Dear Recommender,

Thanks for your comments for round #2 of this revision. Following the suggestions from yourself and the reviewers, I have addressed several changes across the manuscript. Below, I give the individual comments of the editor and reviewers in black and my responses in blue, preceded by a '>>' notation.

Sincerely,

Ricardo A. Segovia
Adjunt Researcher
Instituto de Ecologia y Biodiversidad (Chile)

Round #2
Author's Reply:by Joaquín Hortal, 2021-03-04 06:34
Manuscript: https://doi.org/10.1101/836338 version 2 Moderate revisions

Your manuscript has significantly improved, and I agree with the two reviewers of this version in that it is a fantastic work, and indeed merits recommendation. I asked a new reviewer expert in quantile regressions to take a look at the criticisms on the method raised before. S/he agrees with your arguments, but also points to a couple of simple methods to address its limitations in what refers to providing metrics of goodness-of-fit. Please follow her/his advice about this issue, and in particular the test for the deviation of the slope from -0.65; I think it will certainly strengthen your work.

>> Thanks for this revision. Following the suggestion of revisor 2, I've addressed two new analyses. Firstly I compared the slopes of quantile regressions from 0.5% to 95%. I did this twice, the first one for the transformed data and the second one for the raw data (please see my response for this suggestion in reviewer 2 section). Finally, I decided not to add these results because I think are counterintuitive and difficult to read. Secondly, I tested if linear slopes statistically differ from the theoretical predictions of the kinetic model. I added these results in Table 1 and in the Results section. I agree with you that this simple analysis really strengthens this manuscript.

Besides that, Rafael Molina-Venegas provides a number of minor comments that will be quite useful to improve this final version of the manuscript. I'm sure that, after all these revisions are done, the manuscript will be ready for my recommendation.

>> Thanks, I really appreciate the comments and suggestions from Dr. Molina-Venegas.

Thanks again for sending this beautiful work to open discussion throuhg PCI.

Reviews

Reviewed by Rafael Molina-Venegas, 2021-01-28 12:54

I have revised the new version of the pre-print by Dr. Ricardo Segovia entitled "Temperature predicts the maximum tree-species richness and water and frost shape the residual variation", and I do think the research is simply fantastic. This piece of work clearly shows how the kinetic hypothesis of biodiversity gradients may have been largely underrated due to the use of central tendencies rather than upper bounds (i.e. quantile regressions) in previous modelling exercises, and it also illustrates on the interacting effects of other environmental factors besides mean temperature such as incidence of freezing events and water availability. The former variable is particularly interesting given that environmental information pertaining such drastic, eventual and stochastic climatic events has been comparatively less used in the macroecological literature. I also acknowledge the new cross-biome analysis, which I found largely informative. My criticism is fairly minor, and it mostly revolves around the feeling that that some of the results are perhaps loosely discussed (see comments below). It follows a few comments and suggestions that might hopefully help to lift the manuscript to its maximum informative potential. Congratulations to the author for his fine research.

Comments

The topic is introduced in an excessively sharpy manner (line 2), so I would suggest rewording a bit, perhaps including a short sentence to more smoothly introduce the reader to the topic.

>> Thanks for this suggestion. I've added a new first sentence to introduce the central problem, namely the variation in species richness across space.

Line 86. Please, remove the "a" (i.e. allow for breakpoints in […])

>> Done, thanks for catching it.

Line 97. I do not think you need the "actually" here

>> I still need a connector between the broad interpretation and a specific result. I've replaced "actually" by "in fact".

Line 99: I would suggest using "informative" instead of "robust" or alternatively "better fitted" instead of "more robust"

>> I've repaced "more robust" by "better fitted"

Line 101: I am missing half a sentence here (are not significantly different from those of the linear models)

>> Actually, I want to say "both of the slopes in the segmented models towards the upper bound are not significantly different from each other". I've replaced it in the text.

Line 105: The active voice does not work here. Rewording suggestion: "a linear negative relationship towards the upper bound, where both models fit the better, cannot be rejected"

>> Thanks, I've replaced this sentence following your suggestion.

Figure 1 caption. It should be noted that the SIC criteria was arbitrarily subdued to the t-test when interpreting evidence for model support. As such, the linear models at 75% and 85% bounds were considered as better supported because the non-significant t-tests despite the SIC criteria suggests that the breakpoint models better fit in all cases. Therefore, and given the information the author is intending to show in Figure 1, I think it should be clarified that the breakpoint models were only considered as superior to the linear ones in case both sources of statistical evidence suggested so, and that otherwise the simplest model (i.e. linear) was considered as "best supported".

>> I've added this information in Figure 1 caption.

Line 126. "across biomes" instead of "by biomes"

>> Done

Line 131. No need for "actually" here

>> Removed

Figure 2 caption. "across the main biomes […]"

>> Replaced

Line 179. "model that mechanistically"

>> Replaced

Lines 213-215. I think that environment-driven diversifications are nowadays well acknowledged in the literature, so that there is consensus that diversity is not just a function of time and diversification rate. I think that the point here would be simply that previous attempts to explain spatial variation in diversification rates as a function of temperature might have failed due to the use of central tendencies instead of upper bounds. Yet, I am not sure that such contrasts have been conducted so far (i.e. using diversification rates instead of species richness as the response variable in spatial temperature-based models).

>> I agree. However, I would like to keep this paragraph in order to discuss some potential consequences of the metabolic upper bound model determining a carrying capacity of assemblages on other fields of evolutionary biology. As you say it would be cool to explore the implications of the upper bound model on the estimation of diversification rates, but some methodologic issues still need to be solved to be able to do that.

Lines 217-220. I do not get this, seems too cryptic to me.

>> I've reworded this sentence to make it clearer.

Line 228. I am not an English native speaker, yet I think that testing a relationship "across regions" or "across biomes" actually means exploring the shape of the relationship IN every biome (i.e. the results shown in Figure 2) rather than considering all biomes/regions under the same model.

>> I've reworded this sentence following your suggestion because what you wrote is what I wanted to say.

Lines 254-255. I think this idea could be expanded. For example, well-documented tropical-montane evolutionary radiations (e.g. Hughes 2015, https://nph.onlinelibrary.wiley.com/doi/full/10.1111/nph.13230) probably blurred the macroecological signature of the kinetic hypothesis within tropical biomes.

>> I agree that would be a good idea to discuss the potential influence of biogeographical processes like the migration/radiation of extratropical lineages into tropical latitudes affecting the patterns of species richness. However, I think that is out of the scope of the discussion in this paragraph and the manuscript in general.

Lines 255-259. Actually, your models show greater residual variation towards temperate, non-tropical latitudes, so I think this idea is incorrect. Perhaps the author wanted to stress that the -0.65 slope predicted by the kinetic model should only be expected in temperate regions? Besides, the expression "less environmental arrangement" is weird.

>> I've reworded this sentence to make it clearer. First, I am discussing previous ideas from literature. So, I've added the references. Then I rewrote the weird sentence in a more explicit way.

Line 265. "factors", in plural.

>> Done

Lines 288-289. Rewording suggestion: "that prevents dispersion and diversification into "harsh", extra-tropical environments." Besides, recurrent disturbance (i.e. glaciations) may have also played a key role here…

>> I've reworded this sentence following your suggestion.

Lines 296-300. This idea is loosely discussed and too cryptic.

>> I've reworded this sentence to make it clearer.

Line 298. "dropping"

>> Done

Line 304. See comment to lines 254-255

>> Done

Line 308-311. I do not get this idea.

>> I've reworded this sentence to make it clearer.

Line 322-324. In my opinion, this is a strong claim that suggests that previous studies are somewhat wrong, and I think it should be toned down. You may simply state that your contribution may serve to reevaluate the metabolic hypothesis in future latitudinal diversity gradient analyses.

>> I've removed this sentence to toned down the concluding paragraph.

Line 429. Please, describe the exact breadth of the ordered categories here and in Figure 5 caption.

>> For visualizing this data I have used a method of clusters (kmeans) to discretize the variable. Therefore, the size of clusters (i.e. the breadth of the ordered categories) can vary through the raw data variable.

Reviewed by anonymous reviewer, 2021-03-03 13:34I liked this manuscript and I really enjoyed reading it. It is very clear and very well-written, and the topic and idea on how to address the richness-Tª relationship is nice. Upper limits instead of responses to mean values is something quite common in ecology, and it is surprising the amount of effort that has been put on analyzing mean tendencies instead of upper limits; the research topic of this manuscript is a clear example. I have also read the author´s answers to the previous comments raised by the referees and I am quite happy with the arguments made by the author and the way he addressed some of the points. I have not much to say about the manuscript. The methodology is sound and robust. I understand the concerns raised by the referees and the recommender about the lack of a goodness-of-fit measure in quantile reg. similar to the R-squared, but the author is right when he explains this issue. Unfortunately, R1 does not have the same meaning as R2, and it does not inform about the explained variance. Maybe, one way that could be used to address this concern, somehow, is to check if the slope progressively decreases (or increases in absolute value) from the 5th to the 95th percentiles. This has been previously used by other authors such as:

VanDerWal, J., Shoo, L. P., Johnson, C. N., Williams, S. E. (2009).Abundance and the environmental niche: Environmental suitabilityestimated from niche models predicts the upper limit of localabundance. The American Naturalist 174: 282–291.

Jimenez-Valverde, A., Aragón, P., Lobo, J. M. (2021). Deconstructing the abundance-suitability relationship in species distribution modelling. Global Ecol Biogeogr. 2021: 30:327–338.

>> Thanks for this suggestion. To evaluate if the size of the effect of temperature on species richness increases with higher quantiles of species richness by the measure of the slope is a nice analysis. However, my variables are a bit transformed following the original proposal for the kinetic hypothesis. Thus, I am using a log-transformed species richness and the inverse of temperature. My results are (in "quantile=slope value, for log(species richness) and the inverse of temperature in kelvin") 5%= -1.204, 15%= -1.063, 25%= -0.97, 35%= -0.867, 45%= -0.812, 55%= -0.781, 65%= -0.789, 75%= -0.823, 85%= -0.808, 95%= -0.661. Therefore, the measure of the slope is getting close to zero to higher quantiles, i.e., the effect seems to be decreasing towards higher quantiles. However, if I estimate the slopes per quantiles with the raw data (i,e, directly temperature and species richness) I found an increase in the size of the effect of temperature on species richness toward higher quantiles. The results are (in "quantile=slope value, for species richness in function of temperature in celsius") 5%= 2.755, 15%= 3.622, 25%= 4.489, 35%= 5.032, 45%= 5.464, 55%= 6.205, 65%= 7.402, 75%= 9.666, 85%= 13.085, 95%= 17.244). Thus, with the raw data, I could show a progressive increase in the size of the effect of temperature on species richness. However, these results could be counterintuitive, because all the analyses are carried on with the transformed variables. So, I prefer to avoid this analysis for this manuscript.

As a minor comment, it would be easy to check if the slopes statistically differ from -0.65.

>> Thanks for this suggestion. I've added the results of this analysis in Table 1. In concrete, I've added an asterisk on those values for linear slopes that statistically differ from the theoretical values of -0.65. I also added information for these results in the main text and I informed the t-tests to compare the values of the

linear models with the theoretical values for the slope expected by the kinetic model in the Materials and Methods section.

Congratulations to the author for this enjoyable manuscript.