**Editor Decision:**

I have now received three reviews about your manuscript, all of them from stream ecologists working with a variety of organisms. I have also read carefully the paper myself. Like all reviewers, I also believe that the manuscript has potential, but authors would have to work hard to improve the writing and almost completely re-analyse the data. Data analysis is by far the major issue with the manuscript.  
  
The several linear regressions and the nMDS (specially the way it was interpreted) are not adequate. It's also quite confusing that the ms deals with multiple dimensions of Biodiversity, from diversity indices, species richness, and species composition without a clear integration among them. You should definitely do a better job presenting these topics in a more coherent way.  
  
The relatively small sample size of 10 streams is also a key issue that limits the breadth of analytical approaches that can be used. As a result, I do not recommend relying on linear regressions with such a small sample size. R1 and R2 also have a similar opinion. In this context, and for the questions being asked, multivariate analysis is more reasonable. However, I have to say that using nMDS for purposes other than data visualization is not good practice, due to its several drawbacks (see also Legendre & Legendre 2012), such as instability and not correctly dealing with the mean-variance relationship (Warton et al. 2012). Besides, you didn't indicate which dissimilarity coefficient you used or if you transformed the species composition data, e.g. using Hellinger distance, as recommended (Legendre & Gallagher 2001). If you want to draw inferences from multivariate data, you definitely should use more recent model-based methods, such as those available in the gllvm and HMSC R packages. In addition to those, the package ecoCopula also does model-based ordinations for data visualization. Those are much more adequate to model mean-variance relationships.  
  
I'll only consider a much-revised version of the manuscript, which should indicate in the rebuttal letter the response to each critique made both in the review and in the PDFs reviewers sent.  
  
[# PeerJ Staff Note: Please ensure that all review comments are addressed in a rebuttal letter and any edits or clarifications mentioned in the letter are also inserted into the revised manuscript where appropriate.  It is a common mistake to address reviewer questions in the rebuttal letter but not in the revised manuscript. If a reviewer raised a question then your readers will probably have the same question so you should ensure that the manuscript can stand alone without the rebuttal letter.  Directions on how to prepare a rebuttal letter can be found at: <https://peerj.com/benefits/academic-rebuttal-letters/> #]

The editor has attached an annotated manuscript to their decision:

Reviewer: Sylvain Doledec

**Basic reporting**

As far as I can judge, professional English is used throughout the manuscript. However from place to place, authors have made either typographical errors (and miss words sometimes) or do no use the right tense.  
li 24. why "also"?  
li 43. "changes in water temperature "  
li 93. "for" evaluating.  
li 95. replace "to climate" by "the climate"  
li 125. "was"  
li 138. "were"  
li 150. "field rinsed"???  
li 195. "we need to know if this was "all" environmental variables or "each" environmental variables (it seems it is all.  
li 219. "positively related with precipitation"  
li 240. "included"  
li 286. "Pisidium" in italics  
li 290. Oksanen (2013) is not in the references. I guess you mean Oksanen et al. (2019)  
In some place the paper is rather heavy on-going. For example the long description at li 223-230 or at li 239-250 are not very useful as they are and could be simplified. In fact li 251-260 are a direct product of what is described in li 239-250 and the two parts could be mixed and lighten.  
Li 378-384. What a very, very long sentence.  
  
Li 404-406. I hardly see the strength of this sentence. Do we need it? Either too mich or not enough. Why do not you use traits in this paper? The study would be probably benefit from this.  
  
Li 409. Acknowledgements look strange?  
  
Li 442. Strange references. Is it complete?  
  
In the version I have label of Figure 1 is for Figure 2 and vice-versa? Generally speaking I find the references very "US"oriented. You may take advantage of literature abroad. May I suggest to have a look at DOI: 10.1111/j.1365-2486.2007.01375.x (sorry if I indicate a paper I contribute to. It is just an example, there are probably many others ) and also DOI: 10.1007/s10750-012-1244-4.  
  
Raw data Are shared including the statistics, which is very nice from authors. Almost nothing is hidden and the objectives of the paper are clear but probably too ambitious with the present data set. On li 47-49 "clarifying mechanistic links between climate drivers and instream biological communities will improve our ability to predict to the effects of anthropogenic climate change on lotic ecosystems." In the present version, the approach is rather correlative than causative. This is already OK to show correlations and it is necessary to have large scale observational studies but it cannot be further argued that causal relationships are detected there.  
  
As a result, I totally disagree with the statement on li 314-317. Correlation is not causality. By the way the observed correlation is notre straightforward. Why should precipitation influence conductivity and nutrients directly? Is it because precipitation wash of ions? In addition the next sentence is ackward as you tell us here that one of your site is biased.  
I am also not convinced by you sentence on li 324-327 for the same reason. In addition could we have similar compositional shift in other environmental situations?

**Experimental design**

Li 158. In the appendix it is strange to see the same numbers for different taxa and different sites. Why is this?  
  
This study is original though it would gain to add some literature that has addressed similar topic with different angles.The research question are well-defined and consist mainly in identifying patterns of diversity and composition of fish and invertebrate communities associated to changes in precipitation and identify environmental drivers that mediate the effets of climate on community processes. From the the first objective I do not see why precipitation should have a direct effect on diversity and composition. It seems that the answer lies in the second objective but it is a bit tricky and the present version is not that convincing. We would probably need some model of the cascading effects from precipitation toward aquatic communities to better catch the ideas of authors. By the way, I do not see which community processes are addressed in the present version (dispersion? reproduction)  
  
My main concern goes to the number of sites used to address this ambitious questions. I do not think at this scale with 10 sites one can catch all the natural variability of aquatic biota even if other factors are controlled (geology, elevation, temperature).

**Validity of the findings**

I am quite skeptical about the validity of findings because the statistical analyses lacks strength. As I said above, authors sampled 10 sites all possible regression between pairs of environmental variables and between envir onmental variables and biological metrics. In the appendix-regression 125 linear regression were performed. We have another set of 52 p-values in the appendix-ordination. The chance that one p-value become <0.05 just by chance increase with the number of tests. Authors should thus use a correction to the p-values. I corrected the 125 p-values of their appendix-regression and found that from the 29 significant results, false discovery rate adjustment put to the fore only 6 pairs. I think the NMDS approach is much more useful and consistent than the single regression approach. By the way why not a multiple regression approach which would help to answer the second objective. Of course with only 10 sites, no more than 3 uncorrelated environmental drivers should be used in the model.

**Comments for the author**

There is the general interest in using space-for-time studies to address potential environmental shifts in aquatic biota. However, in this paper, I think we need a better integration of the statistical testing and underlying hypotheses. Maybe an additional tables showing the mechanistic relationship among environmental variables could help to better discuss results and how these relationships scale up to aquatic biota would be helpful. The question remains whether 10 sites (representing a substantial amount of work) are sufficient whereas large data surveys are currently made in the US and could provide much more sites along the precipitation gradient. Maybe the NMDS thoroughly commented would be enough. Finally, the introduction addresses the environmental filers framework. It is strange that natural disturbance is not mentioned. Precipitations will change water quantity but also the extreme events will be accentuated by climate change. Natural disturbance is a very highly structuring factors of aquatic communities and dozen of papers have addressed this. This driver could be better addressed in the present paper.

### **Reviewer: Victor Saito**

#### Basic reporting

I thank the opportunity to review the manuscript by Kinard et al. In this study the authors conducted field samplings for fish and invertebrates in 10 wadeable streams along a gradient of natural precipitation. They considered it as an interesting natural experiment since other environmental variables are varying little in the region, not confounding the causal linkages between precipitation and biological changes. They found a strong compositional shift in both biological groups but only fish diversity responded positively to rainfall increase.  
  
Overall, this is a robust study in terms of field methods, biological sampling, and processing, as well as taxonomic identification and counting. The experimental design looks indeed interesting and the results are promising. I commend the authors for the detailed biological consideration of each species found, as well as for the detailed description of biological processing. However, I have some concerns regarding the chosen analyses and mostly with the discussion. The analyses cannot fully show what the authors claim during discussion whereas the discussion itself is too focused on subjective links between species and habitat characteristics. It could be better to focus on the main point of the introduction, talking about communities in the general terms, as well as to making inferences about the future impacts of climate change.  
  
Answering to the specific points by PeerJ:  
  
English: English is good, but some subjective adjectives used could be avoided.  
  
Literature: Literature is updated, but I miss some key references when discussing intermittent streams (e.g. T. Datry, N. Bonada).  
  
Structure: Some parts of the introduction could be shorter and only included in methods. The figure must be improved and better organized in rows and columns.  
  
Results and hypotheses: Results seem robust and well sampled, but the chosen analyses made understanding difficult.  
  
My detailed comments can be found in the attached document that I hope can be helpful.  
  
Regards,  
Victor Saito

#### Experimental design

This is one of the strengths of the study. The experimental design is within a natural experiment and the samples were based on well-known protocols with large evidence of suitability to the questions made. Only small detailing is needed.

#### Validity of the findings

The data seem robust, but could be better presented. The NMDS is not informative on the relative abundance and fidelity of taxa to a given type of stream. I include some ways to improve it in the attached document.

#### Comments for the author

This is an interesting study and can be a good contribution to stream ecologists. I hope you can modify it in a way that strengthens your findings and make the conclusion clearer.

**note** The reviewer has attached an annotated manuscript to this review.

### **Reviewer 3**

#### Basic reporting

The manuscript has an interesting focus. It is well written, but the introduction lacks context. A more robust hypothesis would make the authors' objective clearer.  
More details in the PDF file.

#### Experimental design

There are some problems with the methods. lack of references. and the analyzes used are not the most suitable for the data. See comments and suggestions for analysis in the annexes.

#### Validity of the findings

At some points the conclusions are vague. they need to be more connected with the results obtained and the objectives of the study.

#### Comments for the author

The theme of the manuscript is current and the way in which the authors justified the need to assess climate gradients in the face of climate change is very interesting. However, the data still need to be better organized to show the existing patterns. I strongly suggest rethinking some of the analyzes used. It is a robust database and contains important information about biological community changes in streams.

**note** The reviewer has attached an annotated manuscript to this review.