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Interactive comment on "The dynamic of annual carbon allocation to wood in European forests is consistent with a combined source-sink limitation of growth: implications for modelling" *by* J. Guillemot et al.

Anonymous Referee #1

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General comments This manuscript discriminates between the drivers of woody growth at close to 1,000 site.years. Because some possible drivers – GPP, water stress, . . . - are not directly measured in the experimental plots, a process-based model is used to estimate these drivers instead. Key conclusions are a hierarchy of all possible drivers according to their ability to explain either inter-annual woody growth variability or spatial woody growth variability. This hierarchy is – in my view – the main novelty brought by this manuscript, and a welcomed one. The method underlying this classification is partly questionable: the authors claim that the statistical models they use are robust

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to collinearity in explanatory variables but this claim is not sufficiently justified (see detailed comments). However, even if collinearity ends up being a problem, this would only add uncertainty to the results and would not greatly reduce the interest of the manuscript. As a result, and although some details could be improved, I think the the manuscript is readable and worth being published.

Detailed comments The claim that field measurements are combined with process-based simulations (e.g. p2214 l. 9, p2218 l3) disturbed me for a long time as I sought to distinguish between what results came from the model structure and what really was observed on-site. Then I remembered that simulations are only used when measurements are not available, and that the dependent variable – woody growth – is measured (the confusion coming from the fact that the model also simulates woody growth). Unless I missed something, the study is mainly empirical, and only supplemented by process-based modelling to estimate some possible drivers of woody growth when more direct methods are not applicable. Therefore, although I do not dispute the "combination", I would recommend clarifying this by removing the term "combination" throughout the text, and highlighting that modelling is only here to estimate explanatory variables (or drivers) when these can't be measured.

P2215 l4 This 60% figure does not directly come from Pan et al (2011). Their estimate of 2.6 PgC yr-1 divided by AR5's estimate of 8.3 PgC yr-1 for fossil fuel emissions would land around 30%. How do you get this 60%?

P2215 l26 An overview of the mechanisms potentially underlying cambial activity other than C availability would be useful here (they are provided later in the discussion).

P2219 l16-22 The key features (e.g. list of explanatory variables) of the empirical model and the allometric function should be provided in the manuscript itself (not in the SM).

P2219 l22 What does "historical basal area" mean? Basal area measured before the measurement years? Since when?

P2220 l6-12 Although SWC and SNA can unquestionably be put in the "plot fertility" category, putting LAI and LNC in the same category is debatable as these variables depend among others on management and stand age as well as fertility.

P2222 l1-4 How are these water stress indices defined? As water stress ends up being high in the hierarchy of drivers of woody growth, it is important to explain it is characterized.

P2222 l24-26 See general comment. The statistical models used are apparently: Pearson's correlations, multiple linear regression, and random forest. The robustness of the first two to collinearity in explanatory variables is not justified (and indeed, it would be difficult to justify it) and the justification provided for random forest (p 2224 l23-25) is that the collinearity problem is diminished by the random selection of variables participating in the classification. I'm clearly not an expert of this algorithm, but I fail to see how random selection reduces collinearity: if two variables (eg. GPP and water stress) are strongly correlated in the sample, selecting sometimes one and sometimes the other will not diminish the fact that they both tend to provide the same clusters and that therefore it's difficult to say whether it's GPP or water stress which explains why the clusters are "different". I acknowledge that I may be missing some background here, but so will many readers so your claim should either be better justified or dropped. If dropped, then the results should be discussed with the collinearity problem in mind.

P2225 l10-15 How variable is Ra? If GPP and NPP are both correlated with AWBI and Ra is not, an obvious possibility is that Ra is broadly constant in CASTANEA. Can you rule this out?

P2226 "between" sites is more appropriate than "among" I think. There is a typo in line 5: "did not revealed".

P2228 l5-10 One of your key explanations is species difference. Would species difference reconcile these cited works? More generally, how do your results help in solving the apparent paradox between these cited works?

Table 4 Which data has been centered and scaled? Why?

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