**Reviews and their reconciliation for manuscript submitted to Transactions of the American Fisheries Society, Meyer et al. 2022**

5/24/2022

*A note to reviewers and co-authors*

In February 2022 the manuscript, “Landscape characteristics influence projected growth rates of stream-resident juvenile salmon in the face of climate change in the Kenai River watershed, southcentral Alaska" was submitted to Transactions of the American Fisheries Society (TAFS) by Meyer et al. The manuscript was accepted conditionally, pending revisions. The reviews from two peer reviewers and one associate editor were received by email on April 7, 2022.

The reviews and recommended edits are listed below in black text, along with responses and reconciliations below each subject in blue text.

These edits and reconciliations are subject to USGS review prior to resubmission to TAFS. We look forward to providing TAFS editors with a manuscript incorporating the recommended revisions.

Sincerely,

Benjamin Meyer

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---------- Forwarded message ---------  
From: **Trent Sutton** <[onbehalfof@manuscriptcentral.com](mailto:onbehalfof@manuscriptcentral.com)>  
Date: Thu, Apr 7, 2022 at 12:10 PM  
Subject: Transactions of the American Fisheries Society - Decision on Manuscript ID TAFS-2022-0016  
To: <[benjamin.meyer.ak@gmail.com](mailto:benjamin.meyer.ak@gmail.com)>  
CC: <[tmsutton@alaska.edu](mailto:tmsutton@alaska.edu)>

07 April 2022  
  
Dear Mr. Meyers:  
  
Thank you for submitting your manuscript entitled "“Landscape characteristics influence projected growth rates of stream-resident juvenile salmon in the face of climate change in the Kenai River watershed, southcentral Alaska" to Transactions of the American Fisheries Society (TAFS). Comments on your manuscript were received from two peer reviewers and the Associate Editor (AE) assigned to your manuscript.   
  
The review team, including myself, agreed that, subject to a revision, your study and resulting revised manuscript could provide a useful contribution to the fisheries literature that is of interest to the readership of this journal. The review team provided a number of comments, questions, and concerns that will serve you and your co-authors well in guiding your revision (the comments are included at the bottom of this letter for the reviewers and the AE).   
  
I have a couple of additional comments, with the first in regard to color figures – do the authors wish to retain the figures as color (all figures except figure 4) or go with black-and-white? If the former, then there is an additional color page charge (I wanted to be sure that the authors were aware of that expectation).

TBD

My second comment is in regard to following AFS format guidelines. The equations list on page 51 needs to be removed, that information can be incorporated within the text.

The equations and symbols page has been removed. The equations remain in place in the main text, and the two symbols (degree symbol and plus/minus sign) are common enough as to not require stand-alone definition.

The largest concern that I have is that this manuscript appears to be a thesis chapter and, as a result, is much too long for publication (96 manuscript pages). We shoot for manuscripts in the 40-50 page range. Even without the appendices and supplement, the manuscript is 63 pages long. I think that there are some easy ways to cut this down (e.g., the aforementioned equation page, the footnote page [page 35]. However, some editing will require more work.

For example, you have 14+ pages of references which is too much, so please cut down considerably this section to only those references that are most needed.

The reference list has been reduced from a list of 14 pages to XX pages

The text itself is 32 pages, which includes a management implications section (TAFS is not a management journal, so this needs to be removed).

The management implications section has been removed, and integrated with the other text where possible.

The conclusions section is a page and a half long. TAFS does not regularly publish manuscripts with separate conclusions sections and conclusions are typically a single paragraph.

The conclusions section as a stand-alone item has removed. Its contents have been shortened and integrated under “Discussion / Juvenile Salmon Growth Under Future Warming Scenarios” where possible.

There are several other areas that should be trimmed as well, in particular the methods. In short, this manuscript needs to much more streamlined to be published in Transactions of the American Fisheries Society.

The methods section has been reduced where possible, particularly in the text regarding how end-of-summer size was chosen as the growth metric to compare among groups.  
  
In order for us to evaluate your revision, I highlight the requirement to prepare a point-by-point response to the comments and questions. It is important to note that to be considered for publication in TAFS, the manuscript will need to meet each of these stipulations. To start the revision, please click on the link below:  
  
\*\*\* PLEASE NOTE: This is a two-step process. After clicking on the link, you will be directed to a webpage to confirm. \*\*\*   
  
<https://mc.manuscriptcentral.com/tafs?URL_MASK=87a2cc9587674567b338e91dae89b815>  
  
This will take you directly to the section of the site where you can submit your revision. Please (1) update the information provided as needed, (2) upload the file(s) containing your revised manuscript, and (3) upload a separate file containing detailed, point-by-point responses to the reviewers’ and editors’ comments. Using the drop-down menu, designate the manuscript files according to type (“Main Document,” and so forth); designate the file containing your responses as “Response to Decision Letter.”  
  
This link will remain active until you have submitted your revised manuscript. If you begin a revision and intend to finish it at a later time, please note that your draft will appear in the “Revised Manuscripts in Draft” queue in your Author Center.  
  
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IMPORTANT:  Your original files are available to you when you upload your revised manuscript.  Please delete any redundant files before completing the submission.  
  
Because we are trying to facilitate timely publication of manuscripts submitted to Transactions of the American Fisheries Society, your revised manuscript must be uploaded within 30 days (no later than 07 May 2022).  If it is not possible for you to submit your revision by this date, we may have to consider your paper as a new submission.  
  
Once again, thank you for submitting your manuscript to Transactions of the American Fisheries Society. I look forward to receiving your revision of this manuscript.  
  
Sincerely,  
Trent Sutton  
Editor, Transactions of the American Fisheries Society  
[tmsutton@alaska.edu](mailto:tmsutton@alaska.edu)  
  
  
Reviewer(s)' Comments to Author:  
  
Reviewer: 1  
Comments:  
The submitted manuscript highlights results from a bioenergetics model used in the Kenai River watershed, Alaska. The authors ran model simulations to estimate the effects of increasing temperatures on the growth rates of juvenile Chinook and coho salmon. Findings from this manuscript have important implications for the management of sensitive salmonid species and their habitats given projected climate change. The manuscript was a pleasant read and was well written. I would be excited to see this published in TAFS following moderate revisions. Namely, I found the authors’ air temperature-water temperature model to be overly simplified for the purposes of this study, and the poor model fit is reflective of this. My comments are provided below.  
  
Major Comments  
  
Lines 288-304: I think there needs to be more justification for the linear air temperature-water temperature model and the incorporation of water temperature in the bioenergetics model. So you used weekly average air temperature and water temperature values to fit a temperature model, the outputs of which were used in a bioenergetics model run on a daily time step for monthly climate estimates? There are a lot of confounding time scales in there…Why not use present-day average air temperature and increase by x number of degrees according to climate estimates?

You could take things further and incorporate daily variability in multiple model runs to estimate uncertainty. Another thing to consider is that air temperature and water temperature are both autocorrelated processes, which means the temperature on day x is more likely to be similar to the temperature on day x-1 than on day x-10. By using a weekly/monthly value, you’re glossing over a lot of complex thermal processes and natural variability that is likely to influence your bioenergetics model output.

We note and appreciate the reviewer’s input regarding the water temperature modeling approach used in this manuscript. We agree that other approaches could more accurately represent natural processes. However we argue that our approach is appropriate for the application in this manuscript and revising the temperature modeling approach would not produce outcomes that would change overall interpretation.

First we will again describe the temperature modeling approach. The manuscript text has been revised (lines 264-269) to clarify and reflect the description below:

Step one, growth simulations based on 2015-2016 field observations: we used observed daily average temperatures as inputs for model runs on a daily time step.

Step two, growth simulations based on projected temperatures: we used weekly average air and water temperatures to fit linear regressions for each field site. We then used monthly decadal mean projected air temperatures to project monthly mean projected temperatures, which were run at a daily time step.

In the approach proposed by the reviewer, in which we would use increase present-day average air temperature by x number of degrees according to climate estimates, we would still need a site-specific way to relate air temperature to water temperature with sensitivity values, which returns to the approach we employed. We followed the approach outlined in Mauger et al. 2017, which linearly regressed weekly mean air and water temperatures. For such regressions, weekly or monthly mean values produce best correlations (Erickson and Stefan 2000).

While this approach oversimplifies natural thermal variability, it is suited to the time scales in this application, which employs downscaled projected monthly decadal mean air temperatures for 2030-2039 and 2060-2069. Projecting water temperatures at a more temporally granular daily scale is arguably not a good match for projected monthly decadal mean temperatures.

Modify discussion to incorporate caveats …. Or refer to existing caveats already in there…

Lines 337-340: You will need to include a more detailed description of the bioenergetics model for readers who are not familiar with it. Please specify the structure of the model (Growth = C – R – SDA – F – E). How did you run the model, in the R Shiny app? I see that you included details in the Supplementary Materials, but it would still be nice for readers to have more explanation in the main document.

I modified the section at line XXX to state the following: “We used Fish Bioenergetics 4.0 in R Shiny, which allows users to simulate fish growth based on the Wisconsin Bionergetics model (Hansen et al. 1997; Deslauriers et al. 2017). The model treats growth rate as the net balance from energy intake (food consumption) minus energy costs (metabolism, activity, digestion).” The end of this section also points readers towards the supplementary information section.

It’s still unclear how the temperature data were incorporated into the bioenergetics model for the climate scenarios. Were values extrapolated from month to month or did you just assign a single value to each month?

Figure 5 indicates that for the simulations using projected temperatures, we used monthly water temperature values.  
  
Minor Comments  
  
Line 20 – remove extra parentheses

I have removed the parentheses in this sentence.  
  
Line 53 and throughout – I think TAFS capitalizes “salmon.” Check for consistency.

Throughout, I have capitalized the word Salmon in the cases of “Chinook Salmon,” “Coho Salmon,” or “Chinook and Coho Salmon.”  
  
Line 55 and throughout – It’s generally best to move references to the end of a sentence unless you’re expressing two separate ideas within a single sentence. This makes the document easier to read.

I have moved references to the ends of sentences in cases where the sentence expresses a single idea.

Line 91 – although applied research like this isn’t always rooted in hypothesis-based science, per-se, it’s still helpful to explicitly list study expectations for readers.

Added sentence, “We anticipated the largest changes in summer growth under future warming climate scenarios would be observed at sites with the highest air-water temperature sensitivity.

Line 142 – format as “Mauger et al. (2015)”

I have reformatted this and other references where parentheses were erroneously located.  
  
Line 164 – Did you consider gape limitations at all when you decided on this method of capture? By using salmon eggs as bait, you might have excluded some smaller sized fish (see Armstrong et al. 2010, Jaecks and Quinn 2014).

In this study we used egg-baited minnow traps attract juvenile fish to minnow traps. The eggs were contained within containers with very small perforations and we generally did not experience issues with eggs leaking out. It is the scent of eggs here rather than the shape/size of eggs and fish gapes that are are attractive to fish in this case. Anecdotally, I have regularly witnessed juvenile salmon attempt to consume eggs even though their mouths are too small, which suggests to me they are attracted to the scent regardless of size.

Lines 217-226 – In the introduction section, you already mention how end-of-summer weights are correlated with marine survival. It seems like final weight is the biologically “intuitive” metric to select, especially because other bioenergetics studies have used it to compare climate scenarios as well (see Davis et al. 2021). I hate to ask authors to remove detailed information about their methods, so I guess I’m just looking for a clearer statistical explanation for why this was done.

During the analysis process we did not assume it was an intuitive choice to use end-of-summer weights. As recent literature such as Davis et al. 2021 was published, it was reassuring to see that other researchers had also discovered this as a logical choice for a growth metric. The section has been edited for length in this resubmission.

Line 313 – May be easier to say “gravimetric composition,” so as not to confuse people when you’re talking about the proportion of maximum consumption on line 321.

Please clarify suggested edits. Which term is “gravimetric composition” replacing?  
  
Line 318 – How did you account for prey items that are digested faster than others? i.e., wouldn’t diets disproportionately reflect consumption of prey that take longer to digest?

See response to the following question.  
  
Line 319 – If you sampled fish monthly, I’m assuming you had measurements of diet through time. Salmon diets have been shown to vary ontogenetically. Why didn’t you incorporate temporal dietary differences into your bioenergetics model?

Our initial modeling efforts using field data from 2015 and 2016, which used diet inputs that were not pooled, produced model results that were unrealistic (very large or very small fish size at the end of simulations). Due to the “snapshot” nature of diet samples, even a pooled sample size of a dozen gastric lavage samples from one sampling event may not be appropriate as a representative model input. The fact that some prey items are digested faster than others additionally supports our decision to pool data at the coarser scale of watershed/fish age/fish species, rather than the more granular scale of sampling site/year/fish species/fish age. Though digestion rates of prey items differ, when pooled at this coarser scale the inputs are more likely to approximate an average diet for that cohort in each unique watershed/fish age/fish species.  
  
Line 366 – Please include size class distributions by site in the main document.

Added note to table 3, to do

Line 394 – See major comment above. These r2 values are not great. In Figure 4, some sites look like a shotgun blast. I suspect it has something to do with glacial outflow buffering against high air temperatures in the spring and summer. It could also have something to do with the lack of autocorrelation term in your linear model (although since you ran it on a weekly scale, maybe this is moot). This tells me that your bioenergetics model may be missing out on crucial climactic shifts that will occur as glacier/snow melt is reduced through time. How can you say your modeled air temperature-water temperature relationship will hold into the future?  
  
agree with comments, modify text to reflect caveats … project what actual changes likely to be, would require more complex hydroclimate modeling to address

Line 561 – What about salmon actively seeking out thermal refugia and prey-rich areas?

… connect or refer to comments at line 489 (under Temperature Effects) … growing body of evidence suggests…  
  
Figure 2 – Wouldn’t the prey input be energy/energy density, rather than dry weight mass?

Probably correct … see other examples and change once sure  
  
Figure 4 – Did you plot observed and predicted values overlaid through time to see whether there were temporal trends in accuracy?

Yes … add to result/discussion if room allows

Figure 5 – So the red line is predicted from RCP and the orange one is observed from your water loggers? Looking at the plot, I can see there’s already quite a bit of variability between observed values in 2015-2016 and predicted values from 2010, and that variability is likely not captured in your bioenergetics model. This warrants a little more discussion.  
  
Agreed, connect to above comment

Figure 6 – Would like to see this parsed out by size class, even if in a supplementary figure.

Refer to supp fig or table  
  
Table 4 – Unless I’m mistaken, this table is redundant with Figure 8 and does not need to be included.

Move table or fig to supp material  
  
Reviewer: 2  
Comments:  
This manuscript explores how diverse habitats across a watershed influence variability in growth potential for juvenile Chinook and Coho salmon in response to climate warming during the growing season (May-Sep). The authors focus on the Kenai River watershed in south-central Alaska. The approach uses bioenergetics modeling that incorporates empirical inputs for contemporary diet and growth, and air-water temperature relationships and climate projections to examine feeding and growth performance in response to various decadal climate scenarios for the 2030s and 2060s. These analyses were performed on 3 geomorphically different subbasins and mainstem habitats to examine how diverse landscape features influenced juvenile salmon growth potential to future climate scenarios.  
Overall, I thought this manuscript was well-crafted and will be a valuable contribution toward understanding potential responses to climate change. Unfortunately, one important issue should be resolved before accepting this paper for publication, and the correction will cascade through all of the simulations.

Diet is reported and used in terms of dry mass contributions, whereas the bioenergetics modeling framework is based on wet-mass proportions of the diet for inputs. As applied in this paper, the dry-mass proportions will significantly underestimate the actual contribution of immature invertebrates and overestimate the importance of the more energy rich (and lower water content) prey like adult or terrestrial insects and fish eggs. The prey energy densities applied to each diet group are in wet-mass values, so there is an inconsistency between the diet proportions (in dry mass form) and prey energy density (in J/g wet mass).  
The best solution will be to convert the original dry mass of prey into wet mass and recalculate diet inputs into wet mass proportions.

TBD

Consider another enhancement to the simulations as well. Railsback (in press) has a recent paper that highlights the complexity of interpreting bioenergetic p-values when applied to examining growth potential scenarios across various thermal regimes. He [correctly] advocates for using more straightforward approaches for dealing with consumption in growth scenarios by either using a constant ration of g/d or a specific ration of % body weight.  
In this manuscript, I’d recommend using a constant g/d input for consumption which is easily accomplished in the Fish Bioenergetics 4.0 software. From the p-fit step, extract the total consumption over the growing season from each cohort-site specific simulation, then divide total consumption by total simulation to estimate mean g/d consumed and apply that to the present and projected simulations. A quick scan of the initial-final weights and resulting low-moderate p-values suggest that there shouldn’t be any problems using the constant g/d input approach but do check that the inputs never exceed Cmax for any of the temperatures or body masses used in the simulations (again I don’t foresee any problems like that). This approach will simplify a direct accounting of feeding rate in direct terms understandable by all and can lend itself more directly to scenarios where food supply is changed by a fixed percentage up or down. The p-value approach involves a bunch of non-linearities in how consumption is computed which are less intuitive to explain.

Check reference for clarification, make plan for how to execute. See comments from Erik.

I want to emphasize that this is an excellent paper. The overall structure and analytical framework is solid. The major revisions would only really relate to the updated simulations: revising values and interpretations in the text where changes emerge. I suspect that many of the outcomes and conclusions should remain the same in relative terms, although the magnitude of changes could differ considerably or perhaps reverse in a few cases.  
  
Specific Comments by Line #:  
L102. Fig 2 listed before Fig 1. Please renumber

Made note in text

L102. Fig. 2 caption For the Inputs for size and diet, the word “pooled” is confusing. Please clarify or replace with “grouped by…” or “stratified by…”

Location of text in question unclear; maybe within figure itself??

Additionally, diet inputs for the bioenergetics model should be in wet mass proportions. If % dry mass (water content) differs significantly among some major prey categories, then the reported diet proportions would overestimate the proportional contribution of prey with lower water content, as realized by fish feeding on natural prey. This bias then has implications that cascade through feeding or growth estimates via errors in the energy density of composite diets and the total energy consumed by diets of various prey compositions.

See major comments  
  
L115. Fig 1 should be Fig 2 here and in L149, 151

Made comments in text

L177. Rephrase as: “…fork length to the nearest mm and weighed to the nearest 0.1g.”

Done

L185-190. Sampling salmon FL>50 mm for aging and diet makes sense to ensure scale formation and to accommodate gut lavage techniques. But please clarify that ALL sizes of juveniles were included in size measurements to enable recording of representative size frequency data. Please also inform the readers that fish smaller than 50 mm FL were [not much/much] smaller than 50mm FL and represented a [small/large] fraction of age-0 Coho [and/but not} Chinook

Put comment in side bar

L 201-202. Why exclude the age-2 Coho and age-1 Chinook, especially the presumed presmolt migrants? Sometimes that “plus growth in the month(s) just prior to outmigration can be critically important. For those individuals that require an additional year of stream rearing, reporting on their feeding and growth performance under current and projected future conditions would be important as well. Perhaps samples sizes were too low to enable adequate simulations within streams or comparisons among streams? At a minimum, add another sentence describing your rationale for excluding this component of the populations, and revisit the implications of this knowledge gap in the Discussion. These age classes could become very important in the future and exhibit some very interesting adaptations, so don’t simply dismiss them.

Agreed. Excluded because catch too small. Revisit in discussion.

L203-210. Nice description of your criteria for retaining or excluding samples.

In the cases of apparent weight loss, consider examining the growth trajectories of the scale circuli (in addition to size at annulus) to infer whether weight loss (e.g., abnormal lack of positive growth between circuli relative to other cohorts) versus size-selective migration of larger individuals was a more plausible explanation. While I suspect this goes beyond the scope of the original project and might not be feasible, examining growth trajectories could help salvage some cohorts.

Way beyond scope of this paper

L230-233. Note that a 15C threshold is more a reflection of a temperature-dependent response to food limitation (P < 1.0 or lower energy density prey) than an outright limitation to growth. This 15C threshold would probably hold true for most situations here, as indicated by all but ~2 of the P-values reported in Table S3 were well below 0.5. So perhaps note here (as part of the screening criteria) or in the Discussion that this threshold could theoretically exclude rare circumstances of prolonged high availability of high-quality food (salmon eggs or adult/terrestrial insects). As climate warming progresses, on outcome could be increased quantity or quality of food that would enable salmon to benefit by elevated temperatures for some period before metabolic demand outstripped the boost in food supply.

Read this a few more times to understand how to integrate

Diet  
L241-245. Please clarify what the prey length reconstructions will be used for here: report the size distribution of prey exploited by various cohorts of consumers (good info), versus trying to reconstruct the diet composition based on intact prey which is fraught with potential errors at many stages of the process and reliant on many unnecessary assumptions.

If I understand this correctly, the request is to reformulate the diet inputs based on frequency vs what we observed … check new merit and cummins paper… ?????

L245-248. Again, most of these gut contents are legitimate prey, albeit some of not much value. If excluding caddis casings, then you must be logically consistent with other calculations referring to weight proportions of prey, energy density of prey, and indigestible fractions of prey.

OK?

L249-266. The bioenergetics model operates on the assumption that prey composition in the diet and associated energy density of each prey category are based on wet mass (NOT dry mass) values. In other words, the model calculates feeding and growth based on prey in their natural (wet mass) form as experienced by the consumer. This has ramifications for potential limitations due to mass-based gut fullness and capacity from which then cascades through a number of other processes.  The prey energy densities in Table 2 are reported in terms of J/ g wet mass, but diet proportions were calculated in dry mass terms. This inconsistency results in potentially large overestimates of prey with less water content (adult aquatic and terrestrial insects have very low water content, followed by eggs, compared to much higher water content in immature aquatic insects).  
This inconsistency fundamentally affects the calculation of baseline p-values and the subsequent growth scenarios. The solution is to convert each of the major prey categories into wet mass proportions (i.e., divide by proportion dry weight) and then re-run the simulations and analyses with the updated diet proportions.

TBD

L313-314. Table 2. Please clarify whether diet composition is reported as proportion dry weight or proportion wet weight. As indicated above, the simulations will use these inputs in wet weight terms which can bias the actual proportional contribution of prey and significantly change estimates of consumption, growth.

See major comment

L357. I assume this should be P < 0.05, but I’d recommend deleting the p-val info here

Fixed, unclear why p-val should be removed… refer to ITA

L373. It was stated earlier that temperatures did not exceed 18 C and was used as the rationale for not using the Plumb & Moffitt parameters for higher temps. Please clarify.

Simulated temps did not exceed 18, observed did

L372-418. Consider condensing this section on temperature by using more generalizations in patterns related to air-water regressions and projected temperatures

Unclear how to further generalize

Diet:  
L 421-424. Delete this paragraph

Will do, refer to table instead

L425-428. The proportional contributions of these high energy, low water content prey are overestimated as diet inputs and collectively underestimate the wet-mass contribution of immature aquatic prey. They would represent a much smaller proportion if calculated in terms of wet mass as intended in the bioenergetics model framework. Since these prey represent >40% of the diet by dry mass, this overestimation becomes important when fitting p-values (will become much higher with wet mass proportions of prey), interpreting energy budgets, evaluating growth potential under current and projected climate conditions.

Will be addessed by major comment when sims are re-run

L434-436. Delete this sentence and cite Fig 7 in the topic sentence instead.

Added sidebar note

LL480. Artifact misspelled. Perhaps the montane streams also experienced less fog or cloud cover than the lower elevation sites?

Fixed spelling  
  
Table 1: I think there may be error(s) in the Average gradient (%) column. 23% gradient in the main stem Kenai?

Check gis calcs

Fig 3. Define what the box, whiskers, points and mid-line (median? Mean?) represent in the water temperature graph

Will do  
  
Fig 5. “Time” is redundant with “period”

Check out

Fig6. Again, the concern about how dry mass proportions were used as diet inputs and the potential errors that propagate if not accounting for the MUCH higher water content in immature aquatics versus Adult aquatics and terrestrials with eggs in between.  
  
  
Table 2. The energy densities of each prey group are reported in wet (natural) mass form, so the diet proportions in the table must similarly be reported in wet mass form. Otherwise, the diet inputs would strongly bias against the actual wet mass contributions of immature aquatics, which would typically dominate the diets, and overestimate the wet mass contributions of Adult aquatics, Terrestrials, and eggs.  
  
See major comment

References-capitalization is inconsistent for some titles and many of the journal names

Associate Editor  
Comments to the Author:  
We have now received two reviews of the manuscript (ID TAFS-2022-0016) entitled “Landscape characteristics influence projected growth rates of stream-resident juvenile salmon in the face of climate change in the Kenai River watershed, southcentral Alaska.” Both reviewers and I agree that model simulations to estimate the effects of increasing temperatures on the growth rates of juvenile Chinook Salmon and Coho Salmon in different watersheds is interesting and appropriate for Transactions of the American Fisheries Society. Furthermore, the data set and modelling approach are leading to some interesting comparisons. The reviewers, however, were consistent in their recommendations and identified several concerns in their reviews that need to be addressed. Foremost, are concerns about the air temperature-water temperature model and diet composition. Reviewer #1 points out concerns with the air temperature-water temperature relationship and suggests ways to improve the approach. Reviewer #1 concerns are highlighted by their points on Figure 4. Reviewer #2 identified a need to address how diet composition is handled and the inconsistency between dry mass and wet mass of the prey that underestimates the wet-mass contribution of some aquatic prey. Reviewer #2 also makes suggestions that will make the modeling more robust. Both reviewers also point out text that need clarifications on the assumptions for prey digestion, diet changes through time, and prey length. Addressing the other comments from reviewers will further improve the manuscript.   
  
  
My additional comments, which I tried not to duplicate with reviewers:  
  
Impact statement: I think the impact statement could be expanded beyond considering just the Kenai watersheds.

Modified to include Gulf of Alaska region

Line 20: I think diet considerations should be worked into the sentence on line 14-15.

Unclear how to restructure sentences as requested.

Line 46: I am unclear on the argument as to why Chinook Salmon and Coho Salmon are especially sensitive from these sentences.

Added “…due to their longer freshwater residency times.”

Line 51: The transition to describing the subarctic growth pattern is awkward in this paragraph.

Unclear how to restructure sentence, open to suggestion.

Line 54: In describing temperate relative to subarctic, I think the spatial relationships need to be laid out more clearly for the diverse TAFS audience and making comparisons outside of Alaska.

Changed “southcentral Alaska: to “the Gulf of Alaska region” and changed “subarctic regions of Alaska” to “subarctic regions of interior Alaska.”

Line 80-83: I think this point should have a stronger connection to the response complexity across the landscape described above.

Request is unclear

Line 88: I think I would re-phrase this as “To accomplish these goals, we characterized feeding…”

Modified sentence per recommendation.

Line 98-110: I am not sure this paragraph is necessary.

Added sidebar note

Lines 113-131: Could the description of the sites be incorporated into some predictions

I think it reads as better organized with just a plain description of the sites

Line 143: The description of making sites representative seems appropriate for the main text.

Modified sentence to, “Appendix A describes site locations, methods used to ensure sites were representative of local environs using channel transects, equipment calibration, and logger check procedures.” These practices are already well documented in Mauger et al. 2015, which is referenced in the prior sentence.

Line 168: Would deployed be a better word than suspended?

Suspended is a useful description because traps potentially operate differently if they were sitting on the stream bottom.

Line 203: I am unsure as to why these are excluded.

In order to maintain minimum sample size …. Figure out how to state

Line 465-469: I would move this text down to section describing limitations of study.

Added sidebar note

Line 473-478: I would point to this text as an example of a place to consider a larger spatial scale in comparing locations, not focusing only on Alaska systems, thereby broadening the inferences from this study.

Added sidebar note

Line 478-486, 527-533: A lot of text on the limitations and caveats before providing conclusions of the study.

Noted; re-order

Line 581: I would suggest phrasing this as “a strength of our study…”