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Reply on Editor comments

Laura Anahí Macario-González et al.

Author comment on "Geodiversity influences limnological conditions and freshwater ostracode species distributions across broad spatial scales in the northern Neotropics" by Laura Anahí Macario-González et al., Biogeosciences, <https://doi.org/10.5194/bg-2021-298-AC1>, 2022

Dear Editor,

We appreciate all comments and suggestions made to improve the manuscript. Please find below answers for each of your comments. In case we were unable to follow them, we provide an explanation.

General comments

Comment 1) I strongly side with a reviewer comment about the usage of the word "geodiversity". You kindly provide definitions, which clearly include geodiversity being linked to the RANGE (DIVERSITY) and VARIETY of geological conditions. You never compute nor use any measure of geodiversity. This could be done for a region and meaningfully linked to biodiversity at a larger spatial scale, for instance for limnological subregions, but it's not the subject of your study. Please, reword the manuscript and title, refer to "geological conditions" or "environmental conditions" instead.

Answer: Definitions provided clarify the coverage of the term geodiversity, the variables that integrate it and its applicability at spatial scale. Our study integrates at least five variables that constitutes geodiversity such as geological units and ages, mineralogy, elevation, and hydrological features of landscape. In addition, the study area covers a region >1300 km N-S for which we accounted large spatial scale. The suggested terms „geological conditions and environmental conditions" are inappropriate for our database as both are too broad and unspecific. We consider that the usage of the term geodiversity in our study is supported by our data and we retained in the title and through the manuscript.

Comment 2) A similar yet less prominent problem pertains to the usage of the word "limnology", which literally means "the STUDY OF inland waters". You use this word as a hypernym for limnological conditions, defined as including biological variables. However, in fact, you never include any biological variables (a classic would be chlorophyll-a) and instead analyse the relationship to a biological dataset. Here, I simply suggest to use "limnological conditions" (then clearly defined as excluding biological data) or - maybe even better - "water chemical conditions".

Answer: Following the suggestion, we replace the term „limnology" by „ limnological conditions". Please note that limnological conditions is also used in the title. Definition of

the term is provided in the introductory section of the manuscript (line 73). We retained the biological component of the term as it may not be solely regarded to chlorophyll but to a broad range of biological diversity. Limnology as discipline must be considered as integrative. Please see Limnology - an overview | ScienceDirect Topics.

Comment 3) I believe that the reviewer concern about low species richness being potentially based on low sampling effort and low counts of individuals has been addressed only partly by providing additional information on sampling and abundances. Please, also include a proper discussion of this problem of sampling effort. This seems specifically necessary as there is a lot of text discussing differences to more species-rich systems elsewhere that were likely covered with a much higher sampling effort. It's quite natural that single Lake Ohrid is sampled with more effort than >70 systems in a single study.

Answer: The goal of this study was to take advantage of the high degree of geodiversity in the research area, and to show the linkage between geodiversity and biodiversity using species assemblages of ostracodes as example. We, however, discussed briefly the relative low sampling effort and how temporality may affect species richness.

Comment 4) I have to also side with reviewer concerns about the SEM. First and foremost, I have difficulties to see the added benefit of the SEM to an already long manuscript. The SEM is rather complicated derived with several steps and the procedures can barely be followed. For instance, similar to Reviewer 1, I have difficulties to understand the SEM output and how a metavariable like "limnology" can be included, which consists of several variables yet is then finally presented with only one path coefficient to other variables in the SEM. Similarly, I do not see a clearly justified procedure how an individual variable like conductivity is isolated from the "limnology" dataset and maybe even used twice? Honestly, the simplest way to address this issue would be to just drop the SEM from the manuscript. Alternatively you may provide a better explanation of the procedures and clearly provide the added benefit to the paper.

Answer: SEM analysis was retained, because is the only test in the study able to evaluate relationships between latent variables (variables composed or formulated out from other measured variables) such as geodiversity and limnological conditions and test their statistical significance. SEM analysis is justified in our study, as the main objective of the manuscript is to evaluate the relationship of the geodiversity and limnological conditions with species composition to infer direct or indirect influences between them. Such analysis can only be addressed using tests able to handle causality assumptions on a multivariable framework, which is the case of SEM. Please see the application of SEM to ecological studies Applications of structural equation modeling (SEM) in ecological studies: an updated review | Ecological Processes | Full Text (springeropen.com)

The reviewers concerns were in fact, the variable selection and how they were *a priori* related, they did not question it uses on the analysis. The way how the individual variables were used to fit models evaluated are clearly explained in the main text and in supplementary material. For instance, performance of individual variables were not in any case tested double.

Comment 5) One of two objectives of the paper is the linkage between geology/limnology with ostracod assemblages. I honestly think that this objective has been addressed rather poorly. Also considering the storyline of the paper that is so much based on ordinations. There are several straightforward options to improve the paper in this regard: Consider opposing the maps resulting from the various ordinations - do groupings based on geo data overlap with those based on ostracods? Consider using clusters resulting from geo/limno data as color codes in the ordination based on biological data or even in the PERMANOVA. Last, there is a CCA hidden in the Supplementary that could be graphically improved and presented in the main text. I consider these options as cleaner solutions to address this objective than the SEM. Please consider these suggestions as late editorial ideas to improve the readability and accessibility of your manuscript. You may not follow them.

Answer: All suggestions were tested, one important problem is the interpretation of such graph overlaps, they all must be analyzed based on observation which may imply subjectivity. Another important issue is that geodiversity and limnological conditions are composed of a set of variables. In an ordination it is not possible to clearly distinguish the signal of composed variables, because its components are dispersed in the ordinations. The same applies to limnological conditions. Therefore, we cannot infer relationships between variables of interest. The CCA or any other ordination can only support SEM results. In correspondence with the next comment (shortening the manuscript), we decided to exclude the CCA from the manuscript.

Comment 6) Last, please consider shortening the manuscript. Apart from several rather long text sections I can see two clear options: Move table 1 to the SI. And consider computing just one PCA instead of two. I see no justification of computing two PCAs for two subregions anyway. It's also unclear why the variable selection was done only for one PCA and not for the other. Also, the two PCAs clearly are similar (especially PC1) anyway. Instead of presenting two PCAs you may consider just one, but then present three PCs rather than just two. Also, the contour plots could be based on PC-scores instead of selected variables.

Answer: Table 1 was removed from the manuscript as it shows a synthesis from table S1, which is the entire data set used in the manuscript. In regard of the PCA, lines 199-200, describe how the analyses were performed. Using the cluster results, we constructed two databases which were subject to two PCA runs. The first run was done for variable selection. In order to keep the manuscript short, we decided not to include 4 PCA graphs, but only the second PCA runs. The contour plots were based on variable importance indicated by PC scores as pointed out in the figure caption. Maps show the spatial distribution of variables.