these environments are not present in the Platte?	Pools were included in the classification scheme (see Table 10). However, pools were not observed in the field survey, so are considered to be mostly absent from this section of the Platte. A very small area of isolated pools was however predicted in the final habitat classfication shown in Fig 37.
30	Gaeuman	Fluvial Geomorphology		Page 20	top: re-states that the model is well calibrated. See comment 22.	See response to Comment 22
31	Gaeuman	Fluvial Geomorphology		Page 20	numbered item 1: velocity units are given as ft.	Editorial comment. Will be edited if Program elects to issue a revised final report.
32	Gaeuman	Fluvial Geomorphology		Page 20	numbered item 2: Was the simulated error applied to each node independently? Or to put it another way, would adjacent nodes be assigned uncorrelated errors? That would clearly be incorrect – for example, if a given node had a large positive error in depth, all nearby nodes (and maybe every node in the model) would probably also have positive errors. Assigning each node an error that is independent of all the other errors would cause the random errors to cancel, and probably result in very little net change in the proportion of particular habitat types.	They were assigned independently. The criticism isn't necessarily valid. Acknowledging Comment 22 above, the model is well calibrated, so the error in the actual WSE should be relatively small. For purposes of this sensitivity analysis, we considered the calibrated model to be the baseline and evaluated the potential effect of uncertainty associated with local variability in topography and hydraulic conditions. The uncertainty in depths and velocities in this context stems primarily from variability in the local bed topography, and these would mostly be caused by micro-scale bedforms. As a result, the errors would not be correlated among nearby nodes in the model.
33	Gaeuman	Fluvial Geomorphology		Page 21	The text says that Table 11 shows variation among transects and among sample episodes, but it doesn't show that. Is a "sample episode" a day?	Table 11 shows variability among episodes, which are the 3 different dates in the table. Variation amoung transects is shown in Figs 39-41.

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
34	Gaeuman	Fluvial Geomorphology	Table 12	Page 22	The table suggests that conductivity and turbidity behave in the same way with respect to different “phases” (what’s the independent variable here, discharge maybe?). Meanwhile, Figure 42 shows that they behave in opposite ways. What point is being made with these statistics anyway?	Independent variable is the date, which essentially makes discharge the independent variable. Table 11 addresses whether WQ data are statistically different between the sampling events, or "Phases". It does not address direct or indirect relationships with discharge. Fig 42 addresses direct/indirect relationships with discharge. The point of Table 11 is that WQ for high flow event (July 2008), mid level flow (May 2009), and low flow (Sep 2008) are significantly different (i.e., are parameters influenced by flow). The point of Fig 42 is how WQ parameters change with flow (i.e., direct or indirect relationship).
35	Gaeuman	Fluvial Geomorphology		Page 22, 3rd paragraph	What is meant by “bottom velocity?” This must refer to some height above the bed.	Yes, peer-reviewed pallid sturgeon literature refers to bottom velocities as the velocity at a height of 0.5 m above the channel bottom. This is a relevant depth for pallid sturgeon spawning.
36	Gaeuman	Fluvial Geomorphology		Page 22, 3rd paragraph	The explanation for why run and plunge habitat is considered most suitable is not very convincing. Where are the sturgeon actually found? Do the cited publications refer to run and plunge habitats?	Discussion of habitat in report and development of habitat "categories" is based on peer-reviewed pallid sturgeon literature and best available information on pallid habitat use and occurrence.
37	Gaeuman	Fluvial Geomorphology		Page 23, 1st paragraph	The gaging error magnitudes defined in the hydrology sections are applied here. I suspect that the interpretation of gage errors may have a problem – see comment 46.	See response to Comments 46 and 4.
38	Gaeuman	Fluvial Geomorphology		Page 23-24	The actual changes in the availability of various habitat types may change more with discharge than is indicated. It appears that the percentages given for habitat types are the percents of the total submerged area. It would be more meaningful to report this in terms of actual area or as a percentage of the model domain area because the extent of the submerged area changes with discharge.	Percent changes are presented relative to submerged area as commenter suggests. This is more meaningful than % of total area, because dry areas are definitively not considered pallid sturgeon habitat.
39	Gaeuman	Fluvial Geomorphology	Hydraulics and Geomorphology	General	The hydrology studies presented in the two USFWS reports and incorporated into the Stage Change Study leave much to be desired in terms of both technical credibility and the clarity of the presentation. Some of the problems with the original reports are noted in the specific comments below. The authors of the Stage Change Study apparently reproduced the analyses described in the USFWS reports. That would require sorting out the details regarding what those analyses involved. Having done that, I would expect the authors of the Stage Change Study to provide a better description of what they did than simply referencing and copying text from the Appendices.	Beyond the scope of the study.
40	Gaeuman	Fluvial Geomorphology	Hydraulics and Geomorphology	General	The flow losses due to evaporation, transpiration, and seepage estimated in these reports are, in my opinion, unreliable. The reported total loss figures become more credible if they are considered to be generic losses, not attributable to any particular sink. Nonetheless, I agree with general conclusion that small discharge augmentations upstream of Grand Island of the magnitude discussed will not be very noticeable at Louisville. This is not so much related to gaging uncertainty (which I think is overestimated in the reports), but is instead due to the fact that the augmentation volumes discussed are small compared to everything else that is going on. Changes in flow on the order of 100 cfs would be difficult to distinguish even if the gages were perfectly accurate, because the changes can be swamped by much larger flow fluctuations caused by a variety of other factors.	Beyond the scope of the study.
41	Gaeuman	Fluvial Geomorphology		Page 2, end of 2nd paragraph	States that the selected flows are considered appropriate for modeling, but doesn’t explain why. Does anything about pallid sturgeon habitat enter into this determination?	The range of flows considered in the hydraulic analysis (3,700 cfs to 40,000 cfs) covers the range of the median historic flows shown in the Louisville hydrograph (Figure 2). Additionally, the median discharge from April to June (months during pallid sturgeon migration and spawning) is approximately 7,000 cfs (as described in the Interpretation and Analysis section), which is well within the range modeled for this analysis.
42	Gaeuman	Fluvial Geomorphology	Table 2 and associated text		Meaning of the headings indicating time periods are unclear. These look like periods of record for the gages, but are not. Time periods listed for the Loup near Columbus include times when there are no gage records. It takes careful picking through the text to figure out how to interpret these dates. I’m unsure of what is meant by “period of analysis.” This could refer to the period from which flow records were drawn to quantify the hydrologic characteristics of the gage site, which could then be extrapolated to other years, or it could mean that consideration of the gage site was entirely confined to that time period.	As indicated in the title of Table 2, the locations and periods of record are pertaining to the Historic Loss Analysis completed for this study. If a revised final report is issued, the text immediately preceding Table 2 will be clarified to indicate that the data in the table pertains to the historic loss analysis.
43	Gaeuman	Fluvial Geomorphology		Page 3, 3rd paragraph	This paragraph is very hard to follow. It does not clearly identify what is being estimated – language like “the USFWS analysis” and “these flows” do not identify the gages and dates for which flows were being reconstructed.	Editorial comment. Will be edited if Program elects to issue a revised final report.
44	Gaeuman	Fluvial Geomorphology		Page 3, last paragraph	A new gage can apparently supply better information about powerhouse return flows, but was not used. This information could have at least been used to check on the accuracy of the method in the USFWS analysis.	Beyond the scope of the study.
45	Gaeuman	Fluvial Geomorphology		Pages 4-5	The Study basically just sends the reader to Appendices A and B. There appears to have been little or no critical review of the USFWS reports by the Study authors.	Beyond the scope of the study.
46	Gaeuman	Fluvial Geomorphology		Page 5, last paragraph	This interpretation of gage accuracy seems overly simplistic. It is stated that the USGS considers 95% of the gage readings to be within 10% of the actual discharge. This report follows the USFWS reports in translating that into error bounds of plus or minus 10%. Assuming the errors are independent random variables, the actual error bound should be related to the number of samples used to generate an estimate. For example, the USGS error estimate could be interpreted as suggesting that the individual errors have a standard deviation of around 5% (because close to 95% of a normally-distributed population is within 2 standard deviations of the mean). Whether the standard deviation is 5% or something else, the standard error of the estimate is equal to the standard deviation divided by the square root of the sample size. If the estimate is monthly mean flow, the sample size is about 30. These numbers suggest that the error bound for the monthly mean might be around 2% at the 95% confidence level. I am not a statistician, and the details of this example may not be exactly correct. For example, the errors on sequential days are probably correlated to some degree. The point is simply that the 10% error bounds assumed in the reports need to be re-examined.	Beyond the scope of the study.
47	Gaeuman	Fluvial Geomorphology		Page 7	In repeating the USFWS reports, the Study incorporates an abundance of errors, confusing explanations, and obscure objectives. Page 7 discusses what happens to an incremental increase in flow at Grand Island by the time it reaches Louisville. The discharge increments considered seem arbitrary. It would be most helpful if the Study would explain why these particular increments are relevant, and more generally, what “Program water” or “First Increment water” is. After consulting the Biological Opinion, the Adaptive Management Plan, the Record of Decision, the Platte River Recovery Implementation Program Final Environmental Impact Statement, and the Platte River Recovery Implementation Program, I’ve determined that First Increment water refers to 130,000 to 150,000 acre-feet of water annually, perhaps in the form of baseflow discharge targets or (undefined?) pulse flows. Spread evenly across the full year, that volume of water is equivalent to about 200 cfs, which is in the range of increases being evaluated. I speculate that the documents I’ve consulted are ambiguous about Program water because it has not yet been fully determined how that water is to be used. If so, the hydrologic analyses in the Study seem to be putting the cart before the horse. They seem to ask: if the upstream flow is bumped by X, could it be detected downstream, and would it materially improve habitat? Would it not make more sense to go about it other way around? That is, to ask: How much of an increase in flow is needed in the lower river to materially improve habitat there, and how much discharge needs to be added to upstream flows to hit that downstream target? Perhaps this is how the question is being approach, but it’s hard to tell from what’s written.	Beyond the scope of the study.
48	Gaeuman	Fluvial Geomorphology		Page 7, 5th paragraph	The paragraph begins and ends describing evaporation trends, but refers to total volume lost in the middle. It’s unclear whether this means total volume lost through evaporation, or total volume lost including seepage losses. It’s also unclear whether evaporation here includes transpiration. More generally, the analysis contained here and in the USFWS reports is often muddled in this regard. Terms like evaporation and ET do not seem to be used in a consistent manner throughout. However, the distinction may be an unnecessary complication, given the methods used to estimate these losses. See comments on that later.	Beyond the scope of the study.
49	Gaeuman	Fluvial Geomorphology		Page 8	The section on hydrograph translation is difficult to interpret. It could be greatly improved by telling the reader more specifically what the EA flow was. Four paragraphs into the section it is noted that “the peak of the EA flow at Duncan is estimated to be approximately 2000 cfs above base flows.” From this, a reader might infer that something like 2000 cfs was released from somewhere upstream or otherwise generated somehow. Is there some reason that what was done and where it was done can’t be clearly stated?	Editorial comment. Will be edited if Program elects to issue a revised final report.
50	Gaeuman	Fluvial Geomorphology	Appendix A	Page 1	The report discusses evaporation and seepage losses. Are there no diversions or pumps to consider?	Peer review of this appendix beyond the scope of the review.

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
51	Gaeuman	Fluvial Geomorphology	Appendix A	Page 2	The Figure 1 referenced here is missing. The same or a similar figure 1 is missing from Appendix B as well. The missing figures seem to be maps showing where all these gages, reaches, and tributaries are.	Peer review of this appendix beyond the scope of the review.
52	Gaeuman	Fluvial Geomorphology	Appendix A	Page 5	Estimated lag times are very crude. All are integer days, and variations in lag time with discharge are not considered. This component of the analysis deserves more attention than it was given.	Peer review of this appendix beyond the scope of the review.
53	Gaeuman	Fluvial Geomorphology	Appendix A	Page 5	Figure 2 referenced here is missing.	Peer review of this appendix beyond the scope of the review.
54	Gaeuman	Fluvial Geomorphology	Appendix A	Page 6-7	It would make sense to look at channel width during the time of year when evaporation losses are greatest. Seasonal trends in channel widths were considered indirectly through the application of "liberal" and "conservative" widths. Seasonal differences in width could be addressed more directly.	Peer review of this appendix beyond the scope of the review.
55	Gaeuman	Fluvial Geomorphology	Appendix A	Page 8	The use of pan evaporation rates to estimate river evaporation rates is a big leap. I suspect that the temperature of the pan is quite different than the temperature of the river. The pan coefficient might be intended to account for that, but no explanation or justification for the factor of 0.7 is given. The adjustment factors used for ET losses also lack explanation. These things need to be explained.	Peer review of this appendix beyond the scope of the review.
56	Gaeuman	Fluvial Geomorphology	Appendix A	Page 9-10	Seepage losses are calculated as the difference between the net inputs to a reach (inflows minus E/ET losses) and the outflow from the reach. This raises the question of why the analysis even bothers to estimate E/ET, because its magnitude is irrelevant to the result. If the estimate of E/ET was arbitrarily increased by 20 cfs, for example, the corresponding estimate of seepage loss would come out 20 cfs lower. The total loss, however, would remain the same regardless of what value was used for E/ET. It would be simpler and equally useful to simply define "losses" as the difference between inflows and outflows without regard to whether they are E/ET or seepage.	Peer review of this appendix beyond the scope of the review.
57	Gaeuman	Fluvial Geomorphology	Appendix A	Page 10	States that "Percent ungaged gains were not calculated, as this quantity is not relevant to this analysis." I'm not sure how to interpret this statement, but I do not agree that gains are irrelevant. It's also unclear whether "gain" refers to ungaged tributary input only, or to all gains (such as groundwater inflows and return flows from diversions).	Peer review of this appendix beyond the scope of the review.
58	Gaeuman	Fluvial Geomorphology	Appendix A	Page 11	Seepage loss estimates are called "conservative." It would be clearer to say the reported losses underestimate the actual losses. It would also be good to say something about the magnitude of underestimation.	Peer review of this appendix beyond the scope of the review.
59	Gaeuman	Fluvial Geomorphology	Appendix A	Bottom of Page 12	"Total estimated daily evaporation + ET losses" are given in units of cfs, that is, rate units instead of volume. And again on page 14. The figures referenced in this text give the losses in percent of flow.	Peer review of this appendix beyond the scope of the review.
60	Gaeuman	Fluvial Geomorphology	Appendix A	Page 18, 1st paragraph	This paragraph is unnecessarily confusing. The example discusses a reach, a subreach, a stream gage, and added Program water with no explanation of the geographic relationship between these elements. That difficulty would be partly relieved if Figure 1 wasn't missing from the report. It is stated that flow is 1000 cfs at Duncan on a particular day. It then refers to the "historic Platte River inflow," which, from the arithmetic that follows, appears to refer to the 1000 cfs at Duncan. Then, 200 cfs of Program water is introduced, although it's not clear how or where. Again, from the arithmetic, it seems that the Program water is also an inflow at the top of the reach, so that the flow at Duncan is actually 1200 cfs, not 1000 cfs. The presentation of the arithmetic is also overly complicated. It could be presented as three simple operations: determine the volume of inflows (including distance weighted gains), calculate the proportion of the inflows that are lost to E/ET (equal to losses/inflows), and multiply the Program water volume by that proportion.	Peer review of this appendix beyond the scope of the review.
61	Gaeuman	Fluvial Geomorphology	Appendix A,	Page 19, 5th paragraph	The sensitivity analysis for open water width needs more explanation. It seems to me that, according to how the total losses are calculated, changing the open water width would have zero effect on total losses because E/ET is subtracted from inflows before computing seepage losses. Could it be that the authors of this report applied 2 different estimates of E/ET to the same analysis? That is, did they subtract the original estimate of E/ET from inflows, then calculate seepage losses, then use those seepage losses with new, larger estimates of E/ET to arrive at new total losses? That would clearly be incorrect.	Peer review of this appendix beyond the scope of the review.
62	Gaeuman	Fluvial Geomorphology	Appendix A	Figures 9 and 10	Why do these graphs present different results than the similar graphs in Appendix C of the other USFWS report included as Appendix B (Page 17 in Appendix B)? Graph titles and axes labels are the same in both appendices, but the plotting positions differ.	Peer review of this appendix beyond the scope of the review.
63	Gaeuman	Fluvial Geomorphology	Appendix A	Page 23, 1st paragraph	States that there are no major diversions below Grand Island. What about numerous small diversions? Has that been evaluated?	Peer review of this appendix beyond the scope of the review.
64	Gaeuman	Fluvial Geomorphology	Appendix B	Page 5, 6th paragraph	Mentions a Tri-County supply canal system. I didn't see that mentioned anywhere else. I wonder where that is, and if it is, or should be, considered in the analysis presented in Appendix A.	Peer review of this appendix beyond the scope of the review.
65	Gaeuman	Fluvial Geomorphology	Appendix B, Table 2		Uncertainty is assumed to be 10% of the measured flow. See comment 46.	Peer review of this appendix beyond the scope of the review.
66	Gaeuman	Fluvial Geomorphology	Appendix B, Table 3		I'm wondering why the effect of First Increment Program activities is to cause negative changes in flow in some months. Here would be a good place to provide some explanation as to what First Increment Program activities include.	Peer review of this appendix beyond the scope of the review.
67	Gaeuman	Fluvial Geomorphology	Appendix B	Page 16	These travel times could be used to improve the Appendix A analysis.	Peer review of this appendix beyond the scope of the review.
68	Gaeuman	Fluvial Geomorphology	Appendix D	Page 18 of Appendix B and text on pages 9-10	Would be appropriate to define what the "OPSTUDY Model" is.	Peer review of this appendix beyond the scope of the review.
69	Guy	Fisheries Ecology and Aquatic resource Management	SOW Question 1		The Stage Change Study does address the overall objective of the RFP for a specific area in the Platte River. I believe that the study could have been more robust by extending the spatial extent of the study. The objective clearly states '…from the Elkhorn River confluence to the Missouri River confluence,' but the study was conducted on a reach from the Nebraska highway 50 bridge to the Chicago Rock Island and Pacific Railroad pedestrian bridge. I would agree that this reach is likely representative of much of the lower Platte River and is an area where pallid sturgeon have been located (Peters and Parham 2004); however, the Platte River at the confluence with the Missouri River is likely quite different and should have been included. The confluence is central to these analyses because much of the use of the Platte River by pallid sturgeon occurs near the confluence (Peters and Parham 2004). Had the investigators conducted measurements in at least two reaches (i.e., the current reach and one at the confluence), preferably more than two reaches (i.e., also include a reach near the Elkhorn River confluence), the precision, understanding of uncertainty, and inference space would have been greater with respect to Program water management activities. Further, the confluence reach is unique given that discharge in the Missouri River can influence the habitat dynamics in the Platte River which in turn will affect the results of Program water management activities, most likely different than the reach near Louisville, Nebraska. This criticism is especially relevant to the 2D modeling exercise which provides the most useful information for pallid sturgeon conservation. Understanding the effects of Program water management activities for additional reaches in the Platte River is instrumental if the Governance Committee is going to use this information to determine the effects of discharge on physical parameters thought to be important to pallid sturgeon. The effects of stage changes on physical parameters appears to be well studied for the reach near Louisville, Nebraska and should provide information needed to evaluate Program water management activities in that area. With that said, it would be beneficial if the investigators made it more clear regarding the discharges under which empirical data were collected, it is difficult to determine as currently written.	It is beyond the scope of this study to model several sections of the lower Platte River, and as a result the reach modeled was chosen because of its general representativeness of the lower Platte River. The study area is representative of the lower Platte River, including channel width and energy grade. The only exception would be areas influenced by unique hydraulic situations such as backwater effects like at the confluence with the Missourri. However, effects of Program flow changes on habitat classification would be even less detectable at areas with deeper flows like at the confluence with the Missourri. Considering that flow changes would not result in discernible changes to habitat area in the modeled reach, the same would likely be true at the confluence with the Missourri. Dates of empirical data collection are stated in 3rd paragraph on p. 1, and associated discharges for those dates are given in Table 7.
70	Guy	Fisheries Ecology and Aquatic resource Management	SOW Question 2		The selected physical parameters seem reasonable given the current state of knowledge regarding pallid sturgeon ecology. However, it is unclear what aspects of the pallid sturgeon life-history are targeted by Program water management activities. Providing habitat for adults is likely quite different than providing habitat for larvae. I realize this was not part of the scope of research for the investigators, but should be considered by the Governance Committee. This will help refine the effects of Program water management activities and how they relate to specific aspects in the conceptual models. Defining the life-history aspects of interest will also make the physical parameters more scientifically defensible. It is becoming clearer that habitat diversity and complexity are important to riverine fishes. Thus, combining metrics into a richness or diversity value and evaluating those data as a composite with varying Program water management activities might be more ecologically relevant than studying each parameter separately.	Primarily a hydrology study, not a study of pallid sturgeon life history or habitat use/occurrence.
71	Guy	Fisheries Ecology and Aquatic resource Management	SOW Question 3		The selected habitat classifications seem reasonable given the current state of knowledge regarding pallid sturgeon ecology. It may be implicit in some of the habitat classifications, but a more detailed analysis of the thalweg dynamics would have been informative (e.g., thalweg depth and migration under varying discharges). I believe understanding the dynamics of the thalweg given varying Program water management activities would be highly beneficial given that several studies indicate that pallid sturgeon are typically found in or near the thalweg. I recognize that the investigators are aware of the importance of this habitat type because they allude to it when they discuss run and plunge habitat. Again, it is important that the life-history aspect of interest is well defined because habitat use likely changes with ontogeny. As stated above, combining habitat classifications into metrics that describe the richness or diversity of habitat may be more ecologically meaningful.	Primarily a hydrology study, not a study of pallid sturgeon life history or habitat use/occurrence.
72	Guy	Fisheries Ecology and Aquatic resource Management	SOW Question 4		Yes, given the error associated with the Louisville gage and the results from the 100, 500, and 1,000 cfs additional Program water at Grand Island reaching Louisville as summarized in Figures 3, 4, and 4a. However, the amount detected varies temporally.	Comment noted.

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
73	Guy	Fisheries Ecology and Aquatic resource Management	SOW Question 5		Yes, relative to stage and velocity, but not temperature, turbidity, substrate, or channel morphology because those are not measured by the gauging equipment. It is clear in the results that there is temporal variation in water quality metrics and that the variation can be detected given the sample sizes, but it is not clear how the variation in water quality metrics relate to Program water activities.	Primarily a hydrology study, so most important to consider stage and velocity. The purpose of the study was not to make a statement about the importance of water quality parameters such as turbidity and temperature for pallid sturgeon or to quantify the effects of Program actions on those parameters.
74	Guy	Fisheries Ecology and Aquatic resource Management	SOW Question 6		In general, I believe the conclusions are supported by the data, although the conclusions are not clearly articulated. I am concerned that most of the analyses and measures of variation represent pseudo-replication. This relates to my comments in the first question. I believe the best way to determine the effects of Program water activities on physical parameters that are thought to be of significance to pallid sturgeon would be to conduct the Stage Change Study in multiple reaches (i.e., the reaches are the experimental unit). Although one could argue that reaches are not independent, I surmise that it better represents available habitat for pallid sturgeon and the influence of Program water activities on that habitat. The most important aspect of having multiple reaches is that one will have a better understanding of the uncertainty of Program related water activities on pallid sturgeon habitat.	It is beyond the scope of this study to model several sections of the lower Platte River, and as a result the reach modeled was chosen because of its representativeness of the lower Platte River. The only exception would be areas influenced by unique hydraulic situations such as backwater effects like at the confluence with the Missourri. However, effects of Program flow changes on habitat classification would be even less detectable at areas with deeper flows like at the confluence with the Missourri. Considering that flow changes would not result in discernible changes to habitat area in the modeled reach, the same would likely be true at the confluence with the Missourri.
75	Guy	Fisheries Ecology and Aquatic resource Management		Page 1, 2nd paragraph	"*bed topography at low to intermediate flows*" Why not bed topography at high flow?	Bed topography was collected at low to intermediate flows to facilitate bed surveys (i.e., difficult to access and survey at high flows). This study focuses on hydraulics of the existing bed, and does not involve sediment transport and mobile bed dynamics, which have a much less significant influence on habitat classification that is primarily driven by hydraulics.
76	Guy	Fisheries Ecology and Aquatic resource Management		Page 1, 3rd paragraph	"*Within the Study Reach, depth, velocity, turbidity, water temperature, dissolved oxygen, and conductivity measurements, as well as bed topography, were obtained …*" Why not sediment transport or large woody debris?	Beyond the scope of the study.
77	Guy	Fisheries Ecology and Aquatic resource Management		Page 5, 4th paragraph	"*Water Quality Measures*" These are commonly measured, but why? What are your hypotheses related to these or how do they relate to a conceptual model	Parameters of importance to the Program.
78	Guy	Fisheries Ecology and Aquatic resource Management		Page 5, 4th paragraph	"Data collected from each phase of sampling were then used to conduct a power analysis to determine whether sample sizes were adequate…" This is true at one site, but wouldn't it be better to measure these at multiple reaches and treat those as the experimental unit?	Since the objective of the study was to determine whether Program changes to discharge could affect pallid sturgeon habitat, discharge was assumed to be the independent variable driving water quality. Changes in water quality between locations would not necessarily be related to Program changes to discharge (assuming all sites are downstream of the Program action).
79	Guy	Fisheries Ecology and Aquatic resource Management		Page 7, 3rd paragraph	*"The results, assuming 100, 500, and 1,000 cfs of additional Program water at Grand Island, are summarized in Figures 3, 4, and 4a, respectively"* Very informative.	Comment noted.
80	Guy	Fisheries Ecology and Aquatic resource Management		Page 7, 6th paragraph	"*Comparison with USFWS Analysis*" Was this part of the original RFP?	USFWS analysis was always considered as important for evaluation for the stage change study.
81	Guy	Fisheries Ecology and Aquatic resource Management		Page 11, 3rd paragraph	"These comparisons indicate that the low-flow channel or channels tended to deepen during the high spring flow events and tended to become shallower in response to periods of low flow..." I find this very informative given pallid sturgeon tend to use the main channel, i.e., thalweg. We have found that pallid sturgeon avoid shallow, small tributaries.	Comment noted.
82	Guy	Fisheries Ecology and Aquatic resource Management	Figure 23	Page 13	Why so few samples at high discharge? Also, does the variation in the number of samples collected influence the results?	Few samples at high discharge because of limited access to survey at higher discharge. Additionally, the "higher" discharge sample points were considered supplemental to the "lower" discharge sample points, due to the relatively small difference in WSE at this range of flow (range of 3,700 cfs to 6,000 cfs). Highest flow points shown in Fig 23 (25,000 and 37,000 cfs) were collected from other agencies and events not related to this study. Variation in WSE between samples did not affect model calibration.
83	Guy	Fisheries Ecology and Aquatic resource Management		Page 13, 4th paragraph	"in conjunction with the topographic data on which the hydraulic model is based (Figure 24)." Some statistics on the regression would help reduce this subjective statement. Why is one of the data points missing from this figure? It is the outlier in Figure 25. Am I missing something?	Editorial comment. Will be edited if Program elects to issue a revised final report.
84	Guy	Fisheries Ecology and Aquatic resource Management	Figure 26	Page 13	Seems like a lot of scatter, should you explain the variation?	Editorial comment. Will be edited if Program elects to issue a revised final report.
85	Guy	Fisheries Ecology and Aquatic resource Management		Page 16, 2nd paragraph	This paragraph and the following two paragraphs are difficult to read.	Editorial comment. Will be edited if Program elects to issue a revised final report.
86	Guy	Fisheries Ecology and Aquatic resource Management		Page 16, 2nd paragraph	"(d* = D50{(SG-1)g/v2}1/3)) " I think the parentheses are off a bit.	Editorial comment. Will be edited if Program elects to issue a revised final report.
87	Guy	Fisheries Ecology and Aquatic resource Management		Page 17, 2nd paragraph	"*is the sediment transport strength defined as (τ'/τcr-1)*" -I don't think this is defined?	T' should be T'o. Will be changed if revised final version is issued. Tcr is defined in the following sentence.
88	Guy	Fisheries Ecology and Aquatic resource Management		Page 17, 3rd paragraph	"*Based on six grab samples of the surface bed material*" -Is six good enough? Why six?	6 is adequate considering the relative uniformity of the bed material
89	Guy	Fisheries Ecology and Aquatic resource Management		Page 18, 1st paragraph	"Evaluation of the areas occupied by dunes indicates that the median predicted dune height increases from 0.45 feet (~5.4 inches) at 3,700 cfs to 0.81 feet (~10 inches)…" These data are very interesting. Especially from a fish ecology aspect because we believe fish use these as velocity refuge. Any measures of variation with these data?	Comment noted.
90	Guy	Fisheries Ecology and Aquatic resource Management		Page 18, Figure 34	Excellent figure!	Comment noted.
91	Guy	Fisheries Ecology and Aquatic resource Management		Page 18, 2nd paragraph	"Plunge areas represent a complex habitat that is characterized by not only a rapid change of depth, but also its spatial location relative to bars and banklines within the detailed study reach…" This information and the bullets below are a bit difficult to follow.	Editorial comment. Will be edited if Program elects to issue a revised final report.
92	Guy	Fisheries Ecology and Aquatic resource Management		Page 19, 1st paragraph	"*Slackwater, Riffles, and Runs*." Why caps now?	Editorial comment. Will be edited if Program elects to issue a revised final report.
93	Guy	Fisheries Ecology and Aquatic resource Management		Page 19, Figure 36	Excellent figure.	Comment noted.
94	Guy	Fisheries Ecology and Aquatic resource Management		Page 19, 2nd paragraph	"The procedure used to develop the uncertainty bands in Figures 38a-d are described in the next section..." This is good, but make it clear what uncertainty you are measuring. I don't think this is uncertainty related to Program water activities, which is the central question.	Editorial comment. Will be edited if Program elects to issue a revised final report.
95	Guy	Fisheries Ecology and Aquatic resource Management		Page 20, Figure 44a	Very useful information.	Comment noted.
96	Guy	Fisheries Ecology and Aquatic resource Management	Table 11	Page 21	Measures of variation?	Editorial comment. Will be edited if Program elects to issue a revised final report.
97	Guy	Fisheries Ecology and Aquatic resource Management		Page 22, 3rd paragraph	"*…it can be concluded that changes in habitat areas as a result of 100 or 500 cfs environmental releases would have a negligible influence on pallid sturgeon habitat in the lower Platte River.*" I agree. Nice work.	Comment noted.
98	Guy	Fisheries Ecology and Aquatic resource Management		Page 23, 1st paragraph	"Finally, the increase in discharge does not move the conductivity, turbidity, temperature, or dissolved oxygen outside the typical range preferred by pallid sturgeon (Figures 42 and 43)." Not sure we know what typical is for pallid. Can you reword to avoid 'typical' and 'preferred?'	If Program elects to issue a revised final report, this sentence will be re-worded to avoid "typical" and "preferred". Also waiting for input from mark Pegg as to whethere there is an identified range or ranges for these WQ parameters for pallids.
99	Guy	Fisheries Ecology and Aquatic resource Management		Page 23, 3rd paragraph	"*Based on this stage change study, the % habitat in the lower Platte River experiences a relatively high rate of change for flows ranging between 4,000 cfs to 6,000 cfs.* " Not true for all habitats see Figures 44 and 45.	The flat and the run habitat types experience the highest rate of change for these flows. Run habitat areas meet habitat criteria for pallid sturgeon (deep and swift flow), which is why this is emphasized.

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
100	Guy	Fisheries Ecology and Aquatic resource Management		Page 24, 1st paragraph	*"The Flat classification would have been increased from approximately 30% (± 7%) to 40% (± 8%) of the habitat area…" Do you mean ±9?*	Editorial comment. Will be edited if Program elects to issue a revised final report.
101	Guy	Fisheries Ecology and Aquatic resource Management		Page 24, 1st paragraph	*"The decrease in discharge does not move the conductivity, turbidity, temperature, or dissolved oxygen outside the typical range preferred by pallid sturgeon (Figures 42 and 43)."* see comment #24	Assume "comment #24" refers to overall comment #98 in this spreadsheet.
102	Guy	Fisheries Ecology and Aquatic resource Management		Page 24, 3rd paragraph	*"Spring is likely the most critical period so that should be protected as best possible."* What does this mean? I don't think we can say this with much confidence.	Editorial comment. Will be edited if Program elects to issue a revised final report.
103	Guy	Fisheries Ecology and Aquatic resource Management		Page 25, 4th paragraph	*"Therefore, the results from this Study should be used as one part of a larger perspective on available habitat rather than an absolute factor in driving conclusions and decisions related to population dynamics."* Yes, nice work!	Comment noted.
104	Helsel	Environmental Statistics	SOW Question 1		The Study adequately addresses the relative magnitude of stage change due to management activities in relation to existing flows and habitat of the pallid sturgeon. It does not discuss the proposed changes in light of existing appropriations and any current legal constraints on flow in the Platte River. In other words, if these diversions were implemented would they impact the water rights of existing rights owners? The method for extrapolation of miSSing record to the Loup River at Columbus is flawed, and so the resulting errors on the analysis are unknown.	Beyond the scope of the study.
105	Helsel	Environmental Statistics	SOW Question 2		The data themselves are presumably scientifically defensible. They are fairly routine parameters with established protocols for collection. The amount of data is adequate. Analysis ofthe data is not adequate, if the purpose is to determine whether proposed flow augmentation and withdrawals for storage will significantly affect those parameters.	Analysis is adequate for the scope of this study. Reviewer did not provide specific points for inadequacy of the analysis in this comment.
106	Helsel	Environmental Statistics	SOW Question 3		This is not my area of expertise.	Comment noted.
107	Helsel	Environmental Statistics	SOW Question 4		Yes. Given that equipment and gauging error is listed as 10% (presumably +5% and ·5%0, the Study determined that flow changes such as those on page 24, going from 5,040 cfs to 3,290 cfs, are expected to be much greater than 5% (the direction is known), and so will be detectable as different from base flow conditions.	Comment noted.
108	Helsel	Environmental Statistics	SOW Question 5		No. Determination ofdifferences in water quality parameters using Analysis of Variance is flawed because the serial correlation in the data was not accounted for. The current analysis is not sufficient to determine whether there are significant impacts for these parameters.	Flow and habitat conclusions are most important; water quality parameter conclusions less so.
109	Helsel	Environmental Statistics	SOW Question 6		The Study's conclusions in regards to flow are supported by the data and analysis. The conclusions in regards to water quality parameters are not. The conclusions in regards to effects on habitat. are beyond my area of expertise, but appear to be the most thoroughly supported portion due to the modeling work.	Flow and habitat conclusions are most important; water quality parameter conclusions less so.
110	Helsel	Environmental Statistics	General		One fundamental problem with the Study is that many analyses were based on two apparently unpublished reports by the USFWS (2002 a and b). Results hinge so much on these draft reports that some statement from the Service should be included that verifies that the analyses, spreadsheets, etc. in these reports are valid, and that they received peer review and were considered accurate, even though the reports were never published. Or if this is not the case, a statement to the effect that the analyses were never peer reviewed or verified. Citations in this Study to those two reports usually do not discuss the methods that produced the conclusions, or speadsheets, or whatever product is being cited. The citations imply that what was reported is accepted as truth. What were the quality of these methods? Are there any plans for reviewing, verifying and publishing these 10-year old reports?	Beyond the scope of the study.
111	Helsel	Environmental Statistics		Page 3	An example of the dependence on these two reports is the method used for extrapolation from one gage to another using regression. This procedure has for years been known to dampen variability in flows, as regression predicts mean values. So the predicted daily flows for 30 years at the Loup River at Columbus (1978-2008) relied upon in this report will not be as variable, high or low, as would have been the actual record ifit had been measured. Other methods for extrapolation (one is often called MOVE or LOC) are preferred when the probability ofhitting a high or low flow is at issue, which it is here. These probabilities of high and low events will be underestimated, as regression by design predicts values towards the center. Given that the referenced report was never taken beyond draft, methods in that report including this one may be less than 'industry standard'.	Beyond the scope of the study.
112	Helsel	Environmental Statistics		Page 4	Please make the method for estimating missing evaporation data more clear. Were simply long-term monthly averages used? That is what is implied in the text. Or were monthly temperatures for the period to be estimated incorporated as well, so an unusually hot June for example had higher evaporation than the long-term average for June?	Editorial comment. Will be edited if Program elects to issue a revised final report.
113	Helsel	Environmental Statistics		Page 4	Isn't the statement that "the effect of flow changes in the central Platte River for the magnitude currently envisioned under the Platte River Program are not likely to be detectable at Louisville, Nebraska" (USFWS, 2002b)" one ofthe questions that this Study is to answer? Why then cite the answer, from a draft report at that, here, with implied great authority? No background or insight into the method the USFWS used to make this conclusion is presented here. I'd suggest you delete this statement until later after you have presented your analysis ofthis question. From my reading of the analysis, the Study finds that the flow changes will certainly be detectable at Louisville, decreasing II ...the flow at Louisville from 5,040 cfs to 3,290 cfs" (from page 24). So if not deleting the statement, make sure it is clear that this report finds a different result.	Beyond the scope of the study.
114	Helsel	Environmental Statistics		Page 5	Data are not "illustrated" in a table such as Table 5. They are "listed". If they should be illustrated, draw a figure. Tables don't illustrate anything.	Editorial comment. Will be edited if Program elects to issue a revised final report.
115	Helsel	Environmental Statistics		Page 5	What is the objective of determining whether "water quality data can differentiate between flow conditions"? This implies that the flow data cannot differentiate, and that water quality might be needed to do this. Or do you mean "water quality is different at different flow conditions"? The latter is focused on water quality, rather than on using it to say something about flow. Clarify the objective for why this analysis is being undertaken.	Editorial comment. Will be edited if Program elects to issue a revised final report.
116	Helsel	Environmental Statistics		Page 5	Your title "Accuracy Assessment of USGS Stream Gage Measurements" is misleading. You aren't doing an assessment of the accuracy of their methods. No data were collected to do so. You are just using their own accuracy assessment to compute the magnitude of 10 percent of observed flows. You should rename this section. Then you compute tables of differences in uncertainty estimates (Tables 4 and 6) without stating what these are good for, or how they came about. Was the method used in the USFWS report different from yours, and therefore the differences? If so, what were the two methods and why do you think they differ? Or are these the same methods just applied to different time intervals, and no change in the physical system has occurred? If this is true, then discuss how this helps you and how the difference in flows between 1975-1994 and 1995-2008 produce the observed differences listed in Tables 4 and 6	Editorial comment. Will be edited if Program elects to issue a revised final report.
117	Helsel	Environmental Statistics		Page 8	I have no idea what "Program staff also provided some preliminary information evaluating the pulse flow event to the Grand Island gage" means. Please reword or delete if not important.	Editorial comment. Will be edited if Program elects to issue a revised final report.
118	Helsel	Environmental Statistics		Page 9	So your conclusions here are that a release of 13K AF upstream is not really discernable by the time it travels downstream to Louisville. What are the implications of this for your later findings, given that the later findings seem to disagree with this?	This conclusion is consistent with later findings. Hydrograph translation described on p. 9 indicates that the small "peak" from the 13k AF release may be within the "noise" of the Louisville gage and the peak is less than the accuracy of gage readings. This is the same conclusion in the 1st paragraph on p. 23 that states the flow change is approximately equal to the gage uncertainty, and thus would be difficult to accurately detect.
119	Helsel	Environmental Statistics	Modeling		You found that you have well-calibrated models, and that the Platte acts like most other rivers in scouring the bed during high flows, increasing channel depth. You have a handle on the types of bedforms and bars likely present at differing flow regimes. This was translated into models of the amount of habitat available for different flow regimes. You evaluate uncertainty in habitat computations based on differences between measured and modeled flows. However this underestimates the true error; as errors for calibration data are always smaller than verification data not used to calibrate the model. A verification step of some sort, possibly a cross-validation procedure, should be used to quantify uncertainties instead. Yours are very likely too small.	As described in the last paragraph on p. 20, a test run at 6,000 cfs indicated that uncertainty in modeled depth/velocity contributes a very small portion of the overall variability in habitat estimates, and that most of the variability is from the uncertainty in hydraulic criteria used to identify habitat types. Considering the minor uncertainty associated with modeled depth/velocity, the approach using variability between modeled and observed values is appropriate.
120	Helsel	Environmental Statistics		Page 21	These daily values are not independent. Analysis of variance (as well as other standard statistical tests) assume independence of observations, that there is no sequential correlation. There certainly is for day to day measures of temperature and water depth, and probably for the other parameters as well. The result is that sample sizes are incorrect, that 46 observations for September 2008 for example may have the equivalent information of 20 independent observations. Therefore the test should be run using n=20 rather than 46, and the differences between months may with reduced sample sizes actually not be significant. Because this was not considered, these tests do not prove that differences actually have occurred between months. The tests should be run by correcting for serial correlation, which can be done with more complex software, or by more simply computing the 'effective sample size' that is a function of the magnitude of correlation between observations in the time series.	The point of the water quality data is to determine a relationship between water quality and Q (e.g., that turbidity increases with Q). WQ data collected for this study were supplemented with USGS WQ data (Figs 42 and 43) for flows well above the bankfull Q of 40,000 cfs. The final dataset included WQ data for flows for the entire range of historical Q at Louisville (Fig 2). As a result, data independence was assumed for the wide range of data in Figs 42 and 43.

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
121	Helsel	Environmental Statistics		Page 21	Serial correlation similarly invalidates standard power calculations. No detail on how power was calculated is given here. Standard ANOVA power calculations assume both independence and a normal distribution, and turbidity and depth data are probably not normally distributed (the others may be based on working with similar data). Much more detail should be given here on the procedure of the power calculations.	See response to previous comment. Due to the large WQ dataset that included USGS gaged water quality data, data independence was assumed.
122	Helsel	Environmental Statistics		Page 22	Even more importantly, the questions that the power analysis and ANOVA are addressing should be explicitly stated. What is the value in these analyses? State why you are performing them.	See response to previous 2 comments. Power and ANOVA analyses are somewhat irrelevant considering the large USGS gage dataset used to supplement WQ samples collected for this study. The total dataset shown in Figs 42 and 43 cover the range of historical Louisville Q, and as a result we have enough data to make predictions whether Program-related changes to Q would affect WQ.
123	Helsel	Environmental Statistics		Page 22	Figures 42 and 43 are stated as being composed of only the May 2009 data. Yet on page 23 they are used to compare to conditions at other additional times. This isn't valid, certainly for temperature. In addition, the data should be tagged and color coded by rising and falling stages of the hydro graph. Part of the large variation for similar discharges is due to differences between water quality when the storm is rising versus falling. Turbidity can certainly be expected to be very different for the same discharge depending on which limb of the hydro graph it occurs on.	Editorial comment. Will be edited if Program elects to issue a revised final report.
124	Helsel	Environmental Statistics		Page 23	The meaning of the statement" the magnitude ofthe change in discharge is subject to the same uncertainty as the overall flow" is unclear. Be more specific or delete this.	Editorial comment. Will be edited if Program elects to issue a revised final report.
125	Helsel	Environmental Statistics		Page 23	The statement" the increase in discharge does not move the conductivity, turbidity, temperature, or dissolved oxygen outside the typical range preferred by pallid sturgeon (Figures 42 and' 43)" is too broad and sweeping of a statement considering that the figures are based on data only from one month, and you've already stated that based on an ANOVA the levels of these parameters differ between months. Graphs of the relationship between these parameters and discharge should be based on data from all four months of interest where diversions are expected (note that May is not one of those months and so is incorrectly used for the data in these graphs), while considering variation due to rising vs falling hydrograph and to temperature effects. In short, you cannot use the current graphs to make the conclusion you are heading toward.	Editorial comment. Will be edited if Program elects to issue a revised final report.
126	Helsel	Environmental Statistics		Page 24	a typo? The Run classification would be reduced from 45% to 34%, a decrease of 1 %??? Plus, you report different values in Appx G. Please clarify.	Editorial comment. Will be edited if Program elects to issue a revised final report.
127	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	SOW Question 1		The report does adequately address the overall objective as stated. The report is logically organized and compete, however, it would be helpful to include a background section early in the report that describes the type of flow conditions being considered to place the study in context.	Comment noted.
128	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	SOW Question 2		Yes, the physical parameters are adequate and scientifically defensible. Clearly, the need for improved scientific understanding of selection and utilization of specific, local flow conditions (both hydrodynamics and water quality) and habitat-scale flow patterns that pallid sturgeon prefer is still needed, but outside of the scope of this project. The report does a very good job of describing available data and current understanding and utilizing this information to reach the conclusions.	Comment noted.
129	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	SOW Question 3		Yes, the habitat classifications are adequate and scientifically defensible. In addition, to the uncertainty analysis and quantification of habitat areas by type, it would be helpful to include a broader discussion about the space-time utilization of individuals that may be residing or moving through the area. For instance, “what is known about adjacencies or distributions of habitat types”, this may be important for habitat utilization and may be impacted by stage change. From the information it did not appear that distribution or adjacency would change, but would be good to include this in the discussion.	Comment noted.
130	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	SOW Question 4		Yes, the report clearly addresses the detectability of the stage change from Program Water activities. It would be helpful, within the discussion section to refer to the stage discharge curves for the reach.	Comment noted.
131	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	SOW Question 5		Yes, the report addresses the impact of the stage change on the river parameters listed. It would be helpful to list other parameters that may be important, such as flow shear lines, and eddy structures, however, less is know about these features than the parameters given. With that said, some acknowledgement that the parameters considered may not be the only flow features that determine habitat function and utilization would be useful. The second to last paragraph of the report provides some comments towards this, but could be expanded.	Comment noted.
132	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	SOW Question 6		Yes, the findings of the study and conclusions reached are supported by data and sound engineering and scientific analysis. It would be beneficial to include an executive summary of the report and a clear conclusions / summary section in the report	Comment noted.
133	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	General		Scientific Soundness – The methods and approaches were based on sound engineering and science. Unfortunately, although there is literature and past studies that describe general habitat preferences and utilization, there is little available information from a first-principles understanding of specific habitat needs for the species of interest. This short-coming is, however, common in most aquatic restoration and management programs. The project report uses sound, available engineering and science to address this inherent uncertainty in its habitat evaluation. Although further studies and fundamental research could improve this understanding, it is clearly outside of the scope of this project.	Beyond the scope of the study.
134	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	General		Organization and Clarity – The report logically presents the engineering analysis of the hydrologic conditions of the study reach; data collection programs; hydraulic model construction, calibration and utilization; geomorphic assumptions and analysis, flow habitat assumptions and habitat discrimination technique; and conclusions. Uncertainties of methods, models and approaches are adequately described throughout the report.	Comment noted.
135	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	General		Conciseness – The report is well written and presents an appropriate amount (both depth and breadth) of information. The report also, includes relevant information in the appendices and adequately sites previous and related published work.	Comment noted.
136	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	General		Degree to which the conclusions are supported by the data – The report provides a logical progression from hydrologic conditions of the study reach through final conclusions, including the uncertainty of information utilized in the decision process.	Comment noted.
137	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	General		Cohesiveness of conclusions – The formulation of the conclusions is based on sound engineering and science. The conclusions/summary statements should have been explicitly organized in a closing, Conclusion or Summary section in the report rather than simply woven into the Discussion section.	Comment noted.
138	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling			In the discussion of minimum and maximum flow selection, a flow recurrence /exceedance plot would be helpful to place the selected flows in context, rather than referring to figure 2. Also the period of record should be stated for this analysis in the Study Flows section.	Editorial comment. Will be edited if Program elects to issue a revised final report.
139	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	Figure 2		x-axis of figure 2 should use the first day of the month for each major grid line and label	Editorial comment. Will be edited if Program elects to issue a revised final report.
140	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling			A better location map would be helpful to locate the study reach within the state and along the Platte River Stream network.	Editorial comment. Will be edited if Program elects to issue a revised final report.
141	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling			It would be helpful to explicitly state that the 2D SRH model is a fixed bed model andthis geometry is used throughout for all simulations. How this impacts the local flow conditions for higher flows should be addressed.	Editorial comment. Will be edited if Program elects to issue a revised final report.
142	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling			Figures 24, 25 and 26 are useful data plots, however, it would be helpful to see the distribution of the difference between model and field data on a spatial image of the study area. This would be helpful to understand the performance of the model, but likely does not negatively impact the use of the model results.	Editorial comment. Will be edited if Program elects to issue a revised final report.
143	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling		Page 24, first paragraph after table 13	….45% (+8%) of the habitat area to approximately 34% (+8%) of the habitat area, a decrease of 1%. The “1%” should be “11%”.	Editorial comment. Will be edited if Program elects to issue a revised final report.

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
144	Weber	River Hydraulics and Mechanics, River Restoration, and Computational Modeling	Discussion		In addition to the text description, it would be helpful to tabulate the changes to habitat classification in the discussion section. This to compare across conditions of interest, and to show the impact of the management actions.	Editorial comment. Will be edited if Program elects to issue a revised final report.
145	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	SOW Question 1		Yes, subject to comments	Comment noted.
146	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	SOW Question 2		Yes, to the extent that they can actually be meaningfully evaluated by the methods used.	Comment noted.
147	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	SOW Question 3		This is a good example of a subject that can't be evaluated if one considers the report in isolation, because habitats get minimal attention in this report.	Primarily a hydrology study; no intention to focus on habitat.
148	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	SOW Question 4		Yes and No. Yes the study answered the question; no, program activities (as to flow) cannot be detected. Effects of other activities (sediment mobilization for example) were not assessed.	Comment noted.
149	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	SOW Question 5		N/A	Comment noted.
150	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	SOW Question 6		Yes, especially given the conclusion is "did not find".	Comment noted.
151	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		I consider the core elements of the study to be technically sound and useful. With some exceptions noted below, the work satisfied the scientific and technical scrutiny that was within my expertise to apply, and within the peer review budget to investigate. The study report appears to satisfy the objectives of the RFP.	Comment noted.
152	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		In my experience, a role of peer review is to focus on potential weaknesses or limitations in a study. Thus the critical nature of my comments should not be taken to suggest the study is seriously flawed, but rather as my effort to provide constructive input to future work. In the specific comments, I observe the following aspects of the study that I thought might be in most need of improvement or of further evaluation.	Comment noted.
153	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		For purposes of organization and clarity, it would be beneficial to provide an introduction that puts the study in context. See specific comments on p. 1.	Editorial comment. Will be edited if Program elects to issue a revised final report.
154	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		I suggest reconsidering the methodology and results of the loss analysis. See specific comments on p. 2.	Further input on this topic from this reviewer is in Comment #171 below. The loss approach used in this analysis is based on the common mass balance technique using known input and outputs and gaged flow between 2 points. The alternative approach suggested by reviewer in Comment #171 (i.e., modeling the flow exchange between surface and ground water based on ground water heads) is beyond the scope of this study.
155	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		The effects of flow modification by hydropower appear to be potentially profound and need further evaluation. See specific comments on p. 8.	Beyond the scope of the study.
156	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		The apparent rigor of certain of the analyses does not fully capture the uncertainty in the bottom line results. See specific comments on p. 20.	Specific comment referenced here is overall Comment #184 below. This boils down to an editorial comment suggesting that a full acknowledgement of unertainties and limitations be added to the report. This will be added if the Program elects to issue a revised final version of the report.
157	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Scientific soundness*. The technical aspects of the document were generally good, with possible exceptions noted under Specific Comments.	Comment noted.
158	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		Organization and clarity. The Specific Comments (especially regarding Pages 1 and 9) identify ways the organization and clarity of the report could have been improved by providing additional background discussion. That being said, within what was actually presented, the report was well organized and well written.	Comment noted.
159	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Conciseness*. Good.	Comment noted.
160	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Degree to which conclusions are supported by the data*. Hard to say without copies of the data sets, spreadsheets, and models.	Comment noted.
161	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Cohesiveness of conclusions*. Ok within the context of the report. But there is so much unsaid, that a stranger to the process might not be able to properly judge the end results.	Comment noted.
162	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Presentation: Is a tightly reasoned argument evident throughout? Does the manuscript wander from the central purpose?* The true central purpose is never stated. Within the organization as presented, the report does a good job of walking through the methods, data and results without any wandering.	Comment noted.
163	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Methods: Are they appropriate? Current? Described clearly and with sufficient detail so that someone else could repeat the work?* Except for the evaluation of losses, the methods are appropriate and current. The level of detail in methods is good. I don't know enough about the models to know if one could repeat the work, but I suspect it would be necessary to get the actual model I/O files to do so.	Comment noted.
164	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Data presentation: When results are stated in the text of the manuscript, can you easily verify them by examining tables and figures? Are any of the results counterintuitive? Are all tables and figures clearly labeled? Well planned? Too complex? Necessary?*Good marks on all of this.	Comment noted.
165	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Statistical design and analyses: Are they appropriate and correct? Can the reader readily discern which measurements or observations are independent of which other measurements or observations? Are replicates correctly identified? Are significance statements justified?* A lot of attention is paid to statistical determinations, but there is a fair amount more that could and probably should have been said. See comments on P. 20.	Comment noted.
166	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Conclusions: Has the author(s) drawn conclusions from insufficient evidence? Are the interpretations of the data logical, reasonable, and based on the application of relevant and generally accepted scientific principles? Has the author(s) overlooked alternative hypotheses?*I found the overall results acceptable, since they agreed with what was fairly evident even without the study, that no significant relationships can be quantitatively established.	Comment noted.
167	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Errors: Point out any errors in technique, fact, calculation, interpretation, or style.* My review was not in depth, but I found nothing of concern except for the loss analysis (see comments on P. 2).	Comment noted.
168	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology	General		*Citations: Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in the manuscript?* It's a good reference list.	Comment noted.
169	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 1	In my first paragraph of general comments, I said the study appears to satisfy the objectives of the RFP. I used the word "appears" because neither the RFP nor report does a good job of placing the study objectives into context, i.e. explaining to what ultimate purpose the work was being done. To understand the work, I relied on the Biological Opinion and the limited discussion in the Protocol. I don't fault the authors for this necessarily, as it isn't clear from the RFP that they were tasked to provide context in the report. Nonetheless, the lack of context made reading and evaluating the report much more difficult than it should have been (at least for me). The standard organization for a scientific paper includes an introduction that presents the background knowledge necessary for the reader to understand the findings of the paper. This is especially important when, as here, there is no executive summary to bring everything together. In this case the following would have been useful in providing the reader with important background knowledge: 1) A brief synopsis of the nexus between stage and sturgeon as it is now understood. Note that the fact that this paper is about pallid sturgeon isn't even mentioned until halfway through the report (p. 14). 2) One or more hypotheses about how the Program could impact that nexus (including a "non-detect" hypothesis). This would disclose the current thinking about why the study reach is important to sturgeon, and why we are interested in predicting impacts to depth, velocity, bedforms, topography and the like. 3) A clear and succinct statement of the methodological approach to evaluating the hypotheses. This might be a flow chart indicating that first we have to route Program flows to the reach; then model their impact on the parameters of interest; which means very complex hydraulic models and interpretations relating especially to bedforms; and finally translate that to impacts to sturgeon habitats. It may seem obvious, but that doesn't mean the report shouldn't be clear about what is being done.	Editorial comment. Will be edited if Program elects to issue a revised final report.

Comment #	Reviewer	Expertise	Section	Page	Comment	Response
170	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Pages 1 and 2	Figure 1 would benefit from an inset location map.	Editorial comment. Will be edited if Program elects to issue a revised final report.
171	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 2	The loss analysis is an update of a FWS study provided in Appendix A. It is difficult to fully evaluate the method without a copy of the spreadsheet. Nonetheless, I was very surprised about the results, and wonder if the Program is approaching this important issue correctly. I did not review Appendix A in sufficient detail to know for sure that my concerns are valid, so please consider this discussion accordingly. My two primary concerns are as follows. Some of the loss rates reported are much higher than I have seen, even in arid western rivers. If it has not been done, I strongly recommend each element of the loss be independently verified. For example, analytical methods using groundwater head data can be used to independently estimate seepage losses. It appears that the method calculates Program losses in proportion to flows. An alternative (and in my experience more appropriate) approach is to calculate them on an incremental basis. If the current procedure has not been affirmatively deemed more appropriate than an incremental approach, the incremental method should be To illustrate my concern, consider the result of the accounting done by the Bureau of Reclamation for the loss of water imported into the Rio Grande Basin (this loss rate is important for quantification of endangered species impacts as well as available water supplies). Based on quantification conducted by the Rio Grande Compact Commission, a loss rate has been calculated for the reach from Heron Reservoir (near the Colorado border on a tributary of the Rio Chama) to Albuquerque (a distance roughly comparable to Grand Island-Louisville). The loss rate applies to the flow added to natural flow by imported water. There are elements of the rate calculation that are not entirely apples-apples to that made for the Lower Platte, but these would have a modest effect at most. The Rio Grande loss rate is 2%. Given this result, it is difficult for me to understand loss rats as high as 90% in eastern Nebraska.The subject of losses above Grand Island is not considered, but it would be of interest to know the Louisville flow as compared to an upstream reservoir release. The following comment is not related to the above, but to the reference to selection of “appropriate” flows on page 2. Appropriate how? With no discussion of matters such as sturgeon habitat, the reader cannot know. It is also confusing to indicate that a flow of 39,000 cfs is of “primary interest”, without explaining why it was then appropriate to use 8,000 cfs as the high end of flows selected.	Beyond the scope of the study.
172	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 3	I did not understand how the study made use of two different periods of record for extended analysis.	Editorial comment. Will be edited if Program elects to issue a revised final report.
173	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 4	The new spreadsheet analysis probably should be provided in an Appendix.	Editorial comment. Will be edited if Program elects to issue a revised final report.
174	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 5	The power analysis probably should be provided in an Appendix.	Editorial comment. Will be edited if Program elects to issue a revised final report.
175	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 6	The focus on gage uncertainty may cause readers to overlook the uncertainty in the USFWS spreadsheet which estimates impacts of Program flows.	Comment noted.
176	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 7	In addition to the plots in Figures 3, 4 and 4a, it would be interesting to see the data plotted as flow duration curves.	Editorial comment. Will be edited if Program elects to issue a revised final report.
177	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 8	This page presents Figure 5 and makes note of the “obvious” intraday flow variation. The discussion focuses on how to smooth that out so the pulse can be translated from Grand Island to Louisville, which is certainly appropriate. However there is no discussion whatsoever about the fact that the hydropower effect causes a 1 foot diurnal change in stage, which is far greater than the transformed impact of the pulse. The implied premise of the study is that stage impacts habitat, through effects on velocity, depth and bedforms. If so, how is it that the effects of such a large and rapid stage change are not considered at all? Had the study found that Program releases did impact habit in the study reach, that conclusion would have been called into question because the interday flow variation was not considered and could be such that it swamped out any Program impact.	Diurnal flow variations in Fig 5 are a result of Loup River hydropower production, and are not related to Program actions. The large & rapid change in stage associated with Loup River hydropower production may have an impact on pallid sturgeon habitat, but assessing those impacts is not a Program responsibility and is beyond the scope of this study.
178	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 9	Another aspect of context that wasn’t effectively presented was the cause-effect relationship being studied. The stated objective puts “stage” as the focal point, whereas after reading the report, I perceive the operational objective was to evaluate the impact of flow (cfs) as it directly impacts water depth and velocity, and the consequent effects on sediment, bedforms and habitat. Stage as such seemed not to be that much of a consideration, or a particularly good surrogate, especially in terms of assessing velocity and its consequences. The lack of hypotheses was surprising given the nature of the Adaptive Management Plan.	Beyond the scope of the study.
179	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 10	Given that stage is the focus of the study, are two water surface data points sufficient for the cross-sections?	Two water surface data points are adequate for validation of a 1-dimensional model, which assumes that water surface elevation is constant at a given cross section.
180	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 11	It would be useful to have an assessment of the change in roughness with flow, and especially whether it is reasonable to interpolate values.	Editorial comment. Will be edited if Program elects to issue a revised final report.
181	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 12	I did not follow the explanation of the very low n values for the 2D model.	Editorial comment. Will be edited if Program elects to issue a revised final report.
182	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 15	The entire bedform discussion would benefit from illustrations.	Editorial comment. Will be edited if Program elects to issue a revised final report.
183	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 19	I found Figure 36 hard to interpret.	Editorial comment. Will be edited if Program elects to issue a revised final report.
184	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 20	The use of a Monte Carlo analysis to assess uncertainty gives an impression of statistical rigor to the results. Certain other aspects of the work give a similar impression. However if one starts at the very beginning of the work, i.e. an increment of flow at Grand Island (with unstated uncertainty), and carries it through to the end, many other issues become apparent – the loss estimates, hydrograph translation, error bars on model inputs (median grain size is a good example), and more. This cascade of uncertainties would have undermined the results had a positive relationship been found. As the bottom line of the report did not assert any relationships had been statistically demonstrated, these issues are perhaps not critical. Still, I would have liked to see (in the discussion section) a recap of all the assumptions, limitations and uncertainties in the work.	Editorial comment. A summary of assumptions, limitations, and uncertainties will be added to the report if Program elects to issue a revised final report.
185	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 22	Of interest given prior discussion, the models are (correctly) said to evaluate depth and velocity, not “stage change”. One question not posed previously: why is the release being evaluated so small?	Potential Program releases as per the Program document were evaluated.
186	Wilson	Hydrology, Environmental Impact Assessment, Geomorphology		Page 25	Perhaps emphasize that lack of statistical significance does not equal lack of effect. In fact, qualitatively one can say that a release probably does have at least marginal benefit (this is a bit more affirmative than “no additional stress”).	Editorial comment. Will be edited if Program elects to issue a revised final report.