Interactive comment on "Towards ground-truthing of spaceborne estimates of above-ground biomass and leaf area index in tropical rain forests" *by* P. Köhler and A. Huth

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In the following we will address all comments of L. Anderson, which helped to focus the MS. We will implement the content of this discussion in the revised version of the MS.

The comments of the referees and our replies are in detail:

- *I consider this study very relevant, given the scarcity of field data and more impor-*

tantly due to the inaccessibility of many forest sites. However, I do not agree with the author s perspective in this study by arguing that remote sensing (RS) would be the only practical solution for estimating and monitoring the above ground live biomass in tropical forests. First, due to the spatial resolution of the appropriate remote sensing data-sets for the biomass estimation of large forested areas (e.g. 20 x 20 m, emphasised by the authors) makes this task not achievable. As the temporal resolution is also dependent of the spatial resolution, the monitoring of these forests is, therefore, not realistic. Secondly, comparing the most recent above ground live biomass estimations for Amazonia (Malhi et al., 2006, Saatchi et al., 2007), one can conclude that there is no high disparities between both maps/estimations. Hence, in primary forests, I think that the biggest contributions for uncertainties is the estimation of the below ground biomass, which can not be carried out with RS data.

Taking into consideration these two points, I believe that the main contribution of the model and methodology developed by the authors are the potentialities for estimating and modelling changes in above ground live biomass over time in disturbed forests. Therefore, I would suggest for the authors, in order to make a more pertinent and actual point with their study, to re-write the introduction and discussion, exploring the importance of estimating and predicting above ground live biomass in disturbed forests for not only the better understanding the spatial patterns of AGLB and carbon fluxes, but also as a main supporting information for the REDD policies.

Malhi, Y., Wood, D., Baker, T.R., et al. (2006) The regional variation of above-ground live biomass in old-growth Amazonian forests. Global Change Biology, 12(7): 1107-1138.

Saatchi, S., Houghton, R., Avala, R., Yu, Y., Soares, J-V., 2007, Spatial Distri-bution of Live Aboveground Biomass in Amazon Basin, Global Change Biology (2007) 13, 816 837."

We rephrased the respective sentences, included the 2 references and added some details on the usage of the approach within REDD. In doing so we hopefully sharpened both the introduction and the discussion as suggested.

• *Abstract: The abstract might be changed if the authors decide to follow my suggestions. Nonetheless, they should make clear that they are estimating above ground live biomass. Also, line 15-19 is a bit confused: is one of the objectives to test the effects in different spatial scales? Include the p value for the correlations. It is not clear how the authors evaluate the 0.04 ha and make a statement about the 1 ha in the conclusions."*

One of the objectives is indeed to test the effect of the spatial scale in the data analysis. Our whole analysis was performed for the two spatial scales 0.04 and 1 ha and therefore we do not entirely understood the last sentence of the comment above. However, to clarify the MS we clearly mention in the abstract and in the introduction that testing two spatial scales is one objective. We furthermore clarified, that we talk about above-ground life biomass and revised this, whenever mentioned, including the title of the manuscript. All regressions are significant ($P < 0.01$) and we therefore restrict the mentioning of the P-value to the caption of Table 2.

• **Introduction:**

• *Page 3229, line 10: Include reference for the carbon emission estimates.*

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Done. Reference is Global Carbon Project 2008.

• *Line 18-26. Please specify that these studies are for the Amazon. Also, would be interesting to include something in relation to wood density. Is wood density more important than canopy height for a more accurate biomass estimation?*

It is not true, that these studies are restricted to the Amazon. The paper of Slik et al 2010 analysed forests on Borneo, South-East Asia, while the studies of Chave et al are pan-tropical. Our goal is to analyse how good biomass can be calculated once canopy height is given, an information on wood density would not be of help here. However, Chave et al 2005 has already shown, how good correlations between wood density and biomass is with respect to other possible regression, and we clarify explicitly the content of that study again. Furthermore, a recent study showed, that the estimation of biomass from LIDAR might be improved if additional information obtained from optical remote sensing on the vegetation structure is included (Ni-Meister et al 2010, JGR). This will be mentioned in the introduction and discussion.

• *Line 25: Please consider excluding the reference Phillips et al., 2009, and including Aragao et al., 2009.*
Aragao, L. E. O. C., Malhi, Y., Metcalfe, D. B., Silva-Espejo, J. E., Jimenez, E., Navarrete, D., Almeida, S., Costa, A. C. L., Salinas, N., Phillips, O. L., Anderson, L. O ., Baker, T. R., Goncalvez, P. H., Huaman-Ovalle, J., Mamani-Solo rzano, M., Meir, P., Monteagudo, A., Penuela, M. C., Prieto, A., Quesada, C. A., Rozas-Davila, A., Rudas, A., Silva Junior, J. A., and Va squez, R.: Above- and below-ground net primary productivity across ten Amazonian forests on contrasting soils, Biogeosciences Discuss., 6, 2441-2488, doi:10.5194/bgd-6-

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2441-2009, 2009."

We changed the reference as suggested, but used the finally version of the article of Aragao et al., published in Biogeosciences, not that one in Biogeosciences Discussions as suggested here.

• *Page 3230, line 6: Please re-write taking into consideration Saatchi et al., 2007.*

Done.

• *Line 20-21: For which landscapes/forest types?*

This is for both temperate forest in Europe and the US and for tropical rain forests in Indonesia. We clarified this accordingly.

• *Last paragraph: It is not clear if the authors attempt to test the effects of different spatial resolutions. Please specify the objectives."*

Yes, indeed, the effect of the spatial scale is one objective which is now mentioned more explicit in the final paragraph of the introduction.

• **Methods**

• *Page 3232, Lines 6-16. Please provide an average value for typical crown size.*

The idea of the modelling concept is that the average crown size of emergent trees is 20×20 m. We clarified the sentence to make this more clear.

• *Page 3232, Line 23. Please provide the time step for the leaf area.*

The model works with the principle time step of one year, thus no faster events / changes are resolved. This is now explicitly mentioned.

• *Page 3232, Line 29. Please specify why senescent trees die. Can you also provide details for the dynamics of the evergreen trees?*

In the model tree mortality is a stochastic event, e.g. the group-specific mortality rates taken from field data are taken as dying probability against which the model draws random numbers. This is now specified in the manuscript. Providing more details on the dynamics of evergreen trees is not within the scope of this paper.

• *Page 3233. Line 11. Please specify a.s.l .*

"A.s.l." means "above sea level" and is written out now.

• *Page 3234. Line 20-25. Please re-write, it is very confusing."*

Done.

- **Results**

- *Pg. 3236, Line 1-7. Do all trees that die falls?*

No, only a fraction of trees that die fall over, here 40%, a number which was achieved from the analysis of field data. This is now explicit mentioned.

- *Pg. 3236, Lines 7-14. Figure 1b is not mentioned. Please specify p value. The authors may want to consider removing the power-law equation, as you add complexity without a great improvement in the results."*

Figure 1b is replotted again as Fig 3a and then compared with field data plotted in Fig 3b. We now also refer to Fig 1b. It is furthermore (together with Fig 1c) used to guide the eye, how the scatter plot in Fig 1a can be understood and to see where the scatter density is largest.

For the relationship between biomass and diameter of individual trees power laws are often used (Zianis et al., 2004; Chave et al., 2005). Furthermore, tree diameter and tree height are related with simple equations. Thus, we see this as an argument to also rely our biomass-canopy height relation on this kind of function. We nevertheless extend the manuscript on these details.

- *Page 3237, Line 11-22. Would be possible to add a comparison of the number of emergent trees between disturbed vs. undisturbed forests?"*

Emergent trees to our understanding are trees with a crown above the closed canopy, thus with heights above 36m. Because the crown size of emergent trees

is about the same as the plot size we can assume that only one emergent tree per plot exists. With these assumptions we can calculate from our result files the ratio of plots with emergent trees. In undisturbed forest (site P1) about 75% of the plots contain a canopy height of 36m and more (thus one emergent tree), while this fraction declines to 39% in the disturbed forest simulations. The information is now included in the manuscript.

- *Discussion and Conclusions. Please see general comments section."*

- *Figures quality Ok."*

Nothing to do.

- *Tables Table 2. Add p value."*

Done.

- *References Appropriate. Please check again references in the text vs. reference list."*

References changed and updated as suggested and cross-checked.

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