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Interactive comment on “Influence of wood density in tree-ring based annual productivity assessments and its errors in Norway spruce” by O. Bouriaud et al.

O. Bouriaud et al.

obouriaud@gmail.com

Received and published: 14 July 2015

We are grateful to referee 1 for his constructive suggestions and comments. The manuscript has been revised based on the suggestions and questions, and the response to the major and minor comments are listed below. Following the editorial instructions, the response to each comment is structured as: 1- comments from the referee; 2- author's response and 3- changes in the manuscript.

##

Comment: Climate does get one short mention in the first and last paragraph but could be stressed more.

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Response: That is true, thank you for the suggestion. A sentence was added in the discussion. The emphasis cannot be put too much on climate though, since it is not studied directly.

Changes in the manuscript: First paragraph of the discussion section: “Climate is indeed probably the most important driver of WD variations with influences at both inter- and intra-annual time steps (e.g. Gindl et al. 2000, Bouriaud et al. 2015).”

##

Comment: If short-term variations in production is not the question, the radial increase in density is still important and will result in an error if only a site- or species specific mean density is used. This is not given as the objective of the study, but is still an important consequence of the study and data.

Response: Thank you for the suggestion. A paragraph was added. Since the list of references was already quite long (60 articles), no new reference was introduced here.

Changes in the manuscript: “Even if using a site-specific WD value, neglecting the radial increment of WD (i.e. the age-related trend) will also lead to under-estimating the biomass increment. This source of error can unfortunately not be compensated by a larger sampling since it affects all the trees simultaneously. This has consequences not only for the annual productivity estimations but also for periodical productivity assessments, such as those conducted on permanent sample plots over a 5 or 10-year period.”

##

Comment: Since the authors also measured axial variation in WD, it would be good to give an estimate of potential errors for not accounting for this or a method to estimate axial variation if WD is only measured from the tree base, as is generally the case with tree ring studies.

Response: We agree, the axial variations in WD are another potential source of varia-

tion. They are acknowledged in the manuscript (P 5891 L22-28). As described in the methods section (P 5877 L13) the measurements of WD were realized on the breast height discs only. The axial variability was thus not analysed in this study. The tomography offers very interesting perspectives of investigation of possibly of modeling and will help efficiently address this issue in the future.

##

Comment: An additional difference between productivity estimates via remote sensing or Eddy covariance and tree ring based approach is the carry-over effect from one year to the next in ring width and probably also WD.

Response: Thank you for the suggestion. A sentence was added in the discussion.

Changes in the manuscript: “Another issue in using the tree-ring parameters (width and density) to produce annual productivity estimations is the presence of autocorrelation or carry-over effects in the series, which are reflected in the derived productivity estimations but are generally not observed in the carbon fluxes measured or modeled (Babst et al. 2014a, b, Ramming et al. 2015).”

##

Comment: Other/technical comments. Introduction page 5874/ line 13 “annual wood density was proved to be related to ring age or to tree diameter, with higher values close to the pith in many species”. I believe it is much more common to see a radial increase in WD.

Response: We agree but there are possible variations related to the growing conditions and the stand dynamic, and to the species. We stated that WD is related to age or diameter without specifying the sign of the slope, which is more general.

##

Comment: “Molto et al., 2013” is not in references.

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Response: Thank you, that was corrected.

Changes in the manuscript: 2012 changed to 2013.

##

Comment: It is unlikely that bark thickness increases linearly with diameter (bark is shed, wood is not), but the error of this assumption would be small.

Response: In the absence of a bark thickness model, it seemed to be the most efficient solution.

##

Comment: 5878/21 “several independent variables were tested, such as the diameter and the ring cambial age” – Please provide a full list of parameters tested, even if they were found not useful for the models.

Response: The formulation was misleading, the variables tested were based on diameter or cambial age and their transformation (inverse, square root) but no other variable was used. The text was modified.

Changes in the manuscript: In the Material and Methods, section 2.2.1 the sentence was modified to: “Following recent publications on Norway spruce wood density (Franceschini et al., 2010; 2013), the diameter and the ring cambial age (as counted from the pith) were used as independent variables.”

##

Comment: Why does, in scenario 3 and 4, WD scale with $(\text{ring width})^{0.5}$ and $1/\text{DBH}^{0.5}$, and not another exponent? The exponent could have been taken from the RW : WD relationship in Fig. 1b and a similar one for WD : DBH, or include the exponent as a parameter to be estimated in the model.

Response: At first the exponents have been left free and fit using nlme. The values

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obtained for these parameters were very close to 0.5 and the simplification of the model (forcing the parameters to 0.5) was tested. The reduction of the fit quality caused by this forcing was very small and the LRT test confirmed the validity of this simplification, which was therefore implemented. This is described in the section 3.2 of the results (P5883 L 15-23).

##

Comment: Results 5883/1 "Variations in WD were mostly related to ring width with a linear correlation of 0.75" – The correlation in Fig. 1b is very strong, but apparently not linear, so fitting a nonlinear function would result in higher r2. The same paragraph continues "Although not really linear, the decrease of WD with ring width had a rate of 0.48 kg m-3 mm-1 , meaning that density is divided by two when ring width is doubling." If one assumes a negative linear correlation, the conclusion that density is halved when ring width doubles is wrong. Anyway, such a relationship is not seen in Fig. 1b and it would be good to fit a non-linear function and provide parameters.

Response: The sentence seemed to introduce confusion and the estimation of the rate of decrease of WD was erroneous. It was therefore discarded. Fitting a model would introduce other difficulties: it would not be used further and could create confusion with those used in the MCMC.

Changes in the manuscript: The sentence creating confusion was dropped.

##

Comment: Figure 6. Comparison of plot-level annual biomass increments and prediction intervals (a) for the 4 scenarios" - Fig. 6a has 13 panels with number of trees from 31 to 62. Why the variable nr. of trees, and where are the 4 scenarios? If the point of the 13 panels is to show the effect of n, this would be better to show in a n : variance correlation or something similar.

Response: We agree. The figure 6 was meant to display both the difference between

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prediction intervals and the inter-annual variations. The new figure displays the prediction error as a function of the number of trees. It put in lights the fact that prediction error in scenarios 2 and 4, both of which use a random tree-level effect, display a slight reduction in the error with the number of trees.

Changes in the manuscript: Figure 6 was replaced by a new. The text in the result section (results, section 3.6.2) was changed accordingly to the new results brought in evidence: “The variation between years in the prediction error was also very low (Fig. 6) despite contrasted ring widths. The error of the predictions based on regression errors only (sc1 and sc3) did not vary with increasing number of trees in the plot (Fig. 6). In contrast, the predictions error decreased slightly with increasing number of trees for the scenarios that used a (tree-level) random-effect term (sc2 and sc4).”.

##

The other technical modifications suggested were all implemented (e.g. product/produce; $SD \pm 2.1$ years).

Interactive comment on Biogeosciences Discuss., 12, 5871, 2015.

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