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Biogeosciences Discussions

Interactive comment on "Mapping tropical forest biomass with radar and spaceborne LiDAR: overcoming problems of high biomass and persistent cloud" *by* E. T. A. Mitchard et al.

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The authors would like to thank this reviewer for the detailed comments given, and for the encouragement as to the general merits of the methods we have used. We strongly agree with the reviewer that LiDAR in combination with other RS data is a very powerful method of mapping AGB, and that in general combining multiple datasources is a useful approach. We also share the reviewer's general concerns about the applicability of the method in the absence of good local field data (though we do not think this any worse in this case than any other method), and the unfortunately absence of spaceborne LiDAR data in the near future. We also value the comments on the section on

uncertainty, though fear we do not have the necessary data to allow for a more precise determination of the errors here (i.e. our error estimates are by necessity themselves uncertain).

Our response to the specific comments raised by the reviewer are listed below, along with changes we have made to the original manuscript in order to prepare a new version for submission to Biogeosicences.

1: Reflecting the regional character of the study in the title:

We have modified the title to "Mapping tropical forest biomass with radar and spaceborne LiDAR in Lopé National Park, Gabon: overcoming problems of high biomass and persistent cloud"

2. p 8787 line 14 – please check sentence

We have changed this sentence to: "Methods for estimating AGB using the methods above are relatively well established, and have been employed as part of the monitoring schemes for many pilot REDD+ projects (CCBA, 2011)

3. p8787 line 25-27: not sure this is a good reasoning. E.g., one could create cloudfree mosaics by exploring the full depth of the Landsat archive

We agree we could have made a cloud-free mosaic, and indeed we put a lot of effort into trying to make one. However, the extensive cloud-cover, in particular a prevalence in this area of frequent small, low-level clouds which are hard to mask, made this very difficult, and the necessity of using many different images, at different seasons and sun-angles, made the resulting composite unsuitable for classification. Also post-2003 Landsat 7 has the problem with its Scan-line corrector, which made its data next to unusable for these purposes: therefore any mosaic would either have a low quality or need to be from pre-2003. As our field data were from 2009, there would have been too long a time between our field data and the remote sensing imagery.

We therefore have not modified this sentence.

4. p.8792 line 27 – please change ‘altitude’ to ‘elevation’:

We have modified this sentence to “We therefore used the radar backscatter (in HH and HV polarisations), RFDI, and elevation (from the DEM) to develop an unsupervised classiïňĄcation of LNP (Fig. 2).”

5. Methods section: it would be good to include a ïňĆowchart outlining the individual steps taken to derive the ïňĄnal biomass map

We have added such a flow-chart: it forms the new Figure 2.

6. The ïňĄrst three section of the results chapter are currently a mix of methods (which model types, etc) and results (e.g., parameter estimation). On the other hand, some analyses are not described in the methods section at all (biomass estimation, DBH estimation, etc). Would be good to include all steps taken in the methods (see above comment) and only show the results of these analyses in the results section.

By adding the flow-chart we have we hope removed some of the problems you have here, and we have modified the text slightly to move some of the ‘methods’ text out of the results. But some is necessary as we feel we cannot include parameter estimation in the Results without some explanation as to how the parameters were estimated.

7. p8793 line 19: please provide a reasoning for the type of model (equation, e.g., why second-order?)

We have added the following clause “A second-order regression was chosen because it produced a good fit to the observed data, and is backed up as an appropriate shape by larger datasets (Feldpausch et al. 2011).

8. P8794 line 15: Which different models were compared? - It would be good to have the error estimation as a part of the results section (not a separate chapter).

We also compared linear and quadratic models, but this model works best here because it accurately models the saturation part of the curve: linear obviously does

not, but quadratic models also cannot model saturation at high biomass. We do not have the space to discuss this in detail here, and we do not think it’s necessary to include a discussion as the main focus of this paper is not estimation using radar directly (as we have done elsewhere, e.g. Mitchard et al. 2009, GRL, http://www.agu.org/pubs/crossref/2009/2009GL040692.shtml).

We were advised by reviewers before the paper was accepted to Biogeosciences Discussion to include a separate Error Estimation section, and we feel this greatly adds to the clarity of the paper. As you say this is a complex analysis, and we feel separating out the error estimation makes it much easier to follow.

9. I also think that the authors could get rid of the subchapter headings and simply have a single section for 5, 5.1, and 5.2.

We would like to leave them in if that is okay: we see your point but we have tried removing them and worry it reduces clarity, and makes finding an appropriate section of this text harder to follow.

10 Some of the error estimations seem to be a bit shaky. Examples of this include p8797 lines 27-28 and p8789 line 12-13. Why 10%? Why 5%? Any justiïňĄcation for these ‘best guesses’? - Given the rather arbitrary choice of individual errors, I am not sure how useful the estimation of a conïňĄdence interval around the biomass estimation really is.

We understand precisely what you mean here. We do not have the space to further explain these ‘best guesses’, and as you insinuate given they are only estimates we’re not sure how much certainty further estimation would give. They are estimates based on our knowledge of the data and other studies, and are probably in the right ballpark. We agree that these uncertainty estimates are themselves uncertain, but we do not think that means they should not be included: we think this section is very useful for providing an idea of the uncertainties. Just because we cannot accurately quantify these values, it does not mean that we should not include them.

11 Also, why is the lower bound of the conïňĄdence interval a better estimate than the median or mean of that interval? This sounds, at least from a statistical point of view, somewhat counterintuitive.

We would happily remove this if the reviewer prefers. However, in the writings of UN-REDD and GOFC-GOLD on the REDD+ process, a principle of 'conservativeness' prevails. In this the mean estimate may not necessarily be the value used, but instead a smaller value about which there is a lot of confidence that the true value is at least as big, and highly unlikely to be smaller. The results we present here allow a sensible way of estimating the minimum likely value of carbon stocks in an area: in an area such as this uncertainties are large, so this value will be considerably smaller than the mean.

12. p. 8801 line 14-18: please revise this section to reïňĆect the current (at best uncertain) status of the DESDynI mission.

We have, with regret, removed all references to DESDynI from the revised manuscript.

Interactive comment on Biogeosciences Discuss., 8, 8781, 2011.