

Interactive comment on “Shift of seed mass and fruit type spectra along longitudinal gradient: high water availability and growth allometry” by Shunli Yu et al.

Anonymous Referee #3

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The manuscript takes advantage of an impressive sampling of seed mass at community level across a sunshine/rainfall gradient in East Asia to address a series of correlations, including climatic, phylogenetic and functional syndromes. The authors find a decrease of seed mass and vertebrate-dispersal with decreasing rainfall among other relationships. The authors then go on to use the correlation to propose the underlying mechanisms. I am positively impressed by the amount of functional data at the community level across a series of sites along strong environmental gradients, most notably rainfall. However, the analyses can be better resolved, the manuscript structure improved, the mechanistic rationale refined and the results better presented. This means that some new analyses and extensive re-formulation is needed. Therefore, I

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would propose major revision. I do not, however, expect major changes in the findings themselves, which are not necessarily ground-breaking, but I do believe are highly interesting for publishing, alone by the sheer amount of data, sampling effort and area of study. I highly encourage the authors to address the comments because these analyses will be of interest for ecologists and biogeographers in general.

Major points:

- The concept of longitudinal gradients seems to be more commonly evoked in Asia. Whereas this is ok, longitudinal gradients have limited information without further details on what environmental variables are actually being varied. Other evoked diversity gradients (latitude, elevation, depth) have clear environmental gradients (i.e. energy, temperature, light), which makes them intuitively understandable. Longitudinal gradients don't. Hence, I would rather suggest the authors to combine the name of the gradient with water availability/precipitation gradient, for example. That is more intuitive in this case. The use of the term 'longitudinal' here is important because you then control for confounding effects, such as seasonality, light/energy availability.

- L. 45-48: Note that these are not mechanisms. There are variables. If you want to mention the mechanisms (which I was hoping for), you need to mention the eco-evolutionary processes which these variables are influencing. In other words, the mechanisms are the underlying processes. I suppose you cannot state the mechanisms, as you mentioned in the sentence that follows: 'a deep understanding of the factors that underlie . . . is missing'.

- L. 71-74: note that correlation is not causality. All the results are correlations, from those the authors discuss their way to propose this set of relationship as a mechanistic framework. I am ok with that, but please add in the respective discussion part that for addressing causality, you need to develop mechanistic models or lab experiments. Also, at this point, you do not need to refer to the figure, which I find more appropriate to be referred only in the end of the discussion, so it becomes a conclusion figure and

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thus receive the last number.

- Study questions and Analyses: the authors list 4 study questions (L. 76-82), so it would be nice if the authors use this structure to present their analyses. This means, what analysis did you do to tackle each question. With this said, I find most analyses quite redundant. Why haven't the authors done a phylogenetic spatial GLMM or similar (e.g. ape package)? Plot or biome could be given as random effect to account for community assembly effects, whereas spatial models would account for spatial autocorrelation and the phylogeny for phylogenetic autocorrelation. Not sure how well the authors can fit such more complex correlative models, but there are also spatial or phylogenetic models. It just seems odd to tackle each of these correlations in separate. I generally find ok to address phylogenetic signals and I'm ok of separate tests, I'm just intrigued by the choice of models, because at least some of the analyses might be combined. Please clarify why not combining the analyses if you choose not to.

Ref: Kaldhusdal, A., Brandl, R., Müller, J., Möst, L. and Hothorn, T., 2015. Spatio-phylogenetic multispecies distribution models. *Methods in Ecology and Evolution*, 6(2), pp.187-197.

- The authors need to sort out the writing style, presentation of results, figures and tables. These are not in the same format or style.

Minor points (some are almost major though):

- Abstract, L. 20: at this point the reader does not know these five communities. So, you have to first mention the five communities. Moreover, I suppose a better term here would be biome.

- L. 26: note that this generalization rising longitude does not make sense (see major point above), as this pretty depends at what continent, part of the continent, latitude and direction you are addressing the longitude gradient.

- L. 93-94: So you have rather a sunshine and rainfall gradient that happens to be in

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that particular longitudinal direction. I would ditch the longitudinal rationale and focus on the sunshine/rainfall gradient, which would make the gradient generalizable. With that said, what is the range of sunshine duration and intensity?

- L. 99: what you mean about recent?

- L. 107-109: why using just these two? Why don't you have quantification of sunshine hours or light intensity or cloud cover as well? I'm asking that because the authors mention the sunshine gradient before.

- L. 120-122: I know that you cannot re-do the sampling, which I find already very impressive. However, it is a pity that you removed wind-dispersal structures, which is part of the propagule. I suppose these would have relative low impact in the overall seed mass. Although for some small seeds, that could play a role. Can you say something about this loss of seed mass by the removal of this structures?

- L. 123-125: So what was it: based on ornamentation and appendages or based on Kew Gardens/literature? This is confusing as you provide two ways to determine dispersal mode. Please, clarify.

- L. 175-176: You can calculate MANOVA for non-independent response variables. Or the authors could be a PCA and perform regressions with the axes loadings, hypervolumes or centroids to the explanatory variables. In this way the authors would address the entire functional syndromes (i.e. trait correlations).

- L. 186-187: you can delete the half-sentence after the comma. The same in the follow-up sentence. Anyway, this entire paragraph is confusing, as you talk about average seed mass in different ways, but it reads the same.

- Tables 2-3: use the same format. Check journal formatting.

- Fig. 1: It might nicer if the authors further identify (i.e. graphically distinguish) the ecological/physiological processes (e.g. the mechanisms), related traits and the external drivers (i.e. abiotic conditions). Also, it is strange to refer to this figure before present-

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ing the results. I see this figure more like a conclusion figure than a result figure, in which the rationale would be explained over the course of the discussion.

- Fig. 2: please, add lettering to the panels. In the top panel, add title to the legend and the units to the numbers (or the legend title). Explain the lettering in the boxplots in the figure caption. Add the statistics as well (I suppose the tests provided in the main text).
- Fig. 3: add letters to the panels and explain statistics in the caption.
- Fig. 4: the same
- Fig. 5: why were those sites excluded? And please check journal style for lettering the panels.
- Fig. 6: Use the same names and ordering to the communities as previous figures.
- L. 204: use italic font for 'p' (check throughout, there are other occasions in which the p is not in italic font)
- L. 226-227: do not use parentheses side by side, just open and close parentheses once then. This happens elsewhere as well, so check throughout.
- L. 228: check paragraph indentation
- L. 263: respectively should go inside the parentheses with the p values.
- L. 269-270: "however, mean seed masses increased from typical grasslands to desert grasslands and desert ecosystems and then to forests (Figure 2)" => This is just repeating the first half-sentence. Actually, this entire sentence is confusing, because you start the sentence mentioning the longitudinal gradient of Fig. 4, but using the community types whereas the reader does not have a clue the ordination of community types along the gradient and in Fig. 4 whatsoever. Please use in the rationales only thoughts the reader can follow.
- L. 271-272: you cannot draw mechanistic explanations from correlations simply like

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this. You have to explain how you think MAP and MAT might be affecting these trends, based on the indication that they might play a role due to significant relationships. I think this is a general comment that apply to other reasoning in the text (using correlations as causation). Please check throughout.

- L. 275: Note that vegetation syndromes involve trait values, you just listed trait names, not the values (e.g. the combination of low seed mass and of low fruit water content would be a syndrome).
- L. 290: larger leaves
- L. 294-295: so can you argue that the plants invest can invest more in (seedling) survival than in competitive strength?
- L. 302: can you back this up with references or with more details on this wide range?
- L. 382-283: please sort out the font sites and style. It seems it make more sense to start with the equation for St , then for Sa .
- L. 384: Sa is the average seed mass per species taken from the community total (St/n).
- L. 388: the authors should better connect this text with Fig. 1. For example, place the same symbols as parameters of the respective processes in Fig. 1.
- L. 399-401: So these are the key gradients, not longitude.
- L. 402-404: this can become its own sentence.
- L. 406-407: it is odd to say we need further studies when you just did this. Exclude this sentence, or make it more detailed about what else can be tested.

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