The authors have made an excellent job addressing the previous reviewers' comments. However, there are still some minor corrections that should be incorporated into the paper in this final round of review.

Like R1, I'm not sure what you mean by "RDA constrained by precipitation". Did you mean you model Hellinger-transformed abundance data as a function of precipitation? Also, consider his/her comments on the use of Shannon entropy. Questions about diversity indices are better framed in terms of Hill numbers, since they allow you to use diversity profiles increasingly weighting rare species (see several papers by Anne Chao and the R package iNExt), differently from using a single diversity measure.

All raw data displayed in tables should be moved to the supplemental material, as well as background information on test results.

I have also made several comments in the pdf attached. Many of these comments are about data analysis and visualization, which I think should mostly be remade. Please, pay close attention to them while working on the revised version of the text.

The editor has attached an annotated manuscript to their decision:

Reviewer: Victor Saito

Basic reporting

The study largely improved from the previous version. The authors did a good job in addressing the major points from my review. There are however some points that still need consideration (see other boxes).

1- English and writing are clear and professional.

2- Literature is appropriate.

3- The structure and figures are much better than the previous ones.

4- Results are relevant and can be used to tackle the hypotheses. Some parts are too speculative, but I detail them below.

Experimental design

The study design is much clearer now and the advantages of the study area for the hypotheses tested very well written.

1- The study is an original primary research.

2- The research question is well defined.

3- The investigation methods and analytical techniques are appropriate.

4- Methods are well described, but I miss a reference or more information for the constrained RDA.

Validity of the findings

The findings are sound in general, but I have one point of concern. The discussion around invertebrate community changes is too speculative and not based on proper results. The idea of an abrupt change is also not well based or explained. Moreover, the correlation between precipitation and diversity seen to be impacted by an outlier that could be tackled by robust regression.

1- Replication within a stream is appropriate. Replication among streams is small but the discussion around the results are appropriate and concern mostly the studied region.

2 - Necessary data was clear for this assessment.

3 - Only the discussion of invertebrates' abrupt changes are too speculative.

4 - Speculative about invertebrates are taken too far supporting the conclusion.

Comments for the author

Abstract:

-change USGS gauged to ‘monitored streams’

-remove ‘significantly’

-‘a variety of omnivores and piscivores fish’

-why consider the changes abrupt?

L47: ‘in general’

L 68: Perhaps complement with 'as long as species can disperse to suitable habitats'

L 114: ‘characterization of fish and invertebrate communities and quantification of environmental variables.’

L 121: ‘The link between stable conditions and habitat heterogeneity is not very clear.’

L 147: Ok, so the sampling effort was proportional to stream width, but not equal among sites. Am I correct? How do you compare abundances among sites? Using individuals per square meter?

L 169: You need to describe this type of information for fish as well.

L 186: gathered?

L 216: Because of this property, diversity indices are commonly criticized because the changes you are seeing cannot be interpreted as pure changes in richness or equitability (e.g. Magurran and McGill's book - Measuring Biological Diversity). I would recommend you to have also a pure metric of evenness, even if only to show on the Sup. Material. This would allow us to understand the changes in Shannon Diversity.

L 233: And which function for the constrained RDA? Also, I think the specific method for constraining an RDA is new (I did not find the information on the two references provided), so a reference would be nice.

L 250: How correlated they are? Some of the Shannon diversity changes could be due to changes in evenness and this would be nice to show.

L 261: This is because it is constrained before the RDA, right?

L 277: One site with very low precipitation looks like an outlier here. You could try a robust regression that accounts for extreme values because overall the scatterplot shows a decreasing trend in diversity with increasing precipitation.

Paragraph 401: This paragraph is mostly based on non-gathered results. You do not have data on basal resources, competitive specialization, competition among species, and predation pressures. Altogether, this logical construction is too fragile. Moreover, it is not well explained why these changes could be considered as abrupt ecosystem shifts. Abrupt shifts have breaking points and your RDA shows a continuous change in composition.

Reviewer 3

Basic reporting

"no comment"

Experimental design

Materials and methods:

Line 158: sweep (15 s duration) sampling from a representative distribution of best available

R: Isn't it a short time? is there any reference that can help saying that this time is enough for the collection?

Line 162-164: In the lab, samples were spread across a gridded sampling tray and randomly selected grid cells were picked to completion until the total count was > 300 individuals (USEPA 2015). Samples containing less than 300 individuals were

R: see this sentence with a different font.

R: In the end of the “Analyses” section authors can add that all scripts were available in the supplementary material.

R: In figure 3, the graphs could be enlarged. They were very good, but small.

Validity of the findings

Line 347-349: (Williams 1999; Dehedin et al. 2013). We also noted but did not quantify higher concentrations of silt in the semi-arid streams with prohibitive implications for nesting species (Jones et al. 2015).

R: I think this is speculation, maybe I would remove that sentence or identify.

R: The result of the regression between canopy cover and shannonfish remained to be discussed further. I suggest that it is not an expected result (Dala-Corte et al. 2020, Journal of Applied Ecology), but the authors need to discuss more about this because this result brings new information that is the influence of the precipitation gradient associated with canopy cover. Are precipitation and canopy cover correlated variables? It would be relevant information. Until maybe authors use the factor as a covariable. So authors can see if the canopy cover can influence the fish divesity however it depends on precipitation gradient. Also, this result “fish shannon negatively with canopy” is in the abstract ... I think that the authors can remove from abstract and discuss more about it in the discussion section.

Line 371-372: Red shiners (Cyprinella lutrensis) were curiously absent from semi-arid sites and were only present in four mesic and sub-humid sites.

R: I suggest modifying the word "curiously". Example: “Contrary to expectations Red shiners are absent….” I think the language gets more scientific.

Line 374: from the rainfall-gradient effects and coextended with Rosgen (stream morphology) and

R: I suggest removing "Rosgen" leaving only "stream morphology", since the authors have already explained that it is a proxy in the material and methods.

Line 445 -450: hey warn that regions expected to become more arid, like Central and Western Texas (Jiang and Yang 2012), could expect a loss of competitive taxa with low environmental tolerances as observed here with centrarchids, ephemeropterans, and trichopterans. And that in their absence, rugged and euryhaline taxa (like livebearers, burrowing gastropods and predatory invertebrates) flourish.

R: This part need to be summarized on the abstract it is a very important conclusion of the research.

Comments for the author

Abstract:

Line 36-37: These results indicate that small future changes in precipitation regime in this region may result in abrupt transitions into new community states.

But what would these new communities look like? Modify something? I think here authors have to make it clear that the modification would not be very good as it would increase/decrease diversity? Or change competition? Function on community?

Introduction:

Line 112-119:

This study conducts the first bioassessments of stream biota in this region. So, we established a framework for discussing abiotic and biotic filtering processes in community assembly, followed up by several expectations based on existing literature and precipitation-driven patterns that can be observed at a glance within the region.

R: I understand that this is a pioneering work. But it does not justify the lack of some information. Throughout the introduction, the authors talk about changes in the biota of fish and macroinvertebrates. I think the authors can be more specific on this issue. for example: diversity increases with rainy and decreases in the dry region ... Or decreases the richness of fish species in a given place. I want to make it clear that the authors have already put references on this, I just think that adding specific information, like the one I mentioned above, would help to understand the objectives and predictions of the manuscript. In my opinion, using results from work done in drought and wetter regions would only improve the introduction of the manuscript.

References:

R: The manuscript needs a revision of the references because they are not standardized with the journal's norms.

R: For example: names are different abbreviated in the first and write out in the second.

Line 510 - Fetscher, A. Elizabeth, Meredith D. A. Howard, Rosalina Stancheva, Raphael M. Kudela, Eric D. Stein, Martha A. Sutula, Lilian B. Busse, and Robert G. Sheath. 2015. “Wadeable Streams as Widespread Sources of Benthic Cyanotoxins in California, USA.” Harmful Algae 49 (November): 105–16. https://doi.org/10.1016/j.hal.2015.09.002.

Line 514 Fréjaville, Thibaut, Albert Vilà‐Cabrera, Thomas Curt, and Christopher Carcaillet. 2018. “Aridity and Competition Drive Fire Resistance Trait Covariation in Mountain Trees.” Ecosphere 9 (12): e02493. https://doi.org/10.1002/ecs2.2493.