Biogeosciences Discuss., 11, C4803–C4812, 2014
www.biogeosciences-discuss.net/11/C4803/2014/

Biogeosciences
Discussions
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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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Received and published: 29 August 2014

Dear reviewer,

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.

Sincerely yours,

On behalf of the authors, Maxime Réjou-Méchain

##

This paper poses and attempts to answer several important questions that are significant in the context of current efforts to infer large scale biomass maps from remote sensing and to make more general inferences on landscape scale biomass from a set of sample plots. In fact, the paper is not really about remote sensing per se, but about how accurately one can extrapolate measurements at one scale to a larger scale. In general, it illustrates that the sampling error when small plots are used to represent the average biomass of a larger area can lead to signiïňĄcant errors in the regression relation between the two. This is of special importance when training remote sensing data with plots that are signiïňĄcantly smaller than the resolution of the instrument. Though these conclusions seem fairly sound, the methodology could be improved, and there is some misleading text.

The following are the main scientiïňĄc issues:

1. The wavelet approach is unhelpful for the purposes of this study. Given the autocorrelation structure of the data, it is relatively straightforward to calculate the variance associated with multiple samples. The wavelet analysis does not help for this and it is not at all clear why the authors have used this tangential approach rather than a less complicated and more informative autocorrelation analysis.

Response: In the new version of the manuscript, we have added empirical variograms for 20 x 20, 50 x 50 and 100 x 100 m subplots. These additional analyses consistently revealed a weak spatial autocorrelation at scales < 100 m. Because the wavelet analyses provide useful additional scale-wise information that will be of interest to some readers (including the other referee), we retained these in the main manuscript. We agree that the wavelet approach may be difficult for many readers to understand and to interpret, so we endeavored to better explain the usefulness of this approach and the meaning of the results.

2. On a related note: the statement about autocorrelation giving rise to a dependence of form sËĘ(-gamma) is wrong, as is clear from an analysis based on autocorrelation.

Response: We did not mean to imply that spatial autocorrelation necessarily results in such relationships, and we agree that our wording here was misleading. We have modified the wording to clarify that a relationship of the form sˆ-0.5 is expected in the absence of autocorrelation, and that positive (negative) spatial autocorrelation will lead to a less (more) rapid decline in the CV with increasing sample size over relevant spatial scales.

3. In their discussion of dilution bias, the authors mix up two effects. The motivation in the text concerns errors in the ground measurements; this is not the same as accurate measurements of a variable quantity. The implications of this distinction need to be clariïňĄed in their analysis.

Response: The term “sampling error” is commonly used in the literature to refer to errors in estimating a true value for a population when measurements (however accurate) are done for only a sample. This term applies perfectly to our situation, where we are concerned with errors in estimating the true value for a larger area based on samples of a smaller area. We have revised the text to clarify this. That said, we agree that we used “sampling error” too broadly in the previous version, and have modified our text accordingly.

4. Why wasn’t Deming regression used? This takes account of errors in both dependent and independent variables?

Response: The Deming regression is a special case of Reduced major axis (RMA) regression, which we used in our study. Both these approaches take into account both the error in x and the error in y. We chose the RMA approach because, unlike

the Deming approach, RMA does not require prior knowledge of the ratio of the error variances in x and in y. This ratio is difficult to assess in practice, as it would require detailed knowledge not only of the true biomass density over the footprint of the remote sensing instrument and the sampling error of the ground plots but also of the errors in the remote sensing measurements. This is now mentioned in the discussion.

5. The authors have not properly understood the implications of negative autocorrelation in sampling to estimate a quantity. In particular, the second sentence of para from p.5727-p.5728 is not true. In fact, if there is negative correlation then averaging reduces the variance, so gives a better estimate; if there is no correlation it makes no difference what the spacing of the plots is.

Response: We agree that negative spatial autocorrelation in AGB would theoretically lead to a better estimate from a single large plot rather than multiple distant small ones. Given that our analyses of spatial structure have been modified with the addition of the variograms, the associated discussion has also been strongly modified. The new text is consistent with the reviewer’s comment.

6. The authors allude to it only once, but an issue that is at least as serious as the topic of this paper, certainly in the tropics, is how representative the available set of plots is. This should be discussed somewhere, as it has effects very relevant to but well beyond the remote sensing problem and is important for REDD+. Response: We agree. This is now mentioned in the discussion. A weakness of the paper is its slipshod use of language, which may be because the ïňĄrst author is not a native English speaker (but many of the co-authors are!), but some of which is carelessness. These language issues are scientiïňĄcally signiïňĄcant, as they change the meaning of many pieces of text. Examples of such (and related) issues include: a. Heterogeneity is not the same as variability, and in most cases the authors mean the latter. This is fundamentally important for discussing statistical properties which rely on an underlying homogeneous population.

Response: We replaced heterogeneity by variability throughout the revised manuscript.

b. In related vein, what is meant by topographic heterogeneity, given the meaning of heterogeneity?

Response: We replaced “topographic heterogeneity” by “topographic variability”.

c. The authors consistently talk about biomass when they really mean average biomass per unit area. This distinction is crucial as without it much of the paper is wrong. The initial text in Section 2.2 is therefore misleading.

Response: As suggested by referee 2, we now use AGBD for Aboveground Biomass Density (Mg. ha-1) and AGB for Aboveground biomass (Mg). We thus modified section 2.2 accordingly.

d. They misuse “uncertainty”; in several cases they mean “error”

Response: “uncertainty” has been replaced by “error” in most places of the revised manuscript.

e. On p. 5719 there is an appeal to the Central Limit Theorem, but this is spurious: the result quoted is just a standard result on averages of independent samples. On the same page, what does $\sim$ mean?

Response: We remove the reference to the Central Limit Theorem and rephrased the sentence including the $\sim$ symbol.

f. The labelling of some of the Figs is misleading, e.g. Fig. 2a does not show sampling error; Fig.3a does not show spatial correlation, nor does 3b; it is wavelet variance.

Response: These labels have been changed.

Following are some more detailed comments on the text: - On p.5717, l.5, it states that small ground samples will have large sampling errors if there is substantial local “heterogeneity”. That is a tautology.

Response: We agree that this sentence is rather a truism but given the questions investigated in our study, we believe that this statement must be made clear and unequivocal, even if it is a tautology.

- The end of the 1st para talks about the need to correct various errors, then fails to comment further on this.

Response: At the end of this paragraph, we state that there is a “need to quantify” the errors due to the spatial mismatches between sensors and field measurements. We address this issue by simulating circular footprints and square calibration plots (Fig. 6) and by investigating how the error associated with such spatial mismatch scales with both calibration plot and footprint areas (Fig. S10). Because subsequent comments highlighted a lack of clarity in these analyses, we improved the description of the methods.

- How meaningful are measurements at 5 m scale (p. 5719), given their dominance by edge effects?

Response: We agree that measurement at 5-m scale are not relevant for remote sensing measurements. We included quantification of spatial variability at this scale to increase the range of scales over which we could investigate the decay of spatial variability with sample area. When more realistic simulations were done (e.g. for the dilution bias analysis), the smallest plot size was set to 0.04 ha (20x20 m), a sample area regularly used in remote sensing studies, even if large edge effects also occur at that scale.

- The use of the word “grain” instead of “scale” is unnecessary and confusing.

Response: We replaced “grain” by “scale” throughout the revised manuscript.

- On p.5720 there is a reference to an area sËĘ2, but s is an area.

Response: This was indeed an error. This is now corrected.

- On p.5721 what does the phrase beginning “was perfectly perceptive : : :.” mean? That the remote sensing measurement is assumed to be correct??

Response: Yes, this meant that the remote sensing measurement was assumed to infer the exact above ground value that would be measured in the field. We rephrased this sentence.

- On p. 5721 it implies that remote sensing ïňĄelds of view are circles (or ellipsoids earlier); this may approximately be true for optical data but not for radar, where they are typically rectangular.

Response: We simulated remote sensing footprints as circular to illustrate the general issue of mismatch between remote sensing field of view and ground measurements. We now make clear that this is merely a simple example. More realistic approaches would require sensor-specific 3D simulations. Radar products are indeed post-processed to represent rectangular areas. However, the original footprint do not precisely match the rectangular area as measured on the ground because radar is measuring the distance to features in slant-range rather than the true horizontal distance along the ground (i.e. Slant-range scale distortion occurs). We now address these issues both in the introduction and in the discussion.

- It is the root mean square error, not the mean error.

Response: Please see next response.

- In (2) is it a condition that the ïňĄeld plot lies entirely within the circle? And why is the term ErrCV used;? This is misleading as it is not a CV and its connection to CV is not explained.

Response: In this simulation, field plots were centered in circular remote-sensing footprints; thus, they were entirely within the circle when field plots were smaller than footprints or they sampled slightly different areas when field plots and circles were of similar size (e.g. the corners of the squares were not sampled by the circular footprint).

As suggested by reviewer 2, we moved figure S2 to the main text in order to make this clear.

The term ErrCV is used because it is the ratio of the RMSE to the mean AGB, which is thus analogous to a coefficient of variation. The formula was split into three equations to highlight the connection of ErrCV to the coefficient of variation.

- There are repeated statements in para 2 on p. 5722. What is meant by a “realistic reliability study”? Why and how is the ICC used? ICC is relevant to measurements made on units that are organized into groups. What are the groups here? The whole of this para following (4) is unclear.

Response: We entirely rephrased this paragraph and provided more details on the ICC calculation.

- On p.5724, in para. 1, it seems strange not to mention at this point that the Asian sites show more elevation change, hence more AGB variation. This is not pointed out until several pages later.

Response: We agree, this is now mentioned in the revised manuscript.

- It is unclear what the sentence about lower gamma values is meant to be saying.

Response: Slopes greater than -1/2 indicate positive autocorrelation in AGB at the relevant scales, as illustrated by simulations (see Fig. 1 at the end of this document).

- What does “expected” mean in Fig. 4? Is it being used in some statistical sense?

Response: This corresponds to the slope that would have been obtained without bias. We have modified the text for clarity.

- p.5725. I could not see how the ïňĄgure quoted tells us about shape effects, and the text does not explain this.

Response: We have revised the legend of the figure S10 and associated manuscript for clarity.

- In para 2 what does “such models” refer to? This sentence is unhelpful overall. It should really say that “if the ïňĄeld measurements have large errors, etc. : : :.”.

Response: “Such models” referred to OLS-based models. We modified this text accordingly.

- As noted above, the authors are mixing up errors in the ground measurements with accurate measurements of a variable quantity.

Response: see above.

- 1st para. in Section 4: “spatial” should be omitted. Where does 26% come from and what does it refer to?

Response: The word “spatial” has been removed. 26% is the average CV at the 0.25-ha scale. For clarity, we added a reference to table S2.

- p.5727. the ïňĄrst sentence confuses detection of change with estimation of biomass change.

Response: This sentence has been removed.

- p.5730, Conclusions: there have been numerous studies of the errors in ïňĄeld sampling and their effects on carbon estimates. How do the authors suggest topographic variation be accounted for?

Response: We now provide more details on how topographic variation might be explicitly considered in sampling designs in the discussion.

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

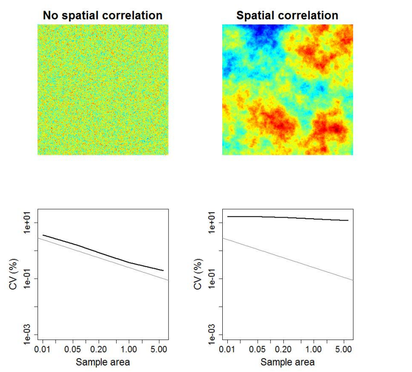

Figure 1: Simulation of the relationship between the coefficient of variation (CV) and the sample area under two different spatial autocorrelation schemes. Random fields with no spatial correlation (upper left) and with a positive spatial autocorrelation (upper right) were generated in a 500x500 grid with an exponential variogram model (sill of 0.025 and ranges of 1 and 100 respectively). As can be seen, with no spatial correlation (left panels), the logarithm of CV decreases linearly with the logarithm of the sample area with a slope of -0.5 (the -0.5 slope is illustrated in light grey). When positive spatial autocorrelation occurs, the slope is much shallower (right panels), with a slope of -0.05 in this particular simulation case.

Fig. 1.