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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.

Anonymous Referee #2

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This manuscript presents an analysis of tree ring width and d13C for quercus nigra from 4 bottomland hardwood sites in Texas. The sites range from relatively dry to waterlogged and therefore capture a range of hydrological conditions despite their relative proximity and therefore similar atmospheric forcing. The data show that growth at all sites was positively correlated with mid-season precipitation. Though the relationship is weaker at the waterlogged site indicative of less drought stress. The d13C of the trees at the dry sites were correlated with early season precipitation indicating a physiological response to early season hydroclimate conditions. Limited evidence of correlations

with previous years indicate the d13C is driven by the current year's assimilates. There was overall little evidence of either growth or physiological response at the wet site to flooding indicating that waterlogging did not have clear detrimental effects in terms of physiological changes or reduced overall productivity. This is an interesting study documenting dynamics in a complex and changing environment. The design of the study allowed for the effects of soil moisture stress to be removed from one site by using a water logged site. That said, I think the study could benefit from some additional discussion and analyses which I describe below.

(1) Other than soil moisture, the authors did not make any substantive effort to consider other differences between the wet and dry sites. The wet sites would have enhanced ET (more E) and therefore higher humidity in the canopy, this would effect VPD and evaporative demand on the leaves. The surface water would also lead to a different bowen ratio and perhaps less extreme temperatures for the soils. The reduced heating at the soil surface would reduce convection and mixing into the canopy that might influence flushing of the canopy. My general concern is that while the broader regional meteorological conditions might be similar between sites, the actual conditions in the canopy could be very different. This could indeed modulate growth and d13C in a way that is not really about "physiological resilience" as the authors state but more simply that the trees are just experiencing different physical forcing.

(2) The authors should take a more mechanistic approach to interpreting their d13C data. This does not need to involve a highly sophisticated model but a simple approach that considers d13C as a function of atmospheric 13C and A/g (as in the Farquhar model). While I think it is qualitatively fine to discuss d13C variability as merely a "stress indicator" this not necessarily true. Notably, the trees at the wet and dry sites have similar growth rates but very different d13C values. One question that I have is whether there is a source of depleted CO2 that emitted from the waterlogged sites that influences the d13C of the cellulose there. For example, if these soils produce isotopically depleted methane that is oxidized to CO2, this could deplete the isotopic

ratio of the soils. The most interesting finding in the study is that d13C for the wet sites show their more depleted values when growth rates are lowest. This is a very unique and exciting result. A series of simple sensitivity tests with a d13C cellulose model could be very helpful.

(3) The design of only measuring d13C during extreme years is not ideal with respect to understanding how a previous year influences d13C. The authors would need to measure the isotopic ratio of the cellulose for the year following the extreme year as well to see if that signal shows up. It is too late to ask this, but the authors would have a more compelling argument regarding previous years if they had used this approach.

(4) This is a rather simple comment, but I was surprised to see the authors did not use VPD as an explanatory variable. Particularly as to whether the response of wet site to precipitation was a result not of soil moisture stress (i.e. precip.) but actually because drier years were associated with higher VPD.

Small comments: 100: How was the frequency of flooding characterized between sites?

254: The actual number of d13C measurements and presentation of the data is sort of obscured. It would be nice to see a timeseries of all the d13C even if the timeseries is discontinuous.

Fig 3: Were BC and DB not different as well?

269: The presentation of the actual correlation analyses would be nice to have in the main text even if the correlations were insignificant. This Especially true for the lag analyses.

272: "…that for the wettest site, growth rates would decline due to flood stress…"

285: With respect to previous season effect on d13C, it would seem the biggest effect would be from the late growing season of the previous year.

297: It is noted that d13C and precip. are not correlated for the wettest site. However, it is interesting that the distance between d13C at the wettest site and the regression between d13C and precip. for the driest sites increases as it gets drier. A potentially useful variable would be ïĄĎ13C defined as d13Cwet-d13Cpredicted from dry sites.

307: You say growth was sustained at wet sites even during dry years but it is also true that growth did decline somewhat during dry years, so there was some sensitivity.

Figure6: As mentioned above, the points on the lower left of the graph (low d13C and low ring width) are extremely interesting. I would like to see a more mechanistic approach to explain them.

314-326: The first paragraph of Discussion is too long. It is fine to revisit some of the context and motivation for the study here, but this can be shortened to 1-2 sentences and then jump into discussing the results.

333: I was a little confused about the comment of more heterogenous growth at waterlogged sites. I don't remember that result being presented and it is difficult to understand why. On the one hand, waterlogging does not have a big effect on growth but its presence or absence on the local scale does drive differences in growth. Please clarify. 338: As noted, there is an argument for physiological resilience at the wet site but also an argument that because of the site's hydrology, soil moisture changes persist across years and reduces the response to precipitation variability. This could therefore be a physical/hydrological not physiological process.

As noted, I think it is worth addressing whether there could be low d13C in the atmosphere of the waterlogged site.

400: The role of previous season's conditions on d13C is not sufficiently established here for reasons described above. More attention and discussion needs to be paid as to why the analysis might be limited i.e. you would need to measure the d13C following an extreme year.

406: Not “harsher” conditions, per se, but conditions less favorable to the species.

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