Answers to the comments from Referee #2

We would like to sincerely thank you for the time you spent on our manuscript and for your insightful comments and suggestions.

Because you pointed some misspelling in your comments, the entire manuscript was edited for correct English language and style.

Very well written and presented study, though the interpretation of the results at times could be viewed as overreaching. The main issue is that the C source is modeled, and therefore the relationship (or lack thereof) between C source and biomass increment are highly dependent on the accuracy of the modeled C source. Unfortunately there is no data available to estimate the accuracy of the modeled C source at the studied sites (with the exception of Puechabon, though no validation is presented here). The authors claim that the model has been widely validated at European sites. Of course practically all models are, but the quality of their extrapolation to other sites remains subject to the idiosyncrasies of those sites.

The issue is that authors repeatedly highlight the lack of relationship between source dynamics and biomass increments as evidence for a sink limitation, but do not acknowledge that there could be errors in the modeled C source that are responsible for the lack of relationship. Indeed the modeled C source is regularly presented in a way that could lead the reader to believe the authors are presenting observed C source. The C source should be referred to as the modeled C source at all times, and the impact of

the potential disjoint between modeled and actual C source should be discussed in an open and non-defensive way.

Regardless of the above, the findings and approach used in this study are a novel and valuable contribution to a growing area of interest. I have no doubt it will be of interest to the readers of Biogeosciences.

We acknowledge that process-based modelling is a source of uncertainty that was not properly discussed in the first version of our paper. As a consequence, we clarified throughout the text whether the considered explanatory variable was simulated or measured (e.g., l354, l360, l366, l392). We additionally discuss in section 4.4. the potential impact of model uncertainty on our results and the fact that the quality of our simulations remain subject to the idiosyncrasies of the sites used in this work. Among other considerations, we acknowledge that "A third factor that hampered the ability of our empirical models to explain the annual growth variability is the potential disagreement between the CASTANEA outputs that were used as explanatory variables and the corresponding actual drivers. […] In particular, a number of past disturbances such as insect outbreaks, windthrow or unreported commercial thinning could have temporarily induced large discrepancies between the actual and simulated C fluxes (Grote et al., 2011; Hicke et al., 2012). The error that is attributable to model performance unfortunately remains unknown because of the absence of EC measurements at our study sites (except for the Puéchabon site, see Delpierre et al., 2012)." (l583-594).

However, as stated in the paper, the uncertainty of the simulated C fluxes was considered in both spatial and temporal analyses using a bootstrap procedure (Chernick, 2011): all linear models were fitted 1000 times, randomly sampling at each iteration the C fluxes values within the root mean square error of the CASTANEA simulations (Appendix S9) to obtain for each variable a

parameter estimate distribution. We finally retained explanatory variables with parameter estimate distributions excluding zero value in a bilateral 5% probability level. Consequently, the results discussed in the paper proved to be significant even when considering the reported uncertainty of CASTANEA flux simulations (l321-326, l.349-350, l409).

In the following, we address each of your specific comments.

Detailed comments:

Page 2215

Line 4: 60% of the fossil fuel emissions. Please check this. The airborne fraction ranges between 30-40%, meaning oceans and terrestrial ecosystems together take up about 60%. Estimates suggest the ocean accounts for 30%, which would leave 30% for global ecosystems (which includes all ecosystem types, not just forests).

We evocated the gross carbon uptake by the established forests and tropical regrowth forests, which amount to 73 PgC over the period 1990 to 2007, "equivalent to 60% of cumulative fossil emissions in the period" (Pan et al., 2011). We acknowledge that this statement was confusing as 1) we did not explicitly mentioned in the manuscript that we addressed gross carbon uptake 2) Literature usually reports the figures that you mentioned i.e. the proportion of anthropogenic C emission captured in established forests. We therefore modified our sentence based on Pan et al. (Table 3) as follows "Inventory-based estimates indicate that established forests have been a persistent carbon sink for decades, sequestering almost 30% of the world's total anthropogenic C emissions between 1990 and 2007 (Pan et al., 2011)." (l51-53) We chose the term "established forests" rather than "global ecosystems" because it makes more sense in the context of our work,

as Pan et al. reported that “within the limits of reported uncertainty, the entire terrestrial C sink is accounted for by C uptake of global established forests”.

Page 2216:

Line 7: Constant – constrained.

We followed your suggestion and corrected the manuscript (l84).

Line 7: This sentence structure is confusing. Perhaps rephrase as: “The above experimental evidence suggests that growth is mostly limited by the direct effects...”

We followed your suggestion and corrected the manuscript (l85-86).

Line 15: The statement that rising CO2 could increase the terrestrial sink only if growth is a source limited process is misleading. There are multiple pathways through which CO2 can increase the sink. For example, increased root exudates under elevated CO2, and increased allocation to roots in general, is now widely accepted. This is thought to alleviate nutrient limitation. Water use efficiency also increases under elevated CO2. This can alleviate water limitation, thus enhancing sink strength.

We acknowledge that our statement was naive and we accordingly corrected the sentence: ”The extent to which wood growth is under source or sink control is of paramount importance for predicting how trees will respond to global changes and specifically how increasing atmospheric CO_2 will affect forest productivity and the future terrestrial C sink.” (l89-91)

Line 19: the future of forest? Perhaps be more specific.

We further explained our statement in a new sentence :" The implementation of the respective roles of source and sink controls on growth in TBMs is therefore a substantial challenge for modellers, because it may determine our ability to project future forest C sink, diebacks and distributions" (l93-95).

Page 2217

Line 13: the intensity effects of which

We corrected the manuscript and clarified this sentence. "The woody biomass increment therefore appears to be under the control of multiple factors. The effects of these drivers are expected to strongly vary in space and time." (l114-116).

Line 26: in forests

We followed your suggestion and corrected the manuscript (l127).

Page 2218

Line 5: I would suggest increasing the font on this schematic. It will be quite small in the final print version.

We increased the font of the schematic for a better readability.

Line 25: ‘allowed to extensively assessing’. Please revise for proper language use.

We corrected the sentence as follows: “This hybrid approach allowed us to assess and disentangle the effects of previously reported environmental and endogenous drivers of C allocation to wood growth (Fig. 1).” (l154-155).

Page 2220 Line 10: used to rank

We rephrased this sentence (l204).

Page 2221

Line 6-7: The level of CASTANEA agreement with observed interannual variability in the Delpierre et al., 2012 study is indeed impressive. Are we to believe, however, that the model does similarly well for site for which there is no calibration data such as the ones included in this study? It is quite likely that when it was first applied to the sites in the Delpierre et al. study it did not do well at all, until some site specific characteristics were accounted for by adjusting parameters. This is the weak point of this study – we have no way of knowing if the model does a good job of reproducing variability in NPP at the studied sites. If it does not do a good job, then it is no surprise that modeled C source diagnostic variables were not found to be related to actual carbon allocation.

We acknowledge that the use of process-based modelling is a source of uncertainty that was not properly discussed in the first version of our work and that remain unknown in the absence of EC measurements at our study sites. Dedicated discussion of this point can be found in our above general answer.

Line 23: 2 day resolution

We followed your suggestion and corrected the manuscript (l241).

Page 2222

Line 12: State which carbon fluxes.

The considered C fluxes are GPP, NPP and Ra. We clarified this point in the sentence (l270).

Line 15: The age related trend.

We rephrased this sentence (l274).

Line 22: I'm not sure collinearity is the right word here, as the relationships are not necessarily linear. Perhaps covariance?

We agree that covariance is better here, as a more general statement (l281). Thank you for this suggestion.

Line 23: could hamper

We followed your suggestion and corrected the manuscript (l282).

Page 2223

Line 5: gathered – grouped.

We followed your suggestion and corrected the manuscript (l296).

Page 2224

Line 14: important – large.

We followed your suggestion and corrected the manuscript (l333).

Page 2225

Line 7: The elementary components of the simulated seasonal forest C balance. It should be made clear in all instances when referred to Castanea output that this is indeed model output.

We clarified in all instances whether the considered variables is simulated or not (e.g., l354, l360, l366, l392).

You are not simulating C balance, as the forest C balance also includes heterotrophic respiration. Please revise throughout the manuscript.

We acknowledge that this statement was confusing because we only simulated tree-atmosphere C fluxes. We therefore changed all the occurrences of "forest C balance" referring to our study to "tree C balance" (e.g., l237, l281, l286, l534).

Page 2226 Line 7: "no retained models included..." Please state whether this is for all species or just the temperate oaks.

This statement is for all species. We clarified this point in the manuscript (l377).

Page 227

Fig 5: These partial dependencies are very interesting. It would help the reader if the footer contained information on how they were derived.

We added to the Figure caption a short technical description of partial plot in random forest : " The marginal effect of a given variable *X* was obtained by fixing the value of *X* and averaging the RF predictions over all the combinations of observed values for the other predictors in the dataset (Cutler et al., 2007). The marginal predictions were collected over the entire range of *X* in the training data using a regular grid."

Page 2227

Line 25: "Our results have far reaching...." This is unnecessary.

We removed this sentence (l423).

Page 2228

Line 23: fluxes – flux

We followed your suggestion and corrected the manuscript (l445).

Page 2232

Line 21: It is disingenuous to cite a paper over a decade old in support of the claim that current models do not simulate the IAV of growth well. There are multiple studies

that show quite accurate simulation of tree ring IAV using GPP driven modeling (e.g., http://www.biogeosciences.net/11/6711/2014/bg-11-6711-2014.html).

We acknowledge that recent studies report very satisfactory simulations of annual growth based on C source modelling. We rather would like to emphasize that there is a risk of "getting the right answers for the wrong reason" (Fatichi *et al.* 2014) because of the high correlation usually found between GPP and cambial activity. Because the simulated fundamental processes are different, even if they obtain similar performances against observations, this could be of great importance for productivity projections under climate change. We therefore modified our sentence : "This C-centric perspective overlooks the possibility of sink control of growth and thus ignores results such as those presented in this study and those of earlier local studies (reviewed by Fatichi *et al.* 2014). Consequently, this perspective possibly hampers the ability of TBMs to project future forest productivity (Fatichi *et al.* 2014)." (l547-550).

Line 20-25. You need to add another explanation here – the possibility that modeled C source is not accurate enough. Even if your model is globally applicable with absolute confidence as it is presented here, it will still be unable to account for forest disturbances such as insect outbreaks, and various extreme events. This is a very important issue and must be discussed.

We added a further discussion of the possible implications of process-based modelling on our results, based on your suggestions. A dedicated discussion of this point can be found in our above general answer.