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

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Dear Reviewer,

We are thankful for your careful assessment and dedicated efforts towards the improvement of our manuscript. We were very pleased to account for your positive and constructive comments. Please, find below our point-by-point response to your comments. The changes marked in the revised manuscript are given within quotation marks after response.

On behalf of the authors,

Nidhi Jha

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Discussion paper

Reviewer #2: This study combines field, airborne (Lidar) and satellite (Landsat) data to estimate biomass stocks and accumulation in a regenerating tropical forest in Thailand. The authors use multi-temporal Landsat images to identify and target pixels that went from “non forest” to “forest” since 1972. They estimate the rate of biomass gain of these pixels by regressing the recovering time and biomass estimations from a locally calibrated AGB model. Their approach is a clever and effective way to assess biomass dynamics in the absence of multi-temporal biomass maps, and will probably encourage similar future studies in other areas. Recovery from forest disturbance is an important but still poorly understood topic, and this study will definitely contribute to advancing this field. The paper is very well-written, clear and well organized. The methodology is on point and the authors use high quality validation methods, making the whole study very robust. All the methods used in this study have already been used in various studies, but the way the authors combine them is unique. The paper could be improved by making some minor changes presented below:

Response: Thank you.

General comments:

Reviewer #2, C1: My main concern is that only 3 plots with biomass <100Mg/Ha were used to make the AGB lidar model. This is an important point, since the study is focusing on low biomass/recovering areas. This should at the very least be addressed in the Discussion.

Response C1: Please see our response to Reviewer #1 comment C1. As argued in our response, we agree that we have a rather limited number of low AGB value field plots, but we do believe that it did not much impact the LiDAR model. As said in the response to reviewer 1, we added the following sentence in section 4.1:

“Due to a limited number of field plots in low-biomass areas we were, however, unable to test whether predicting errors vary with AGB or not.”

Reviewer #2, C2: Although I understand why the authors chose to focus on pixels that went from “non-forest” to “forest", I think it would be nice to also talk about the pixels that remained “non-forest”. There is no information on these in the paper, as the authors are not making any distinction between the “non-changing” pixels (pixels that remained non-forest vs. pixels that remained forest). I think it would be very informative to see how much of the degraded pixels did not recover since 1972 (excluding roads). It would give a more complete picture of the state of the forest. Ignoring these pixels implies that all disturbed areas have recovered. It would be nice to mention this somewhere in the manuscript, and also perhaps making this distinction in Figure 4.

Response C2: Thank you for this suggestion to which we agree. We thus highlighted areas that remained non-forested since 1972 in Figure 4 of the revised manuscript. About 5% of the study area stayed non-forested since 1972. Most of these areas correspond to areas that were continuously cleared by the Park for administrative or tourism purpose, such as building and camping areas, or wildlife watchpoints. We have added the distinction of the areas as follows which remained forested from all the non-changing pixels in the result section 3.2 in the revised manuscript.

“Figure 4a illustrates the resulting spatialized time series of non-forest-to-forest shifts over the study area and showed that most (83%) of the landscape did not experience such shift at 60-m resolution, out of which 5% of the area remained permanently non-forested over the 42-year study period. Most of these non-forested areas were continuously cleared and mostly correspond to National Park buildings, including tourist shops and guest houses or camping location”

Revised Figure 4 is attached herewith as Fig.1.

Specific comments:

Reviewer #2, C3: L.89: How long before? Is there any historical information indicating when it started?

Response C3: Unfortunately, we do have very limited information on the history of this area. The rough estimate of start of swidden agriculture is about the end of 19th century or early twentieth century (Chanthorn et al., 2016, Theoretical Ecology). For instance, Cumberlege & Cumberlege (1964, VMS Cumberlege Nat. Hist. Bull. Siam Soc 20), who studied orchids in Khao Yai National Park, mentioned that some secondary grassland patches in 1964 were the result of 80 years of swidden agriculture by villagers (hence starting around 1880). All villagers were then expelled by 1962 when the National Park was established (Chanthorn et al., 2017, Forest Ecology and Management). We briefly added estimates in the revised manuscript in section 2.1 as follows:

"Before establishment of the park, some areas were used for low-intensity agriculture activities (Brockelman et al., 2011, 2017) that started at the end of 19th century or early twentieth century (Chanthorn et al., 2016)"

Reviewer #2, C4: L.111: "SIS; n=3". This low number should be addressed in the discussion.

Response C4: Please, see our response to Reviewer 1 comment C1.

Reviewer #2, C5 L.114: If SES forest is 35-40 years old and OGS forest is more than 200 years old, what is in between?

Response C5: Please see our response to Reviewer #1 Comment 5. Second-growth forests mostly have regenerated since the Park was established about 40-50 years ago. As a consequence, old (50-200 years) second-growth forests are very rare in the landscape so that the understory re-initiation stage is absent from our study. To better explain this, we added the following sentence in the revised manuscript.

"The classification is based on the framework of Oliver and Larson (1996) who studied successional gradients in temperate forests. Although the original framework considered four successional stages, we did not find any area corresponding to the understory

re-initiation stage in the study landscape. Most second-growth forests have regenerated since the Park was established about 40-50 years ago so that older second-growth forests, where understory re-initiation occurs, is very rare in this area. In our study, the SES stage is represented by forest of upto 35-40 years, while other SES area in the landscape may typically range upto 55 years (since 1962), as suggested by some hand drawn historic maps (Smitinand, 1968; Cumberlege & Cumberlege, 1964). On the other hand, OGS forest stands mostly correspond to forests with no obvious sign of human disturbance during the last 100 years (Brockelman, 2011)."

Reviewer #2, C6: L.160: "(see below)". Replace by "see Table S1"?

Response C6: We agree, "(see below)" is replaced by "see Table S1" in the revised manuscript.

Reviewer #2, C7: L.199-200: address "pixels that remained non-forest" (see General comment number 1)

Response C7: Please see our response to your C2 comment.

Reviewer #2, C8: L.206-208: This part of the methodology should be explained in more details, or differently. I wasn't sure what you meant until I saw Figure 5.

Response C8: We agree that the sentence was quite unclear and reformulated it as following in the revised manuscript:

"We thus assigned to each pixel the year of the last non-forest to forest shift, if any, and considered this year as the forest recovery starting time. The AGB predicted by the LiDAR AGB map in 2017 was then used to estimate how much AGB was stored between the forest recovery starting time and 2017 through a non-linear power model."

Reviewer #2, C9: L.225: separate the pixels that stayed non forest and those that stayed forest.

Response C9: Please see our response to your C2 comment.

Reviewer #2, C10: L.228: Do we know why? Is this addressed somewhere?

Response C10: All cultivated areas were abandoned for natural reforestation after the park establishment in 1962 and a US Army camp was maintained open until the end of the Vietnamese war in 1975 (Chanthorn et al., 2016). By 1990's most of the study areas were thus reforested except few patches such as a golf course that reforested after 2001 (Chanthorn et al., 2016). Because this historical dynamic is not accounted for in our analyses, we removed this sentence from the revised manuscript.

Reviewer #2, C11: L.233: Why only eight? Mention Figure S4?

Response C11: We only considered the eight available secondary plots for which forest recovery start felt during the study period. The remaining field plots belong to the old-growth forest type and were forested during the whole study period (see also Figure S4 of the original manuscript, now Figure S5 in revised manuscript). To be more explicit we slightly reformulate the sentence as followed: "Using field AGB estimates at two different census dates from eight secondary forest plots that started regenerating during the study period (see Figure S5) ,we showed that the observed rate of AGB accumulation was similar to the one predicted by our model and also tended to increase with forest age (in blue dots in Fig. 5)"

Reviewer #2, C12: L.270-271: If the rate of accumulation is increasing, shouldn't the rate over 40 years be higher than the one over 20 years?

Response C12: Thank you for identifying this counter-intuitive result. To produce those estimates, we used the formula of the model presented in Fig. 5 dividing the AGB predicted after 20 and 40 years by 20 and 40 respectively. If this approach would have been correct with no intercept in the model, you are right that it led to a counter-intuitive result with the existence of an intercept in the model. Because the model should have an intercept to accurately capture the AGB dynamics, we now only report the rate of C accumulation during the first 20 years of succession to avoid ambiguity. We revised the line as following in section 4.2: "AGB accumulation in our study corresponds to a

net carbon uptake of 3.4 Mg C ha−1yr−1 for the first 20 years."

Reviewer #2, C13: Figure 1: add "(TCH)" in caption Figure 4: add grey classes to legend. Suggestion: Make distinction between "pixels that remained forest" and "pixels that remained nonforest"

Response C13: Done, thank you.

Supporting information:

Reviewer #2, C14: Table S1: Highlight the results of TCH and mention in caption that it is the best metric.

Response C14: Done.

Reviewer #2, C15: Table S2: Keep same name conventions as in TableS1. Are these the best 4 models?

Response C15: We changed the names in Table S2 with the same name as given in Table S1. We tested TCH with additional metrics but adding a second predictor did not reduce the relative LOOCV-RMSE by more than 1% (mentioned in L171 in the original version of manuscript), so only TCH was selected as final predictor to avoid overfitting issues.

Reviewer #2, C16: Figure S1: It would be nice to add the sub classes I mentioned about Figure 4, if possible.

Response C16: Subclasses from Fig. 4 derived from the time series illustrated in Fig. S1 (now Fig. S2 in revised manuscript). At a given year, the only information that we can report for a pixel is its forest or non-forest status so that we cannot report the sub classes in the new Fig. S2.

Reviewer #2, C17: Figure S2: Compare the histogram presented in Figure 3 to this one. Also, it would be nice to have some results from the random forest analysis somewhere in the Supporting Information and add that reference in the main text.

Response C17: As recommended, we modified said Figure S2 (now Figure S3 in the revised manuscript; attached as Fig.2.) by superimposing the density distribution from Figure 3. We also added the results of Random Forest showing the average variable importance in each Landsat sensor in Figure S1 (attached as Fig.3.) in the revised manuscript.

Minor comments:

Reviewer #2, C18: L.36: Replace “The previous study” by “A recent study” Response C18: Done.

Reviewer #2, C19: L.98: “which the plot officially joined”: please rephrase Response C19: We rephrased the sentence as followed: “Center for Tropical Forest Science (CTFS) network with which the plot is officially associated since 2009”

Reviewer #2, C20: L.122: Replace “into the ground” by “into ground” Response C20: Done.

Reviewer #2, C21: L.138: Replace “cannot” by “could not” Response C21: Done

Reviewer #2, C22: L.211: For consistency, keep the order you present forest classes the same throughout the paper (SIS, SES, OGS)

Reviewer #2, C22: We have now maintained the consistency of the forest classes as SIS, SES and OGS throughout the revised manuscript.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-280, 2019.

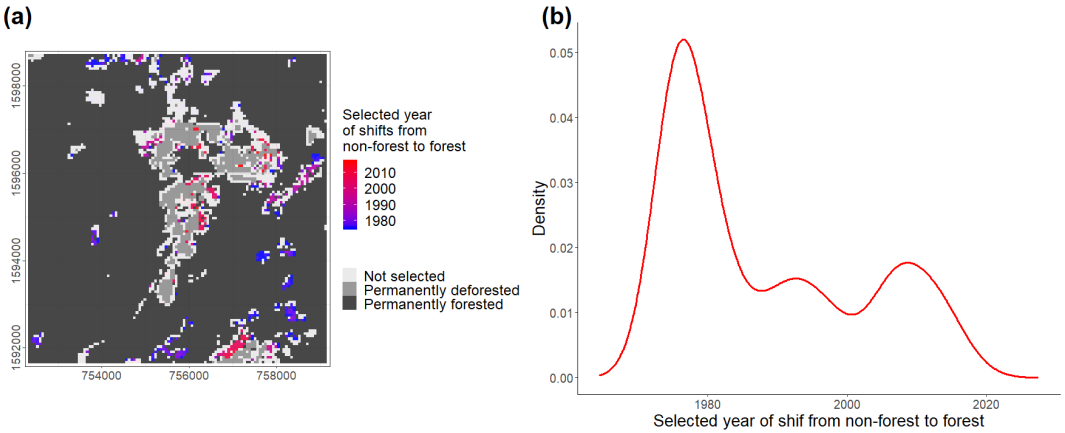

Fig. 1.

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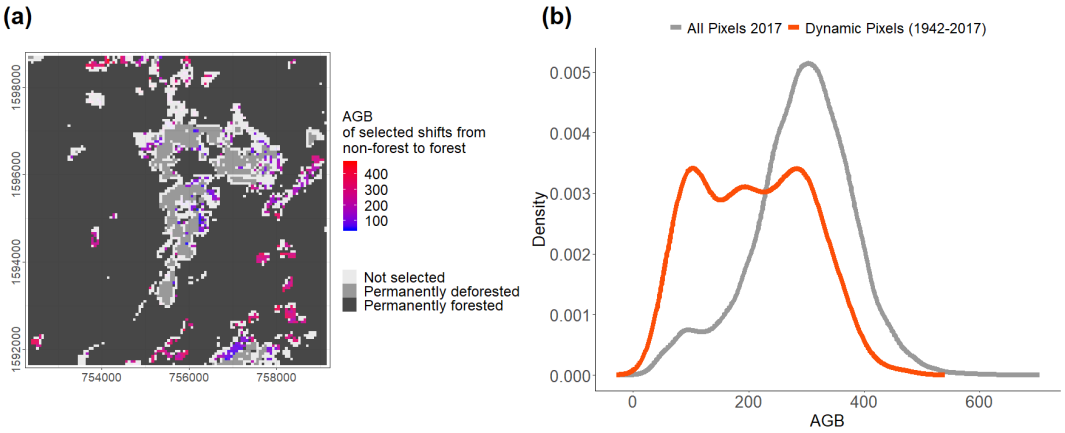

Fig. 2.

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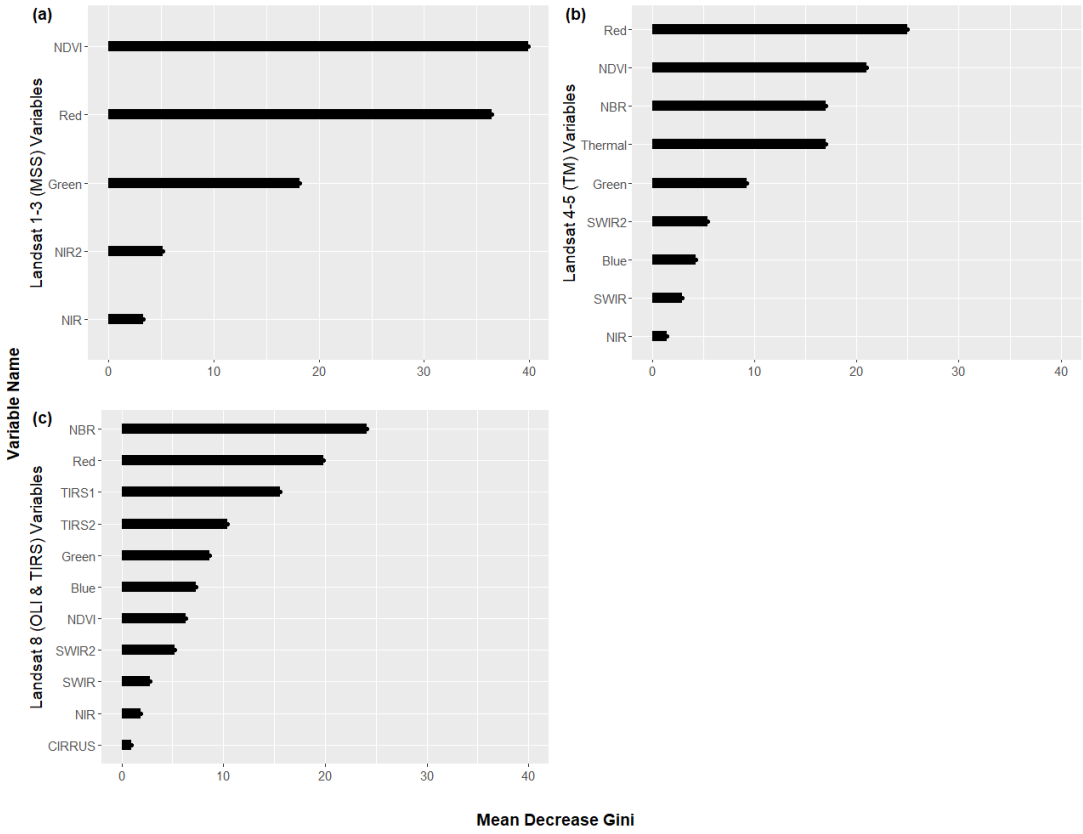
(a)
Landsat 1-3 (MSS) Variables
NDVI
Red
Green
NIR2
NIR
0
10
20
30
40
(b)
Landsat 4-5 (TM) Variables
Red
NDVI
NBR
Thermal
Green
SWIR2
Blue
SWIR
NIR
0
10
20
30
40
(c)
Landsat 8 (OLI & TIRS) Variables
NBR
Red
TIRS1
TIRS2
Green
Blue
NDVI
SWIR2
SWIR
NIR
CIRRUS
0
10
20
30
40
Variable Name
Mean Decrease Gini

Fig. 3.