PONE-D-22-16898

Northern Pikeminnow Abundance in Deadwater Slough, Salmon River, Idaho, and Potential Impacts to Local Chinook Salmon

PLOS ONE  
  
Dear Mr. Ackerman,  
  
Thank you for submitting your manuscript to PLOS ONE. After careful consideration, we feel that it has merit but does not fully meet PLOS ONE’s publication criteria as it currently stands. Therefore, we invite you to submit a revised version of the manuscript that addresses the points raised during the review process.  
  
The reviewers have raised a number of important points that will need to be addressed if your paper is to be accepted. Please revise accordingly. Both reviewers question, and I agree, the validity of the assumption that 60% of the diet of Northern pikeminnow is composed of fish, which is not supported by your own empirical diet data from gastric lavage which stated that only 11.7% of the stomach contents of sampled pikeminnows was composed of fish. A reasoned justification for this assumption is needed*.* There are a number of other assumptions made that seem questionable and will need some justification or further explanation. The Deadwater Slough at 1.9 km is a small area of such a large system and additional verbiage is needed to explain why this is representative of the predation issue of pikeminnows and salmonids in the larger overall system*.* I am also unconvinced that angling is a random sampling method that will give accurate population/abundance estimates of pikeminnow. A brief discussion and citations corresponding to the use of angler data for suchestimates should be included. I also question how realistic the assumption of no growth is "We assumed no growth in individual Northern Pikeminnow, resulting in equal start and end weights." Again some additional verbiage explaining the rationale of this assumption is needed.  
  
Please submit your revised manuscript by Dec 04 2022 11:59PM. If you will need more time than this to complete your revisions, please reply to this message or contact the journal office at [plosone@plos.org](mailto:plosone@plos.org). When you're ready to submit your revision, log on to <https://www.editorialmanager.com/pone/> and select the 'Submissions Needing Revision' folder to locate your manuscript file.  
  
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We look forward to receiving your revised manuscript.

Kind regards,

Mary Peacock, Ph.D.  
Academic Editor  
PLOS ONE

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2. In your Methods section, please provide additional information regarding the permits you obtained for the work. Please ensure you have included the full name of the authority that approved the field site access and, if no permits were required, a brief statement explaining why.

3. Thank you for stating the following in the Acknowledgments Section of your manuscript:

“The authors extend much appreciation to the many volunteers that assisted with field efforts including collaborators from the Bureau of Reclamation, Idaho Governor’s Office of Species Conservation, Idaho Department of Fish and Game, and the Lemhi Regional Land Trust, among others. Special thanks to Braden Lott, Jared Barker, Brian Hamilton, Tulley Mackey, and Chelsea Welke for their help in the field. We further appreciate the administrative support and guidance from staff at Inter-Fluve. This manuscript benefited from reviews by Sean Gibbs, Mark Roes, and Ian Courter. Funding for this study was provided by the Bureau of Reclamation, Pacific Northwest Regional Office (contract No. 140R1021F0018) with assistance from the Idaho Governor’s Office of Species Conservation.

We note that you have provided funding information that is not currently declared in your Funding Statement. However, funding information should not appear in the Acknowledgments section or other areas of your manuscript. We will only publish funding information present in the Funding Statement section of the online submission form.

Please remove any funding-related text from the manuscript and let us know how you would like to update your Funding Statement. Currently, your Funding Statement reads as follows:

“The funders had no role in the study design, data collection and analysis, decision to publish, or preparation of the manuscript.”

Please include your amended statements within your cover letter; we will change the online submission form on your behalf.

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Reviewers' comments:  
  
Reviewer's Responses to Questions

**Comments to the Author**  
  
1. Is the manuscript technically sound, and do the data support the conclusions?  
  
The manuscript must describe a technically sound piece of scientific research with data that supports the conclusions. Experiments must have been conducted rigorously, with appropriate controls, replication, and sample sizes. The conclusions must be drawn appropriately based on the data presented.

Reviewer #1: Partly

Reviewer #2: Partly

2. Has the statistical analysis been performed appropriately and rigorously?

Reviewer #1: No

Reviewer #2: N/A

3. Have the authors made all data underlying the findings in their manuscript fully available?  
  
The [PLOS Data policy](http://www.plosone.org/static/policies.action#sharing) requires authors to make all data underlying the findings described in their manuscript fully available without restriction, with rare exception (please refer to the Data Availability Statement in the manuscript PDF file). The data should be provided as part of the manuscript or its supporting information, or deposited to a public repository. For example, in addition to summary statistics, the data points behind means, medians and variance measures should be available. If there are restrictions on publicly sharing data—e.g. participant privacy or use of data from a third party—those must be specified.

Reviewer #1: Yes

Reviewer #2: Yes

4. Is the manuscript presented in an intelligible fashion and written in standard English?  
  
PLOS ONE does not copyedit accepted manuscripts, so the language in submitted articles must be clear, correct, and unambiguous. Any typographical or grammatical errors should be corrected at revision, so please note any specific errors here.

Reviewer #1: Yes

Reviewer #2: Yes

5. Review Comments to the Author  
  
Please use the space provided to explain your answers to the questions above. You may also include additional comments for the author, including concerns about dual publication, research ethics, or publication ethics. (Please upload your review as an attachment if it exceeds 20,000 characters)

Reviewer #1: General comments:  
  
The manuscript “Northern Pikeminnow abundance in Deadwater Slough, Salmon River, Idaho, and potential impacts to local Chinook Salmon” is well written and will be of interest to fishery managers charged with managing salmon fisheries in the Salmon River drainage. The methods seem appropriate; although I question why the authors present multiple abundance estimators. However, there are a large number of assumptions used within this manuscript; assumptions that are not validated in the current study. As such, I believe that the strength of the conclusions should be tempered given that the management implications could result in development of a program to reduce a native fish.  
  
My interpretation of the authors’ data is that the diet of Northern Pikeminnow in Deadwater Slough is composed of 11.7% (by weight?) “fish parts” during emigration of DSR and NRR (lines 313-315); and that this 11.7% could be made up of some combination of Chinook Salmon, Redside Shiner, Largescale Sucker, sculpin species, and Mountain Whitefish (lines 402-405). Yet the modeled consumption potential of Northern Pikeminnow relied on assumed proportion of fish in the diet of 30-90% and the calculated effects on Chinook Salmon were assumed at a minimum proportion of 18% of the diet composed of Chinook Salmon (60% fish in diet with 30% of fish being Chinook Salmon) to a maximum of 39% (60% fish in diet with 65% of fish being Chinook Salmon); hopefully my math is correct, if not, my apologies. If this all look correct, I suggest the authors include a bioenergetics model run where fish comprise 11.7% of the diet and include a run of their “sensitivity analysis” where fish comprise 11.7% of the diet. Essentially, I see no reason not to use the empirical data from Deadwater Slough as opposed to only relying on assumed data. Conducting gastric lavage and diet analysis on 1,558 Northern Pikeminnow is a massive undertaking just to disregard the results of the effort. If these diet analyses are so poor as to ignore them in their bioenergetics and consumption potential assessments, then perhaps the authors should indicate that the next step in understanding the potential effect of predation by Northern Pikeminnow on Chinook Salmon is getting a better idea of just what the diet of Northern Pikeminnow is in this area; as opposed to suggesting large-scale habitat modifications or native species suppression.  
  
  
Line-specific comments:  
  
Lines 165-167: I am having trouble understanding how this does not violate the closed population assumption. It seems that completely removing these fish from the data set would be more appropriate because whether they are available for future capture or not, these dead fish would seem to have an influence on the estimated N (I think as either the n parameter or the m parameter, or both). Perhaps the authors can elaborate on this issue. Also, it would be valuable for the reader to know how many fish died during sampling (is this information in Table 1: i.e., something like [(Fish Caught + Marked Fish Caught) – Marked Fish Returned]?  
  
Line 208: Some journals do not allow abbreviations at the beginning of the sentence. I am not sure how PLOS ONE deals with this, but you may want to check their guide for authors and adjust here (and throughout the document) if necessary.  
  
Line 218: “Whirl-Pak” is a registered trademark name and may need to be cited appropriately. You may want to check PLOS ONE guide for authors to find out how to deal with this.  
  
Lines 220-221: “lowest taxonomic unit” is somewhat vague. Do you intend ‘lowest taxonomic unit possible”, or are you referring to genus, species, subspecies?  
  
Lines 239-242: The equation appears to convert fork length to weigh, but you also state that you converted TL to FL. What equation did you use to make this conversion from TL to FL (please cite source and/or describe methods).  
  
Lines 249-251: The authors conducted diet analysis of 1,558 Northern Pikeminnow, which included 350 non-empty stomachs, and identified contents to the “lowest taxonomic unit”. Based on the results section, it appears that the lowest taxonomic unit reported is “fish or fish parts” (line 311). It also appears that “fish remnants made up 11.7% of all stomach contents” (lines 314-315), overall; I assume that this is by weight, given that line 314 discusses mean weight from individual fish, and I also assume that this is only for non-empty stomachs. Consequently, and if I read this correctly, the proportion of fish in the diet of Northern Pikeminnow in Deadwater Slough is known based on the authors data. The author’s empirical data from Deadwater Slough (11.7% fish parts) differs substantially from the values used in the bioenergetics model (30-90% fish prey items). The authors provide some discussion related to why their diet analysis may be incorrect, or “conservative” (lines 402-417); however, it is also plausible that the proportion of fish prey in the non-empty stomachs was unbiased. Consequently, I believe at a minimum a bioenergetics model run and estimate of “Impacts to Chinook Salmon Populations” should be conducted based on the empirical data from the authors own study. This could be compared and contrasted with model runs from “assumed proportions of fish prey” (i.e., 30-90%), but to completely omit a model run (or runs) based on empirical observation from the study area during the time of the study seems misleading at best.  
  
Lines 281-285 (and throughout): PLOS ONE has a broad audience (i.e., it is not a regional journal, nor a taxon-specific journal). As such, it may be difficult for the average reader to understand how “Lower Granite Dam” fits into the geography, or importance to Pacific Salmon ecology. Also terms such as “Granite-to-Granite” may not make intuitive sense. Consider the audience you are writing to and phrase your manuscript appropriately.  
  
Lines 282-285: It would be valuable for the authors to expound on what factors influence SAR (briefly in the Methods and more fully in the Discussion). How SAR is influenced by additive or compensatory mortality, and how population growth rate (or abundance) is influence by mortality during rearing/migration versus by contemporary ocean conditions and upstream migration will be important to understanding how appropriate it is to convert juvenile predation to assumed adult returns. Specifically, the manuscript implicitly suggests that there is a direct relationship between putative predation on Chinook Salmon by Northern Pikeminnow and the number of Chinook Salmon returning from the ocean; however, this seems to assume that mortality is additive, which may or may not be accurate.  
  
Line 302: How was the confidence interval estimated for the spring 2021 sample? The equation on line 201 appears to use the point estimate from the fall abundance estimate and the ratio of CPUE for spring and fall to estimate the abundance in spring. This seems like a reasonable approach; however, it is likely inappropriate to use the same method to calculate a confidence interval for your spring 2021 sample.  
  
Lines 296-301 (and lines 170-196): The authors should state why they chose to use multiple abundance estimators. Lines 296-297 indicate that the “sampling design most closely matched the multiple census estimator…”, so I am unsure why the other estimator was used. The objectives of this study do not include an assessment of different abundance estimators, and there is nothing in the interpretation of the results that objectively compare the results of the abundance estimators. Consequently, I am unsure why the authors did not simply come up with a study design and use the most appropriate analytical methods to analyze their data. Essentially, reporting the results from other estimators does not provide any useful information in the context of this study. If the authors believe that the delayed mixing Schnabel estimator is most appropriate for their study, then that is the only one that needs to be presented in the Methods (with justification for why they chose it) and reported in the Results and Discussion. Alternatively, the authors should include an objective related to comparing the performance of different abundance estimators and provide a formal and objective assessment of the different abundance estimators.  
  
Lines 300-301: The Methods indicate that the delayed Schnabel estimator assumed that marked and released fish were not available for recapture for 48-h (lines 188-190). Where there instances in your data where this assumption was violated (i.e., you recaptured a marked fish in < 48 h)? And if so, how did you account for this.  
  
Lines 398-399: Suggest rearranging to “…the catch-and-release record for Northing Pikeminnow in the State of Idaho, measuring 639 mm TL, was caught…”.  
  
Lines 402-406: It may be valuable for some readers for you to indicate in the manuscript what the proportions of these identifiable remains were by fish species. I understand that it was difficult to accurately identify all fish remains; however, this is the first instance that the reader is provided with information that some of the remains were identifiable. Given the large number of assumptions presented in this manuscript (from assumptions related to abundance estimates to assumptions about the proportion of fish in the diets of Northern Pikeminnow and assumptions about the proportion of Chinook Salmon comprising those diet contents), I for one, would be interested in understanding what the species composition was (percent by species) for the identifiable stomach contents, and seeing an analysis based on the assumption that the proportion of Chinook Salmon in the identifiable stomach contents is similar to the proportion of Chinook Salmon in the unidentifiable stomach contents.  
  
Line 417: What is meant by “conservative” here? Overestimate number of empty stomachs in the population? Underestimate Chinook Salmon in the diets? Something else?  
  
Lines 456-458: Perhaps I don’t fully understand your manuscript, but I am having difficulty understanding how your diet scenarios are based on conservative assumptions given that those assumptions suggest a greater reliance on salmon than your empirical stomach content data (11.7 percent of prey was fish in general).  
  
Lines 459-470: This would be a good place to provide a discussion (supported with citations) related to compensatory vs additive mortality and other sources of mortality that are not considered in your study (e.g., ocean conditions, etc.). Essentially, the discussion is a good place to put your study in a broader context.  
  
Line 503: Change “refugia” to “refuge”.

Reviewer #2: This is an interesting and generally well-written manuscript that provides a multifaceted analysis of the effect of pikeminnow on Chinook populations in Deadwater Slough in the Upper Salmon. The key finding is that pikeminnow eat a lot of out-migrating Chinook. Based on the reported results, I think that indeed is true. But if I’m reading this correctly, the authors estimate that the magnitude of the consumption approaches 100% of adult escapement equivalents, indicating that if there were no pikeminnow in this reach, SARs would double… which would put the entire Upper Salmon MPG on the path to rapid recovery. That would be amazing, but strains credulity. The magnitude of this estimate hinges on the assumption that pikeminnow eat 60% fish, which is 5 times the authors’ own estimate of 12% fish consumption based on gastric lavage.  
In the discussion the authors explain that gastric lavage may not be effective for pikeminnow, but they never provide any evidence or argument that their estimates of fish consumption might be biased low because of this ineffectiveness. Therefore, it’s really unclear why they adopt an estimate of fish consumption that’s so inconsistent with their own data. Maybe there’s something unstated here? Something the authors assume is obvious to the reader, but I’m missing because of my ignorance? If so, please spell it out because there may be other folks in the same boat as me.  
In short, the key result that pikeminnow are eating an implausibly large amount of Chinook depends on assumptions that are not clearly supported given the empirical results reported. I think taking a more conservative approach, in which the authors use their own data as the basis for their calculations, would actually yield an estimate of piscivory that would be more believable while still clearly conveying that pikeminnow predation in Deadwater Slough is a huge problem for Chinook.  
  
A few detailed comments follow.  
  
64. Thrive goes way beyond tolerate. I would cut “tolerate and”.  
71. I’d end the sentence after “basin” and start a new one with “Considerably…”  
88. I’d cut “Importantly, the”  
197-200. Something is grammatically amiss here.  
208-209. It’s unclear why PSD is calculated and reported. Even after reading the full paper, I’m still not clear. Wouldn’t it be enough to report the mean TL for pikeminnow relative to other locations?  
249-250. This statement is confusing. What’s the point of the lavage study, if not to answer this question?  
261-262. Also confusing. It’s worth explaining here or previously that the gut contents can’t be ID’d to species.  
301. Why? This seems like an arbitrary decision.  
306. The significance of the PSD is still unclear.  
318. Why 60%? In this whole section, it becomes clear that the results are dependent on the assumptions of fish prey diet %, but it’s really unclear why the 12% reported in the previous section have no bearing on the values used here.  
331. This is not a sensitivity analysis in the usual sense of the term.  
455-456. “total DSR and NRR” is referring to the full MPG, or the Lemhi?  
517-518. This says that pikeminnow are feeding on the adult returns, but you mean adult equivalents, right? Consider rewording.

6. PLOS authors have the option to publish the peer review history of their article ([what does this mean?](https://journals.plos.org/plosone/s/editorial-and-peer-review-process#loc-peer-review-history)). If published, this will include your full peer review and any attached files.  
  
  
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Reviewer #1: No

Reviewer #2: No

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