

APPENDIX III- ANONYMOUS PEER-REVIEW COMMENTS ON "A SYNTHESIS OF THE COAST-WIDE DECLINE IN SURVIVAL OF WEST COAST CHINOOK SALMON"

Responses to 5 Reviewers and editor's comments from two rounds of reviews at Fish and Fisheries. (Our detailed responses describing how we modified the manuscript are also included).

From: Paul Hart <onbehalf@manuscriptcentral.com>
Sent: Monday, May 11, 2020 6:43 AM
To: David Welch <David.Welch@Kintama.com>
Subject: Fish and Fisheries - Decision on Manuscript ID FaF-20-Mar-OA-084 [email ref: DL-SW-4-a]

11-May-2020

Dear Dr. Welch

I write to you regarding manuscript # FaF-20-Mar-OA-084 entitled "Review of the Coast-wide Decline in Survival of West Coast Chinook Salmon (*Oncorhynchus tshawytscha*)" which you submitted to Fish and Fisheries.

Editor Comments to Author:

The three reviewers give a mixed assessment of your paper with one having only minor suggestions for change but the other two recommending major revision and resubmission. I have gone for the latter because it would be good to have expert opinion as to how you have coped with the reviewer comments. As a resubmission I will send your new version out to be reviewed again.

Although it has not caused me any trouble I would like to point out that both the websites you mention in your Cover Letter that include names of people you didn't want as reviewers, were closed to me. I was denied access to both sites.

Response: I must apologize for the trouble. It seems that these websites have been revamped and are now locked down—I no longer had access either.

If you choose to send the revised manuscript out for review once again, we can provide the list of people we would like to avoid as reviewers, but so long as the reviewers you select are from outside the Columbia River basin region we have no concerns. We are also happy with any of the reviewers we suggested from the original submission that were members of the ISAB/ISRP Columbia River review groups as they are charged with scientific oversight in the Columbia River basin but do not conduct primary research there, so we feel they can provide appropriate perspective and balance on some of our findings.

Reviewers' Comments to Author:

Reviewer: 1

Comments to the Author

This paper combines coastwide data on chinook ocean survival to present a comprehensive story of the historical changes that have been seen, and the result that will surprise many, that survival of Snake River fish is better or equivalent to most west coast chinook stocks.

The paper is clearly written and illustrations are well done and appropriate.

The authors have avoided discussing any causes of the decline in ocean survival which is probably a good idea as it is a totally different paper, but I think it would be worth mentioning the range of explanations that have been put forward.

Response: We added one paragraph in the Discussion, summarizing the potential causes of poor marine survival and listing the authors that identified these factors over the past decade. However, we do not want to get drawn into a debate about why survival is dropping in the ocean, because no one really knows—there is much speculation and some correlation-based analyses, but nothing definitive.

At this point we want to keep the primary focus on the fact that survival (SARs) has fallen everywhere and that this has not been recognized by the fisheries management and research communities, despite the fact that they have been generating more and more of these data sets over the past half-century. This is new information and we feel the community should focus on these results.

My only detailed comment is that lines 100-102 should mention the good performance of Alaskan sockeye fisheries, which are the most valuable salmon fisheries on the west coast

Response: We emphasized the major decline in southern populations of commercially important salmon species in our original manuscript because we want to focus on the broader issues and not get drawn into some of the exceptions. As we mention in the Introduction, the lower-valued species of Pacific salmon are doing well. (And, apart from crediting "climate change/global warming" right now I don't think anyone has any actual idea why Bristol Bay sockeye in the Bering Sea are doing so well).

Reviewer: 2

Comments to the Author
Review of Faf-20-Mar-OA-084

Welch et al. examine broad scale patterns in Chinook salmon survival (smolt-to-adult return rates: SARs) by collating data from multiple sources and regions along the Pacific coast of North America. The analysis synthesizes data from most regions where Chinook are monitored (excluding populations further west of SE Alaska), parses data into relevant life history strategies (sub-yearling vs. yearling) and hatchery vs. wild stocks (data more rare for these), and is transparent with sources of data.

With some exceptions, authors found similar and relatively poor SARs based on CWTs for hatchery stocks (both sub-yearling and yearlings) across regions, including those with relatively pristine freshwater conditions. For regions where the time-series of SARs combined across populations were long enough (extended back to 1970's), there was evidence for a synchronous 3-4 fold decline in SAR to approximately the same contemporary level (~1%).

Next, median regional SARs from 2010-2014 were referenced to the Snake River within the Columbia River basin since survival there is generally considered poor (although data suggest currently on the rise?) and SAR recovery targets are in place (2-6%). In general, standardized SARs were not statistically

distinguishable from the Snake River (typical case for yearlings) or were lower than the Snake River (typical case for subyearlings).

Patterns in PIT-based estimates of SAR within the Columbia River basin generally aligned with CWT-based estimates, but were not directly transferable, and relationships between the two were population-specific and different between yearlings and subyearlings indicating that a general conversion was not possible. Authors point out that unlike CWT-based estimates, PIT-based estimates may not adequately account for harvest or other components of migration sequence which likely contribute to the lack of transferability.

Primary conclusions included (1) given similarity in regional SARs and seemingly congruent decline in SARs to similar contemporary levels, the notion that survival could be driven more by broader oceanic factors rather than local freshwater factors cannot be dismissed (I feel like relaxing language to something along these lines would help reduce knee jerk reactions and help stimulate conversation and advancement of knowledge)—in other words, actions (presumably small scale??) that try to alleviate ecological bottlenecks during freshwater life may not compensate for ocean conditions as they are generally perceived (or hoped) to do (Snake River dams are highlighted as key example), (2) more careful consideration of the role of harvest and migratory life-history in influencing SAR estimates (particularly PIT-based estimates) is needed to reduce potential bias and increase clarity in patterns of survival for conservation efforts, and (3) more rigorous technical standards are needed for measuring SARs.

Overall, I thought the paper was well written and that the authors effectively distill a large amount of information and present the key elements and patterns. I also do not find myself disagreeing with their conclusions. Given the scale of this assessment, perceptions that it challenges, and new actions that it calls for with the data to support, I feel it is suitable for Fish and Fisheries. However, I do have some suggestions that may help improve the manuscript and that authors and editors should consider:

First, the Abstract seems a little vague to me and primary conclusion (1) above is never explicitly stated and probably should.

Response: We extensively revised the abstract to better clarify the summary points that Reviewer #2 have outlined, and in fact incorporated Reviewer #2's phrasing: "given similarity in regional SARs and seemingly congruent decline in SARs to similar contemporary levels, the notion that survival could be driven more by broader oceanic factors rather than local freshwater factors cannot be dismissed" (slightly modified to limit the word count).

Next, I feel like authors could provide a bit better road map for readers by expanding the Introduction and better linking to the Discussion. Essentially including an additional paragraph at the end of the Introduction that makes it clear what is coming down the pipe. As written, all the reader knows is that authors are examining broad scale patterns in SAR, but don't really know what that entails and how it helps us evaluate the importance of ocean conditions vs. freshwater conditions (main conclusion is related to this)—something we don't know much about but is current focus of Introduction.

Response: This is an excellent point. We added a paragraph to the end of the Introduction to outline the methods, explain how our analyses provide evidence of the relative importance of oceanic versus freshwater conditions,

and to clarify the segue from the SARs compilation to the evaluation of the importance of harvest .

Another example is that the transition to PIT-based estimates of survival in the Columbia are never mentioned in the Introduction, but end up seemingly dominating the Results and Discussion. There needs to be a stronger link between what was explicitly examined in the analysis and how different Results regarding SAR methods/patterns would lead to one profound conclusion vs. another to better prime readers for why this assessment is needed and what it all means. If the Introduction could be modified to be a bit more hypothesis driven, maybe that would help?

Response: We have added this material into the Introduction, in order to frame the issues better.

Similarly, the Discussion reads much like repeated Results and most every Figure was cited again. I'm wondering if authors could rework some material that is currently in the Discussion and incorporate into the Results section. I happen to like Results sections that include more context/consistently remind readers as to the how and why, and feel that would be appropriate for a paper like this. The Discussion could then dive a bit deeper into the different profound conclusions. For example, what might be the "broader factors" driving survival? Do we need to ramp up monitoring of wild stocks? Maybe the real answers could be found in those stocks?

Response: We felt the need in the Discussion to reference back to the specific figures as we develop our observations and conclusions—there is a great deal of data boiled down into these figures and we feared losing the readers if they were unsure which data sets/analyses we were gauging our specific statements on.

We added a paragraph providing a literature review of the past decade of Chinook salmon studies (2010 to present) examining aspects of the survival conundrum, categorizing the papers by the mechanisms they propose. However, we don't want to go any further—all of these papers are either conjectural or, at best, correlational. Bluntly put, we don't want to stray into that particular morass when we don't have real data to allow us to discriminate what is going on...we feel it is important to keep the focus on the key points we have identified, not speculate on what is causing poor marine survival when we have no data to contribute.

(And as for the reviewer's point that maybe the "real answers" could be found by ramping up monitoring of wild stocks, this senior author is deeply cynical... he has watched the community start more and more monitoring programs without ever really looking at their data and asking why the survival estimates were so similar everywhere? It should not have taken my group, divorced from the monitoring programs, to point this out... But it did. Starting more wild salmon monitoring programs will simply be a displacement activity that absolves major governmental organizations from responsibility for going out to sea and finding out what is really causing poor survival. But that is for a different paper).

The other major comment I have pertains to the selection of data. Authors mention excluding a large amount of data in this analysis in their conclusions. Given the call for a broad evaluation of SAR methods and development of more standardized methods for estimation, I feel presenting the rejected data is just as important as presenting the accepted data. The criteria for each is not very clear to me. Do the rejected data tell a different story, meaning that more digging and Discussion with folks on the

ground is needed? Do the accepted data represent and true random sample? Or is there some systematic bias involved? Maybe including rejected data on the map and in supplemental material would help improve transparency and allow us to gauge how representative selected data are?

Response: Including rejected data on the map is simply infeasible. We found in our efforts to find CWT-based SAR data for the Columbia River basin specifically that there are a number of regional databases, but in a number of cases the "SARs" that were calculated in those databases only included some components of adult returns (for example, only hatchery rack returns (the adults used to start the next generation of fish in the hatchery)). In a number of cases we variously found that reported SARs did not include hatchery-origin adults spawning on the spawning grounds, counts were not expanded for sampling portions, data were coded as experimental or above dam numbers were not counted. In short, there seem to be very lax standards as to what was being collected & reported. However, we don't want to get into a battle over this—the major focus of the paper is on much more important scientific issues—so we have called for a major inter-agency review of the issues without getting too critical of specific groups. That review will presumably also want to expand our current analysis and make sure that other datasets of acceptable standards are consistent with our own findings. However, we have already done a huge amount of unanticipated work here assessing the quality of the data, and we simply need to publish now and move to the next stage.

Lastly, the standardization procedure authors used could be sensitive to the time-frame examined and I would like to see more rationale for the selection of 2010-2014. Regime shifts are mentioned, with the last one stated to occur in 1998. So, why not consider all data from that point, particularly if the major conclusion from this analysis is that broad scale drivers are more important than local drivers? Doesn't quite line up logically to me.

Response: We have modified the text to provide more rationale for the selection of the 2010-2014 period. We looked closely at Reviewer #2's suggestion but found that extending back over the entire post 1998 period included calendar years where some populations were just not available. We wanted to choose a time frame including the same populations and relatively constant environmental conditions. Unless the populations contributing to the analysis are stable over time, the resampling procedure can potentially include populations which might have different productivity characteristics, distorting the analysis in subtle ways. In addition, 2008 was recognized as a year of major global environmental change (see Arguez et al. (2020)) that was atypical and may have been a regime shift on the west coast of North America (we are still debating that as a community, because there are pros and cons). After 2014 the delays in getting data into the various government information systems meant that few populations were available—so we chose 2010-2014, a 5-year period. This rationale (and the reference to Arguez et al) is now presented.

LINE-BY-LINE COMMENTS:

Lines 360-361: Remind readers that time-series were then combined regionally to better set the stage for the following sections. **Response:** We added the sentence "These populations were then aggregated by geographic area to compare regional SARs."

Lines 376-380: I would provide this general reminder at the start of the Results section. **Response: Moved.**

Lines 382-383: Two paragraphs above, the mid-Columbia is listed as showing declines in SAR?

Response: We think it is clear from the text and Figure 2 that the major 4-fold decline is referencing those time series that extend back prior to 1978 but that there was some modest increase in numbers since the early 80s & 90s. The two paragraphs up statement said (in part): "Most regions of west coast North America with CWT time series extending back prior to the 1978 regime shift show an approximate four-fold decrease in SARs for hatchery populations (Fig. 2). This applies to subyearling Chinook from ... and the mid-Columbia River... average CWT-based SARs for all regions are now approximately 1% or less". Lines 382-383 that the reviewer is questioning says "...Chinook from all regions of the Columbia show some increase in CWT-based SARs since the 1980s and early 1990s".

Lines 384-385: At least not yet. And, it was mentioned in Methods that SARs from Raymond are probably inflated when compared to those estimated more recently. Why doesn't that matter here? I think more explanation would be useful to improve transparency.

Response: We harbour some concerns that the original downstream smolt abundance estimates from Raymond's time may be distorted because of the technology of the day (possibly high doses of the anesthetic MS-222 and use of freeze branding to mark the fish, which could reduce smolt survival and thus understate the resulting SAR estimates). We had debated including this comment in the manuscript but decided against it because there appears to be no way to quantify these impacts (the original data sets that Raymond generated are apparently lost). In addition, the Raymond SARs data (even if underestimates), are the official recovery targets so, if anything, understate the true level of decline in survival.

We don't want to have this issue become a distraction from the main messages of the paper—and, in any event, we can't quantify our concerns—so would prefer to not get drawn into an unproductive side debate that cannot be resolved.

The other aspect which Reviewer #2 may actually be commenting on is from Line 252 of the original manuscript "... (Raymond, 1988). These SAR estimates are inflated relative to the CWT-based estimates". However, we think that the manuscript actually makes clear that the PIT tag-based SARs are generated in essentially the same way as the Raymond estimates were (i.e., from dam to dam, not spawning ground to spawning ground). As we go to considerable lengths later in the manuscript to describe our attempts to achieve a consistent conversion factor between PIT & CWT-based SAR estimates (and fail for the reasons described), getting further into this rather murky issue here again seems unlikely to be productive.

Lines 397-404: I assume we should still be looking at Figure 3 here? Might be worth also highlighting any populations that fall well below where others cluster regionally if applicable. Also, on the log scale, some of the differences among medians observed could be quite large. Seems small changes in survival could result in much larger or much fewer adults returning. So, instead of saying that they simply cluster, give us more information by saying that they are variable but fall with XX-XX orders of magnitude of each other etc... Then highlight clear outliers.

Response: The reviewer raises a good point about several populations with unusually low SARs that fall well below the regional clusters. We originally opted not to belabor this point in the manuscript simply because the management response on the west coast of North America is all about trying to recover the SARs that were achieved 4-5 decades ago. The few populations we could identify that do achieve these recovery targets point to where to look if future research is to focus on this question. However, we did add a short sentence to the Discussion section pointing out that a number of populations with clearly lower SARs than typical for their geographic region could also be worthy targets for investigation: *"Similarly, a few populations with anomalously low SARs relative to regional medians also are evident (Fig. 3). If the underlying reasons for higher or lower survival can be identified it might be possible to improve hatchery productivity more broadly"*.

Line 406: Provide new section heading related to normalization and regional comparisons prior to this paragraph.

Response: Added the subheading "Comparison between regions".

Line 408: Not clear to me what is meant by "Interannual timing" here.

Response: Rephrased the paragraph to read: *"To compare the current status of regional CWT-based SARs we included the five most recent years of consistently available SAR data (2010-2014) in a resampling procedure to statistically quantify relative SARs. We chose this time period because there were a consistent number of populations contributing to each regional grouping used in the comparison period and it avoided including 2008, a year of unusually cold conditions) Arguez et al. (2020)"*.

Lines 408-409: Briefly remind us why Snake River was chosen as baseline.

Response: Done.

Line 469-470: What are the profound conclusions here? Seems these should be outlined more explicitly in the first paragraph of Discussion to provide a better road map.

Response: We extensively re-wrote both the Discussion and Conclusions to better outline the major findings and the conclusions that stem from them.

Line 470-471: Rephrase to "How comparable are estimates of SAR's among agencies...?"

Response: Done.

Line 475: Shouldn't Figure 2 be referenced here? Overall, the extensive re-referencing of figures (aside from Figure 8) in the Discussion is odd and I don't think should be needed.

Response: The reviewer is correct, the reference should be to Fig. 2, not Fig. 1 (now corrected). We included the extensive referencing to figures to ensure that the readers do not get lost in what aspects of the analysis we were referring to. We can remove/reduce this if it doesn't fit with journal policy, but are inclined to keep the referencing to the figures as it currently is to minimize confusion.

Line 479: Be more explicit about what these "broad drivers" could be? What kind of spatial or temporal scale are we talking here? Rather than relying on citing other papers, give us the key elements in a review like this.

Response: Unfortunately, apart from likely occurring in the ocean, these "broad drivers" remain opaque. One objective for publishing this paper is to try to get governments to start taking the marine issues more seriously, rather than repeatedly falling back on doing more work on freshwater habitat issues (the current default). Straying into conjecture here about why marine

survival is so poor might be interesting but may shift the needed debate from whether our paper is correct in our current findings to squabbles over a side-issue—whether we have correctly identified the drivers of poor marine survival.

We did adopt a compromise here for the Discussion. We added one paragraph listing the possible mechanisms of poor Chinook marine survival, citing all of the papers we are aware of that touch on each proposed mechanism—a one paragraph mini-review of the literature, if you will. However, it is important to recognize that the cited papers are ALL either conjectural or correlational. There are no scientific papers that are actually testing mechanisms, which is what is really needed to move the field forward. (We are saving that topic for an entirely different paper!).

Line 482: Relative “shortness”? I’d rephrase.

Response: We delated “relative”.

Line 684: Consider different word choice than “frustrated”.

Response: Changed to “ineffective or misleading”.

Line 703: I believe University of WA hatchery ended their Chinook program.

Response: Yes, but this is not relevant to our paper.

Figure 2: I don’t see where hatchery vs. wild vs. mixed is being shown as indicated in the caption. I’m assuming that the individual data points on the different panels are coded with the different letters? Also, three regimes shifts are highlighted (1977, 1989, 1998) – what do these signify? Initial decline, low point, and some rebound, respectively? Don’t recall these being defined in main body of text, but would be nice to describe these more explicitly to help with interpretation of results.

Response: The reviewer needs to zoom in to the individual panels under high magnification to see this—the data points are shown as H, W, & B (=both). We have re-phrased “*The major regime shifts of 1977, 1989, and 1998 are indicated by vertical dotted lines*” to read “*The timing of the major regime shifts starting in 1977, 1989, and 1998 are indicated by vertical dotted lines*”.

Reviewer: 3

Comments to the Author

This manuscript has a lot of potential but it is extremely rough. The paper compiles existing tagging data needed to estimate changes in smolt survival to adulthood for Chinook salmon along the North American west coast. This topic will be interesting to lots of people but the manuscript is very poorly written. There is little coherence in the manuscript and the results are presented as a long list of specific examples, all of which seem to be exceptions to the general trend. This is only interesting to people who are intimately knowledgeable about these data. More effort is needed to improve the writing and provide a more accessible message with the data.

Response: This was a complex paper to write because it condenses a lot of data, has several related but disparate messages, and touches on controversial topics. Given that the first two reviewers thought that the paper was well-written and that Reviewer #3 hasn’t provided much detail as to what might be improved, we would welcome the editor’s input. We do note that we have extensively re-worked parts of the paper to respond to the specific comments from Reviewer #2 and we think that the new discussion and conclusions provide a readily understood summary of the major findings (and the exceptions, where they occur).

The core message that SAR has declined in most places along the west coast, in both heavily impacted and intact watersheds, across a broad swath of latitude, is an extremely important message. The current paper highlights this in the Abstract, but the paper then buries the result in a long list of exceptions to the rule.

Response: Although we have re-worked the discussion to better highlight the findings, we do wish to disagree with the reviewer here. If our results are to have credibility, we also need to demonstrate that we have carefully analyzed the data in a number of ways to see how robust this conclusion is. The delayed mortality theory (that passage through many dams subsequently reduces survival at sea) is a prominent theory that is very persistent. Despite making this aspect of the paper rather Columbia River-centric, we have taken the opportunity to review this theory in depth because the proponents missed the datasets (including their own!!) that did not support this theory. We are setting that shortcoming to rest.

The paper needs to be rewritten in a more streamlined and coherent fashion. It is currently a hodgepodge of results and observations and is difficult to follow. Those very familiar with the data might have a better time with it but this reads like a management technical report more than it does a coherent paper for the peer-reviewed literature.

Response: The Discussion & Conclusions have been extensively re-worked, and we think it now reads quite well. Suggestions from the editor would be most welcome, of course.

Technically, the paper seems to be reasonable, though I would have liked to see more effort put into statistically describing how the trends in SAR are shared among stocks and locations. The Dorner et al. paper shows one example of how to do this. A chronological clustering approach might as well. Again, statistically quantifying this shared trend would improve the quantitative nature of this paper.

Response: We considered a "chronological clustering approach" early on, but the reality is that the starting time of various populations' survival time series varies. Aggregating the data in the way that Reviewer #3 is suggesting risks introducing artifacts into the analysis where time series of regional SARs suddenly jump up or down as data for additional populations with unusually high or low productivity becomes available. It was for this reason that we chose the statistical approaches that we did. We would also like to point out that wherever possible we show the statistical confidence bounds on the data sets (Figs. 2, 4, 7) or use box & whisker plots (Figs. 3 & 5) to quantify the uncertainty for the readers.

Finally, it is important to point out that our statistical comparison of survival relative to the key Snake River stocks (variously listed as endangered or threatened under the US ESA) used the most rigorous modern statistical re-sampling methods we can identify... they are free from requiring us to make assumptions about the form of the underlying statistical distribution and let the data speak for themselves in the most relevant time frame—the most recent (Fig. 4), and in the supplementary information (Fig. S1) we have shown the results using all possible regions as the basis for comparison (to address the potential question about whether there is something unusual about the Snake River region—there is not).

One relevant paper that is worth looking at that provides an earlier analysis of recent changes in Chinook productivity is:

Ohlberger et al. Ecosphere 2016. Population coherence and environmental impacts across spatial scales: a case study of Chinook salmon.

Response: We missed this reference. We have added citations to this paper as well as a another by the same author to the revision.

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**Dear Prof Hart-**

**Thank you for your comments, and the useful comments from the reviewers.**

**I believe we have addressed all of them. Attached (below) is how we addressed each point raised.**

**Sincerely, David Welch**

-----Original Message-----

From: Paul Hart <onbehalf@manuscriptcentral.com>

Sent: Monday, July 20, 2020 2:35 AM

To: David Welch <David.Welch@Kintama.com>

Subject: Fish and Fisheries - Decision on Manuscript ID FaF-20-Jun-OA-162  
[email ref: DL-SW-2-a]

20-Jul-2020

Dear Dr. Welch

Manuscript ID FaF-20-Jun-OA-162 entitled "Review of the Coast-wide Decline in Survival of West Coast Chinook Salmon (*Oncorhynchus tshawytscha*)" which you submitted to Fish and Fisheries, has been reviewed. The comments of the reviewers are included at the bottom of this letter.

The reviewers have recommended some minor revisions to your manuscript. Therefore, I invite you to respond to the reviewers' comments and revise your manuscript.

Because we are trying to facilitate timely publication of manuscripts submitted to Fish and Fisheries, your revised manuscript should be uploaded as soon as possible. If you feel that you will be unable to submit your revision within two months please contact me to discuss the possibility of extending the revision time.

Once again, thank you for submitting your manuscript to Fish and Fisheries and I look forward to receiving your revision.

Sincerely

Paul Hart

Editor, Fish and Fisheries [pbh@le.ac.uk](mailto:pbh@le.ac.uk)

Editor Comments to Author:

The first reviewer also saw your original submission. The other two are new and have a fresh take on your paper but both agree that minor revisions are appropriate.

The second reviewer makes a very important point relating to the geographical scope of the paper. As it states in our Aims and Objectives "A paper in Fish and Fisheries must draw upon all key elements of the existing literature on a topic, normally have a broad geographic and/or taxonomic scope, and provide points of generic value, which make it compelling to a wide range of readers whatever their geographical location". To fulfil this objective it would be valuable if you could make some comment about the SAR status in other parts of the chinook's distribution. Has any work in Japan been done on this? I don't expect a comprehensive survey of what's happening on the western side of the Pacific but it would be useful to readers who are not salmon specialists to be able to put your results into a wider context.

**Response: You & the 2<sup>nd</sup> reviewer raise an interesting point—salmon survival rates in Asia. I checked with two of my very knowledgeable colleagues and neither are aware of any survival time series for Chinook from either Russia or Japan. Thus "high quality" survival data does not seem to exist. (Chinook are basically absent from Chinese or South Korean waters).**

**We therefore reviewed the historic pattern of Chinook productivity data for Asia (Russia and Japan) relative to North America (Canada & USA) using data from the NPAFC website (<https://npafc.org/statistics/>). Asian commercial catch and hatchery releases are both small, with Asian hatchery releases consistently <1% of the North Pacific hatchery release total for the species (thus explaining why SAR data is not available—it is much easier to measure survival for hatchery populations). The Asian commercial catch of Chinook averages just under 10% of the total North Pacific catch of Chinook in the 1970–2019 period.**

**I have thus inserted the following summary into the Discussion:**

*"Although survival data for Asian Chinook salmon populations appear to be lacking, Asian populations have had similarly large decreases in abundance relative to North America, suggesting that the drop in Chinook survival is not restricted to North American populations. The reported Asian commercial catch of Chinook averaged just under 10% of the total North Pacific Chinook catch for the 1970–2019 period (NPAFC, 2020). Russian catches for the most recent decade, 2010–2019, were only ¼ of the 1970–79 average. For Japan, catches in the 2010–2019 period were only 1/60th of the 1970s (NPAFC, 2020). Some of the decrease in Japanese catches is likely attributable to regulation changes, particularly the 1977 Law of the Sea Treaty which extended coastal state control out to 200 nautical miles (320 km), and resulted in the transfer of harvesting opportunities from Japan to other coastal states. However, the combined Asian catch still declined to only 17% (~1/6th) the level of the 1970s. Thus, although we only have survival data for North American populations, the decline in Chinook abundance due to decreased survival appears to be Pacific basin-wide".*

Please also pay attention to the Instructions to Authors which gives details as to how the manuscript is laid out. At present it does not follow the instructions. If the paper is accepted you will have to make the necessary changes anyway so you might as well do it now.

**Response: We reviewed the Author Guidelines from your website: <https://onlinelibrary.wiley.com/page/journal/14672979/homepage/forauthors.html>.**

We are currently non-compliant in one area (odd bracketing for some of the references and some arcane inclusion of authors' initials in a very few of the text references). Unfortunately, this seems to be a bug in my referencing software (EndNote). It seems that the only way to fix this is to replace the dynamic links with pure text, but that will drop the reference from the reference list at the back. I expect that it is best for the copy editor to address this, as I have been unable to find a fix.

Reviewers' Comments to Author:

Reviewer: 1

Comments to the Author

I've carefully reviewed the re-submitted manuscript and response letter. The paper has really improved structurally in my opinion and I appreciate the authors willingness to incorporate numerous recommendations brought up during the first round of review. I also appreciated their thorough and detailed responses to my original comments. Between the substantive revisions that I see and thorough response letter, I have no additional concerns and satisfied with the paper as is.

**Response: Thank you.**

Reviewer: 2

Comments to the Author

Within the world of "salmonology", this paper will have considerable impact. Its key finding - that there have been fairly consistent region-wide reductions in ocean survival of Chinook salmon - has important policy implications as people consider where to invest in recovery. The authors specifically take aim at attempts to improve freshwater spawning and production, arguing that this may be fruitless if the major cause of declines is reduced ocean survival, including in areas where habitat remains relatively pristine. It is very valuable to have all of the data from such disparate sources assembled in one place. I give credit to the authors for the enormous amount of work that was required to assemble the data and analyze them, while dealing with numerous limitations of the methods that generate survival.

A key issue for this journal is whether this paper will be sufficiently understandable and interesting to a wide audience. It spends a lot of time in the weeds with the details of analyses, specifics of populations, etc. It has to do this, because these details really matter (and it would be good to ensure that the paper is reviewed by people with more experience than I have at analyses of datasets like this, to ensure that issues of comparability of data among sources and across time is adequate to support the authors' conclusions). But the editor should read the paper with an eye as to whether it aims broadly enough with its Intro, Discussion, and Conclusions to serve Fish and Fisheries' objectives.

**Response: Addressed above in Response to Editor.**

Line 126. "If survival across this vast swathe of relatively pristine territory is severe enough to seriously impact salmon productivity, then there is little hope that modifying freshwater habitat in more southern regions will support a newly productive environment for salmon." This seems logical, but I don't think most people involved in stream restoration are aiming for a "newly productive environment". They are aiming to increase the

number of smolts that migrate to the sea, in the hopes that this will lead to stronger adult returns. If ocean survival is cut in half, then if twice as many smolts leave, that MIGHT mitigate the reduced ocean survival, though this depends crucially on the fitness of the smolts when they leave (e.g. reduced survival from hatcheries) and on negative density dependence, which could be severe if the ocean's carrying capacity is lower. All of this is just to say that the wording should be chosen carefully, in terms of objectives and caveats.

**Response: If the freshwater habitat modifications won't produce a "newly (more) productive environment" that increases the number of smolts produced, then it is difficult to see how the people advocating for this work are going to generate more smolts to offset the decline in marine survival.**

The reviewer's comment is a common response I have seen over my career, where freshwater habitat specialists call for more (freshwater) habitat work but then often fail to change smolt numbers... the habitat is "prettier" (or fits what human eyes see as "more natural", and thus beautiful) but this does not translate into more smolts out—which is the key. As a particularly important example of this, consider a 10 year Washington State study published this February using treatment and control groups to evaluate the improvements in smolt production resulting from a \$1.5 Billion US habitat improvement effort. (See Krall, M., Clark, C., Roni, P., & Ross, K. (2019). Lessons Learned from Long-Term Effectiveness

Monitoring of Instream Habitat Projects. North American Journal of Fisheries Management, 39(6), 1395-1411. doi:10.1002/nafm.10381).

The authors of that study convincingly demonstrated that there was essentially zero (no) difference in smolt numbers between the multiple replicated treatment/control comparisons conducted at any point in the decade-long monitoring program (Fig. 3 of that report, reproduced here). Yet they never concluded that the habitat restoration work was ineffective in increasing the number of smolts produced for any of the three species of salmonids monitored at any of the 4 time periods it was assessed (what their study demonstrated). Instead they concluded that the study must have been statistically underpowered, which was not true, as their own graph clearly demonstrated! (The differences in all cases were near zero, as opposed to large differences but with large variance).

This is why our paper is important—we are showing that the marine survival declines are large and the same (or closely similar) for a wide range of populations and habitats—including essentially pristine (northern BC, SE

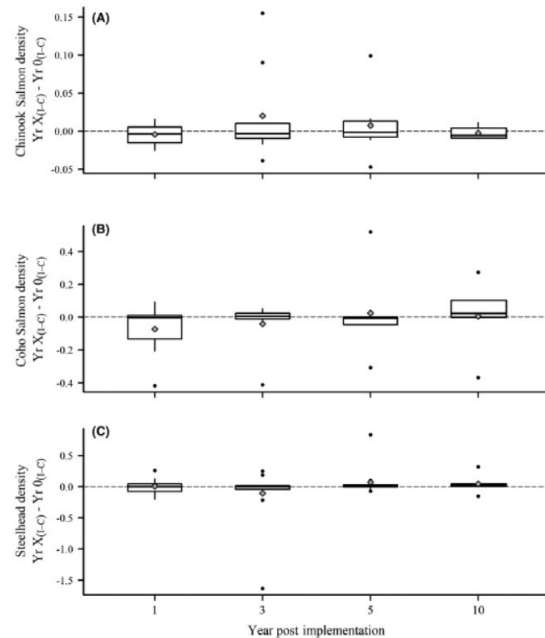


FIGURE 3. Boxplot of the mean difference in the impact (I) minus control (C) for each subsequent year of postproject monitoring (years 1, 3, 5, and 10) minus the impact minus control for densities of year 0 for (A) Chinook Salmon, (B) Coho Salmon, and (C) steelhead. The gray diamonds are the average difference in response between year 0 (impact minus control) and years 1, 3, 5, and 10 (impact minus control).

Alaska and (now) Russia). I don't think it is appropriate to hammer on this point that the reviewer makes further in the paper, but it is something I intend to pick up on in a separate paper dealing with the point that the effect of freshwater habitat improvements on smolt numbers is rarely measured, but where it has been there is little evidence that it has had the intended effect of increasing smolt numbers.

Line 142. "Although not explicitly stated, this seems to be the basis for setting the 2-6% rebuilding standard for the Columbia River." It's interesting that it's not obvious where this target came from, and the explanation doesn't mention 2%. It would be good if the authors could pin down the derivation of the targets, perhaps by contacting the authors of the report.

**Response:** We spent over a week researching precisely this issue in drafting the manuscript. It proved to be very confusing, with recent reports citing earlier reports (often by the same authors) as the justification. In the end, we drew a network diagram of all of the varying reports and their relevant citations before finally narrowing it down that they all led back to a single 1998 report on the issue (which we cite—the reviewer quotes our conclusion from that paper above). That paper (Marmorek et al 1998) does not explicitly state a rationale for choosing the definition of the 2-6% rebuilding target but seems instead to have chosen it because this survival level was what was measured (using different technology) in the 1960s and early 1970s. We found no reports that clearly laid out the justification.

Ultimately, we can't pin down the source—it seems that it transitioned from something that was loosely defined as a sensible aspiration, to something that was almost a legally prescribed definition. We deleted a long section we had written on this issue in an earlier draft of the paper because it added little to the main thrust and we don't want to belabor what is essentially a side issue in the current manuscript,

Line 146. "The SAR is the three-fold product of freshwater smolt survival during downstream migration multiplied by the marine survival experienced over two to three years in the ocean, and multiplied by adult freshwater survival during the upstream migration to the final census point." It would be good to state explicitly how fishing mortality fits in here. When I think of "returns" I usually take this to mean not including fishing mortality, i.e. returns are the number of fish that return to the coast, and then may or may not be caught. This seems to be the correct interpretation based on Line 244, but readers shouldn't have to wait that long to find out.

**Response:** We added the following text to the Introduction: "*Survival should include animals removed by the fisheries; however, as we show later, harvest is not included in PIT tag-based survival estimates, which has significant implications*".

Line 179. "Attempts to improve SARs by addressing region-specific issues such as freshwater habitat degradation or salmon aquaculture in coastal zones are therefore unlikely to be successful." Don't you mean "hatcheries", not "aquaculture"? Aquaculture is fish farming; no intentional releases and nothing to do with attempts to improve fish survival. And again, smolt-to-adult returns may not be improved by addressing habitat degradation, but the total number of smolts that leave may be improved.

**Response:** Salmon aquaculture (fish farming) has been implicated by many as a possible reason for the decline in wild salmon stocks, particularly in British Columbia. We simply want to flag the point that neither dams nor

salmon farms are likely a big driver if regions lacking either industry (Northern BC, SE Alaska) have the same survival issues.

Line 220. Typo. "is to note that that after log-transformation the mean..."

**Response: Corrected.**

Line 221. Something missing here "We therefore use the simpler terminology both for clarity and because. Furthermore, the median is invariant under log-transformation, which is not true for the mean. Pacific Salmon Commission (CWT)".

**Corrected:** *"We therefore use the simpler terminology both for clarity and because the median is invariant under log-transformation, which is not true for the mean".*

Line 274. "should include hatchery rack returns," What is a hatchery rack return?

**Response: Clarified**

Fig. 2 is difficult to read. Consider splitting it in half and present 2 panels one below the other. There is no logical reason to run them all out in a single horizontal row. The caption says "Annual SAR estimates for Hatchery (H), Wild (W), and mixed hatchery-wild data sources (B) are shown...". I don't see those symbols / distinctions among data sources in this figure.

**Response: As discussed previously with the editor, the on-line version of the figure allows the reader to zoom in and see the requisite detail. (We added this statement to the figure caption). Additionally, the published version of the figure will be substantially larger than the one used for review. We chose the horizontal row format to allow easy comparison of the magnitude of the decline for the same life history type (yearling/subyearling) across regions.**

Fig. 3. At first I missed the legend to distinguish whether populations are wild, hatchery or mixture. Why do some population names include additional symbols for these distinctions, but others not?

**Response: Economy of space. We restricted the number of letters in the abbreviated names to reduce the space taken by the axis labels but still allow them to be informative. Symbols to distinguish wild/hatchery/mixed stocks are only included in this figure where necessary to distinguish populations that would otherwise have the same name. The origins for all populations are in Table S1.**

Reviewer: 3

Comments to the Author

Title: Review of the Coast-wide Decline in Survival of West Coast Chinook Salmon (*Oncorhynchus tshawytscha*)

Authors: DW Welch, AD Porter, EL Rechisky Manuscript ID: FaF-20-Jun-OA-162

Summary: In this manuscript the authors present an analysis and review of patterns and trends in the survival of Chinook salmon stocks from southeastern Alaska to California. To accomplish this, they collected the historical smolt and return data for 123 stocks that are tagged as juveniles or smolt using either coded-wire (CWT) or passive integrated transponder tags (PIT). Survival was estimated as the smolt-to-adult return (SAR) rate, where return included both harvest and escapement information. While there are comprehensive programs to sample harvest for CWT across this stretch of coast, no such program exists for PIT leading to SAR estimates that are

concordant but biased compared with CWT-based estimates. The authors find that survivals have generally declined across stocks and almost all are below the rebuilding targets (2-6%) set for Columbia River stocks. Based on the observation that declines are consistent across a geographical scale where freshwater habitats range from highly compromised to almost pristine, the authors propose that the main causes are to be found in the marine environment.

**Assessment:** This manuscript will be interesting to a variety of audiences and will contribute to the ongoing conversation around the current demographic patterns and trends in this species in specific and all Pacific salmon species in general. The concept that one or more critical periods exist in the marine portion of the Chinook salmon life history has been discussed for decades, but the collation of data and presentation of a widespread pattern across this species adds to the discussion. Likewise, the discussion and demonstration that there are significant differences in data depending on the technology and design of application. Sometimes a great technology can't make up for lack of information. I appreciate the time and attention paid to the style and grammar used, which helped with reading and comprehension.

**Recommendation:** Publish with minor edits

**Comments:**

1. Line 130 - This is the first use of SAR and a definition is not supplied until Line 145

**Response:** We added "*(smolt-to-adult return, or survival)*" to Line 130.

2. Line 170 - From the text (e.g. Line 734) the changes referred to at this point happened two decades ago, in 1999. The treaty has been renegotiated twice in that period. This needs clarification.

**Response:** We have clarified that the change we are referring to occurred in 1999 (rather than more recent negotiations). Beyond this, we don't want to divert focus to a history of the Pacific Salmon treaty because Chinook salmon harvest remains "abundance-based" since 1999. All of the battles over further negotiations may be very real to those who live through the multiple meetings each year, but these changes are irrelevant to our big point—that the "abundance-based" management system mandated by the treaty operates to remove much of the variability in adult returns that those working in freshwater are attributing to changes in hydropower dam operations. The two groups are disconnected and working in isolation.

3. Line 182 - Why is the call to funding agencies and not management agencies or trans-jurisdictional management organizations?

**Response:** Because the funding agencies can drive change. It is the management agencies that have spent the funding agencies' money without ever picking up on the critical issues we have outlined. They should have done so years ago.

4. Line 221 - The end of this sentence is missing.

**Response:** Apologies. Fixed as follows: "*We therefore use the simpler terminology both for clarity and because the median is invariant under log-transformation, which is not true for the mean*".

5. Line 225 - "coastwide" is not and appropriate descriptor. The Treaty only covers fisheries from Cape Suckling, Alaska to Cape Falcon, Oregon.



**Response: We have adopted the reviewer's definition:** *"The PSC is a bilateral treaty organization between the US and Canada coordinating coastwide management of Pacific salmon from Cape Falcon, Oregon, north to Cape Suckling, Alaska."*

6. Line 312 - This statement is true, but could be better worded since the point being made concerns the measurement of the "return", a term that is hidden in the acronym, SAR. The finer point here may not be clear to a reader that is not familiar with salmon management. See Line 635.

**Response: We have elected to leave the existing wording** (*"PIT tag-based SARs do not incorporate losses due to harvest (McCann et al., 2018, p. 95) because the commercial and sport catch is not monitored for PIT tags."*) as it currently is. We feel the importance of the failure to account for harvest (catch) is best clearly laid out once, later in the paper where we outline the magnitude of the harvest variations, rather than lightly raised in multiple places.

7. Line 341 - "essentially immune" may be too strong as there can be fishery removals at remote marine locations in the Gulf of Alaska and southern Bering Sea and many yearling stocks are subject to a period of harvest in nearshore fisheries.

**Response: Reworded to incorporate this point.**

8. Lines 350 to 354 - Limiting the years to 2010-2014 is explained, but the rationale is not clear. Not including through 2008 because it was unusually cold while not acknowledging the unusual warmth that these booyears experienced in the marine environment during 2014 and 2015 seems inconsistent.

**Response: We added the parenthetical clause** *"(2014 being the last year with essentially complete data available for all populations)"*.

9. Line 360 - It would help to incorporate the description of this analysis in Lines 444-446 into the description here. The inclusion of SAR\_SNAK,j in the equation was confusing until I was reminded that every region was normalized to the Snake River.

**Response: We inserted "relative to the Snake River" into the sentence to make this clearer.**

10. Line 551 ff - Consider including Howard et al. 2016 and/or Murphy et al. 2017 to the lists for growth and ocean conditions to extend the range of observations

**Response: We added both.**

11. Line 558 - Consider including Seitz et al., 2019 for marine predation

**Response: Added.**

12. Line 607 - This statement is backwards according to the equation at Line 235

**Response: Fixed.**

13. The style and format of the in-text citations varies widely and was actually quite distracting. For example in the paragraph beginning at Line 551.

**Response: We agree. We finally (partially) fixed this, which was an obscure bug in how our bibliographic software (EndNote) formats references. (Some remain to be fixed by the copy editor because EndNote still "corrects" some references to a format inconsistent with the journal's guidelines).**

14. Citation at line 1135 is missing information and difficult to locate. Suggest adding the following information: "Arctic-Yukon-Kuskokwim Sustainable Salmon Initiative retrieved from <http://www.aykssi.org/aykssi-chinook-salmon-research-action-plan-2013/>"

**Response: We updated the reference information.**

15. Figure 2 was too small to be informative.

**Response: The published version of Figure 2 will be larger than the version available during review. As my email to the editor indicates, the figure was designed to support ample magnification to allow inspection of individual panels—the detail is there, so long as the reader zooms in using the on-line version and the figure is kept in its native TIF format. We can change the orientation and split up the rows or columns, but the reader then loses the ability to see the bigger picture and compare across regions. We would welcome editorial advice here, but for now have not made any changes (we have tried many, many variants already).**

**We have added the following text to the figure caption, as it is clear the reviewers did not try to zoom in on the details: "(The on-line version of the figure supports substantial magnification to examine the details of each panel)."**