EXTERNAL EMAIL – Use caution with any links or file attachments.

16-Feb-2021

Dear Mr. See:

Your manuscript entitled "State-Space Model to Estimate Salmon Escapement Using Multiple Data Sources", which you submitted to North American Journal of Fisheries Management, has been reviewed. The reviewer comments are included at the bottom of this letter.

The reviews suggest that following revision your paper could be suitable for publication. Therefore, I invite you to respond to the reviewer(s)' comments and revise your manuscript.

To start the revision, please click on the link below:

\*\*\* PLEASE NOTE: This is a two-step process. After clicking on the link, you will be directed to a webpage to confirm. \*\*\*

<https://mc.manuscriptcentral.com/najfm?URL_MASK=14b9c0159e5445aa8542e50854538db1>

This will take you directly to the section of the site where you can submit your revision. Please (1) update the information provided as needed, (2) upload the file(s) containing your revised manuscript, and (3) upload a separate file containing detailed, point-by-point responses to the reviewers’ and editors’ comments. Using the drop-down menu, designate the manuscript files according to type (“Main Document,” and so forth); designate the file containing your responses as “Response to Decision Letter.”

This link will remain active until you have submitted your revised manuscript. If you begin a revision and intend to finish it at a later time, please note that your draft will appear in the “Revised Manuscripts in Draft” queue in your Author Center.

If the link above does not work, please log into your author center at <https://mc.manuscriptcentral.com/najfm> and click on "Manuscripts Awaiting Revision" to submit your revision.

IMPORTANT:  Your original files are available to you when you upload your revised manuscript.  Please delete any redundant files before completing the submission.

Because we are trying to facilitate timely publication of manuscripts submitted to North American Journal of Fisheries Management, your revised manuscript should be uploaded by 18-Mar-2021.  If it is not possible for you to submit your revision by this date, we may have to consider your paper as a new submission.

Wiley Editing Services Available to All Authors

Should you be interested, Wiley Editing Services offers expert help with manuscript, language, and format editing, along with other article preparation services. You can learn more about this service option at www.wileyauthors.com/eeo/preparation. You can also check out Wiley’s collection of free article preparation resources for general guidance about writing and preparing your manuscript at www.wileyauthors.com/eeo/prepresources.

Once again, thank you for submitting your manuscript to North American Journal of Fisheries Management and I look forward to receiving your revision.

Sincerely,

David Geist

Editor, North American Journal of Fisheries Management

[david.geist@pnnl.gov](mailto:david.geist@pnnl.gov)

Reviewer(s)' Comments to Author:

Reviewer: 1

Recommendation: Reconsider following revision based on review team comments

Comments:

Overall, based on what I do know, I think the work is solid, but I do think the authors mis-aimed a bit in the intro and discussion -- much of the content in the discussion would serve well as a motivation for the paper, and the discussion should treat the issue of "how much does this change matter?"  and "What are the implications of applying this novel method?

In addition, the paper does not deal well with the management setting for these species (see below).  While the authors mention that the species are listed as threatened, they do not provide sufficient context (e.g. explanation of the listing of ESUs, average harvest rates (and what type of harvest), or the implications of their application (e.g. see comment below about the bias caused by unclipped hatchery fish leading to greater risk to the threatened species.).

I'd also suggest cleaning up quite a bit of repetition across the paper, removing methods from the introduction, and making sure that more detail about the STADEM package is provided in the main body of the paper before the paper is considered for publication.

Major Comments

1.  Introduction

a. I’d strongly suggest that the authors not refer to Snake River spring/summer Chinook salmon as Snake River Chinook salmon.  As the authors know, there are two well-defined, and very distinct ESUs of Chinook salmon in the Snake River.  I think there is strong potential for confusion – especially for those readers who don’t read line 40 or gloss over it – such as in line 45 where “aggregate Snake River Chinook salmon” could easily mean the spring/summer ESU and the fall ESU.

Good point. There are locations where it could be ambiguous as to whether we’re referring to spring/summer or fall run types. Line 40 was changed to “spring/summer-run Chinook Salmon (hereafter sp/sum Chinook Salmon)” and we then use the sp/sum throughout to clarify where applicable.

b. Lines 51-53.  Would be worth mentioning that wild SRSS Chinook only have tribal ceremonial and subsistence harvest in the discussion of harvest rates being set by escapement at LGR.  These rates don’t vary much.

The final statement of this paragraph has been revised to read “As an example, tribal ceremonial and subsistence harvest rates in the region are informed by escapement at LGR; additionally, the openings, closures and bag limits of fisheries targeting hatchery fish, both upstream in Snake River fisheries and downstream in mainstem Snake and Columbia rivers, can be informed by escapements at LGR.” to differentiate between fisheries targeting wild and hatchery fish. Additionally, we added a reference to US v. Oregon to support the statement.

c. Lines 84-85.  Much of the previous paragraph describes the issue with missed night-time passage.  This introduces it as if it’s a new (undiscussed) phenomenon.

The paragraph starting on line 84 has been reworked to place the focus on fallback and re-ascension as the new, undiscussed process. And acknowledge nighttime passage as previously discussed.

d. Line 104.   Why is it parsed into weekly strata?   And, shouldn’t this level of detail be in the methods?

We have added a sentence about this within the methods, in the first paragraph of the Model Framework section. How strata are constructed is easily modified, but we chose to construct them weekly to ensure sufficient sample sizes of PIT tag detections for those parts of the model.

e. Line 105.   How are you distinguishing unclipped hatchery fish?  Better explain this (again, better in the methods than in the introduction, though).

This is explained in the Adult Fish Trap Data subsection within Methods

2. Methods

a. Line 120-1 – what specific information from adults captured at traps?

This information includes number trapped, number of previously PIT tagged fish trapped, and genetic origin information, at a minimum. These are called out later in the Methods section, where appropriate.

b. Line 215-6.  How did STADEM adjust the counts?

This section of the Methods has been expanded to contain all the details of the model.

c. State-space models are not my forte, but it looks to me like the lead author worked up a model in R, has it posted on Github, and this is the first time it is published.   If this is the case, then more detail about what the STADEM package actually does is definitely needed.  While Appendix A does provide the details, at least a general description is needed in the Methods for this paper.

Good point. We have moved the methods from Appendix A into the main manuscript.

d. Paragraph beginning at Line 232.  I would like to see a clear articulation of the questions being asked with these simulations.

Following your suggestion, we revised the first few lines of this paragraph to be, “Our objective was to assess STADEM model estimates of origin-specific (wild, hatchery, hatchery no-clip) escapement for accuracy and precision, given a known simulated "truth", under different possible conditions. We developed those conditions from the combinations of two trap rate scenarios…”. We also revised the following paragraph to make clear what metrics we were using to assess accuracy and precision.

3. Results

a. Line 264.  Unclear why wild fish correspond to a medium escapement level.

This description is based on the range of wild estimates from Lower Granite (see Table 3) and wild escapement being between hatchery and hatchery no-clip escapement. This was made more clear on line 326 of the new version.

b. Following comment 2d.  Presenting clearly the questions being asked with the simulations allows you to then present the results clearly.  Make sure that the reader understands why you are presenting these results. (e.g. paragraph beginning at 262).

See our response under comment 2d.

c. Line 272-3.  For what purpose?

As a case study to show how the model works for Lower Granite data. The purpose is now mentioned more clearly to meet your 2d suggestion above.

4. Discussion

a. Line 309-324.   Much of this is good motivation for this study and might be better presented in the introduction, in place of the duplicative methodology descriptions.

Following your suggestion, we re-organized this paragraph and other parts of the discussion to remove unnecessary duplicative statements.

b. Line 329-332.  I think the authors need to acknowledge that the SSRS and the SR Steelhead are ESA listed species, and that accepting a bias due to unclipped hatchery fish (which would always lead to a larger estimate of wild escapement) will result in greater risk to this ESU.

Following your suggestion, we have added a sentence so that section now reads, “The inclusion of this genetic information typically results in a reduction in wild escapement estimates and an associated increase in hatchery no-clip escapement, as phenotypically wild fish are identified as the progeny of hatchery broodstock. Accepting a potential bias due to unclipped hatchery fish can create a greater risk during status assessments of these ESA listed wild populations. However, if in-season management decisions…”

c. I am convinced that the STADEM approach is unbiased and it appears to provide better estimates.  Can the authors speak to how much difference its application would make in management?

We have expanded the discussion to address this and included a column in Table 3 demonstrating the estimated bias of window counts alone.

5. Figures.

a. I’m not finding Figure 1 to be terribly intuitive.  The verbal/word explanation is actually clearer than the figure.

We took this opportunity to change Figure 1 to be more graphical. It is now a directed acyclic graph, illustrating the data sources, the estimated parameters and the results.

b. Figure 4 – would be helpful to indicate in the caption what the reader is supposed to get out of this figure.

We improved the caption to include the meaning of points above and below the 1-1 line. Additionally, we reference the figure multiple times in the discussion for a more detailed explanation.

More minor Comments

1.  Introduction

a. Line 24:  Escapement doesn’t “often” mean this.  It does mean this for salmon.

Passage clarified to read as “Fish escapement refers to the number of adults that survive juvenile and subadult rearing, escape harvest, and achieve a size and age to potentially spawn. For anadromous fishes, escapement is often estimated at a fixed location in a river system prior to fish reaching their natal habitat to spawn.”

b. Line 49:   perhaps “parsed by species, ESU and origin”.  Also this whole sentence is confusing.  It kind of sounds like the parsing happens at the dam, when rather the count is of all SRSS Chinook (or steelhead), later divided out by math, with the help of genetic sampling and so forth.

We have revised this sentence to read, “Many fisheries management and conservation actions are made based on estimates of escapement by species and origin at Lower Granite Dam.”

c. Lines 47-60.   Fair bit of repetition at the end of the first paragraph and the beginning of the second.  Suggest thinking carefully about what you want the main point of each paragraph to be and eliminating the repetition.

We hope that in re-organizing the Introduction and reducing repetitiveness throughout the manuscript has resolved this issue.

2. Methods

a. Line 155.  How about “identified to species” instead of “speciated”, since the latter is used in evolutionary biology to refer to the origin of a new species.

Done.

b. Line 178.  Unclear what “trapping rate” refers to.

Changed to “a trap rate for the adult fish trap”

c. Line 197.  Awkward, unclear wording.  “…of tags passing the fish ladder for both.”

Our intent was to convey that both were estimated using PIT tag observation data and on a weekly basis. Changed to “We used PIT tag observation data from the adult fish ladder to estimate both nighttime passage and re-ascension rates; each estimated on a weekly basis.”

3. Discussion

a. Line 300 – awkward wording

This statement was moved to be the topic sentence to the final paragraph of the Discussion. We hope that placing it in the new context with justification for implementing STADEM elsewhere helps clarify.

4. Appendix B

a. First sentence – wording makes it unclear whether you developed it or the R Core Team developed it. Suggest active voice. Thanks. Changed to active voice.

Additional Questions:

The discussion provides adequate explanation and interpretation of the findings, and integrates the results with the broader literature on the topic.: Disagree

The statistical analyses are appropriate (this question focuses not on what might have been done, but whether what was done is appropriate).: Agree

The methods are appropriate to address study objectives, and sufficient methodological detail is provided.: Disagree

The introduction develops a logical and justifiable basis for the work and includes appropriate context.: Agree

The science described in this manuscript is: Somewhat important to fisheries managers; useful science but in a specific context (e.g, species, location, method)

Reviewer: 2

Recommendation: Reconsider following revision based on review team comments

Comments:

Overall this paper is well-written and presents the findings in a clear and concise manner. The statistical analysis is novel and should benefit managers striving to understand steelhead and Chinook population dynamics in the Snake and Columbia Rivers. I have only a few minor revision suggestions, detailed below.

Minor comments:

Line 46: Readers unfamiliar with the Snake River are unlikely to know where the Tucannon River is, or its significance. A short clause or sentence stating that the Tucannon is a tributary to the Snake River whose junction is downstream of Lower Granite Dam would clarify this.

Thank you. Added a clause in parentheses “(a tributary of the Snake River whose confluence is downstream of Lower Granite Dam)”.

Line 181, 255, Appendix B, possibly elsewhere: Check for consistency throughout the manuscript using "data" as plural.

Thank you. Changed data as plural throughout the manuscript, including lines 181 and 255. However, I couldn’t find an occasion in Appendix B where we referred to data as singular.

Line 266: It appears from Table 2 that only one of the 12 simulation scenarios had an average relative bias less than zero. Estimated bias was indeed very small for each scenario, however shouldn't even a small relative bias fall around both sides of the true parameter? I don't think this is a huge deal, but perhaps a sentence or two in the Discussion pertaining to potential reasons for this anomaly would be helpful.

The relative bias presented in Table 2 is the mean relative bias across all 500 simulations for a given scenario. We added a clause to reference Figure 2 to make clear that the relative bias is sometimes positive, sometimes negative: “Estimates of wild escapement were unbiased, with an average relative bias of 0.2–0.3%, although the bias appeared equally distributed between positive and negative values across the simulations (Figure 2).” We also added a couple sentences in the discussion to explain this.

Lines 285-292: Consider moving this to the Discussion. Your findings of higher nighttime passage than re-ascension for steelhead, but the opposite for Chinook Salmon, are important and deserve more discussion. Citing publications that back up your conclusions would strengthen the argument that the STADEM model is appropriately correcting the biased counts of both of these species.

We have revised the discussion section to focus on these results a bit more, point out the level of bias in window counts that these results point to, and how STADEM can correct that bias.

Additional Questions:

The discussion provides adequate explanation and interpretation of the findings, and integrates the results with the broader literature on the topic.: Agree

The statistical analyses are appropriate (this question focuses not on what might have been done, but whether what was done is appropriate).: Strongly agree

The methods are appropriate to address study objectives, and sufficient methodological detail is provided.: Agree

The introduction develops a logical and justifiable basis for the work and includes appropriate context.: Agree

The science described in this manuscript is: Somewhat important to fisheries managers; useful science but in a specific context (e.g, species, location, method)

Reviewer: 3

Recommendation: Reconsider following revision based on review team comments

Comments:

Thank you for allowing me to review the manuscript entitled “State-Space Model to Estimate Salmon Escapement Using Multiple Data Sources”. The manuscript was well-written and, although I feel like it may have limited application in practice, I would still consider it a useful addition to the literature. I have a few major comments that I think should be considered and a few line-by-line comments.

I appreciate the author’s attempt to streamline the manuscript by putting the model equations in the appendix, but because this manuscript almost entirely focuses on the novel model, I feel like it needs to be included in the main text. I think somebody that is going to read this paper is primarily doing so to learn about the model and making them look to the appendix is potentially a bit cumbersome.

Good point. We have moved the methods from Appendix A into the main manuscript.

The authors state on line 243 that they “…calculated the root mean squared error (RMSE) as the square root of the squared bias in the estimate.” This is not actually the RMSE. The root mean squared error is the square root of the bias squared plus the variance. For instance, if an estimate is unbiased, calculating RMSE as described in the paper would result in a RMSE estimate of zero. But there is still error in the estimates simply because of variance. In the unbiased case the RMSE reduces to the square root of the variance. I think that to truly evaluate the estimator the RMSE needs to be calculated correctly rather than just providing the square root of the squared bias (i.e., bias; see next comment).

We expanded the definition of RMSE to now say “We calculated the root mean squared error (RMSE) as the square root of the mean of the squared bias in the estimate, which is equivalent to the square root of the variance of the estimator plus the square of the expected bias.”

I think the manuscript could benefit by calculating the RMSE in the simulations for when just the window count is used. The author’s suggest that the window counts are biased and do not provide estimates of uncertainty, thus providing justification for their model. When I look at Figure 3 the “estimates” from the window count are pretty close to the estimates from the STADEM model, but the window counts truly do not have variance, unlike the STADEM estimates. Thus, the estimates from the window counts may actually have a smaller root mean squared error (when calculated correctly) than the STADEM estimates. Meaning that although they may be biased, they may be on average closer to the true population parameter than the STADEM estimates which requires a bunch of parameters to be estimated all of which contribute to the variability of the estimate. My suspicion based on Figure 3 is that the RMSE is smaller for the window counts based on the size of the credible intervals for the STADEM estimates relative to the “bias” (i.e., the difference between STADEM estimates and window counts). Obviously we do not know the truth in practice but this could easily be evaluated in the simulations. I appreciate the point the authors make about the uncertainty, but it is possible that managers and policy makers may prioritize having an estimate that is closer to the truth on average, with a little bit of bias, than having an estimate of variance for an estimator that may be further from the truth on average (if the RMSE is in fact smaller for the window count). Providing information on the bias-variance tradeoff for this model and the window counts would be useful when considering such a decision.

Good point. We have expanded Figure 2 to include a comparison of STADEM estimates of unique fish with window counts. The spread of the boxplots provides an indication of the variance of each estimator, and the bias should be clear as well. We’ve added some text pointing out that the variance of the window counts grows as the simulated observer error grows. Indeed, when nighttime passage and re-ascension rates are equal (so window counts are unbiased), the window counts do have a smaller RMSE compared to STADEM. However, when those two rates are not equal (scenarios labeled N-R), the bias in the window counts leads to a higher RMSE compared to STADEM estimates. One point we tried to make is that the RMSE is quite consistent for STADEM across all scenarios (while remaining unbiased), which we imagine would benefit managers and policy makers, especially when considering a time-series of estimates. We also added to the discussion a paragraph describing the implications of using window counts vs. STADEM estimates, as well as a column to Table 3 showing the bias of window counts in each year. Ultimately, managers and policy makers may decide that the estimated level of bias is acceptable, but we point out in the discussion that one justification to developing STADEM was to provide an alternative.

Line-by-line comments.

Lines 25-27: could use a citation or two for this sentence.

We have added two citations as examples.

Line 39: I don’t believe that all stocks of Chinook Salmon are listed under the ESA in the Snake River (e.g., spring-summer Clearwater stocks).

Good point. Changed to “***most*** Snake River spring/summer-run Chinook Salmon were classified as threatened…”

Paragraph starting on line 93: This is mostly methods, except for line 113 which is conclusions, and should be moved to the methods and discussion.

We revised this paragraph to more clearly state our study objectives and to include your suggestions and reviewer 1’s suggestions in comment 2d. We hope our revisions are helpful.

Line 94: The author’s state that they incorporated all known sources of uncertainty, but I don’t see how they incorporated the uncertainty in the expansion for the 50 minutes out of the hour counting (lines 135-136). I acknowledge that I may just not be seeing it in the model.

We did not incorporate that type of uncertainty, it’s true. Hatch (1994) found no difference between expansions of 50 minute observer counts and “full” counts made with 60 minutes of video. We added a clause to that sentence so it now reads, “Counts were provided for each day the fish ladder was open to passage, ***and had already been expanded by 1.2***.”

Line 179: should be “included” not include (past tense).

Fixed.

Lines 243-245. See general comments about RMSE.

See our response above.

Lines 289: need to define “did not match”.

We performed a 2-sample test of equal proportions for each week with sufficient PIT tags to perform this test, and have now reported those results. We revised some of the other wording in this paragraph to make it clearer.

Lines 306: I don’t disagree with this statement, but are these data currently being used in PVAs with the uncertainty at this scale? If so a citation or two would be helpful.

Added a citation to Paulsen (2007).

Lines 311-312: Basically the same comment. Are management decision risks being used at this scale?

We added a couple of citations to better support our statements.

Lines 360-362: Again, I don’t disagree with this statement, but there isn’t really anything in this manuscript to support this as a conclusion. I’m not sure that accounting for the apparently small bias from using window counts (Figure 3), or incorporating the small amount of variance in the STADEM model would actually change any policy or management decisions in practice. To make this statement I think you would need to simulate the population, estimation, and management decision processes and evaluate how they may change by using one method or the other, which wasn’t done in this manuscript.

We added the estimated bias contained in window counts to Table 3 to help readers judge the importance and potential difference in STADEM results as compared to current window count results. We believe removing bias, and understanding the true uncertainty contained within an estimate is valuable in the decision-making process. However, managers select different estimation methods on their ability to meet many criteria, and our aim was only to present an alternative for consideration that accounted for known sources of variation and bias.

Line 518: Need to define е and σ (the error terms), here and throughout.

Thank you, this was overlooked and we have now added definitions where needed.

Line 523: I think it would be helpful to move the equations up before defining the terms in the text.

We have rearranged where the equations appear to be more helpful.

Line 538 equations. I think it would be helpful to move the Y distribution up to the first line because that is the first term you describe in the text.

Done.

Line 543-550: How is it favoring this? Are you manually weighting the datasets? A little more information here would be helpful here.

We have removed this confusing language, but we were attempting to illustrate that a state-space model will tend to put more emphasis on a data stream with less observation error, without requiring any manual weighting.

Additional Questions:

The discussion provides adequate explanation and interpretation of the findings, and integrates the results with the broader literature on the topic.: Agree

The statistical analyses are appropriate (this question focuses not on what might have been done, but whether what was done is appropriate).: Disagree

The methods are appropriate to address study objectives, and sufficient methodological detail is provided.: Agree

The introduction develops a logical and justifiable basis for the work and includes appropriate context.: Agree

The science described in this manuscript is: Somewhat important to fisheries managers; useful science but in a specific context (e.g, species, location, method)

Associate Editor

The article UJFM-2020-0221 “State-Space Model to Estimate Salmon Escapement Using Multiple Data Sources” described a new model to calculate fish escapement rates with the purpose of aiding fisheries resource managers. Three people conducted reviews on this manuscript, who are knowledgeable in fisheries ecology and modeling. A summary of the reviewers’ and my concerns are described below.

1. Reviewer 1 had concerns with the motivation for the paper in the introduction and discussion, indicating more of a “so what?” could be emphasized to describe the importance and usefulness of the new model. Additionally, the reviewer believed further details to the management of the species should be included and the repetition of the STADEM package could be reduced. I agreed with each of these concerns.

We believe our revised manuscript is better organized with less of a repetitive nature, and now accounts for most of the reviewer comments and suggestions.

2. Reviewer 2 had only minor concerns with the paper. The reviewer’s main suggestions were to add a few details in the intro and discussion, and to move some text to the discussion.

We added details to the introduction and discussion sections, and the original Appendix A was moved into the main body for better tractability.

3. Reviewer 3 provided several high-level comments and questions about the model that will be important to address to aid with clarity of the model calculations and uncertainties. This reviewer also suggested moving the equations to the main body of the text instead of an appendix, with which I agreed.

We provided a response to reviewer 3’s concerns regarding the calculation of RMSE and added a clarifying statement to the manuscript to alleviate future reader concerns. Again, we agreed that model details needed to be moved to the main body of the manuscript, as such, the original Appendix A was removed and the details were added to the methods section.

4. Associate Editor. In the methods (e.g., L. 158) please clarify how hatchery fish were able to be identified as hatchery. It appears to be mentioned in the discussion (L. 326) but should be clarified in the methods. Where appropriate in the tables and figures, please define the acronyms (i.e., N-R, Err L, Err H).

The methods of how unclipped hatchery fish were identified as hatchery is described in the Adult Fish Trap Data section in Methods. We tried to use Table 1 to define what all those scenarios refer to, so as not to need to state their definitions in great detail multiple times. However, we have added some references to specific scenarios in the text (e.g. scenarios when nighttime passage does not equal re-ascension rates (N-R)).