Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-291-RC1, 2017

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Gross changes in forest area shape the future carbon balance of tropical forests" *by* Wei Li et al.

Anonymous Referee #1

Received and published: 26 September 2017

General comments

This is an interesting study, pointing out the importance of using gross instead of net land use transitions, distinguish between clearing of primary vs. secondary forest and to define a specific and reasonable time horizon when making land-based mitigation policies. Three main steps were taken: 1) the comparison between different response curves, 2) calculating different theoretical scenarios with a bookkeeping model to show the importance of considering gross forest area change and finding critical and 3) applying the ratio to real net land cover transitions from satellite data. Thereby, step 2 clearly takes the highest priority and consideration.

Still, some revision is needed: the abstract is very long which makes it partly difficult to get the main message of the study. Also, the gap in current research is not carved

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out very well (page 2, ln 16 ff, says that other models have already implemented gross transitions) and the objectives should become clearer. In the introduction a two-fold purpose of the study is mentioned, what about the 3rd step? What was its objective? The 3rd point cannot be found in the method section, it is just roughly described in the results. Thereby some steps remain unclear: e.g. the model considers LC transition to take place at time = t0, but the satellite covers a time series of 12 years. Are all the transitions during these years threatened as if they took place at one time t=0 and then the results for the different time horizons of 20, 50 and 100 years are calculated based on that? Or is the exact time of each transition considered and the time horizons starts to be calculated after the last transition took place? Or do the gross transitions in this case refer not to time (i.e. shift of one LC to another LC and back) but instead refer to transitions within the calculated gridcells of 0.5° resolution, as the satellite data was mentioned to have a 30 m resolution?

One possibility to handle the dominance of point 2 would be to make it to the only objective, and shift point 1 to the method section – the comparison seems to be anyhow just a plot of the different curves that justifies the usage of response curves based on Poorter instead of those from Houghton and Hansis. Another possibility would be to include 3 in the method section and give point 1 and 3 more weight - e.g. by calculating the critical gross to net ratios based on the Houghton and Hansis functions, applying it to the same grid cells and comparing it then with the results based on the Poorter function. This would be interesting outcome and extend the first objective of comparing the different response curves to more than just a simple plot of the different curves in the same graph. Further, it would be very interesting to not only know whether each gridcell was a sink or source but also to quantify the ELUC and sum it to total number – one if everything was primary forest at the first transition, and one as if all was secondary, and the same if the other response curves were used.

Specific comments

Abstract: The abstract should be shortened to better focus on the findings, which would

make it easier to read and understand. E.g. is the 3rd sentence really relevant for the findings of this study? Especially also from line 19 to 27 there might be possibilities to shorten, summarize and simplify. Where shapes of the three different curves relevant for the finding? The finding here is difficult to understand, the sentences a bit complicated and several sentences basically say the same: You found and show critical values of gross to net forest area change above which ELUC of a net a net forest area gain switches from CO2 sink to source.

Introduction: Page 2, ln 8: that is for DGVMs the sub-grid transitions that sum up to net changes, here a reference to e.g. Bayer et al. 2017: doi:10.5194/esd-8-91-2017 could be nice, who focused on the problematic of sub-grid transitions.

3.4 Page 7, ln 14: "we pose the question whether such ratios can be observed in the real world" – but this is not what you are answering with your approach. As far as I understood you just calculate using your rates, whether the regions are a sink or a source.

Page 7, ln 22: "With a too high rotation rate of forests, i.e. a large gross to net area change ratio, a net forest gain could still legate a net carbon source over a long period in the future." I don't agree, as I think long rotation secondary forests should have other response curves than short rotation forest, as short rotation forest don't store as much carbon that can be lost afterwards.

4. discussion You state that the response curves used in bookkeeping models from Houghton (1999) and Hansis et al. (2015) overestimate carbon density – that implies that Porters values are true, while Houghton and Hansis are wrong. But also Houghton and Hansis are based on measurements, right? Maybe just not in the right region? It would be helpful to mention in the discussion where the measurements for Houghton and Hansis models were located.

Is the extent to which gross versus net transitions affect ELUC comparable what other studies investigating gross versus net transitions studied? You mention several studies

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about this issue which were performed with carbon models – they should also appear in the discussion, showing how your results compare with what they found.

Technical corrections

General: In all figures and the text, it might be useful to replace the term “biomass carbon density” by “vegetation carbon density”, as biomass is less well defined and includes in some disciplines also dead biomass and soil microbial biomass which would count here to the soil pool.

Page 1, ln 26: “critical value” should be plural

Page 2, Ln 10: “Gross LUC occurs in tropical regions with shifting cultivation (Hurtt et al., 2011) but also everywhere forests are cut and new plantations created at the same time” Is here a “where” after “everywhere” missing?

Table 1: The table caption uses gamma gross to net but in the table heading and the text gamma Agross to Anet is used.

Fig 1: biomass from primary forest: reference missing; Legend for a) and b) shows biomass, which can only be found in plot c) that has an own legend.

Fig 3: include dashed and solid line in legend. In figure description logarithmic asympthotic should be removed or refered to both – solid and dashed, so it becomes more clear that there is no difference in the response curve between solid and dashed, but just which systems are transformed.

Fig 4 is a bit difficult to understand. The difference between plot a) and b) is hidden in the middle of the figure description in the end of a sentence. Would be better to have it in the description directly following a) resp. b), whereas “net forest gain at t=0” which is true for both plots should either be in the end or before the separation in a) and b). Or/additionally it could be mentioned as title in each plot whether it is primary to secondary or secondary to secondary. The axis title is only in plot a) but not in b) whereas the legend can be found in both plots. Please add the axis title to b) or remove

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the legend from a) or do both and set the legend a bit aside, which would also help the reader to not confuse it with a second y-axis title at a first glance.

Please extend “Exponential carbon loss curve from (Hansis et al., 2015) and logarithmic gain curve from (Poorter et al., 2016) are used in this example” to something like “Exponential curve from Hansis et al., (2015) for carbon loss in all pools and gain in soil pool and logarithmic curve from Poorter et al., (2016) for gain in biomass pool are used in this example, which corresponds to the combinations C1 and C2 from Table 2 for a) and b) respectively.”

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