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Biogeosciences Discussions

Interactive comment on "Bottomland hardwood forest growth and stress response to hydroclimatic variation: Evidence from dendrochronology and tree-ring δ^{13}C values" *by* Ajinkya G. Deshpande et al.

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My primary concerns and suggestions are related to concepts that need to be further developed in the discussion (and sometimes other areas) that are not clear or clearly supported from the data presented. Specifically, the authors invoke soil moisture deficits as the mechanism to explain hydroclimate, stable isotope, and growth relationships. However, beyond the observations that the wet site floods more frequently, there is no measure of soil moisture status among these sites. While I believe the authors that there is variability among the sites, some form of estimation of soil moisture

status would strengthen their arguments when invoking it as a mechanistic explanation for their findings. For example, how do the physical properties of the soil reported in table 1 relate to water holding capacity and plant available water (especially given the large difference in clay content).

Authors' Response: We have now cited a study conducted in 2018-19 at three of our sites (including the wet site) by researchers from the Department of Geology and Geophysics, Texas A&M University. In this Earth Resistivity Tomography (ERT) study, soil resistivity (and conductivity) was used as a proxy for soil moisture while controlling for other effects like soil salinity. After repeated seasonal ERT measurements, the top 1 m soil at the wet site was found to be more conductive as compared to the drier sites as a result of higher soil moisture. We have added the resistivity measurements to Table 1, which gives a more empirical estimate of differences in soil moisture between the sites.

The other area that needs to be more clearly defined and developed is resilience. The authors refer to the resilience of these systems frequently throughout the paper, but resilience is not defined and it is unclear how the data presented fit into a predictive framework for resilience for these systems. For example, the authors do examine possible carry-over effects in 13C from one year to the next, but did not find any relationship. How does this fit into a resilience conceptual framework? Given that the authors emphasize the importance of these ecotone systems, it will greatly improve the impact of the paper to have a concrete framework for resilience and to put their findings within that framework.

Authors' Response: We agree with your and the other referees' comments about the use of the term "resilience" in this study and we acknowledge that defining resilience in this ecosystem would require a more detailed approach. Consequently, we are omitting the resilience narrative from this study and will explain our findings as a result of differences in physical/hydrological conditions between sites.

Specific comments by line number:

65-70: this is pretty standard methods and not necessary in the introduction

Authors' Response: We have edited and shortened this portion but we think a brief mention of these methods might be necessary because we have now converted our δ13C values to Δ13C notation, which is a function of atmospheric δ13C. The description in these lines provides justification for using Δ13C instead of δ13C.

88: Most of your examples and the assumptions related to hypotheses are related to stress which would result in declines in stomatal conductance. Why invoke increased assimilation here? Is that a likely driver in this system?

Authors' Response: We agree with this suggestion. We have removed the use of the term 'assimilation' from line 88 as we are mainly concerned about reduction in stomatal conductance which is the stronger driver in this system.

156: Why does this say "at least eight" when table 3 indicates that there are only eight? All figures with regressions. It appears that regression lines are only include for significant relationships so it is not necessary to also indicate significance with a *

Authors' Response: Edited as per the suggestions. We had earlier mentioned "at least eight" because we had sampled more than eight trees at each site but some samples had to be discarded due to low ring visibility.

291: Physiological resilience is not defined. How is this different from another form of resilience? What do you mean by this term?

Authors' Response: Please refer to our response to comment #2.

302 – 307: What about the BP site? Anything to report?

Authors' Response: Growth at BP declined with δ13C similar to that at the other two drier sites but the slope was not significant, hence we have not reported the numbers.

332 -333: It unclear where the reader was supposed to gather that growth was more heterogeneous in the wetter site. Did this play out in 13C? The relationship to waterlogging is highly speculative. What supports this interpretation?

Authors' Response: More heterogenous growth at the wet site is interpreted from the lower series intercorrelation value for this site shown in Table 3. This lower series intercorrelation value indicates differences in tree-level growth rates within the wet site.

349-351: this seems to be an incomplete line of discussion. Can you say more?

Authors' Response: Sentence rephrased: "Generally, forested wetlands are known are not known to be drought stressed because of ample soil moisture availability. However, it is important to note that in parts of Columbia Bottomlands, depending on site-level hydrologic conditions, vegetation does experience drought stress during some years when dry climatic conditions persist over a longer period."

353: how are dry edaphic conditions defined or measured?

Authors' Response: Resistivity measurements as a proxy for soil moisture from the ERT study mentioned in our earlier comment along with visual assessments are the parameters that we have used to characterize drier edaphic conditions.

376: "severely detrimental to this ecosystem": I'm very unclear what data indicates what hydrologic change will be severely detrimental. Based on the RWI, all of these sites seem to be broadly growing in a similar way. Are there signs of mortality? In general, how is this statement supported?

Authors' Response: The statement in line 376 is supported by Figure 4, which shows decline in growth during years with low precipitation, high temperatures and high drought severity during the mid-growing season at all sites. However, we understand your concern and we will rephrase this sentence by mentioning "reduced productivity" specifically instead of mentioning detrimental effects in general.

402-413: this paragraph needs organizational work. It is unclear from the topic sen-

tence where is paragraph is going. The paragraph is difficult parse until you get to the end.

Authors' Response: Paragraph rephrased as per suggestion: "Climate models have predicted a significant decrease in growing season precipitation and increase in temperature throughout Texas (Jiang and Yang, 2012) and especially in the Brazos River basin (Awal et al., 2016) where our study area is located. This region occurs at the extreme southwestern edge of the bottomland hardwood forest type (Bray, 1906; Putnam et al., 1960), which is also the southwestern edge of the distribution of Q. nigra and many other wetland tree species. Edges of distribution ranges usually experience environmental conditions that are less favorable (drier and warmer) to the species as compared to the range core (Rehm et al., 2015), which makes them more resilient and better adapted to survive in stressful conditions relative to core populations (Gutschick and Hormoz, 2003). Therefore, as climate changes, these native wetland tree populations will play key roles in helping the species maintain their geographic distributions."

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