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Comment

Interactive comment on “Local spatial structure of forest biomass and its consequences for remote sensing of carbon stocks” by M. Réjou-Méchain et al.

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This paper deals with an important and critical issue: the scaling of plot estimates of carbon density to larger areas, in particular to remote sensing pixels. This is vital for REDD+, designing algorithms for predicting carbon stocks using data from new satellites addressing this issue such as BIOMASS (which will produce outputs at a coarse, 200 m pixel size, larger than most plots), and for developing a better understanding of forest dynamics.

Their findings in terms of the influence of topography are novel and interesting: topography has long been discussed as a source of error in comparing field data to remote

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sensing data, but its influence has not been quantified in this way before. Clearly from their findings sampling designs in hilly areas must be stratified by this topography. Also their findings in terms of the use of different sized subplots for developing regression equations are very important. It is known that using OLS regression for small plots would cause an underestimation slope, but it had been thought that RMA or Theil-Sen regression would correct for this: these results show that this is not the case. Finally this study shows that small forest plots (<0.25 ha) should not be directly compared to satellite remote sensing data for spatial sampling reasons, before even considering the known additional errors that result from small plots having a high edge-area ratio and a larger relative geolocation error.

I think the paper is excellent and should be published after minor corrections. I have a number of relatively minor comments below, with just one where I consider an additional analysis necessary. One of the key result of the paper is displayed in Figure 4, showing the expected errors that result from a calibration plot of a particular area compared to a remote sensing footprint of a particular size. This however considers only circular remote sensing footprints, which is not always appropriate. This is discussed in detail below, and should be addressed.

The other points I raise are not critical, and should be treated as suggestions.

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5716 lines 14- 15 – need to explain why monitoring of forest carbon stocks is necessary for REDD+. REDD+ projects will it appears quantify changes in carbon stocks by stratifying areas, assigning carbon density values to each strata, and multiplying the two to get total stocks (and differencing these from year to year to calculate emissions). Thus lines 14-15 to not necessarily follow from your first paragraph. Need to justify space-based mapping of carbon stocks (as opposed to just landcover classes) in terms of degradation, regrowth and enhancement activities.

5717 line 16 – the issue raised here of uneven sampling of ground plots is raised here

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but not covered or discussed in the paper – if this is to be left in the introduction maybe it could also be raised in the discussion? The bias introduced by non-random plot selection is hard to quantify, do the authors have any ideas on how to propagate this.

Page 5719 lines 1-2 – it is strange here to refer to a general paper about the proportion of biomass from lianas, as it is known that this can vary significantly from plot to plot. I think it is fine to exclude lianas, I can see how it simplifies the analysis and should not change the conclusions. But it would have been good to estimate the proportion of biomass from lianas within each plot (or maybe subplots too), and exclude those with a high (say >10 %) contribution of total biomass from lianas. This would prevent the possibility of lianas biasing the analysis.

Lines 3-5: information should be given here on how elevation data was collected for each plot. If this SRTM? Field survey measurements? The description here is too brief, it would not allow for this study to be replicated based only on this information. How is topographic heterogeneity defined? Moreover, given you don't actually use topographic heterogeneity as a variable in the paper, but elevation range, why not in the rest of the paper call this variable what it is, i.e. 'elevation range', rather than heterogeneity?

Page 5719 lines 8-10 and throughout: AGB is being used here to mean both Biomass density (Mg ha^{-1}) and total tree biomass (AGB estimates for each stem, presumably in Mg or kg). This is often the case in the literature, but given the repeated use of AGB in this paper I found this confusing. I suggest the use of something like AGBD for Aboveground Biomass Density (Mg ha^{-1}) and AGB for Aboveground biomass (Mg).

Page 5719 line 12 onwards: I like the use of CV(s) as a measure of divergence between subplot AGB values and the mean of the whole plot. However the comparisons are confused because the size of the large plots vary from 8 to 50 ha. This may not have much impact on the results, but does mean that the CV(1) values you use to test issues such as topographic heterogeneity are not all strictly comparable. This may be fine, but I would like you to test this impact by calculating CV values for 8 ha subsets

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of the larger plots, and seeing whether trends based on 8 ha subplots of all plots are significantly different from using a single CV value calculated per plot. In other words I would like you to test whether cutting all your largest plots into approximately the same size as your smallest plots, and then calculating CV values from comparable sized plots, influences the results. If not, then your current analysis is fine – it is better to have a single value per plot than the pseudo-replication of giving 6 values for a 50 ha plot, but you need to test whether the CV metric is sensitive to large plot size.

Page 5719 line 25: are you sure it is the Central Limit Theorem that implies this?

Page 5721 line 1-: You need to justify decision to choose to represent remote sensing pixels as circles. This may be fair for most optical sensors, and potentially IceSAT GLAS mentioned, though it's not that simple as in both cases more information will pass to the sensor from the centre than edge of the pixel. But for radar and high resolution (airborne) LiDAR, looks/returns are aggregated into rectangular pixels. Radar in particular has no circular features, and the beams of a LiDAR sensor, while circular, from an aircraft are very small and aggregated into rectangular pixels. This needs to be discussed and addressed here and in the Discussion: It is okay to keep the analysis as is, but it must be made clear that the analysis as presented is relevant only to optical, and potentially spaceborne LiDAR, instruments. Alternatively, and perhaps more powerfully, the same analysis could also be performed with rectangular pixels of different sizes, allowing the effect of shape as well as size to be quantified.

The authors could also explain their choice of 'footprint' areas better. They have chosen 0.5, 1, 2 and 4 ha, but these do not match onto any remote sensing instrument I am aware of, and in particular start quite large. Surely more useful would have been to look at a range of pixels including Landsat size (30 m, i.e. 0.09 ha), ICESat 2 (50 m circular footprint, i.e. 0.2 ha) all the way up to MODIS size (250 m, i.e. 6.25 ha). If square pixels were being considered then the 4 ha pixel size makes some sense, as this is the probable size of pixels in the BIOMASS product, but a circular 4 ha pixel does not at all represent the viewpoint of BIOMASS, with this table probably overstating ErrCV in this

case. In my view the analysis should be repeated with a wider, more realistic range of footprint sizes, and square as well as circular footprints.

Finally on this point, if space allows, it would be good to include Figure S2 in the main paper to illustrate this analysis. I do not think the text is terribly clear and this diagram is very helpful in explaining this analysis.

Page 5721 line 17: In contrast the sampling error propagation analysis, including the assessment of regression dilution, assumes a square 4 ha plot (BIOMASS-like) – this is good and this is an excellent and sensible analysis to perform, but the inconsistency with the above is not discussed or explained. It makes the results from the two analyses difficult to compare.

Discussion

The Discussion is excellently written and gets to the nub of the problem, explaining well all the significant findings of the study. Its implications for future research design are clearly stated. The explanation and interpretation of the wavelet analyses is well presented in my opinion.

However, it ignores right until the end the role that high resolution remote sensing can play. High resolution data, especially aircraft lidar, can act as a stepping stone between small field plots and the relatively coarse resolution remote sensing methods discussed here (min considered pixel size in comparison is 0.5 ha). When discussing ‘remote sensing’ in this paper it appears the authors are discussing ‘satellite remote sensing’, whereas much biomass mapping effort uses airborne LiDAR sensors where individual trees can be distinguished and many (though not all) of the trends shown here would not apply. This is mentioned in the final two sentences, but it would be great for this to have its own paragraph highlighting studies that have succeeded in using high resolution data to effectively increase the size and number of plots that can be used to calibrate satellite remote sensing data.

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Fig 1: Forest cover map should be referenced as GLOBCOVER2009 as well as the Bontemps reference, and the different colours of forest should be included in a key or left out, i.e. with a single colour for all forests. Additionally, though optionally, I think it would be useful to have lines showing the boundary between tropical, subtropical and temperate here, given these are used in later figures and analyses.

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