

Studying Chinook Salmon in Northern River Ecosystems through Ecological Methods and Indigenous, Teslin Tlingit Knowledge

Corresponding Author: Mr Jared Connoy

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

This was a joint review between a faculty member and a PhD student, and we separate it into two parts, largely oriented towards big picture comments and more detailed suggestions.

Big Picture Comments:

First off, we very much support Communications Biology going beyond...Biology. What we mean by this is that we think that there is much value in incorporating more than solely empirical field data into our collective understanding of ecological processes. And in this case, learning about salmon-ecosystem dynamics from people with long-term cultural and ecological interactions with salmon and other components of ecosystems can provide much value.

The challenge of any paper with multiple information sources, such 'mixed methods' included, however, is combining the information in a comprehensive, complementary, and coherent way. Candidly, however, we feel that there is a long way to go to achieve these outcomes.

As we expand on below, one striking deficiency is the organization of the writing. It requires a lot of attention. This manuscript deviates strongly from the typical flow of scholarly paper. With an 'hourglass' shape, readers typically first encounter the highest-order conceptual content in the Introduction, laying bare what is known and what is not, then proceeding to objectives and hypotheses. The Discussion takes the opposite organizational form in which the results are unpacked in their narrowest sense (as they relate, for example, to the study system), then the writing broadens out again to the conceptual material first encountered in the Introduction; the authors then should explain what new insight their results (and its inference) contain for the conceptual body of literature.

In its current form, the writing is highly scattered and several 'rules' of writing are violated. As a prominent example, the reader encounters results (but without accompanying data or pointers to figures or tables) on line 152 of the Introduction. Similarly, there is (extensive) discussion-like material added soon thereafter (again in the Introduction).

Its our opinion, even when writing about IK, you might avoid vague phrases, even (and perhaps especially) in praise, like "...immeasurably deep in breadth and complexity". Line 59. "This is obvious to Teslin Tlingit community members". Line 144. We offer this with the best of intentions; we feel that such hyperabundant praise has the opposite effect as desired.

Similarly, the normative statements are likely best reinforced via citing (especially Indigenous) scholars who have made these same arguments. E.g., "With an expansion in interest in IK comes a moral imperative to work with Indigenous communities in a good way, with an understanding of the colonial history of research and academia, and to perform research that recognizes and supports Indigenous sovereignty." line 123...Wilson, Kovach, Reid et al. not cited until several lines later.

Similarly, the Abstract 'goes hard' right away, with first lines declaring: "Biodiversity and ecosystem function are in collapse. This has been caused by colonial pursuits of land and resources in North America. Science and ecology have perpetuated these systems, requiring a re-thinking of research and how it is done". While this statement is valid, its not necessary to frame your study like this 'right out of the gates'. Others (especially Indigenous scholars) have forcefully and compellingly made this argument in more powerful stand-alone pieces. What we are trying to convey is the suggestion to let those powerful pieces stand alone (and reinforce them by citing them) but avoid using the very first line of a study on salmon-ecosystem dynamics to declare it. While we align with your values, we feel that this declarative statement reads like its you making this point anew, which undercuts those who long-ago suggested it.

Line 144. The reader encounters deltaN15 with no background whatsoever (other than a brief 1-sentence mention of 15N on

line 106) about how it's a tracer for marine-derived nitrogen from salmon. Summarizing the 'salmon forest' literature and its tools (like this molecular tracer) earlier in the Introduction is key to understanding the analytical approach, but this is missing. We are pleased to read about community protocols. In particular, the authors should be commended for taking the time and effort to engage meaningfully with Indigenous research partners. This is reflected in the manuscript at various points, especially the inclusion of Indigenous authors, the positionality statement, the footnote to clarify the word 'explore', and the project development section of the methods. But while we see 'consent forms' mentioned, there is no statement assuring the readers that there was also an approved Human Research Ethics application from the university/ies involved (typically citing the application/protocol number, so its traceable). This is mandatory for all interview data collected in Canada. We note that the University of Calgary Research Ethics Board was mentioned in the accompanying pdf file, but it does not appear in the manuscript and does not have a protocol number despite this requirement being explicitly stated.

As presented, its hard to ascertain the reliability of the statistical modeling. Not all results, for example, are shared. E.g., Line 184. "For all relationships of temperature, temperature lag-1, precipitation, and precipitation lag-1, p values were >0.16." Readers need to see the data for parameter estimates, their measures of variation, exact p-values and so on. Right now, there is no table or figure referred to with these data (but we noticed the figures start with figure 2...is there a missing figure 1?). Additionally, the reporting of other p-values is unorthodox (e.g., " $p = 0.01 \times 10^{-6}$ "). But these examples are not as important as the absence of any accompanying figures for your main data set (deltaN15 and salmon escapement) and modeling results (as presented)...the only figures presented are those relating to salmon escapement alone (i.e., not central to your modeling). Figure 2 simply shows box plots of raw data (ie not model outputs, accounting for other variables). Additionally, its not clear what type of models you are using; there are not enough details shared to be interpreted (or replicated - the gold standard for Methods). The clues we could assess left us puzzled, like 'null climate models' (line 192), which seems to be an oxymoron; if we understand your intention, they are better described as climate-only models. Even the results narrative had some puzzling phrases, like "All models (including the negative control) had interestingly high Akaike weights for models including an unlagged salmon escapement variable" line 193. As a result of these and other shortcomings, there is not enough information and data presented to assess the modeling.

Line by line comments:

Lines 59 – 63: These sentences at the end of the paragraph here could be combined with the paragraph beginning on line 112.

Line 66: Here is a good example where a citation(s) should be included – there are many works on this topic.

Line 98: The sentence "One particular area of concern regarding these declines is the role of salmon as ecological keystone species (Hyatt and Godbout 1999)." is a awkward. Introductory sentences at the start of paragraphs are ideally straightforward and give the reader an understanding of what to expect from the paragraph. A more direct way of writing this might be "As an ecological keystone species, declines in salmon are likely to have adverse impacts on the ecosystems they inhabit."

Lines 116, 128, 132: The phrase "...critical to..." is repeated three times in about 15 lines here (and elsewhere in the paper). The statements are not incorrect, but consider changing up this phrase. "Key", "essential", or "necessary" could all be useful synonyms. However, be mindful of the overuse of such strong phrases, as they dilute the impact they have.

Line 117: While unintentional, the sentence here seems to relegate IK to "filling gaps" in Western science. While it certainly can do that, it can also do much more, such as transforming how we make management decisions or informing the development of hypotheses. Adding a few examples of this (with citations) would more clearly demonstrate the many ways IK contributes to ecological understanding.

Line 125: The word "good" is a bit vague. The authors expand on it a bit but "just" might be better. Moreover, it would be helpful to clarify what exactly is meant by "good" or "just". While a full treatment of such a topic would be beyond the scope of this manuscript, a few key points (ideally with citations) would be helpful (e.g., reciprocal data sharing, input at all stages of research, etc.).

Lines 149 – 182: The two last paragraphs of the Introduction section read as a Results/Discussion section rather than as an Introduction section. Perhaps this was caused by a formatting error? Most of this content should be included in the Discussion section.

Results section generally: A number of sentences can probably be merged into the paragraphs above and below them. Paragraph breaks are not needed between individual sentences. As well, it is more common to simply report p-values as " $p < 0.001$ " rather than reporting the p-value to the exact decimal.

Line 195: Recommend removing the word "interestingly", as the Results section is typically where the text should be the most objective. If it a result is interesting, explain why in the Discussion section.

Line 223 (Figure 2): Significant differences among groups are typically labelled by asterisks (e.g., *, **, ***) rather than A, B, C. While it is fairly clear what the authors are denoting, using the asterisk convention may help readers to understand the figure much more quickly and easily.

IK Interview Results: Did any interviewees mention changes in vegetation with salmon declines? While observations of changes in tree growth are difficult, as alluded to by one participant on line 289, has anyone noticed changes in shrub/grass health, abundance, or any other vegetation attributes? This information might help to link the interviews with the scientific aspects of the manuscript (namely, the effects of salmon on plant growth).

Line 284: Just a note to check the consistency in use of single or double quotes throughout the manuscript. While minor details such as this can be ironed out with the journal after acceptance, the less inconsistencies, minor errors, and grammatical mistakes the better. Attention to detail in the formatting and presentation of manuscripts reflects attention to detail in other aspects of the research, such as methods and analyses.

Line 315: Two periods at the end of this sentence. Again, these errors are minor, but in sum detract from the communication of the research.

Lines 316 – 322: This content of this paragraph has already been stated in the Introduction section and can be deleted or merged with other sections to avoid repetition.

Line 327: Change "sonar" to "salmon" for clarity.

Lines 355 – 370: This paragraph contains no citations but probably could be enhanced by connecting the statements here to the broader literature. For example, “Low flow could be beneficial to salmon by reducing water velocity and energetic requirements...”.

Line 381: Here (and also line 326) is where it might be beneficial for the reader to be briefly reminded of the hypothesis (rather than the lengthier re-hash of the hypotheses near the beginning of the Discussion section). For example, instead of “The second hypothesis and prediction were strongly supported by $\delta^{15}\text{N}$ results.”, use something like “Our second hypothesis and prediction, that salmon fertilize trees and that salmon-bearing sites would therefore have higher $\delta^{15}\text{N}$, were strongly supported by our results.”

Lines 418 – 422: Here is an example of an area where the authors might be able to connect the work of this study to the literature and explain why specifically the use of IK interviews was valuable. The authors discuss how interviews provided an important baseline for salmon runs, but this point could also be made more broadly to highlight a unique aspect of local/Indigenous Knowledge. Some helpful citations on this topic include:

Eckert LE, Ban NC, Frid A, and McGreer M. 2018. Diving back in time: Extending historical baselines for yelloweye rockfish with Indigenous knowledge. *Aquatic Conservation: Marine and Freshwater Ecosystems* 28: 158–66.

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Lines 437 – 444: This paragraph is important and most sentences within could be elaborated on. For example, “Changes observed in bear behaviour are also concerning for negative human-wildlife interactions.” (Lines 440 – 441). How, specifically, are these changes in bear behavior concerning? The authors allude to the reasons in the paragraph above, but it is never made explicit. Including additional citations in the paragraph would likely help make these arguments. For example: Artelle, K., Anderson, S., Reynolds, J. et al. Ecology of conflict: marine food supply affects human-wildlife interactions on land. *Sci Rep* 6, 25936 (2016).

Lines 445 – 454: This paragraph makes a few generalizations. While the main points being made are reasonable, it would be helpful to be specific about what aspects of this study were enhanced by including IK. The information in the paragraph beginning on line 168 in the Introduction could probably be included around this area. For example, the sentence “By building a narrative through IK instead of interpreting it through statistical analyses, knowledge systems can be braided into a bigger picture without either being subsumed by the other.” is a bit vague. If one of the goals of this manuscript is that it “...demonstrates...the importance of considering multiple ways of knowing to improve research in complex (social-)ecological systems.” (lines 37 – 39), the reader would benefit from hearing about the specifics. For instance, did the interviews inform the study topic or predictions? What would have been missed if the interviews were not conducted? Did the interviews help to build community investment in the project? Answering these types of questions might allow a reader studying a different area or a different species to draw parallels and design their research accordingly. It would also help this manuscript appeal to a more international audience.

Line 501 – 502: This sentence could simply be merged with the paragraph above.

Line 606 (and elsewhere): Recommend denoting R packages with quotes or italics to clearly identify them (in this case on line 606, “car” or *car*).

Line 626 (Interview methods): Would it be possible to provide the interview questions as an Appendix or supplementary information? If not, some additional clarification about the types of questions asked would be useful (and necessary for open science and reproducible work) Lines 639 – 641 provide little information on the “set list” of questions. While semi-structured interviews by nature allow interviewees to expand upon the questions (as the authors note here), the general questions asked, or topics covered, would be useful for the reader to better understand the conversations. In addition, future efforts to repeat or emulate this study in a different area or context would benefit.

Table 3: Recommend using a set number of significant figures for presenting results. In this table, there seems to be switching between different numbers.

Reviewer #2

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Table 3: Recommend using a set number of significant figures for presenting results. In this table, there seems to be switching between different numbers.

Reviewer #3

(Remarks to the Author)

This paper provides a somewhat novel approach of blending Indigenous Knowledge with ecological methods to test for links between salmon and tree growth, including consequences of decline of numbers of fish. This is a powerful approach, and I thought the interviews were very insightful and helpful. Most of my concerns are with the ecological side of this paper. Overall, I felt that results that did not support the predictions were not given sufficient discussion.

We really need to see graphs of the relationships between numbers of salmon and tree growth. Sole reliance on AIC tables is simply not sufficient without a clear visualization of the data. It feels like the authors are sabotaging their own study without such graphs, and I would not support publication without them.

It is not clear how the predicted percentage increases in tree growth due to the influence of salmon were calculated. These are potentially very risky extrapolations, especially if they are based on comparison with just the one control site.

I don't understand the definition of a "positive control site". We are told that many chinook salmon carcasses accumulate there. Without knowing how many carcasses accumulate at each of the salmon sites, it is not possible to tell what "many" means relative to the 3 "salmon stream sites". Why not simply call it a "salmon site"? Note that whatever the authors call it, it should be included in the tally of the number of streams that either did or did not support the authors predictions, with the various analyses. I am suggesting this because by having three kinds of site, and different scales of analyses, it is sometime hard to see the bigger picture.

There is no description of the waterfall site. How can the authors rule out the possibility that the characteristics of the riparian zone that affect tree growth did not differ above the waterfall compared to the other sites? Data should be presented on this if possible, or else a more nuanced interpretation of the results should be given.

The authors state: "The physical and ecological similarity between sites (long-lived, riparian, alder-free, white spruce stands, which only form relationships with ectomycorrhizal fungi), although not accounted for statistically, suggest that none of these factors should vary between them." This is an overly strong statement. What about differences in soil moisture, aspect, and slope? Some mention is made later of the importance of soil moisture for stable isotopes of nitrogen. Isotopic results need to be treated with extreme caution due to the complexity of their interpretation. Physical and biotic data should be presented for each site, if the authors have this information. I was not able to download supplementary information so I'm not sure if it's there.

"Many replicates would have to be included in each site type for a more reliable and informative analysis of differences in mean growth, which was not possible in the study area due to few known locations of negative control sites." There may be few sites with no salmon, but that is not the only kind of comparison used in this paper: the authors could have made use of additional sites that vary in the number of salmon. I'm not saying this should have been done; there are limits to what can be accomplished. But the focus only on lack of control sites is not the only limitation.

My reading of Table 3 is that there was no effect of salmon escapement on tree growth at the positive control site (Teslin). I did not see any discussion of why not. It seems important to the overall conclusions of the paper.

Related to the point above, the fact that one of the four salmon sites had significantly lower tree growth than the control sites is mentioned briefly, but then not again. It should be mentioned more prominently, including in the Abstract, to temper the conclusions. If the authors have any speculation as to why this occurred, it would be helpful to place this result in context with the other findings.

I did not see any discussion of any potential limitations from having interviewed only three participants. These interviews have been very helpful and are an important and novel part of the paper. Is there a way to respectfully consider what additional insights might have been gained if more people had been interviewed?

Minor comments

Much of the literature cited is quite dated. For example, "Pacific salmon experience threats in both freshwater and marine environments during their life cycles, support large commercial fisheries (Clark et al. 2006)..."

There is also a lot of more recent literature available on effects of salmon nutrients on riparian ecology. The findings with respect to bears could benefit from discussion of work by Quinn on relationships between bears and salmon, and the discussion of an increase in bear-human conflict could consider the analyses by Artelle et al.

Table 3 is the first one mentioned in the text.

It would help if the title of Table 3 specified which salmon data were being analyzed (presumably the full Yukon River estimates, based on the text). Table 5 uses just the 5 years of Teslin data.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

I congratulate you for attending to our suggestions, and wish you the best with this and other work.

Reviewer #2

(Remarks to the Author)

This paper is much improved from the previous version, and the authors have clearly put in plenty of additional effort to highlight the value of this study. The language is clearer, the structure is far better, and the results (including tables and figures) are more conclusive. The authors have done well to incorporate the previous feedback and, along with their other changes, have made a more compelling case for the impact of salmon on tree growth in an inland region.

A few suggested edits are provided below that would improve the manuscript further. In particular, however, there are still a number of grammar and syntax errors throughout the manuscript. Some examples include:

Line 233: "Including both a positive and negative control site allows to make comparisons..."

The sentence on line 401 should begin with "The lowest..."

Line 417: "an" should be "and".

Line 495: "...around two weeks years into..."

Line 635-636: "challenging to in cross-site".

Although these are mostly minor errors individually, they (and others throughout the manuscript) collectively detract from the paper. Additional proofing would help to iron out these details and, in many cases, improve the overall clarity of the message.

Abstract (Line 36): The sentence "This study was limited by available sites and data, however demonstrates that the health of salmon and riparian forests on the TTC Traditional Territory are deeply linked, and the importance of considering multiple ways of knowing to improve ecological research." is awkward and grammatically incorrect. I would recommend breaking this into two separate sentences, as considerable time is spent in the discussion explaining the substance of this sentence. The Abstract is perhaps the singular most important part of a publication, as it is what the majority of readers will engage with first (and for many, the only thing they will read unfortunately). As such, it should be well-polished and free of such errors. In addition, the authors do not do themselves favors by bringing up their limited sites and data. While explaining limitations is important and necessary in the paper, the results should still be able to stand in the abstract. Perhaps a better way of phrasing it would be something along the lines of "Although this study was limited by available sites and data, we demonstrate that..."

Lines 63 – 69: I would recommend moving or integrating this portion about the value of Indigenous Knowledge systems to the paragraphs beginning on line 143, where there is more thorough treatment of the topic. As is, the opening paragraph begins with biodiversity/fish losses and its impact but by the end is discussing IK systems. I would suggest making the paragraph more directly about the effects of declines in freshwater, and perhaps expanding a bit on the effects of fish declines, especially for Indigenous peoples.

Line 66: Here, the phrase "...with a long way still to go..." is an example of vague language that is not very 'scientific'. People can interpret this phrase in a number of ways, and it provides little added detail. I would suggest being more direct and linking it to the next sentence a bit more. For example... "Indigenous Knowledge Systems are increasingly valued in scientific literature and management, especially in fisheries, though efforts to combine knowledge systems to transform research processes is slow and often fraught with social and analytical challenges. Yet, this transformation is needed..."

Line 85: Suggest some wordsmithing to make this sentence flow a bit better, such as: "Pacific salmon are a significant marine nutrient subsidy to otherwise unproductive aquatic and riparian environments, as the vast majority of salmon growth occurs in the ocean and mortality and decomposition is concentrated in freshwater environments after reproduction."

Line 94: Recommend moving this sentence "Numerous vertebrate species..." to earlier in the paragraph after the sentence beginning on Line 85.

Line 97: Recommend briefly defining "15N" the first time it appears.

Line 173: Recommend replacing the dashes with commas in this sentence for clarity. While it is not grammatically incorrect, there are four dashes that appear in a single line which is a bit confusing at first glance.

Figure 1: A good convention is to remove the grey grid lines in figures and replace them with x and y-axis. This would also make the legend in plot B more visible.

Figure 2 is difficult to assess, as it appears in the PDF that the x-axis has been obscured by the figure caption text, though I assume it is Yukon River escapement data. This figure would benefit from a bit of formatting though, specifically by eliminating the grey lines and increasing the font size of the axis.

Figure 3 and figure 4 are overall well designed and clear figures. The large font on the axis, for example, is likely to be helpful for readers. I would recommend adjusting the other figures to have similarly large text on the axis so that the information is very visible. For figure 3, I would recommend briefly defining the asterisk values that denote significance in the caption.

Table 3 is quite 'cluttered' in format, but this may be fixed by the journal formatting specifications. Additionally, in the caption, "... *** p < 0.00" should be "**** p < 0.001". Finally, and I may be missing or confusing something from the main text, but why

was escapement used as abundance in the Upper Morley model, when all others used the 2-year average? Table 4 also indicates that this was the only model with an AIC weight that wasn't escapement, which is curious. Can this be clarified?

Line 511: Here, and in a few other areas of the paper, there are paragraphs composed of only a couple sentences. These should be eliminated where possible and combined into the paragraphs above and below.

Line 565: Can you explain more clearly why "Model Akaike weights indicated that nutrients are made available to, and taken up by, trees primarily within the same year, and into the following after a given salmon run..."? I think I know what is being alluded to with regards to escapement models being weighted higher, but clarity is needed here. Also, it seems as though the Upper Morley site was the only site where any model other than annual escapement was ranked higher (in this case, 2yr escapement average). I am a bit confused by the phrase "...and into the following after a given salmon run,..." when none of the lag-1 models were weighted highly, and line 409 states "Escapement lag-1 models were weighted very low at all sites."

Line 614: This opening sentence does not make sense and awkwardly states the second hypothesis. But here, and elsewhere, some confusion is created by terms such as "annual mean growth". Because multiple measures of tree growth were used in this study, it is helpful for readers to easily be able to distinguish them. I recommend picking a term for each and sticking to it throughout the paper, or clarifying within each sentence what is being talked about (e.g., "tree growth measured as...").

Line 662: I suggest switching the order of the first and second paragraphs of the "IK Interviews" section. Overall, they read well and offer some insight, but the second paragraph discusses the results of this study, while the first paragraph, as well as the one beginning on line 702 speak more broadly about IK and interviews.

Line 705: Here, the authors state that IK helped in "...refining research questions..." among other things. Yet, there is no mention of this in the introduction when the hypotheses are introduced, and this feels a bit 'attached' to the end of the paper. The authors have included some great explanations of the benefits of IK in the 'Project Development...' and discussion sections, but it would also be good to highlight the value of the partnership when the hypotheses and objectives are laid out in the introduction. Even simple additions such as "Based on consultation with community members..." or something to that effect would go a long way toward reinforcing the benefits of IK/science partnerships at all stages of the research process.

Line 715: I recommend changing this sentence to remove the statistical analysis part, to something more along the lines of "By building a narrative with IK, knowledge systems can be braided...". Information derived from IK can, in some cases, be analyzed statistically. Many authors argue that there are benefits of doing so when done appropriately.

Reviewer #4

(Remarks to the Author)

I commend the authors for their work on this paper. Working in the space of Western Science and Indigenous Knowledge is hard and messy. Yet, as the authors state in the paper, is the avenue forward for decolonizing ecology, promoting reconciliation and platforming indigenous knowledge and ways of knowing.

However, in my reading of this manuscript, it almost felt like I was reading two disconnected pieces of work: 1) a western science approach to look at salmon subsidies to trees; and 2) a tangential survey of indigenous knowledge in the region. Perhaps that is the point, and one of the reasons why there has been slow progress in this area. In my opinion, neither piece on its own would merit a standalone publication, but they aren't as complimentary as I would have hoped reading the title and the abstract.

So, the question becomes, can these pieces be better integrated in this manuscript? Maybe providing a bit more rationale on why the Teslin were interested in looking at tree growth would provide some additional motivation? Alternatively, many of the Nations that I work with have a concept of interconnectedness (e.g. Hishuk ish tsawalk in the Nuu-chah-nulth culture: everything is one) – do the Teslin have something similar that would help tie the strands together? Did this research question come from a similar place?

All this to say, I think there is some important and necessary work here and I would hate to see it dismissed by the broader community because it isn't tied together in the most effective manner.

My other major comment relates to the salmon data used as a predictor variable. Specifically, more detail is needed on what specific data were used and from where, and there are also other data sources that may be more appropriately used as predictors. Specifically:

- In the methods L278-292, are you using border passage here? Or total Yukon returns?

- Connors et al. 2022 uses genetics at border passage to derive an estimate of escapement to the Teslin system. Using this time series would be much more defensible than the Yukon aggregate.

- Overall, this whole section would benefit greatly from a discussion of the diversity of salmon within the Yukon. The Teslin area is one of nine salmon Conservation Units, and within the Teslin Conservation Unit there are a number of populations or streams. These nine Conservation Units appear to largely respond synchronously to environmental drivers (at least in recent years; Murdoch et al. 2024). So, perhaps the assumption that border passage approximates Teslin escapement is warranted. But, either way, some more text is needed here explaining the hierarchy of salmon diversity, why datasets were chosen, and what the major assumptions are (e.g. that border passage~Teslin escapement).

Connors, B. M., Siegle, M. R., Harding, J., Rossi, S., Staton, B. A., Jones, M. L., ... & Sutherland, B. J. (2022). Chinook salmon diversity contributes to fishery stability and trade-offs with mixed-stock harvest. *Ecological Applications*, 32(8), e2709.

Murdoch, A., Connors, B. M., Lapointe, N. W., Mills Flemming, J., Cooke, S. J., & Mantyka-Pringle, C. (2023). Multiple environmental drivers across life stages influence Yukon River Chinook salmon productivity. *Canadian Journal of Fisheries and Aquatic Sciences*, 81(1), 97-114.

Specific suggestions

L 29-32

“Tree growth was positively related to salmon escapement” and “mean site tree growth did not show a clear pattern related to salmon” appear to be contradictory.

Suggest splitting paragraph 1 of the introduction. This one starts with a strong, clear topic sentence, but then meanders into Indigenous Knowledge Systems

L55-57: Interesting focus on freshwater ecosystems here, considering that salmon recruitment is typically linked to changes in the marine environment (e.g. see Pearcy 1992; Beamish and Mahnken 2001; Peterman and Dörner 2012). Perhaps salmon (and other anadromous species) experience the worst of both worlds from both marine and freshwater pressures (e.g. Waldman and Quinn 2022)? I think a more relevant tack for this paragraph may be to focus on the plight of anadromous fishes.

Beamish, R. J., & Mahnken, C. (2001). A critical size and period hypothesis to explain natural regulation of salmon abundance and the linkage to climate and climate change. *Progress in Oceanography*, 49(1-4), 423-437.

Pearcy, William G. (1992). *Ocean ecology of North Pacific salmonids* / William G. Pearcy. Seattle : Washington Sea Grant Program : Distributed by the University of Washington Press

Peterman, R. M., & Dörner, B. (2012). A widespread decrease in productivity of sockeye salmon (*Oncorhynchus nerka*) populations in western North America. *Canadian Journal of Fisheries and Aquatic Sciences*, 69(8), 1255-1260.

Waldman, J. R., & Quinn, T. P. (2022). North American diadromous fishes: Drivers of decline and potential for recovery in the Anthropocene. *Science Advances*, 8(4), eabl5486.

L71: Careful with the word productivity here. Suggest defining it as it means different things to different audiences.

L101: repetitive with above

L111: see also Reimchen and Fox (2011)

Reimchen, T. E., & Fox, C. H. (2013). Fine-scale spatiotemporal influences of salmon on growth and nitrogen signatures of Sitka spruce tree rings. *BMC ecology*, 13, 1-13.

L123-124: should also cite Murphy et al. 2017. The reasons underlying salmon declines are complex and multifaceted, but the strong relationship between Bering Sea catch-per-unit-effort and returns points to a strong marine effect.

Murphy, J. M., Howard, K. G., Gann, J. C., Cieciel, K. C., Templin, W. D., & Guthrie III, C. M. (2017). Juvenile Chinook salmon abundance in the northern Bering Sea: implications for future returns and fisheries in the Yukon River. *Deep Sea Research Part II: Topical Studies in Oceanography*, 135, 156-167.

Paragraph beginning on L116

This paragraph should be tightened up or split. Needs a topic sentence with supporting information.

L182: Not a complete sentence, add ‘we predict that’

The ‘western science’ part of the introduction with tree growth and isotopes generally could stand from a more thorough examination of the literature. Based on the works of Reimchen, Naiman, Reynolds etc. what is the remaining gap and what does this study do to help address it?

L301: Not at all my area of expertise, but would precipitation matter less early in the season when presumably temperatures are above 0 but there is still snow on the ground.

L343-347: way more detail is need here to understand what is going on

L408: d15N’AIR’ variable should be renamed. Unclear if it refers to ‘air’ or the variable A x I x R. See Edwards and Auger-Methe

Edwards, A. M., & Auger-Méthé, M. (2019). Some guidance on using mathematical notation in ecology. *Methods in Ecology and Evolution*, 10(1), 92-99.

Discussion

I like how the discussion is laid out in terms of hypotheses. I would support the inclusion of a table of hypotheses.

L581-591: Could conversely be argued that more extreme environments may be more limited by nutrients

L614: not a complete sentence

L616: Reintroduce acronyms in each section if allowed by the journal

Table 5 (and throughout) – did you account for multiple comparisons? If not, you probably should

Version 2:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Congrats on your efforts to respond appropriately to all of the remaining suggestions by the 2 reviewers of 4 who requested another round. I think you have done a satisfactory job, and this is ready to share with the world.

Reviewer #2

(Remarks to the Author)

The authors have addressed all of my previous comments appropriately.

Reviewer #4

(Remarks to the Author)

The authors did an excellent job of responding to my comments, and I found this manuscript to be much improved from the last version I reviewed. Nice work

made.

In cases where reviewers are anonymous, credit should be given to 'Anonymous Referee' and the source.

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We thank the reviewers for their insightful and constructive feedback. With the recommended changes, we believe the manuscript is better positioned to contribute meaningfully to a wider audience. We have made major changes to our formatting, presentation of results, literature cited, discussion of how our work is situated within the literature, and provide a more nuanced interpretation of results. Please see our responses to the reviewers feedback in blue italicized text below:

Reviewers #1 and 2 (Remarks to the Author):

This was a joint review between a faculty member and a PhD student, and we separate it into two parts, largely oriented towards big picture comments and more detailed suggestions.

Big Picture Comments:

First off, we very much support Communications Biology going beyond...Biology. What we mean by this is that we think that there is much value in incorporating more than solely empirical field data into our collective understanding of ecological processes. And in this case, learning about salmon-ecosystem dynamics from people with long-term cultural and ecological interactions with salmon and other components of ecosystems can provide much value.

We thank the reviewer for seeing the value in this approach.

The challenge of any paper with multiple information sources, such ‘mixed methods’ included, however, is combining the information in a comprehensive, complementary, and coherent way. Candidly, however, we feel that there is a long way to go to achieve these outcomes.

As we expand on below, one striking deficiency is the organization of the writing. It requires a lot of attention. This manuscript deviates strongly from the typical flow of scholarly paper. With an ‘hourglass’ shape, readers typically first encounter the highest-order conceptual content in the Introduction, laying bare what is known and what is not, then proceeding to objectives and hypotheses. The Discussion takes the opposite organizational form in which the results are unpacked in their narrowest sense (as they relate, for example, to the study system), then the writing broadens out again to the conceptual material first encountered in the Introduction; the authors then should explain what new insight their results (and its inference) contain for the conceptual body of literature.

Regarding the writing and structure, this is largely an artifact of this manuscript being transferred from a journal with a different format. We have corrected this so that the manuscript is more traditional in form.

In its current form, the writing is highly scattered and several ‘rules’ of writing are violated. As a prominent example, the reader encounters results (but without accompanying data or pointers to figures or tables) on line 152 of the Introduction. Similarly, there is (extensive) discussion-like material added soon thereafter (again in the Introduction).

Same comment as above.

It's our opinion, even when writing about IK, you might avoid vague phrases, even (and perhaps especially) in praise, like "...immeasurably deep in breadth and complexity". Line 59. "This is obvious to Teslin Tlingit community members". Line 144. We offer this with the best of intentions; we feel that such hyperabundant praise has the opposite effect as desired.

We have removed 'immeasurably deep in breadth and complexity'. We believe it is necessary to state that the fact that salmon are critical to Teslin Tlingit ecosystems is obvious to Teslin Tlingit community members, because it is a true statement, and is made in light of the legacy of science 'discovering' what Indigenous people already know. We have modified the sentence to include a 'qualifier': 'This is obvious to Teslin Tlingit community members, who are deeply connected to these fish...'

Similarly, the normative statements are likely best reinforced via citing (especially Indigenous) scholars who have made these same arguments. E.g., "With an expansion in interest in IK comes a moral imperative to work with Indigenous communities in a good way, with an understanding of the colonial history of research and academia, and to perform research that recognizes and supports Indigenous sovereignty." line 123...Wilson, Kovach, Reid et al. not cited until several lines later.

We have added more citations of Indigenous authors in science and ecology, and earlier on in the introduction, to point to their work without attempting to replicate it.

Similarly, the Abstract 'goes hard' right away, with first lines declaring: "Biodiversity and ecosystem function are in collapse. This has been caused by colonial pursuits of land and resources in North America. Science and ecology have perpetuated these systems, requiring a re-thinking of research and how it is done". While this statement is valid, it's not necessary to frame your study like this 'right out of the gates'. Others (especially Indigenous scholars) have forcefully and compellingly made this argument in more powerful stand-alone pieces. What we are trying to convey is the suggestion to let those powerful pieces stand alone (and reinforce them by citing them) but avoid using the very first line of a study on salmon-ecosystem dynamics to declare it. While we align with your values, we feel that this declarative statement reads like it's you making this point anew, which undercuts those who long-ago suggested it.

We appreciate the authors feedback in this regard – it was challenging to know where to draw the bounds of this study, largely because there are very few like it. We have modified the abstract to shift the scope as recommended. We have added more citations of Indigenous authors in ecology, earlier on in the introduction (and throughout), to point to their work without attempting to replicate it.

Line 144. The reader encounters deltaN15 with no background whatsoever (other than a brief 1-sentence mention of 15N on line 106) about how it's a tracer for marine-derived nitrogen

from salmon. Summarizing the 'salmon forest' literature and its tools (like this molecular tracer) earlier in the Introduction is key to understanding the analytical approach, but this is missing.

We have added background on salmon-derived Nitrogen to the introduction, and how it has been used to study salmon ecosystems.

We are pleased to read about community protocols. In particular, the authors should be commended for taking the time and effort to engage meaningfully with Indigenous research partners. This is reflected in the manuscript at various points, especially the inclusion of Indigenous authors, the positionality statement, the footnote to clarify the word 'explore', and the project development section of the methods. But while we see 'consent forms' mentioned, there is no statement assuring the readers that there was also an approved Human Research Ethics application from the university/ies involved (typically citing the application/protocol number, so its traceable). This is mandatory for all interview data collected in Canada. We note that the University of Calgary Research Ethics Board was mentioned in the accompanying pdf file, but it does not appear in the manuscript and does not have a protocol number despite this requirement being explicitly stated.

We have added ethics certificate information.

As presented, its hard to ascertain the reliability of the statistical modeling. Not all results, for example, are shared. E.g., Line 184. "For all relationships of temperature, temperature lag-1, precipitation, and precipitation lag-1, p values were >0.16 ." Readers need to see the data for parameter estimates, their measures of variation, exact p-values and so on. Right now, there is no table or figure referred to with these data (but we noticed the figures start with figure 2...is there a missing figure 1?). Additionally, the reporting of other p-values is unorthodox (e.g., " $p = 0.01 \times 10^{-6}$ "). But these examples are not as important as the absence of any accompanying figures for your main data set (deltaN15 and salmon escapement) and modeling results (as presented)...the only figures presented are those relating to salmon escapement alone (i.e., not central to your modeling). Figure 2 simply shows box plots of raw data (ie not model outputs, accounting for other variables). Additionally, its not clear what type of models you are using; there are not enough details shared to be interpreted (or replicated - the gold standard for Methods). The clues we could assess left us puzzled, like 'null climate models' (line 192), which seems to be an oxymoron; if we understand your intention, they are better described as climate-only models. Even the results narrative had some puzzling phrases, like "All models (including the negative control) had interestingly high Akaike weights for models including an unlagged salmon escapement variable" line 193. As a result of these and other shortcomings, there is not enough information and data presented to assess the modeling.

We have added several new tables and figures to provide more information on data and models that we believe meets the requests of the reviewers. We have changed the language as recommended from 'null models' to 'climate-only models', and standardized reporting of p-values. We have worked to clarify our modelling approach in the methods section.

Line by line comments:

Lines 59 – 63: These sentences at the end of the paragraph here could be combined with the paragraph beginning on line 112.

Change implemented as recommended.

Line 66: Here is a good example where a citation(s) should be included – there are many works on this topic.

Change implemented as recommended.

Line 98: The sentence “One particular area of concern regarding these declines is the role of salmon as ecological keystone species (Hyatt and Godbout 1999).” is awkward. Introductory sentences at the start of paragraphs are ideally straightforward and give the reader an understanding of what to expect from the paragraph. A more direct way of writing this might be “As an ecological keystone species, declines in salmon are likely to have adverse impacts on the ecosystems they inhabit.”

Change implemented as recommended.

Lines 116, 128, 132: The phrase “...critical to...” is repeated three times in about 15 lines here (and elsewhere in the paper). The statements are not incorrect, but consider changing up this phrase. “Key”, “essential”, or “necessary” could all be useful synonyms. However, be mindful of the overuse of such strong phrases, as they dilute the impact they have.

We have replaced ‘critical’ with synonyms in most instances.

Line 117: While unintentional, the sentence here seems to relegate IK to “filling gaps” in Western science. While it certainly can do that, it can also do much more, such as transforming how we make management decisions or informing the development of hypotheses. Adding a few examples of this (with citations) would more clearly demonstrate the many ways IK contributes to ecological understanding.

This is somewhat intentional, as that is what the scientific literature has done with IK, although this is changing. We have added more language around how the literature has a long way to go in its treatment of IK and that its worth/usefulness should not be determined by colonial systems.

Line 125: The word “good” is a bit vague. The authors expand on it a bit but “just” might be better. Moreover, it would be helpful to clarify what exactly is meant by “good” or “just”. While a full treatment of such a topic would be beyond the scope of this manuscript, a few key points (ideally with citations) would be helpful (e.g., reciprocal data sharing, input at all stages of research, etc.).

'A good way' has a specific meaning here. We have added a citation and additional explanation from the cited paper to reflect this.

Lines 149 – 182: The two last paragraphs of the Introduction section read as a Results/Discussion section rather than as an Introduction section. Perhaps this was caused by a formatting error? Most of this content should be included in the Discussion section.

The MS has been reformatted to be more conventional in form.

Results section generally: A number of sentences can probably be merged into the paragraphs above and below them. Paragraph breaks are not needed between individual sentences. As well, it is more common to simply report p-values as " $p < 0.001$ " rather than reporting the p-value to the exact decimal.

Some paragraphs in Results have been collapsed and p-value reporting standardized.

Line 195: Recommend removing the word "interestingly", as the Results section is typically where the text should be the most objective. If it a result is interesting, explain why in the Discussion section.

Change implemented as recommended.

Line 223 (Figure 2): Significant differences among groups are typically labelled by asterisks (e.g., *, **, ***) rather than A, B, C. While it is fairly clear what the authors are denoting, using the asterisk convention may help readers to understand the figure much more quickly and easily.

Change implemented as recommended.

IK Interview Results: Did any interviewees mention changes in vegetation with salmon declines? While observations of changes in tree growth are difficult, as alluded to by one participant on line 289, has anyone noticed changes in shrub/grass health, abundance, or any other vegetation attributes? This information might help to link the interviews with the scientific aspects of the manuscript (namely, the effects of salmon on plant growth).

Interviewees did not describe any such changes. Interview questions have been included as an appendix. We have added more discussion (mostly surrounding bears) based on this comment and those of the other reviewer, to better link the results from ecological and interview methods.

Line 284: Just a note to check the consistency in use of single or double quotes throughout the manuscript. While minor details such as this can be ironed out with the journal after acceptance, the less inconsistencies, minor errors, and grammatical mistakes the better. Attention to detail in the formatting and presentation of manuscripts reflects attention to detail in other aspects of the research, such as methods and analyses.

Change implemented as recommended.

Line 315: Two periods at the end of this sentence. Again, these errors are minor, but in sum detract from the communication of the research.

Change implemented as recommended.

Lines 316 – 322: This content of this paragraph has already been stated in the Introduction section and can be deleted or merged with other sections to avoid repetition.

Line 327: Change “sonar” to “salmon” for clarity.

Sonar has been changed in most instances to salmon or another appropriate term throughout.

Lines 355 – 370: This paragraph contains no citations but probably could be enhanced by connecting the statements here to the broader literature. For example, “Low flow could be beneficial to salmon by reducing water velocity and energetic requirements...”.

Relevant citations have been added throughout the paragraph.

Line 381: Here (and also line 326) is where it might be beneficial for the reader to be briefly reminded of the hypothesis (rather than the lengthier re-hash of the hypotheses near the beginning of the Discussion section). For example, instead of “The second hypothesis and prediction were strongly supported by $\delta^{15}\text{N}$ results.”, use something like “Our second hypothesis and prediction, that salmon fertilize trees and that salmon-bearing sites would therefore have higher $\delta^{15}\text{N}$, were strongly supported by our results.”

Change implemented as recommended.

Lines 418 – 422: Here is an example of an area where the authors might be able to connect the work of this study to the literature and explain why specifically the use of IK interviews was valuable. The authors discuss how interviews provided an important baseline for salmon runs, but this point could also be made more broadly to highlight a unique aspect of local/Indigenous Knowledge. Some helpful citations on this topic include:

Eckert LE, Ban NC, Frid A, and McGreer M. 2018. Diving back in time: Extending historical baselines for yelloweye rockfish with Indigenous knowledge. *Aquatic Conservation: Marine and Freshwater Ecosystems* 28: 158–66.

Jardine TD. 2019. Indigenous knowledge as a remedy for shifting baseline syndrome. *Frontiers in Ecology and the Environment* 17: 13–4.

We have added context for our results in light of these (and other) sources.

Lines 437 – 444: This paragraph is important and most sentences within could be elaborated on. For example, “Changes observed in bear behaviour are also concerning for negative human-

wildlife interactions.” (Lines 440 – 441). How, specifically, are these changes in bear behavior concerning? The authors allude to the reasons in the paragraph above, but it is never made explicit. Including additional citations in the paragraph would likely help make these arguments. For example: Artelle, K., Anderson, S., Reynolds, J. et al. Ecology of conflict: marine food supply affects human-wildlife interactions on land. Sci Rep 6, 25936 (2016).

We have opted to remove this line from the discussion. We do not want to draw focus towards this particular point as it is not a focus of the study and changes in bear behaviour were only reported by one interview participant.

Lines 445 – 454: This paragraph makes a few generalizations. While the main points being made are reasonable, it would be helpful to be specific about what aspects of this study were enhanced by including IK. The information in the paragraph beginning on line 168 in the Introduction could probably be included around this area. For example, the sentence “By building a narrative through IK instead of interpreting it through statistical analyses, knowledge systems can be braided into a bigger picture without either being subsumed by the other.” is a bit vague. If one of the goals of this manuscript is that it “....demonstrates...the importance of considering multiple ways of knowing to improve research in complex (social-)ecological systems.” (lines 37 – 39), the reader would benefit from hearing about the specifics. For instance, did the interviews inform the study topic or predictions? What would have been missed if the interviews were not conducted? Did the interviews help to build community investment in the project? Answering these types of questions might allow a reader studying a different area or a different species to draw parallels and design their research accordingly. It would also help this manuscript appeal to a more international audience.

We have provided concrete examples on how both knowledge systems contribute to an ecosystem-understanding of our results and how IK shaped our research process.

Line 501 – 502: This sentence could simply be merged with the paragraph above.

Change implemented as recommended.

Line 606 (and elsewhere): Recommend denoting R packages with quotes or italics to clearly identify them (in this case on line 606, “car” or car).

Change implemented as recommended.

Line 626 (Interview methods): Would it be possible to provide the interview questions as an Appendix or supplementary information? If not, some additional clarification about the types of questions asked would be useful (and necessary for open science and reproducible work) Lines 639 – 641 provide little information on the “set list” of questions. While semi-structured interviews by nature allow interviewees to expand upon the questions (as the authors note here), the general questions asked, or topics covered, would be useful for the reader to better

understand the conversations. In addition, future efforts to repeat or emulate this study in a different area or context would benefit.

Interview questions have been provided as an appendix.

Table 3: Recommend using a set number of significant figures for presenting results. In this table, there seems to be switching between different numbers.

Reviewer #3 (Remarks to the Author):

This paper provides a somewhat novel approach of blending Indigenous Knowledge with ecological methods to test for links between salmon and tree growth, including consequences of decline of numbers of fish. This is a powerful approach, and I thought the interviews were very insightful and helpful. Most of my concerns are with the ecological side of this paper. Overall, I felt that results that did not support the predictions were not given sufficient discussion.

We thank the reviewer for their support of our approach. We have added additional figures and table information on the results, as well as given more consideration of uncertainties and predictions that were not supported, specifically surrounding ^{15}N isotopes and site mean tree growth.

We really need to see graphs of the relationships between numbers of salmon and tree growth. Sole reliance on AIC tables is simply not sufficient without a clear visualization of the data. It feels like the authors are sabotaging their own study without such graphs, and I would not support publication without them.

We have added the graphs requested, as well as a more detailed Table 3.

It is not clear how the predicted percentage increases in tree growth due to the influence of salmon were calculated. These are potentially very risky extrapolations, especially if they are based on comparison with just the one control site.

They are not based on comparison with the negative control site, but were calculated based off of the effect size of salmon variables in the tree growth models. The calculation of these values are described in the methods.

I don't understand the definition of a "positive control site". We are told that many chinook salmon carcasses accumulate there. Without knowing how many carcasses accumulate at each of the salmon sites, it is not possible to tell what "many" means relative to the 3 "salmon stream sites". Why not simply call it a "salmon site"? Note that whatever the authors call it, it should be included in the tally of the number of streams that either did or did not support the authors predictions, with the various analyses. I am suggesting this because by having three

kinds of site, and different scales of analyses, it is sometime hard to see the bigger picture.

We have attempted to clarify the purpose and use of the different site types throughout the manuscript. We included multiple site types to compare results across gradients of salmon (none/some/many). The negative control was used in modelling tree growth to look for a confounding variable or upward bias in the salmon variable in the modelling of growth at salmon sites. We discuss the implications of the negative control tree growth model results for the salmon site tree growth models in the discussion.

There is no description of the waterfall site. How can the authors rule out the possibility that the characteristics of the riparian zone that affect tree growth did not differ above the waterfall compared to the other sites? Data should be presented on this if possible, or else a more nuanced interpretation of the results should be given.

We have presented the data available, and pertinent ecological factors that were controlled for (i.e all sites were mature stands of white spruce, alder-free, no slope, 25m from the riverbank, etc.). In lieu of more data, and in light of other comments from the reviewer, we have refined our interpretation to draw more attention to the study limitations than was previously included, and use less certain language around some aspects of our results. We have provided a more nuanced discussion of our 15N results.

The authors state: “The physical and ecological similarity between sites (long-lived, riparian, alder-free, white spruce stands, which only form relationships with ectomycorrhizal fungi), although not accounted for statistically, suggest that none of these factors should vary between them.” This is an overly strong statement. What about differences in soil moisture, aspect, and slope? Some mention is made later of the importance of soil moisture for stable isotopes of nitrogen. Isotopic results need to be treated with extreme caution due to the complexity of their interpretation. Physical and biotic data should be presented for each site, if the authors have this information. I was not able to download supplementary information so I’m not sure if it's there.

We have provided some additional information regarding site physical characteristics in Table 1. We do not have any additional data to provide on these sites. We considered taking soil samples but were informed that this could potentially disturb shallow graves throughout the territory and decided this was not worth that risk.

“Many replicates would have to be included in each site type for a more reliable and informative analysis of differences in mean growth, which was not possible in the study area due to few known locations of negative control sites.” There may be few sites with no salmon, but that is not the only kind of comparison used in this paper: the authors could have made use of additional sites that vary in the number of salmon. I’m not saying this should have been done; there are limits to what can be accomplished. But the focus only on lack of control sites is not the only limitation.

This was our intention with the inclusion of a positive control. We have included more language to describe this in the methods, and discuss the positive control more in our discussion.

My reading of Table 3 is that there was no effect of salmon escapement on tree growth at the positive control site (Teslin). I did not see any discussion of why not. It seems important to the overall conclusions of the paper.

This interpretation is correct – we have added our interpretation of this to the discussion. We believe the accumulation of carcasses at this site results in lower sensitivity to changes in annual salmon abundance, as this site would receive a more consistent supply of nutrients.

Related to the point above, the fact that one of the four salmon sites had significantly lower tree growth than the control sites is mentioned briefly, but then not again. It should be mentioned more prominently, including in the Abstract, to temper the conclusions. If the authors have any speculation as to why this occurred, it would be helpful to place this result in context with the other findings.

We have added this result to the abstract, and added more limitations surrounding this result. We want to make clear that differences in mean tree age between sites makes comparisons of average growth somewhat unreliable in testing the hypothesis/prediction.

I did not see any discussion of any potential limitations from having interviewed only three participants. These interviews have been very helpful and are an important and novel part of the paper. Is there a way to respectfully consider what additional insights might have been gained if more people had been interviewed?

We have added a sentence to the discussion on this, recognizing that interviewees had unique insights, of which more could have been captured with more interviews.

Minor comments

Much of the literature cited is quite dated. For example, “Pacific salmon experience threats in both freshwater and marine environments during their life cycles, support large commercial fisheries (Clark et al. 2006).”

In this instance, and wherever possible, we have tried to provide more recent references.

There is also a lot of more recent literature available on effects of salmon nutrients on riparian ecology. The findings with respect to bears could benefit from discussion of work by Quinn on relationships between bears and salmon, and the discussion of an increase in bear-human conflict could consider the analyses by Artelle et al.

We have removed discussion of human-bear conflict from the discussion as we see it as a minor result (only reported by one participant). We instead draw focus to recent literature, as

recommended, on salmon-bear interactions and the role of bears in delivering salmon nutrients to terrestrial ecosystems. We think this is a very important contribution to the MS and thank the reviewer for this suggestion.

Table 3 is the first one mentioned in the text.

We have corrected table/figure numbers.

It would help if the title of Table 3 specified which salmon data were being analyzed (presumably the full Yukon River estimates, based on the text). Table 5 uses just the 5 years of Teslin data.

Change implemented as recommended.

We thank the reviewers for the time and effort invested in our manuscript, providing thoughtful and constructive critique. We believe the manuscript is much improved because of it, and of high-quality for publication. The feedback from reviewers 1 and 2 have largely been incorporated as-is. We have incorporated the feedback from reviewer 4, with the notable addition of a ‘Bridging Knowledges’ section to more effectively tie together different forms of knowledge in the manuscript. We provide important rebuttal to the recommendation to use an alternative data source in our work, with detailed rationale provided below in the line-by-line responses to the authors. We thank you for the opportunity to have our work reconsidered and look forward to your thoughts.

Reviewer #2 (Remarks to the Author):

This paper is much improved from the previous version, and the authors have clearly put in plenty of additional effort to highlight the value of this study. The language is clearer, the structure is far better, and the results (including tables and figures) are more conclusive. The authors have done well to incorporate the previous feedback and, along with their other changes, have made a more compelling case for the impact of salmon on tree growth in an inland region. A few suggested edits are provided below that would improve the manuscript further. In particular, however, there are still a number of grammar and syntax errors throughout the manuscript. Some examples include:

We thank the reviewers for their positive feedback, and new comments which help to further refine the manuscript.

Line 233: “Including both a positive and negative control site allows to make comparisons...”.

Reworded this sentence (line 297).

The sentence on line 401 should begin with “The lowest...”.

Corrected.

Line 417: “an” should be “and”.

Corrected.

Line 495: “...around two weeks years into...”.

Added context for clarity (line 587).

Line 635-636: “challenging to in cross-site”.

Corrected (line 741).

Although these are mostly minor errors individually, they (and others throughout the manuscript) collectively detract from the paper. Additional proofing would help to iron out these details and,

in many cases, improve the overall clarity of the message.

Abstract (Line 36): The sentence “This study was limited by available sites and data, however demonstrates that the health of salmon and riparian forests on the TTC Traditional Territory are deeply linked, and the importance of considering multiple ways of knowing to improve ecological research.” is awkward and grammatically incorrect. I would recommend breaking this into two separate sentences, as considerable time is spent in the discussion explaining the substance of this sentence. The Abstract is perhaps the singular most important part of a publication, as it is what the majority of readers will engage with first (and for many, the only thing they will read unfortunately). As such, it should be well-polished and free of such errors. In addition, the authors do not do themselves favors by bringing up their limited sites and data. While explaining limitations is important and necessary in the paper, the results should still be able to stand in the abstract. Perhaps a better way of phrasing it would be something along the lines of “Although this study was limited by available sites and data, we demonstrate that...”.

Change implemented as recommended.

Lines 63 – 69: I would recommend moving or integrating this portion about the value of Indigenous Knowledge systems to the paragraphs beginning on line 143, where there is more thorough treatment of the topic. As is, the opening paragraph begins with biodiversity/fish losses and its impact but by the end is discussing IK systems. I would suggest making the paragraph more directly about the effects of declines in freshwater, and perhaps expanding a bit on the effects of fish declines, especially for Indigenous peoples.

We have incorporated feedback on expanding on fish declines and Indigenous Peoples (Lines 71-77). Based on this feedback and from the other reviewer, we have broken the first paragraph of the introduction into two. We think it is important to keep discussion of Indigenous Knowledge at the start of the introduction to keep this a central focus of the paper, and not an afterthought of the ecological aspects of our work (understanding that weaving these narratives is challenging).

Line 66: Here, the phrase “...with a long way still to go...” is an example of vague language that is not very ‘scientific’. People can interpret this phrase in a number of ways, and it provides little added detail. I would suggest being more direct and linking it to the next sentence a bit more. For example... “Indigenous Knowledge Systems are increasingly valued in scientific literature and management, especially in fisheries, though efforts to combine knowledge systems to transform research processes is slow and often fraught with social and analytical challenges. Yet, this transformation is needed...”.

We have expanded this sentence to communicate a more specific meaning (Line 78).

Line 85: Suggest some wordsmithing to make this sentence flow a bit better, such as: “Pacific salmon are a significant marine nutrient subsidy to otherwise unproductive aquatic and riparian environments, as the vast majority of salmon growth occurs in the ocean and mortality and decomposition is concentrated in freshwater environments after reproduction.”

Change implemented as recommended.

Line 94: Recommend moving this sentence “Numerous vertebrate species...” to earlier in the paragraph after the sentence beginning on Line 85.

Change implemented as recommended.

Line 97: Recommend briefly defining “15N” the first time it appears.

We have added a brief description of 15N (line 136).

Line 173: Recommend replacing the dashes with commas in this sentence for clarity. While it is not grammatically incorrect, there are four dashes that appear in a single line which is a bit confusing at first glance.

Change implemented as recommended.

Figure 1: A good convention is to remove the grey grid lines in figures and replace them with x and y-axis. This would also make the legend in plot B more visible.

Changes implemented as recommended (and font size increased as recommended in later comment).

Figure 2 is difficult to assess, as it appears in the PDF that the x-axis has been obscured by the figure caption text, though I assume it is Yukon River escapement data. This figure would benefit from a bit of formatting though, specifically by eliminating the grey lines and increasing the font size of the axis.

Changes implemented as recommended.

Figure 3 and figure 4 are overall well designed and clear figures. The large font on the axis, for example, is likely to be helpful for readers. I would recommend adjusting the other figures to have similarly large text on the axis so that the information is very visible. For figure 3, I would recommend briefly defining the asterisk values that denote significance in the caption.

All figures have been adjusted and standardized as recommended.

Table 3 is quite ‘cluttered’ in format, but this may be fixed by the journal formatting specifications. Additionally, in the caption, “...*** $p < 0.00$ ” should be “*** $p < 0.001$ ”. Finally, and I may be missing or confusing something from the main text, but why was escapement used as abundance in the Upper Morley model, when all others used the 2-year average? Table 4 also indicates that this was the only model with an AIC weight that wasn’t escapement, which is curious. Can this be clarified?

The reviewer has correctly understood the text – the error here was in the superscripts within the table (now corrected). We have also made some small formatting changes and included

additional text to the table caption for added clarity. We welcome any other feedback from the journal editors on formatting.

Line 511: Here, and in a few other areas of the paper, there are paragraphs composed of only a couple sentences. These should be eliminated where possible and combined into the paragraphs above and below.

Change implemented as recommended.

Line 565: Can you explain more clearly why “Model Akaike weights indicated that nutrients are made available to, and taken up by, trees primarily within the same year, and into the following after a given salmon run...”? I think I know what is being alluded to with regards to escapement models being weighted higher, but clarity is needed here. Also, it seems as though the Upper Morley site was the only site where any model other than annual escapement was ranked higher (in this case, 2yr escapement average). I am a bit confused by the phrase “...and into the following after a given salmon run...” when none of the lag-1 models were weighted highly, and line 409 states “Escapement lag-1 models were weighted very low at all sites.”

This statement comes from the non-autoregressive 2-yr average of Yukon River salmon escapement model being weighted highest, as well as the Teslin River salmon abundance lag-1 model being weighted highest at the Upper Morley site. The author is correct that most models support same-year impacts on growth, and additional language has been added to the text to reflect this (lines 661-664). We have also added a line to help clarify this in the Results (line 521).

Line 614: This opening sentence does not make sense and awkwardly states the second hypothesis. But here, and elsewhere, some confusion is created by terms such as “annual mean growth”. Because multiple measures of tree growth were used in this study, it is helpful for readers to easily be able to distinguish them. I recommend picking a term for each and sticking to it throughout the paper, or clarifying within each sentence what is being talked about (e.g., “tree growth measured as...”).

We have modified this sentence (Line 713) and added more specificity where necessary when discussing growth throughout the manuscript.

Line 662: I suggest switching the order of the first and second paragraphs of the “IK Interviews” section. Overall, they read well and offer some insight, but the second paragraph discusses the results of this study, while the first paragraph, as well as the one beginning on line 702 speak more broadly about IK and interviews.

Change implemented as recommended.

Line 705: Here, the authors state that IK helped in “...refining research questions...” among other things. Yet, there is no mention of this in the introduction when the hypotheses are introduced, and this feels a bit ‘attached’ to the end of the paper. The authors have included some great explanations of the benefits of IK in the ‘Project Development...’ and discussion sections,

but it would also be good to highlight the value of the partnership when the hypotheses and objectives are laid out in the introduction. Even simple additions such as “Based on consultation with community members...” or something to that effect would go a long way toward reinforcing the benefits of IK/science partnerships at all stages of the research process.

We appreciate this comment and have added a line to the intro to reflect the co-developed research questions (Line 223).

Line 715: I recommend changing this sentence to remove the statistical analysis part, to something more along the lines of “By building a narrative with IK, knowledge systems can be braided...”. Information derived from IK can, in some cases, be analyzed statistically. Many authors argue that there are benefits of doing so when done appropriately.

Change implemented as recommended.

Reviewer #4 (Remarks to the Author):

I commend the authors for their work on this paper. Working in the space of Western Science and Indigenous Knowledge is hard and messy. Yet, as the authors state in the paper, is the avenue forward for decolonizing ecology, promoting reconciliation and platforming indigenous knowledge and ways of knowing.

We thank the reviewer for their support for this work.

However, in my reading of this manuscript, it almost felt like I was reading two disconnected pieces of work: 1) a western science approach to look at salmon subsidies to trees; and 2) a tangential survey of indigenous knowledge in the region. Perhaps that is the point, and one of the reasons why there has been slow progress in this area. In my opinion, neither piece on its own would merit a standalone publication, but they aren’t as complimentary as I would have hoped reading the title and the abstract.

So, the question becomes, can these pieces be better integrated in this manuscript? Maybe providing a bit more rationale on why the Teslin were interested in looking at tree growth would provide some additional motivation? Alternatively, many of the Nations that I work with have a concept of interconnectedness (e.g. Hishuk ish tsawalk in the Nuu-chah-nulth culture: everything is one) – do the Teslin have something similar that would help tie the strands together? Did this research question come from a similar place?

We thank the reviewer for their feedback, and recognize the value in further describing the rationale of our methodologies to better integrate the manuscript. We have added to our *Project Development with Teslin Tlingit Council* section, as well as added an additional section to the methods, titled ‘*Bridging Knowledges*’. We believe this section will allow readers to better understand our motivations, how the two types of knowledge in the manuscript relate, and prepare them for the discussion section which considers both knowledges side by side and the value in doing so.

We would not categorize the interviews conducted as tangential, and agree with the author that the point of these types of studies are to in fact gather different types of knowledge, that are not directly comparable but rather lend their different strengths to a greater ecological understanding. This is communicated in the manuscript in '*Bridging Knowledges*'.

All this to say, I think there is some important and necessary work here and I would hate to see it dismissed by the broader community because it isn't tied together in the most effective manner.

My other major comment relates to the salmon data used as a predictor variable. Specifically, more detail is needed on what specific data were used and from where, and there are also other data sources that may be more appropriately used as predictors. Specifically:

-In the methods L278-292, are you using border passage here? Or total Yukon returns?

We are using escapement estimates, as stated throughout *Data Sources*. These are generated by the YRJTC from border passage and fisheries data.

- Connors et al. 2022 uses genetics at border passage to derive an estimate of escapement to the Teslin system. Using this time series would be much more defensible than the Yukon aggregate.

See comments below on Connors et al. data.

- Overall, this whole section would benefit greatly from a discussion of the diversity of salmon within in the Yukon. The Teslin area is one of nine salmon Conservation Units, and within the Teslin Conservation Unit there are a number of populations or streams. These nine Conservation Units appear to largely respond synchronously to environmental drivers (at least in recent years; Murdoch et al. 2024). So, perhaps the assumption that border passage approximates Teslin escapement is warranted. But, either way, some more text is needed here explaining the hierarchy of salmon diversity, why datasets were chosen, and what the major assumptions are (e.g. that border passage~Teslin escapement).

We have included more information on Yukon River and Teslin River basins Chinook population structure (Line 347), including . See below for response to other comments.

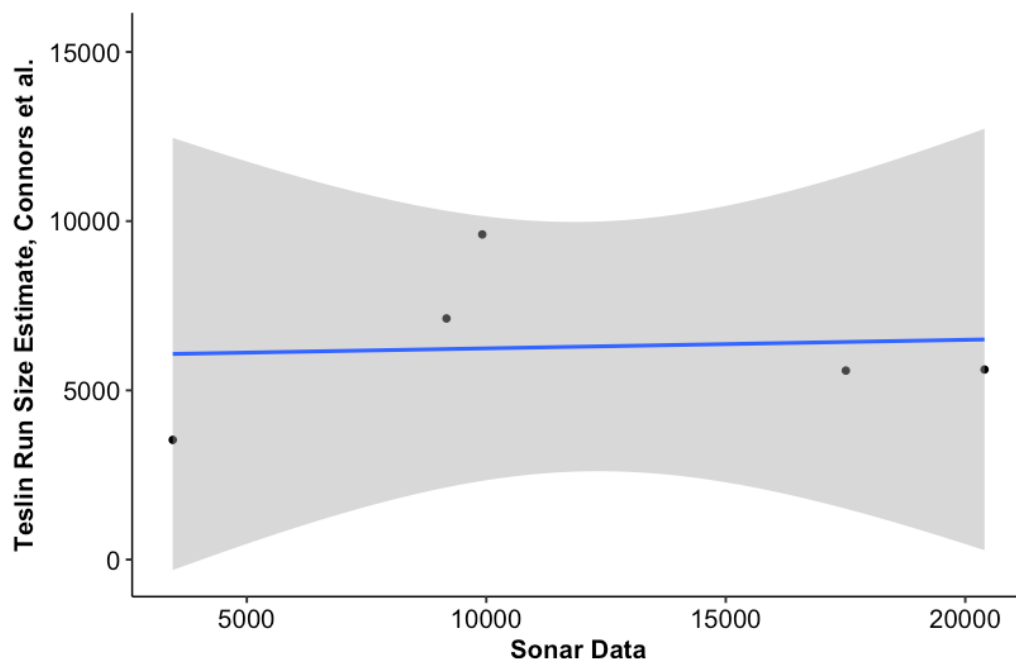
Connors, B. M., Siegle, M. R., Harding, J., Rossi, S., Staton, B. A., Jones, M. L., ... & Sutherland, B. J. (2022). Chinook salmon diversity contributes to fishery stability and trade-offs with mixed-stock harvest. *Ecological Applications*, 32(8), e2709.

Murdoch, A., Connors, B. M., Lapointe, N. W., Mills Flemming, J., Cooke, S. J., & Mantyka-Pringle, C. (2023). Multiple environmental drivers across life stages influence Yukon River Chinook salmon productivity. *Canadian Journal of Fisheries and Aquatic Sciences*, 81(1), 97-114.

We thank the reviewer for directing us towards these resources. Regarding the use of Connors et al. 2022 data for modelling tree growth: These data were not initially considered as analyses

were conducted prior to the publication of Connors et al.'s work. Considering it now, we do not believe it to be suitable in this work for two important reasons:

- (1) These data are total run size estimates, not escapement estimates, so do not reflect the number of fish that actually remained in the system to spawn, i.e. have impacts on ecosystems. The difference between total run size and escapement in the Canadian portion of the Yukon River was highly influential, pre ~2005, and in some years following (as shown in Figure 1 of the manuscript).
- (2) Equally concerning, is that the data for the Teslin system run size from Connors et al. 2022 is highly inconsistent with fish abundance measured by the Teslin sonar (2011-2015). Running a linear regression of the two data sets shows a near 0 relationship (slope = 0.025, $p = 0.903$, see following plot), with measured salmon abundances in the Teslin River falling extremely far outside of Connors et al.'s 95% confidence intervals in multiple years.



On the other hand, Teslin Sonar data is consistent (i.e. synchronous) with the Yukon River estimates, as suggested by the reviewer, leading us to believe this remains the better data set for the purposes of our research.

Specific suggestions

L 29-32

“Tree growth was positively related to salmon escapement” and “mean site tree growth did not show a clear pattern related to salmon” appear to be contradictory.

The difference is in within-site vs between-site comparisons, i.e. within sites growth was positively related to salmon abundance, but this did not result in sites with more salmon carcasses having the highest absolute mean growth, as described in the results and discussion. We see how this can be confusing for a reader engaging only with the abstract

and have added some clarifying language (lines 29,32).

Suggest splitting paragraph 1 of the introduction. This one starts with a strong, clear topic sentence, but then meanders into Indigenous Knowledge Systems

Change implemented as recommended.

L55-57: Interesting focus on freshwater ecosystems here, considering that salmon recruitment is typically linked to changes in the marine environment (e.g. see Pearcy 1992; Beamish and Mahnken 2001; Peterman and Dorner 2012). Perhaps salmon (and other anadromous species) experience the worst of both worlds from both marine and freshwater pressures (e.g. Waldman and Quinn 2022)? I think a more relevant tack for this paragraph may be to focus on the plight of anadromous fishes.

Beamish, R. J., & Mahnken, C. (2001). A critical size and period hypothesis to explain natural regulation of salmon abundance and the linkage to climate and climate change. *Progress in Oceanography*, 49(1-4), 423-437.

Pearcy, William G. (1992). *Ocean ecology of North Pacific salmonids* / William G. Pearcy. Seattle : Washington Sea Grant Program : Distributed by the University of Washington Press

Peterman, R. M., & Dorner, B. (2012). A widespread decrease in productivity of sockeye salmon (*Oncorhynchus nerka*) populations in western North America. *Canadian Journal of Fisheries and Aquatic Sciences*, 69(8), 1255-1260.

Waldman, J. R., & Quinn, T. P. (2022). North American diadromous fishes: Drivers of decline and potential for recovery in the Anthropocene. *Science Advances*, 8(4), eabl5486.

We have shifted some of the focus of this paragraph to anadromous fish specifically, and agree that this is helpful for this paper's focus on salmon (lines 63-67).

L71: Careful with the word productivity here. Suggest defining it as it means different things to different audiences.

We have briefly defined productivity (line 97)

L101: repetitive with above

Removed previous line.

L111: see also Reimchen and Fox (2011)

Reimchen, T. E., & Fox, C. H. (2013). Fine-scale spatiotemporal influences of salmon on growth and nitrogen signatures of Sitka spruce tree rings. *BMC ecology*, 13, 1-13.

We have incorporated this citation on line 167.

L123-124: should also cite Murphy et al. 2017. The reasons underlying salmon declines are complex and multifaceted, but the strong relationship between Bering Sea catch-per-unit-effort and returns points to a strong marine effect.

Murphy, J. M., Howard, K. G., Gann, J. C., Ciciel, K. C., Templin, W. D., & Guthrie III, C. M. (2017). Juvenile Chinook salmon abundance in the northern Bering Sea: implications for future returns and fisheries in the Yukon River. *Deep Sea Research Part II: Topical Studies in Oceanography*, 135, 156-167.

We have incorporated this citation on line 178.

Paragraph beginning on L116

This paragraph should be tightened up or split. Needs a topic sentence with supporting information.

We have split this paragraph and merged the first part with the previous paragraph.

L182: Not a complete sentence, add 'we predict that'

Change implemented as recommended.

The 'western science' part of the introduction with tree growth and isotopes generally could stand from a more thorough examination of the literature. Based on the works of Reimchen, Naiman, Reynolds etc. what is the remaining gap and what does this study do to help address it?

Our contribution in the area of salmon-tree studies is largely through applying the tools developed by researchers like Reimchen and Naiman in a novel environment (the Yukon). We have tried to make this clear in the introduction (line 169), and throughout. Our contribution is also through expanding the methodological tools available to ecologists, in tree-salmon studies or otherwise, through engaging with community knowledge. Our goal was not necessarily to further develop nitrogen isotopes as a tool for studying salmon/their ecosystems.

L301: Not at all my area of expertise, but would precipitation matter less early in the season when presumably temperatures are above 0 but there is still snow on the ground.

The use of growing season precipitation was chosen as precipitation early in the season as snow/rain could have influence later in the season through either later release or through storage. Research from the Yukon shows that in some cases snow pack may actually be

more important to white spruce growth than summer precipitation (which we have added to the manuscript, line 395):

Dearborn, K. D., & Danby, R. K. (2018). Climatic drivers of tree growth at tree line in Southwest Yukon change over time and vary between landscapes. *Climatic Change*, 150(3), 211-225.

However, the effects of temperature and precipitation on white spruce in the Yukon are spatially and temporally variable:

Griesbauer, H.P. and S.D. Green. (2012). Geographic and temporal patterns in white spruce climate–growth relationships in Yukon, Canada. *Forest Ecology and Management*, 267, 215-227, <https://doi.org/10.1016/j.foreco.2011.12.004>.

Chavardès, R.D., Daniels, L.D., Waeber, P.O. et al. (2013) Unstable climate–growth relations for white spruce in southwest Yukon, Canada. *Climatic Change* 116, 593–611. <https://doi.org/10.1007/s10584-012-0503-8>.

L343-347: way more detail is need here to understand what is going on

It would be helpful to know what kinds of details the reviewer is requesting.

L408: d15N' AIR' variable should be renamed. Unclear if it refers to 'air' or the variable $A \times I \times R$. See Edwards and Auger-Methe

Edwards, A. M., & Auger-Méthé, M. (2019). Some guidance on using mathematical notation in ecology. *Methods in Ecology and Evolution*, 10(1), 92-99.

It is our understanding that reporting the standard used for expressing isotopic concentration in the subscript is best-practice in ecology, to reduce ambiguity in how isotopic results are reported:

Alexander L. Bond, Keith A. Hobson "Reporting Stable-Isotope Ratios in Ecology: Recommended Terminology, Guidelines and Best Practices," *Waterbirds*, 35(2), 324-331, (1 June 2012)

We may have caused some confusion with all-caps; this has been corrected and a line was added to the methods for clarity (line 344).

Discussion

I like how the discussion is laid out in terms of hypotheses. I would support the inclusion of a table of hypotheses.

The authors defer to the editors on whether this is appropriate for *Communications Biology*.

L581-591: Could conversely be argued that more extreme environments may be more limited by nutrients

L614: not a complete sentence

Added to the sentence, line 713.

L616: Reintroduce acronyms in each section if allowed by the journal

The authors defer to the editors on whether this is appropriate for Communications Biology.

Table 5 (and throughout) – did you account for multiple comparisons? If not, you probably should

The authors are not sure what the reviewer means by ‘multiple comparisons’.