

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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Dear Dr. Mitchard,

We very much appreciated the careful assessment of our manuscript, and we were very pleased by the positive and constructive review. You made a number of useful suggestions to clarify and strengthen the manuscript, which we have addressed (changes are highlighted in red in the corrected manuscript).

Below we provide detailed answers to all points raised in the review.

Thank you for your time and consideration.

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Sincerely yours,

On behalf of the authors, Maxime Réjou-Méchain

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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 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 results 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

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in detail below, and should be addressed.

Response: We modeled the remote sensing footprint as circular for the sake of simplicity, as now stated and discussed in the revised version of the text. Please see the response to reviewer 1.

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

Response: We agree that this important point was missing and have modified this paragraph accordingly.

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

Response: This is now discussed at the end of section 4.3.

- The bias introduced by non-random plot selection is hard to quantify, do the authors have any ideas on how to propagate this.

Response: We agree that the bias introduced by non-random plot selection is hard to quantify; this is clearly an outstanding challenge, and we believe that this is beyond the scope of this paper. We now briefly mention this issue in the discussion.

- 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

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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.

Response: Lianas were not measured in all plots so that we were not able to include them systematically. However, none of the plots had a high proportion of large lianas; thus, excluding lianas from the analyses is not expected to introduce bias in our results. We slightly modified the sentence to “We [...] excluded lianas from our analyses in the few sites where these were censused. Lianas usually represent less than 5% of the total AGB (e.g. Schnitzer et al., 2012).”.

- 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?

Response: Elevation data were either generated by field measurements with surveying equipment or by high-resolution airborne LiDAR. Topographic variability was defined as the mean standard deviation of elevation within 1-ha subplots (see legend of figure S1). This is now clearly stated in the manuscript.

In the method section and in the results, we systematically used the term “topographic variability” in association with “elevation range” in order to avoid any ambiguity. We chose to keep “topographic variability” in the manuscript because the elevation range was an excellent proxy of it (Fig. S1) and because we believe that this term is more intuitive for readers.

- Page 5719 lines 8-10 and throughout: AGB is being used here to mean both Biomass

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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).

Response: We agree and followed this recommendation throughout the revised paper.

- 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 true, but I would like you to test this impact by calculating CV values for 8 ha subsets 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 true – 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.

Response: We investigated the consequence of calculating the CV for different plot areas. Because the largest possible standardized size was 4 ha (200x200 m), i.e. some plots had a side of “only” 200 m, we calculated a “standardized” CV for each site as the mean CV that would be obtained in 4-ha subplots chosen randomly (n=1000 random repetitions). The results are illustrated in the figure 1 below (see at the end of the document) and show that this “standardized” CV is highly correlated with the “unstandardized” CV for all spatial scales investigated. This is because most variation in AGBD occurs at small spatial scales. Using the standardized CV also gave a highly significant relationship with the elevation range (Spearman’s rho =0.59, p<0.001). Of course, the elevation range itself also depends on plot size, and ideally the standard-

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ized CV of AGBD within 4-ha plots would be correlated with the standardized CV of elevation within 4-ha plots – but the latter is not available for all plots. For the sake of simplicity, we retain the “unstandardized” values in the main text. We now provide the results from this “standardized” analysis in the new Fig. S4.

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

Response: The reference to the Central Limit Theorem has been removed.

- 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.

Response: As discussed above, we have improved the description of the methods explaining why we choose circular footprints. The main reason is to simulate a spatial mismatch between the footprint area and the calibration plot area, a mismatch which may occur for several reasons and take different forms. As now stated in the manuscript, a realistic approach would require a sensor-specific approach and 3D simulations, for example to explore the influence of the slant-range scale distortion in radar. We also note that when the calibration plot is fully inside the footprint, the shape of the footprint (circular or square) does not matter (whatever the shape of the footprint, circular or square, we found highly similar ErrCV values when the subplot was smaller

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than the footprint, not shown). The difference in shape mattered only when the calibration plot and the footprint have the same area, so that each encompasses area not encompassed by the other.

- 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.

Response: We chose these footprint scales to address those large enough that they are often calibrated via smaller plots, and those small enough that we can fit two of them within each of our study sites. The objective of our paper, after all, was to examine the implications of spatial variation in biomass density for calibration of large-footprint remote sensing with data for smaller plots. Thus, we restricted ourselves to simulations of fairly large footprints and small plots within them.

Many existing plots are larger than the footprints of Landsat and ICESat 2 (0.09 and 0.2 ha), and thus our concerns are by and large not relevant for these instruments. Further, as we showed, when calibration plots are small, most errors are due to the field sampling errors, almost independently from footprint size. Hence, with small resolution products such as Landsat or Icesat, sampling errors are likely to be very high if smaller plots are used or if spatial mismatches between the field and the sensor signal occur. We added a sentence in the text about that issue.

We would have liked to have simulated larger footprints such as 6.25 ha for MODIS, but

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even our large plots start to reach their limits as we increase the scale. In the current work, we simulate two nonoverlapping remote sensing footprints within each site. A 4-ha circular plot has a diameter of 226 m, while a 6.25 ha circular plot has a diameter of 282 m. In five of thirty sites the smaller plot dimension is 200, and thus these already get dropped by the 4-ha analysis, leaving us with 25 sites (and 50 simulated footprints). In 18 of 30 sites, the larger dimension is 500 m or less, leaving only 12 sites in which two overlapping 6.25 ha footprints could be simulated, and 13 in which one could be simulated, or potentially 37 simulated footprints total. Thus if we added larger footprints, the sites included would become ever more constrained. We would then either have different sets of sites for different simulated scales, making comparisons across scales difficult, or we would have a much reduced set of sites across all scales, which would fail to draw on what we see as the strengths of the CTFS-SIGEO network for addressing this question.

These constraints, and an interest in seeing how patterns change with the scale of the footprint, led us to restrict our footprints to the range 0.5-4 ha. We chose doubling sizes within this range (0.5, 1, 2, and 4 ha) because of our interest in seeing how patterns change with scale, and the power function nature of most such scaling relationships. We note that even though our results do not specifically encompass other scales, they provide a basis for estimating patterns at other scales through interpolation and extrapolation.

- 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.

Response: We have improved the description of this approach and moved figure S2 to the main manuscript.

- 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

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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.

Response: As stated above, the effect of differing shape between calibration plot area and footprint area only occurs when these areas are of similar size, a case that was not investigated in the dilution bias analysis. Hence the two analyses are directly comparable as having a circular footprint or a square one would have produced extremely similar field sampling errors.

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.

Response: We agree that small-footprint LiDAR may offer a unique opportunity to act as a stepping stone between field plots and large remote sensing footprints. In the revised manuscript the discussion of this approach has been enlarged.

Fig 1: Forest cover map should be referenced as GLOBCOVER2009 as well as the

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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.

Response: The references have been included and the same colour has been applied to all forest types.

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.

Response: Thermal zones from the IIASA classification have been added to the figure.

Interactive comment on Biogeosciences Discuss., 11, 5711, 2014.

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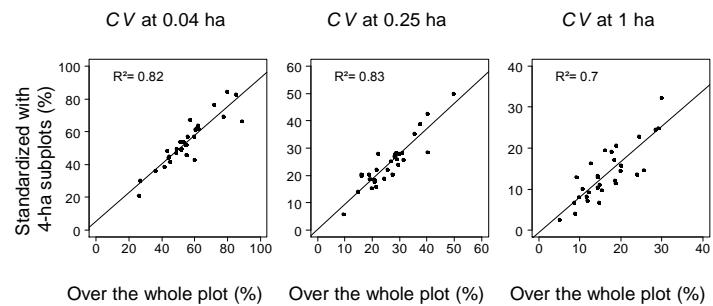


Figure 1: Relationship between the coefficient of variation in Above Ground Biomass calculated over the whole plot (x axis) and the mean CV calculated in 4-ha subplots (mean over 1000 randomly chosen subplots), relative to the 1:1 line. The R^2 of the linear model is reported within each panel.

Fig. 1.

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