

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2022-24

Anonymous Referee #1

Referee comment on "A question of scale: modelling biomass, gain and mortality distributions of a tropical forest" by Nikolai Knapp et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-24-RC1>, 2022

This manuscript considers the issue of property scaling with respect to spatial scale. In particular, the authors examine how the aboveground biomass distributions in forests change depending on the scale they are measured at. Such information may have important implications for global models that incorporate site-level data and run with pixels covering hundreds of square kilometers.

My thoughts on the manuscript can be summarized in three categories, and I have the feeling they are strongly related to the intended audience. A journal like Biogeosciences has a bit wider audience than some other journals, which means that folks will be approaching it from different backgrounds. Indeed, this article is meant to address folks from several different backgrounds. As someone with a modeling background, I may have missed what is evident in other fields. To wit:

- 1) I did not find the problem well-demonstrated

- 2) I felt that the discussion was not well-developed enough to convey the significance of the results and convince a general reader

- 3) I was not convinced that the method was successful compared to the case of not applying a scaling factor

For the last point (3), this seems easy to address by showing a figure like Figure 7 but replacing the green histograms and the blue line with the results of the unscaled

distribution. Would this just be the existing green histograms at 50m? If so, I would really appreciate somehow making this more clear (ideally in the figure, but also adding text would be good). I feel like Figure 7 is the figure showing the method was successful, but I do not see that immediately.

Figure 7 amazed me. I was shocked to see how the distributions shifted. However, I don't think I should be on page 15 of an article before I'm intrigued by it. I feel like the right hand side shows why this issue is important, and relates to my point (1) above. The introduction of the problem seems to occur in lines 75--7 with a single citation (Wong, 2008). In my mind, this should be an entire paragraph to emphasize the point: "Models which fit biomass distributions at 10 m² spatial resolution and reproduce them perfectly at 100 km² are incorrect." However, this represents a Catch-22. Phrasing the problem this way makes it much more appropriate for Biogeosciences, but would also require more evidence in the case of land surface models. However, the authors could (and I believe, do) demonstrate this problem with two simple models. Therefore, the information seems to be already present and just needs some restructuring to be more evident and grab the eye of a non-specialist (which is the case with the vast majority of Biogeosciences readership). More citations to the last sentence of the paragraph on line 80 ("But it is often unclear how scale affects observed and simulated distributions"), in particular with regards to forest plot and larger area modeling related to the carbon cycle, would be very welcome for point (1).

For point (2), it was not clear to me why the standard deviations are different. Figures 5-7 show that they are, but I don't understand why this happens. Section 4.2 mentions that different fitting approaches had different levels of success, and explains what these fits were, but it does not explore why they had different levels of success. Is there something about the underlying data or problem which means this could have been foreseen from the beginning?

Minor comments:

Line 80 and 81: Perhaps "extends" should be "extents"

Line 170: It seems that mortality modeling presents an issue with respect to the scale. The simulation model chosen resets the area of a whole grid cell to zero. For a 10m x 10m pixel, this could be a single very large tree. For a 100m by 100m pixel, this seems like a larger event. Biomass gain, on the other hand, seems to be similar for every size of pixel (if a 100 m² plot grows by 100 g C m⁻² yr⁻¹, then either one tree grows like that on a small pixel or it's spread among many trees on a larger pixel). Does this difference in behavior have something to do with why the simulation results change depending on pixel size?

Table 2: The number of significant figures used seems almost excessive. Is there truly a rationale for mean OVL of 0.883 and 0.887? I guess if the error bars on the distribution

are taken into account, the mean OVL will fluctuate by much more than that. Although the bins are big enough that the measurement errors are likely small. I would be happy if the authors could confirm this for me (a non-experimentalist).

Figure 7: Please add, "On the left are the G and M distributions used as input" or something similar to clarify what the left side of the plot is for the reader.

Line 326: The line beginning with "Theory states that the SD" implies to me that there is rationale behind this. I would appreciate this rationale being expressed more in the introduction to introduce the reader to the fact that this is a well-known problem with both observational and theoretical background. Perhaps it is mentioned in the Wong, 2008 reference, but adding a couple sentences would be welcome. The same for the fact that the mean is stable across all scales (line 338), which indicates it really is just an issue with the standard deviation.