Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-280-AC3, 2019

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Forest aboveground biomass stock and resilience in a tropical landscape of Thailand" *by* Nidhi Jha et al.

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Received and published: 24 October 2019

Dear Dr. Fischer,

Thank you very much for the attentive assessment of our manuscript. We really appreciated your positive and constructive comments. Please find below our point-by-point responses to your comments. The changes marked in the revised manuscript are given within quotation marks after response.

On behalf of the authors,

Nidhi Jha

Rico Fischer (RF): In this study, field data is combined with lidar data to estimate the

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Discussion paper

biomass of secondary forest for a landscape in Thailand. In addition, Landsat scenes were used to distinguish forest and non-forest for the years 1972 to 2017. This made it possible to investigate the biomass dynamic of secondary forest in more detail. The study is very clearly structured and well written. The methodsTro are applied straightforward. This study is very interesting and very important, even if it was only studied for a small region in Thailand.

Response: Thank you

RF C1: However, it is not clear to me why the authors did not used existing products and created their own products instead. A new biomass model is calibrated for the region in Thailand (from ALS with TCH as metric) – although there are already many studies on biomass estimation from ALS available with generalized models. Why calibrating a very specific equation for a small region? The same for the forest/non-forest maps. There are already products available (e.g. from Hansen or Sexton). Why generate your own product? There are certainly good reasons for this, but they should be discussed. In my view, the authors lose the opportunity to generalise this important study and apply them to larger regions.

Response C1: RF is right that a few generalized LiDAR models were proposed in the literature (reviewed in Réjou-Méchain et al. 2019 Surveys in Goephysics). The most well-known generalized LiDAR model for tropical forests is from Asner et al. (2012, Oecologia), then updated in Asner & Mascaro (2014, Remote Sensing of Environment). If those generalized models may be useful with a limited availability of field data, they convey large systematic errors when transposed to new areas. For instance, they were reported to underestimate AGB by 7% (Jucker et al. 2017 arXiv preprint) and 16% (Réjou-Méchain et al. 2015 Remote Sensing of Environment) in two independent sites compared with locally adjusted models. When extensive field data is available locally, as in our case, there is no doubt that a locally-adjusted model is to be preferred, as recommended in Asner et al. (2012) and Asner & Mascaro (2014)'s papers.

A similar reasoning may be applied for the forest/non-forest maps products. While global products such as those proposed by Hansen and Sexton may be useful for some specific, large scale applications, they cannot outperform a locally-calibrated model that was trained with airborne LiDAR data. Even over large spatial scales, Landsat-based Global Forest Watch maps (Hansen) convey large systematic errors, e.g. a 24% underestimation of gross deforestation at the pantropical scale, with important continental variations (up to 92% in Humid tropical Africa, Tyukavina et al. 2015, Environmental Research Letters). Moreover, most global forest cover maps cover rather limited time periods, e.g. 2000- present for Hansen and 1990-present for Sexton, while we here consider a period of 42 years starting from 1975.

Further comments:

RF C2: Title: The title is too general and does not address the specificity of the study – the forest carbon recovery in secondary forests for the last 42 years.

Response C2: We still think that the title well reflects our study as we do also report forest carbon stock estimates. We let the editor decide if we have to change the title.

RF C3: L 160: What are the lidar metrics? “see below” make no sense to me. Please refer to Table S1.

Response C3: As suggested we have replaced “see below” by “see Table S1” in the revised manuscript.

RF C4: Equation 4: What is the spatial scale of the biomass estimation (AGB_L)? Is it 0.5ha? Please add the scale and also the r2 beside RMSE.

Response C4: As suggested we have now added the spatial scale (0.5-ha) and the R2 value (0.85) in section 3.1 of the revised manuscript as following:

“Among all the LiDAR metrics, the mean of top-of-canopy height (TCH, defined as the maximum height of 1-m resolution pixels) was the best predictor of field AGB estimates with a relative RMSE of 14% (RMSE = 45 Mg ha-1; R2= 0.85) at 0.5-ha scale”

RF C5: Fig.2: The abbreviations (SIS, SES, OGS) only become clear if you read the whole text. It would be helpful to briefly explain the abbreviations here as well.

Response C5: We agree. The full form of SIS, SES and OGS abbreviations are now provided in the caption of Figure 2.

RF C6: Fig.3: Assuming Eq. 4 gives the biomass at the scale of 0.5ha, how can you generate with this a biomass map with the spatial scale of 60m?

Response C6: We simply predicted AGB from a TCH estimated at 60-m resolution instead of 70-m resolution. The reason is that we had to calibrate the model with 0.5-ha field data and to produce a map matching the Landsat resolution at 60-m resolution. We agree that this is not an ideal situation but, given that resolution remains very close and given that TCH is a mean, we do not believe that this impacted our results.

RF C7: Fig.4: Why is there no transition from forest to non-forest? Could forest loss play a role in the results of the study?

Response C7: There were only 70 pixels (0.5%) which shows a single transition from forest to non-forest (attached as Fig.1). These pixels represent areas close to human-impacted areas (e.g., roads and national park tourism areas). Since our study area is a protected zone, forest loss is very limited and hence does not play an important role in the overall dynamics.

RF C8: Fig.5: This figure shows that the secondary forest allocates more and more biomass as it becomes older. But if you look at Fig. 3, you can see the highest biomass values between 300 and 400 Mg/ha. Shouldn't Fig.5 therefore show saturation in the AGB recovery at some point? Possibly a power model (red line) is not suitable as a model due to the unlimited growth, but rather a model with a capacity limit

Response C8: As discussed in the manuscript, we agree that saturation should rapidly occur, typically after 50 years. This is the reason why we assume, as previous authors did, that the overall functional form should rather be a sigmoid form. However, our time

period stopped at 42 years so that a power model was more adapted to our data. We, however, agree that this model cannot be used outside the calibration model domain, i.e. for forests older than 42 years.

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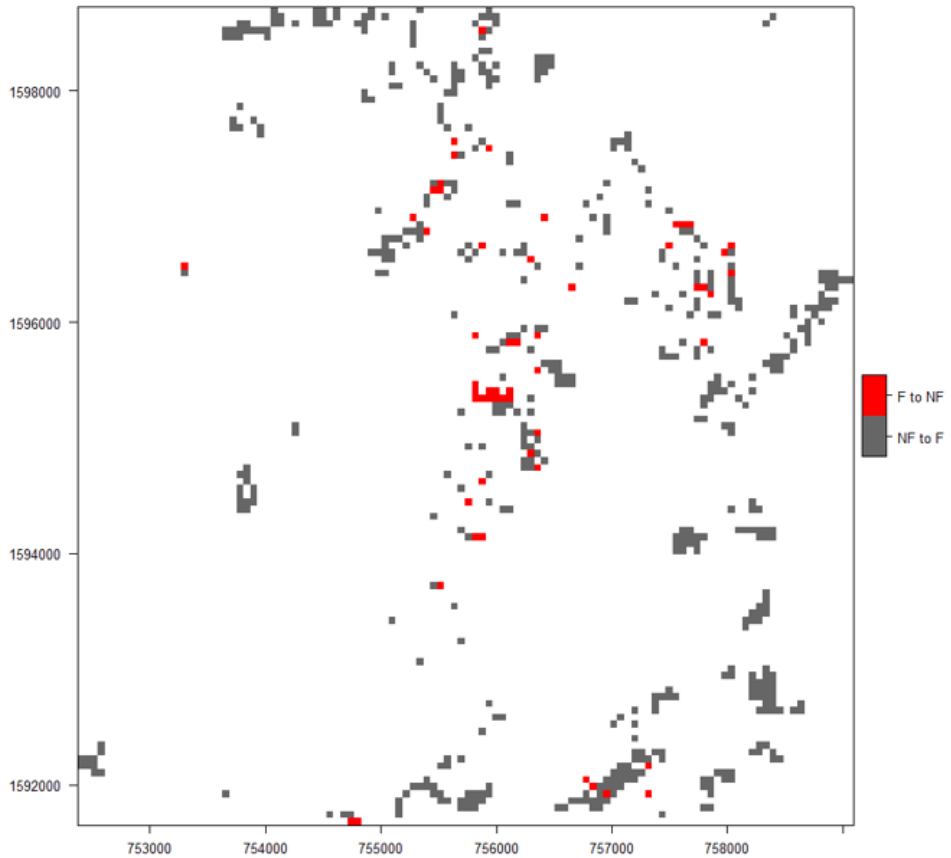

Fig. 1. Pixels in red shows the pixels that underwent Forest to Non-forest shifts (F to NF) with the other selected pixels that has underwent Non-Forest to Forest (NF to F)