

I found significant improvements in this new version of the manuscript. It is well written (the introduction is really informative and the method is clearer for me). The results are still original, and I see great potential for this work. Moreover the replies of the authors to my previous commentaries are well argued.

However, I still believe that discussion could be improved. I give some ways to do that below.

All the best,
Romain Bertrand

NB: line numbers used below do not consider correction in the manuscript.

Main concern:

The discussion is quite confusing for me sometimes. I think the authors can improve it notably by :

- explaining the reason which could lead to different effects of community size on incidence- and abundance-based beta deviation. In the current version of the manuscript the authors detailed potential causes of the negative and positive effect of community size on incidence- and abundance-based beta deviation independently. So the authors did not really confront these opposite results. Why niche selection increased difference in species abundance composition but not in species occurrence? May be it's a matter of environmental sensitivity... Observing changes in species composition (occurrence) among communities required likely more environmental heterogeneity than changes in species abundance. The authors could discuss this point, notably by shorting lines 385-417 to save some space.
- by discussing expectation 3. What about the non-linearity vs the linearity relationship between beta-deviation and community size? If it's not important, the authors could remove this expectation.
- the authors could be more direct in some parts.

Minor comments:

lines 100-101: "...as estimates of β -diversity; these are called beta deviations hereafter."

For me the index comparing beta diversity to null expectation is not beta diversity. I think it's better to use directly beta deviation.

Line 148: "...to be increase..." => to increase

Line 149: Refer to Fig S1.

Lines 157-158: "watersheds dominated by agriculture (mainly pastures, and *Eucalyptus* and *Pinus* plantations)" => I propose to change "by agriculture and forestry".

Fig. 1:

-Is color necessary? If yes please give color meaning in legend.

-E3 => the authors talked about beta diversity in fig 1 while they talked about beta deviation at lines 142-145.

- this figure is good and informative to add. But I was quite confused when I saw the results because in Fig 1 the authors presented relationships grouping both region in a same fitted model while it's not the case in the results. By doing this it's like some expectations consider that Brasil and Finland inform two successive parts of the relationships. But the authors did not test for that in their analysis. May be the authors can verify such expectation of continuous pattern from Brasil to finland observations. But it means that the averages of beta deviation and diversity values at similar community size are closed or

equal, and I am not sure it's the case when I look to the results. The authors have two options: 1) test for that expectation or 2) modify Fig1 and/or provide more explanations.

Line 219: "... β -deviation per region, with one value per watershed".
I propose to add "incidence-based β -deviation".

Lines 243-246: What is the satellite? The image resolution? Give these information or at least a reference if the method has been already described.

Lines 249-250: The authors should provide correlation among covariables in order to evidence no multicollinearity in data.

Results: For some results authors concluded if results supported or not the expectations presented in Fig. 1., but not for every analysis while it could improve the understanding of the results and give to the reader better chances to follow how the authors conducted the work.

Lines 277-278: "Beta diversity of tropical smaller communities were closer to null expectations than those of larger communities (Fig. 3)".
I propose to add: "Incidence-based beta..."

Line 278: Fig. 3 => Fig. 3A

Table 1:

-use the same abbreviation in FigS1 and table 1.

- " b = standardized partial regression coefficient." => What does "partial" mean? I think it's a standardized regression coefficient.

Line 283: Fig. 3 => Fig. 3A

Line 292: Fig. 3 => Fig. 3B

Line 313: Fig. 3 => Fig. 3A

Lines 329-330: " We showed that β -diversity of smaller communities deviates less from null expectations than larger communities".

I agree with the authors for abundance based beta deviation. But for incidence based beta deviation I have a doubt because the range of beta deviation values between Brasil (smaller community size) and Finland (larger community size) are quiet close. Could the authors explicitly test for this difference or use a less general sentence? This comment need to be consider in the abstract too (line 34).

Lines 342-344: "...and because dispersal ability is highly variable among species in riverine systems (Heino et al. 2015b, Tonkin et al. 2018a),..."

But it's not the case here as the authors argued that dispersal is not limited in your system (see lines 359 and 385-417).

Lines 345-356: may be it will be easier and more informative to test if species abundance varies with community size.

Lines 364-366: “As dispersal within watersheds was likely not limited, this positive relationship indicates that niche selection was sufficient to cause non-random variations in genera relative abundance and aggregation patterns among large communities.”

So if it's the case why the authors didn't observe any effect of environmental heterogeneity in the model?

Line 367: “... selection...” => niche selection?

Line 372: please add “incidence based” for a better understanding.

Lines 379-382: “The streams sampled in Brazil and Finland ... structure of stream communities (Hynes 1975, Allan 2004, Roque et al. 2010, Siqueira et al. 2015)”

May be not necessary. I didn't understand why the authors referred to environmental difference among watersheds while the results should be more discussed considering environmental heterogeneity within watersheds to explain the low species composition dissimilarity within metacommunity.

Lines 385-389: “This suggests a tendency towards environmental determinism; ... to be the main driver of spatial variation in genus composition among those communities”

Ok but incidence-based beta deviation is still negative. So it's like despite the authors observed an effect of environmental heterogeneity, it is not able to strongly differentiate community composition within metacommunity.

Lines 418-419: “If our watersheds were larger (in extent), dispersal limitation would likely have played a role and niche selection could have been the major driver of community structure.”

But watersheds of 500 by 300km in Finland are quiet large enough and should evidence dispersal limitation, isn't it?

Lines 456-457: “variations in genera relative abundances are the result of local environmental filtering”
=> “variations in genera relative abundances are **LIKELY** the result of local environmental filtering”