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Biogeosciences Discussions

Interactive comment on "Feedbacks between earlywood anatomy and non-structural carbohydrates affect spring phenology and wood production in ring-porous oaks" *by* Gonzalo Pérez-de-Lis et al.

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REFEREE: Overall, this was an interesting and useful contribution to the ongoing discussion about the roles of NSCs and plant hydraulics on tree phenology, growth, and survival. In this paper the authors studied two congenator oaks of that contrast in their ecological strategies to compare the impacts of winter NSC storage, hydraulic diameter, and budburst on earlywood vessel production (EVP) and the subsequent impacts of EVP, hydraulic diameter, foliar density, growing season length, and NSC on latewood production. Species were evaluated at three sites that form a moisture gradient

in northwest Spain.

This paper was generally well written and well cited and most of my concerns are moderate and should not change the overall results.

ANSWER: We kindly thank the referee for taking our discussion paper under consideration, and for the helpful and constructive comments raised to improve the quality of our research. We agree with the referee that some methodological aspects need to be more clearly stated. In our opinion, we can easily address such points in a revised version of the paper. Our proposed changes are listed next to the points raised by the referee.

REFEREE: Page 2, Lines 22-24: Here you describe one of your study species, but you fail to describe the other. I know Q. robor is more common, but not all your readers will be familiar with its ecology.

ANSWER: In the revised version, this paragraph is modified as follows: "This is the case of the ring-porous oaks Quercus robur L. and Q. pyrenaica Willd., which coexist in NW Iberian Peninsula. The former is widespread in Europe, being abundant in areas with mild-oceanic climate. By contrast, Q. pyrenaica is dominant in various mountainous ranges of the sub-Mediterranean area, hence exhibiting multiple adaptations to cope with summer drought and winter frost, such as late flushing (Pérez-de-Lis et al., 2016)".

REFEREE: Page 3, Line 21: No description is given as to HOW the trees were selected. In particular, I have no idea if the authors put out plots of some standard design, picked 'representative' trees, or picked the 40 biggest, healthiest trees they could find. No description is given of the size threshold or other criteria for inclusion (we could in theory be comparing a sapling at one site to a 100cm DBH tree at another). Unfortunately, ample evidence exists to show that trees and locations chosen subjectively to be 'normal' or 'representative' tend to be far better off than random, which unfortunately would cause all of the ANOVA-based comparative analyses to fall into question and require very careful interpretation of the regression-based analyses. I think in any revision the authors need to provide considerable more information about sampling and the editor should pay careful attention this information in assessing the validity of the work. For the remainder of the review I'm going to assume the sampling was done correctly (randomized locations, randomized trees within location).

ANSWER: The study was carried out at three sites where both study species were present. At all the sites, trees were randomly selected from those belonging to the study species, although suppressed individuals were disregarded. In the revised version, we will include a more detailed description of tree selection.

REFEREE: Page 3, Line 32: How was sapwood area determined?

ANSWER: Sapwood can easily be distinguished by colour. Heartwood in oaks is brown-coloured while sapwood has a pale tone (Figure 1, Supplement). For the sake of clarity, this information will be included in the revised version of the manuscript.

REFEREE: Page 4, Line 37: I'm going to assume growing season length is an individual-level measure and not a site-level measure (as is commonly done), otherwise this effect is confounded with the site random effect.

ANSWER: Indeed, growing season length is an individual-level parameter. In fact, in the discussion paper is said that "Leaf phenology was weekly monitored during 2013 using binoculars (10×) at ca. 10 m distance from each tree" (page 3 line 22). Yet, this sentence will be rewritten as follows: "Leaf phenology was weekly monitored for each tree during 2013 using binoculars (10×) at ca. 10 m distance"

REFEREE: Page 5, Lines 1-2: Here you're talking about averaging over a set of models, but in the paragraph above you only describe a single model. Where does this other set of models come from? Why do you need another set of models? Why is the sum of Akaike weights an appropriate measure of the relative importance of a variable? This quantity is quite challenging to interpret, especially in a GLMM, and fairly unintu-

itive. I'm all for sophisticated analyses when needed, but why not stick to a simpler analysis (e.g. the proportion of the variance [R2] explained by each covariate),which in my mind would be much easier to interpret and a more direct measure of importance. As I tend to look at the Figures before I read a paper, I'll also note that the meaning of 'relative importance' (essentially a weighted number of times that a variable was included in the model) is not clear in the Figure.

ANSWER: What we meant with "set of models" is that we calculated the AIC of the models containing all the possible fixed-effect combination. In the discussion paper we used an information-theoretic approach to identify the most influent fixed effects of the model. According to this procedure, models were compared using their AIC scores (the lower the AIC, the better the model fit). Hence, models were ranked and averaged in order to assess the relative weight of each variable (we averaged 95% of all the fitted models according to their AIC scores). As we mentioned in the discussion paper, this method was detailed in Burnham and Anderson (2002), and has been used in a recent paper analyzing possible limitations of carbon supply on secondary growth published in Biogeosciences (Guillemot et al. 2015).

In order to have a more confident analysis, the proportion of the variance explained by each predictor can also be calculated and provided (either in the main text or as a supplementary material). Although there are different model selection procedures, with different advantages and caveats, we think that combining the information-theoretic approach with the R2 of the covariates (as proposed by the referee) should be a good alternative to improve the robustness of our results. In addition, a table summarizing the estimates and significance for each covariate (in the full model) can be presented as supplementary material.

REFEREE: Page 5, Line 8: You should report the degrees of freedom in the F test (and all other tests). If this is going to be the same for all subsequent analyses state that here at the ïňĄrst usage, otherwise make the df explicit for each analysis.

ANSWER: We agree with the referee. df values will be included in the revised text.

REFEREE: Page 5, Line 34: Be consistent with notation. In all other places you refer to sites by their moisture status, and here you've reverted to a site code, and I'm not sure which site you're referring to.

ANSWER: We apologize for this mistake again; we put "ATL" instead of "Hyperhumid site". This error has been corrected in the revised version.

REFEREE: Page 5, Line 35: Were trees with powdery mildew included or excluded? Why wasn't this included as a covariate? Why is there not more in the discussion about how this could be affecting results?

ANSWER: Unfortunately, powdery mildew infestation has not been quantified. Yet, it is relevant to take into account that all the trees were more or less affected by the pest (it would not be considered as a covariate, but as a part of the site effect). This is a very frequent disease at oak forests in the study region, but their effects during the humid spring of 2013 were higher than usual at the hyperhumid location. Thus, we decided to provide this detail, which probably contributed to impair xylem growth at this site.

Actually, the possible effect of powdery mildew infestation was mentioned in the Discussion (page 6 line 34). Since we did not perform any measurement, we marginally commented this issue.

REFEREE: Page 5, Line 37 to Page 6, Line 3: In the Results (here) and Discussion (below), I'm concerned that the authors are over-interpreting the biological signiïňĄcance of results that are statistically signiïňĄcant but have low R2. Looking at Figure 5, about all I'm comfortable concluding is that SS and tree size have a negative impact on budburst in both species, and that SS had a positive impact on EVP in Q. pyrenaica. Effects in the R2 of 3-6% range (Starch, Q robor EVP) don't seem worth discussing, and those in the 10-16% range (Dh, SS) should be acknowledged as weak.

ANSWER: Thanks for the comment. In the revised version, we will include a more

careful interpretation of these significant relationships accounting for a low variance in the observed parameters.

REFEREE: Page 6, line 31: Tree density effects are speculative

ANSWER: This idea was based on differences in stand tree density (reported in Pag 3 line 12) and basal area (according to stem diameter measurements) among locations. We acknowledge that direct measurements on tree competition were not carried out, however recent work modelled a strong effect of competition in Quercus pyrenaica secondary growth (Fernández-de-Uña et al., 2016). The sentence in Page 6, line 31, will be rewritten as follows: "One explanation could be that lower tree density at the subhumid site might be associated to a lower inter-tree competition, which is assumed to favour both carbon uptake and xylem growth (Fernández-de-Uña et al., 2016)".

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-227/bg-2016-227-AC2-supplement.pdf

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