Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-392-RC1, 2019

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Reshaped acclimation traits of dominant tree species under manipulated rainfall would alter their coexisting relation in a low-subtropical secondary evergreen forest" *by* Lei Ouyang et al.

Anonymous Referee #1

Received and published: 24 October 2019

General comments: In this manuscript, Ouyang et al. present the results of a precipitation manipulation experiment in which the dry season was exacerbated or lengthened (along with a compensating increase in wet season water supply). The authors report a number of traits for the two dominant tree species at their experimental site, along with species-specific transpiration and water-use patterns. They conclude that the two dominant species show contrasting water-use strategies and their findings have important implications for the survival of these species under a changing climate regime. The experimental setup and amount of data collected is impressive, but the manuscript has

Printer-friendly version

Discussion paper

some key issues that I believe preclude publication at this time. Below, I have listed some broader feedback about the manuscript, followed by smaller line by line comments.

1) In general, I think the most novel aspect of this manuscript is the water uptake depth data. However, I think there is significant methodological detail missing about how water uptake depth was partitioned and there seems to be no statistical analysis comparing these data across treatments or species. I also think it should be mentioned that water samples were collected during a very limited time frame and that plant water use could have shifted during the experiment, as could precipitation 18O. Further, the authors make claims about these data (primarily differences between species), that look unfounded to me but could potentially be shown statistically. In sum, I don't think the water uptake depth analyses are valid as is. Without the water uptake depth data, I think the manuscript is fairly simple, merely showing the effects of the precipitation manipulation on transpiration, along with a few traits. 2) Related to the above point, I think there are some large gaps in the methods that make a solid interpretation of the results difficult. In particular, I think the manuscript is missing detail on leaf 13C sampling, lacks a discussion on the age of the bulk material they sampled for 13C analysis and what this means for their interpretations, neglects description of their water uptake depth model, and lacks some additional smaller details on sampling that I've highlighted below. 3) In many places throughout the manuscript, the authors claim their results speak to the competitive ability of these species using words like "competition", "success", "coexistence", "tolerance", etc. I do not think the data support these claims since the manuscript merely shows patterns in water use between the two species and some additional traits. I think this language should be toned down and I've highlighted areas where this issue came up. 4) Certainly, this is not a big issue at this stage, but I think the manuscript could use some significant editing for grammar and sentence structure.

Line by line comments: L1-2: I would really recommend altering the title. I do not

Printer-friendly version

Discussion paper

really think the study pertained to “acclimation traits”, nor were any traits “reshaped”. Further, I think that saying they “altered their coexisting relation” is not supported by the data (my point #3 above). L27: See #3 above about claims of competitiveness. L29-30: How are you defining drought tolerance? It seems like the two species had fairly similar responses to the precipitation manipulation in terms of transpiration and there was no evidence that either species was water stressed (point 3# above). L31: See #3 above about the coexistence terminology. L78-80: I do not think it is well supported in the literature that isohydric species tend to occur in mesic areas. They can and do exist most everywhere. L128-135: It seems like #1 and #2 are really similar in the sense that they both describe traits and water-use patterns. Maybe the authors could separate these objectives out into one about tree water use (and water uptake depth), and one about how traits mediate tree water use patterns. L161-164: I think it would be very helpful for the reader if there was a simple diagram (or perhaps labels added on to a time series figure) showing when precipitation was excluded or added back in for each treatment. L178: In order to interpret the effects of the precipitation manipulation, there needs to be information about how the experimental year’s climate related to average site climate. For example, if this was a really wet year, there may be no reason to expect significant treatment effects in the first place. L216: What depth were these samples collected at? L252-290: I think some text should be added (either in the methods or discussion) that clarifies what the 13C signal would represent. If these leaves had been around prior to the experiment, their bulk tissue 13C would incorporate the 13C signal from climatic conditions at the time of leaf expansion, the carbon used to make those leaves, and any dynamics influencing non-structural carbohydrates since leaf expansion. Any information on the life span of these leaves would help here, or simply a caveat that the 13C signal could be complicated. L275: When were rainfall samples collected? Were these multiple samplings or were there 4 replicates of one rainfall event? If it is the latter, I don’t think you can assume that this rainfall event is representative of all rainfall. L286: Please describe in detail what IsoSource is and the methodology behind how it partitions water uptake depth. L343-350: What are

these percentage reductions in comparison to (i.e., what counted as dry versus wet seasons)? L364-378: As mentioned in point #1, this sections needs some statistical analysis to be able to draw any conclusions from the data. L390-391: Is this saturation model warranted for the data considering that most of the responses seem to be fairly linear? Is there a first principles reason to expect a saturation relationship? L424: I'm confused as to how Huber value can be used to understand how much water a species has access to. L427: See point #3 above regarding the "drought-tolerant" terminology. L433-434: I'm not sure this is supported by the data (especially since there are no stats). The bars seem to be similar in size and the error bars seem to overlap. L489-490: I'm not sure this claim is supported since the treatment effects seemed to be fairly similar in the two species. Perhaps specify or cut this sentence? All figure and tables: Please specify what the +/- means in the tables (standard error, deviation?), what the lettering notation indicates, and what error bars represent. Fig. 1: During what hours were these daily values calculated? It might be more relevant to present mean daytime PAR and VPD. Fig. 3: I think the clarity of this figure could be improved. In general, it is hard to parse out trends due to the experiment since the points are so close together. It is also hard to interpret the panels for each species since they encompass different and overlapping time points. Perhaps clarify what manipulation was occurring in each panel, or maybe make one longer time series graph with all the treatment times labeled?

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-392, 2019.

Printer-friendly version

Discussion paper