

Interactive comment on “Carbon stocks and dynamics at different successional stages in an Afromontane tropical forest” by Brigitte Nyirambangutse et al.

Brigitte Nyirambangutse et al.

brigitte.nyirambangutse@bioenv.gu.se

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Thank you for your comments on our Biogeosciences discussion paper. Below you have our response to each comment. We have also prepared a revised manuscript according to the comments and response (see Supplement), ready to be submitted

Comments from Referee 1

This manuscript fills an existing gap about the carbon pools of African montane forests of different stages from early to late succession. The authors have applied up to date methods to determine the above- and belowground biomass and productivity and soil carbon pools and found that the late successional forests investigated have higher

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carbon pools than montane forests in S-America and SE-Asia. The manuscript title clearly reflects the content, the abstract gives a nice overview and summary. The paper is well structured and presented, written in fluent and precise English. Formulas and abbreviations are correctly given and used. The methods are in general clearly described with only view details, that should be clarified: In the description of the study area and the plots I was missing information about:

RESPONSE: Thanks for kind comments. Below we answer all specific questions and comments.

1. The slope inclination and topographic position of the plots. Only the planimetric area is given, but especially the topographic position (ridge, valey bottom) would be an interesting information, as it is known to have an important influence on forest structure.

RESPONSE: We have included information on slope and topographic position in Table S1.

2. Later in the methods it would be good to clarify that all area-based results (biomass, NPP, C-pools) are related to the planimetric area and not to the inclined area.

RESPONSE: We have included a sentence at the end of section 2.2 “All forest area based information is related to the planimetric area.”

3. In section 2.3 Meteorological data the authors list . . . air humidity, solar radiation, humidity . . . Please clarify what is meant by “humidity”, maybe soil moisture?

RESPONSE: Unfortunately, humidity was repeated twice. The second “humidity” was a typing error and has been deleted.

4. A list of meteorological parameters that were measured are not presented like solar radiation, soil temperature and moisture. So, I would like to ask the authors to present these data.

RESPONSE: We added two sentences to report soil temperature and solar radiation

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in section 3.1. Soil water content was already reported in the same section. “The daily mean soil temperatures were similar to the air temperatures, but with lower mean diurnal amplitudes (1.9 °C in the soil compared to 6.1°C in air).” “The daily mean photosynthetic photon flux density at the four meteorology stations was $289 \pm 10 \mu\text{mol m}^{-2} \text{ s}^{-1}$, with slightly elevated levels during the dry period.”

5. The authors used ingrowth cores to quantify the fine root production. I was wondering if they determined the lack time between installation of the ingrowth core and the time when roots started after the disturbance to grow inside the root free soil cores. Please clarify if you determined the lack time and subtracted these periods for annual fine root productivity, or if you didn't.

RESPONSE: We guess that you mean lag-time here, however we did not include any lag-time which might have underestimated the growth. However, since we used relatively long intervals (6 month) between the harvests, we assume that any lag effect would be of smaller importance in this study compared to the commonly used, 3-4 month intervals. To clarify this we added the following information in section 2.7: “The annual sum of production was calculated in proportion to the time between the harvests (without assumption of any lag-time)”

6. The results are well and traceable presented and support the conclusions. I In Tab 2 – 5 I would like to ask the authors to present also the results of the intermediate successional plots MS. To me that information would be of more interest than the total mean and SD of all 15 plots.

RESPONSE: We believe that the overall message is clearer by mostly presenting data from the two distinctly separated ES and LS groups. However, to show that there is a successional transition between ES and LS plots we suggest extending Table S1 with calculations of the mean for all three groups statistically analysed with a one-way ANOVA to show how MS relates to ES and LS for some of the most important biomass parameters. We also added references to Table S1 regarding MS values in section 3.3,

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3.4 and 3.5.

7. In line 2 of Table 6 “America b” is missing after C&E

RESPONSE: Thanks for pointing out the missing note. It has been added, however it is C & E Amazonia.

8. And please replace Clumsee et al. by Culmsee et al.

RESPONSE: Thanks for noting the spelling error. It has been corrected.

9. On page 12 lines 14-15 it remains unclear if the given recruitment rates refer to the above mentioned species or not.

RESPONSE: Yes, also recruitment rate refer to the two species *M. kilimandscharica* and *S. guineense*. We have revised the sentence to become more clear, and deleted a typing error, as follows: “However, recruitment rate (3.0/1.4%, P = 0.17) did not differ significantly differ between these two species on when analysing plots where they co-occurred.

10. Putting their own work into the context of published studies the authors clearly indicate the origin of the data. Only a few studies are missing in their review on forest structure and above and belowground biomass. Here the authors should include additional results from SE-Asia from Hertel et al. 2010 Forest Ecology and Management and from Kessler et al 2012 PloSOne.

RESPONSE: The two suggested papers are definitely two important studies of stocks and production of biomass in tropical forest. However, the reason why we did not include these in our overview is that they are conducted at around 1000 m a.s.l., while we focused our comparisons with other tropical montane forests on studies conducted at an elevation range of 1600-3000 m, and our lowland overview included only studies conducted < 1000 m a.s.l.

11. On page 14 line 29 it should say “ *M. kilimandscharica*”

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RESPONSE: Thank you for observing this spelling error. It has been corrected.

12.The supplementary material presents important detail information and is well presented. In Table S3 please correct in the first line “species abundance”.

RESPONSE: Thank you for observing this spelling error. It has been corrected.

13.In Fig S1 please give also R2adj when non-linear correlations were applied.

RESPONSE: We have included adjusted R2 values in the legends of all figures with non-linear functions.

14.This manuscript will be an important contribution to Biogeosciences as the results fill still existing gaps about the C pools and dynamics of Afromontane forests of different successional stages. It is nice to read, well presented and comes to interesting conclusions based on a great dataset.

RESPONSE: Thanks!

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/bg-2016-353/bg-2016-353-AC1-supplement.pdf>

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-353, 2016.