Dear Dr. Junker:  
  
Thank you very much for submitting your manuscript "Environmental warming increases the importance of high-turnover energy channels in stream food webs" (ECY23-0749) to Ecology. We appreciate the work you have accomplished. I am willing to consider a revised version for publication in the journal, assuming that you are able to modify the manuscript according to the recommendations.  
  
I got the recommendations and comments from two expert reviewers in the field. Both reviewers agree that the manuscript is technically sound and the data support the conclusions. However, several helpful comments on the introduction and discussion were suggested by both reviewers, and I share their views. So, I'd like to invite you to submit a revised version of the manuscript that addresses the points raised by the reviewers.  
  
Your revisions should address the specific comments outlined in this letter.  
  
Sincerely,  
  
Dr. Hideyuki Doi  
Subject-matter Editor, Ecology  
  
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**We thank the Editor for the opportunity to address the comments and to submit a revised manuscript. Below are our point-by-point responses to the Editor and reviewers, shown in bold blue text (as here).**

**We would like to highlight the addition of a short paragraph in the discussion that we believe is needed to address the role of food resources in our findings, after assessing the reviews in their totality. All reviewers have mentioned, to varying extents, the attributes of the resource pool and the potential difference between algal-based and detrital-based streams. We address these comments in the additional text (lines 452-467) and believe the generalizations and suggestions therein justify the additional length to the manuscript.**

Editorial staff note: In addition to revisions according to the review comments below, you must make revisions to your paper to conform to the journal style at this stage, especially the tables, figures, and supporting information, anticipating the possibility that your paper may be accepted for publication. If the journal style is not followed at submission of the revision, we may return the manuscript to you for further revision before sending it along to the Subject-matter Editor. Closely following our manuscript preparation guide at this stage would expedite the production of your paper for publication, should it be accepted. Please review the full guide here: <https://www.esa.org/wp-content/uploads/2022/05/ESA-Manuscript-Preparation-Guide.pdf>.   
  
Please see the list below for specific corrections that are needed for your resubmission. Any questions regarding these changes should be directed to Brad Walker ([bradley@esa.org](mailto:bradley@esa.org)):  
  
•       The list of key words/phrases should be presented in alphabetical order in BOTH the manuscript and the ScholarOne online submission form.  
  
•       At the beginning of an appendix, the presentation and order of author names should match the title page of the main document. Please present your author names in the following order: Given Name then Middle Name/Initial (if present) then Family Name.

We have made these editorial changes to the document

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Reviewer #2 Comments to the Author:  
  
I read the second version with great interest and the authors addressed most concerns I raised in the first round of reviews. My questions about methods of secondary production and bootstrapping were clarified with the inclusion of new text, a schematic figure about diet proportion estimates, and information about taxa-level responses and traits. As said in the first round, I do see a valuable contribution by the authors on the matters of how energy fluxes change within and across communities along temperature gradients. The field and data analyses are robust in this regard and most of their discussion and inferences are sustained by comparative analyses. I am not aware of any study calculating energy fluxes with such rigorous methods in well-known natural systems along a temperature gradient. Moreover, the skewness approach is innovative and proven to show new ecological patterns. The investigation of how energy fluxes are distributed among species with different body sizes and life cycles is of general interest considering how the scenarios explored here could be happening in different systems and should happen around the globe with climate change. Finally, the authors were also tackling novel hypotheses about the balance between selective and stochastic processes operating along temperature gradients within a metabolic framework.

That being said, I would like to highlight a few points about the last analysis: the randomization and inference of stochastic and selective processes. My points here are 1) a matter of data interpretation and analytical choices that may be more appropriate for a post-publication discussion; 2) does not preclude my support of this manuscript; 3) yet, I would like to share some points that could be relevant and may offer some options for other analyses.  
  
First, let me explain how I understand their analyses addressing stochastic assembly of communities. Junker et al. estimated OM fluxes for each population in each stream and also calculated secondary production and average body sizes of species. With that, they ordered population body sizes or P:B in relation to cumulative OM fluxes. The evenness of OM fluxes would be indicated by a perfect diagonal showing that every population, independently of their body sizes or their production, contributes equally to OM fluxes. When OM fluxes are skewed toward small or large species, this would be indicative that selection could be operating favoring a given body size. This would be especially the case if this pattern follows a temperature gradient. Indeed, the authors applied such methods and inferred that the patterns were equivocal for body size but clearer for production. They sustain these claims based on skewness values along temperature (Panel B in Figures 3 and 4). I may have a distinct opinion on how I interpret these results. Taking into account panel A for Figures 3 and 4, I consider that clear skewness is only present in the extreme temperature values and the intermediate values are hard to interpret given the number of replications and the lack of unidirectionality. This would suggest that strong selective forces on these traits would be acting on very low or very high temperatures.   
  
Nevertheless, defining the level of skewness necessary to infer the strength of selection over random chances could be difficult once you have limited replication to find a definitive trend. So, a fair comparison could be a randomization procedure to create null skewness able to distinguish selection from some type of stochastic sorting. The authors did so and opted for randomizing species ordering within each stream which is the equivalent of considering that the amount of OM fluxes is independent of the body sizes or P:B of the species already there. If the real skewness cannot be simulated in this way (i.e. the real data is more skewed than possible by random ordering), then selection is indeed favoring organisms with some particular traits in this site. The authors found that by doing so, the effect size is strongly mirrored to the raw values of skewness (Panels B and C in Figures 3 and 4), which could suggest that the randomization is strongly constrained in the possible scenarios. Yet, this result is used to infer the selective forces operating strongly with increasing temperature. I am not totally convinced that this pattern is unidirectional. By looking into the Supplementary Material, Appendix S2, I could not clearly tell where the real values were falling within the random distribution. Maybe this could be clarified. Nevertheless, my point here is that a different randomization procedure could clarify the findings. The authors could, for example, randomize all populations for all sites to assemble random communities, constraining the number of populations for each site by the real value, or the total OM fluxes for each site, or both. This null model would actually tell whether, at the metacommunity scale, the species arriving from the species pool is larger or smaller than expected (and the equivalent for production). This would perhaps clarify the history and support or not the unidirectionality of increasing selection with temperature. Also, this null model would be more in accordance with testing the Neutral Theory using a lottery model (Chesson and Warner 1981, Mouquet and Loreau 2002) something that would be of general interest to ecologists.   
  
In any case, I do think that ecologists will have a strong interest in reading this study and that it will generate a lot of healthy discussion around it.   
  
Best regards,  
Victor Saito

We thank the reviewer again for their helpful feedback. We have carefully considered the recommendations and have addressed them in a number of ways. We agree that determining the strength of selection for specific species traits is a difficult task and further that the selection is likely not unidirectional.

Regarding different randomization analyses, we have decided to forego any further analyses for this publication, based on our understanding of the reviewer’s suggestions and the potential insight gained. Our justification is twofold:

1. We observed a fairly clear trend of reduced body size and increased P:B ratios of populations with increasing temperature (Fig. 1b & c). This pattern addresses, in part, the question about whether species arriving from the metacommunity are larger or smaller in body size or have lower or higher turnover than expected. While we cannot speak explicitly to the probability of observing this pattern randomly (and therefore the relative importance of stochastic versus deterministic processes), we believe that any additional randomization analyses would be largely redundant to the patterns shown in Figure 1.
2. In light of our first point, we also find there are some technical difficulties in constructing a randomization analysis across the gradients of species richness and productivity we observe, particularly if we want to interpret skewness indices. These difficulties question the interpretability of any such randomization and may be beyond the limits of this particular dataset. To briefly explain, we believe that randomizing based on species richness or total organic matter flux is possible, but not realistic due both to the ~45-fold difference in total OM flux (Fig. 1a) and the >2-fold difference in species richness (Fig. 2). Randomizing and fixing OM flux would require unrealistically high/low values of species richness in many cases. This would be especially drastic in the warmer, depauperate yet productive sites and the cooler, more speciose sites. In the case of randomizing and fixing species richness, we believe this analysis would only strengthen the skewness patterns we observe, due to the differences in total productivity across the temperature gradient.

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Reviewer #3 Comments to the Author:  
  
I really enjoyed reading this manuscript, which is well written and presents novel and interesting results. While many previous studies investigated the effects of environmental warming on community body size structure, we still know very little about the potential effects of warming on energy and matter fluxes through the food webs, especially from natural ecosystems. The authors used monthly data of invertebrate communities from six Hengill streams, sampled monthly throughout the entire year across a 5-27C temperature gradient. They showed that increase in the mean stream temperature was positively associated with total organic matter flux, mean population biomass turnover rate, but negatively associated with mean population body size of invertebrates. Environmental warming resulted in the relative fluxes of organic matter to consumers to be increasingly skewed toward the taxa with higher biomass turnover (P/B ratio) rates. The analysis of this skewness suggested that this pattern was driven predominantly by non-random deterministic process associated with warming. By contrast, there was no similar pattern of skewness in the relative fluxes of organic matter associated with temperature. This difference is intriguing.   
  
This is a solid and mature manuscript, with a unique data set and advanced analytical methods that brings novel findings. I have several comments and recommendations that can further strengthen the manuscript:

We thank the reviewer for their positive comments and helpful feedback. We have addressed their points below.

1)      One of the two research questions presented in the objective paragraph of the Introduction is (L111-112): “how does temperature shape the role of deterministic vs. stochastic sorting processes in driving relative fluxes of OM through consumer communities”. Although this was identified as a main research objective / question, I felt the introduction mostly discussed the deterministic processes, while the stochastic processes were somehow “brushed under the carpet” / mentioned very briefly. I believe it should be expanded on in the Introductions. For instance - why would we expect warming could increase the stochasticity, under what conditions, stochasticity in what, and how would such stochasticity be detected in the data (how are you planning to separate such stochastic vs deterministic processes)? The authors suggest several hypotheses in the Intro (L126-129), e.g. that they expect that warming would result in more non-random (deterministic) environmental filtering under the raised temperatures. The manuscript would become stronger (and more balanced) in my opinion if you could also propose one hypothesis about the stochastic processes – under which circumstances would you expect such stochastic processes to prevail and why.

We have expanded the background and reasoning behind the relationship between temperature and stochastic processes. Specifically, we outline how reduced population sizes and increased metabolic rates expected with warming may lead to more local extirpations and, therefore, the increase in the relative importance of stochastic community assembly processes. We include a citation to Siqueira et al. (2020) and Saito et al. (2021) to highlight these specific predictions (lines 77-83).

2)      On Lines 117-120, the authors make an argument about these streams relying on autochthonous primary production, which is controlling energy flow. In the next sentence (Line 120-122), they say “We \*therefore\* predicted that annual OM fluxes would increase with stream temperature,…” I didn’t follow the argument. Why wouldn’t the same apply also to streams driven by allochthonous resources with high canopy cover?

Our previous work (Hood et al. 2018), as well as that of others (Demars et al. 2011), has shown that primary production is positively related to temperature in these systems. Further, consumer community productivity is tightly tied to resource availability/productivity (Junker et al. 2020), therefore, a positive relationship is expected between temperature and consumer energy demands (and thereby OM fluxes). In streams driven by allochthonous resources, resource availability is under donor control (e.g., determined by the surrounding forest productivity) and therefore the connection between \*stream\* temperature, resource availability, and consumer productivity are not as straightforward. This is not to say that warming in detrital-based ecosystems may not have similar dynamics, but that the causal connections are not as clearly apparent. We have modified this text to make these connections clearer (lines 125-128) and added some discussion of interactions with the resource pool (lines 451-467,

“Due to the positive relationship of primary production with temperature, we predicted that annual OM fluxes to consumers would increase with stream temperature, mirroring patterns in resource availability.”

3)      I find it strange that consumers diets were very similar in all streams (L313). Could this be because of very low diversity of resources in these streams? Could you provide some explanation in the Discussion? It would be interesting to include discussion about how these findings could be different in ecosystems with great diversity of resources, where consumers could shift to different diet.

Yes, it is very likely the simplified resource base contributes to the high diet similarity across streams, but also our characterization of diets was conducted at a fairly broad scale (e.g., diatom, green algae, etc.). It is possible a more refined identification of diet items would show a distinction among streams. For our purpose of characterizing broader OM flows the coarser characterization is sufficient.

Additionally, we have added a brief paragraph in the discussion to expand on the importance of resources and broader implications of these findings (lines 451-467).

“The ecological and evolutionary effects of warming are likely mediated by their interaction with food resource availability and dynamics (Cross et al. 2015, McMeans et al. 2015). In high-latitude ecosystems where light plays a dominant role in driving resource dynamics, consumer energy demands may be affected by resources more than by temperature *per se* (Huryn and Benstead 2019, Junker et al. 2020). This may be particularly apparent in open-canopied streams where primary production is highly synchronous with seasonal light regimes. Under such conditions, simplified resource dynamics may create strong selection for high growth and turnover rates (and associated *r*-selected life-history traits, such as small body size) because consumers cannot compensate for periods of resource scarcity through diet shifts (e.g., consumptive “portfolio” effects, Gutgesell et al. 2022). Future warming may exaggerate these effects because ectotherms may experience increased temperature-driven energy demands that are increasingly decoupled from resource availability (Huryn and Benstead 2019). Acknowledging these dynamics may lead to a more general framework for understanding different responses to warming across organisms (Greyson-Gaito et al. 2023), latitudes (Gibert 2019), and ecosystem types (McMeans et al. 2015, Gutgesell et al. 2022).”

4)      Although I appreciate the great amount of effort it must have been to collect these unique data throughout the entire annual cycle from six streams, it appears as if three out of six streams have very similar temperatures (around 5C). This doesn’t diminish a great value of this study, but selecting sites with more different (spread) temperatures would allow us to see more clear patterns, e.g. in Figures 4 b and c. I would suggest to highlight this briefly in the Discussion.

There was not a clear opportunity to discuss this in the discussion. Therefore, we have added language in the ‘Methods’ to acknowledge the unequal distribution of sites across the temperature gradient (lines 145-146).

5)      I would like to see some more specific implications of your very nice findings. The conclusions and especially the concluding sentence (L452) is quite vague: “These changes have important implications for the maintenance of biodiversity, as well as for the connections between biodiversity and the magnitude and stability of ecosystem energy and material cycles in a warming world.” Can you explain how these changes impact biodiversity maintenance and the relationships between diversity and stability of energy fluxes? What would faster and more deterministic energy and material flows mean for our future ecosystems – more or less stability? How would your findings be different in streams with closed canopy cover or in the tropics? Would you expect more stochasticity there? Where should the future research go?

We have added a brief paragraph in the discussion to expand on some possible broader implications of these findings (lines 451-467; see above response).

L62: I’m a big fun of the classic May’s (1972) and McCann’s et al. (1998) papers about week species interactions, but I feel that Gilbert et al. (2014) Ecology Letters 17 (8), 902-914 is a more appropriate reference in this context here and directly relevant to warming.

We have added this citation here (line 62)

L67: I would suggest changing to “energy demand and distribution” here.

Original text: “Such relationships depend upon how warming influences functional trait distributions and how functional traits translate to patterns of energy demand”

We have changed this sentence in accordance with this suggestion (line 65-67) to,

“Such relationships depend upon how warming influences functional trait distributions and how functional traits translate to the magnitude and distribution of energy demands.”

L70-71: To avoid some repetition, I would simplify this sentence to: “—leading to changes in the relative abundance of species, and dominance of particular functional traits in ecological communities.”

We have opted to keep this sentence unchanged. We believe it reinforces the connection between temperature change and community taxonomic/functional change that may be lost with the suggested modification.

L79: I would change to “relative species abundances”

This text was removed in response to other comments.

L83-85: I would move these references after the word “temperature” on line 83, i.e. before you mention energy fluxes.

We have made this change (line 86).

L126: I would explain what the acronym P:B stands for, when used for the first time.

We have made this change (line 129).

L104: delete “was”

We have made this change.

L186: please include percentage in which this condition was not met, in how many growth rate estimates?

We have made this change (line 189).

Figure 1: what are the grey shaded areas in panels b and d? Are these the fits to the data? If yes, could you also include the fit to the panel a? Clearly the relationships are non-linear, so GAMs might be a good solution here?

Yes, these are individual fits from bootstrapped linear models. We have expanded the figure caption to clarify this point and have added the model fits to panel A.

Regarding the use of GAMs, we agree that the data do appear non-linear and that a non-linear approach such as GAMs may provide a better model fit. However, what GAMs may make up in fit, they lack in interpretation, because there are no clear ways to estimate model-based effect sizes. Here, we believe determining the mean effect size of temperature outweighs any potential shortcoming of a less well fit model and have opted to maintain the log-linear model fit.

Figure 2: Please explain in the figure caption what are the n values in the panel a. Are these number of samples or taxa?

These values represent the number of taxa present in the “Other” category. We have added this information to the figure caption.