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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 #2

Received and published: 5 March 2015

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

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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.

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).

Page 2216:

Line 7: Constant – constrained.

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

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,

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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.

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

Page 2217

Line 13: the intensity effects of which

Line 26: in forests

Page 2218

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

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

Page 2220 Line 10: used to rank

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.

Line 23: 2 day resolution

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Line 12: State which carbon fluxes.

Line 15: The age related trend.

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

Line 23: could hamper

Page 2223

Line 5: gathered – grouped.

Page 2224

Line 14: important – large.

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

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

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.

Page 2227

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

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Line 23: fluxes – flux

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>).

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.

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