

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.

Anonymous Referee #2

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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.

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

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

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).

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

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

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 unintuitive.

BGD

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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.

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 first usage, otherwise make the df explicit for each analysis

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

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?

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 significance of results that are statistically significant 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. robur* EVP) don't seem worth discussing, and those in the 10-16% range (Dh, SS) should be acknowledged as weak.

Page 6, line 31: Tree density effects are speculative

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