Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-236-AC1, 2019


Geoscientific Model Development Discussions

# *Interactive comment on* "Observation-based implementation of ecophysiological processes for a rubber plant functional type in the community land model (CLM4.5-rubber_v1)" *by* Ashehad A. Ali et al.

**Ashehad A. Ali et al.**

ashehad.ali@uni-goettingen.de

Received and published: 28 February 2019

Anonymous Referee #1

Review of "Observation-based implementation of ecophysiological processes for a rubber plant functional type in the community land model (CLM4.5-rubber_v1)"

We thank the reviewer for reviewing our work. We acknowledge that our model developments involved several assumptions and thus had some level of uncertainty associated with it in the first instance. Therefore, we have removed the sections that involved



calibrating soil respiration. We place the reviewer's comment as "C" and provide our response as "R".

C1: The authors develop a new plant functional type for the community land model CLM4.5 representing the rubber plant to simulate rubber plantations in the Indonesian province of Jambi. They show that their model can reproduce measurements of plant productivity, biomass, soil carbon, and water fluxes at one site.

R1: We respectfully disagree with the reviewer here. We showed model development and calibration at two different sites, where the differences included age of the plantation, soil texture and management intensity.

C2: The major drawback of this implementation is that the model has been calibrated to match observations of the study site with specific environmental and management conditions. Initial simulations with a more generic implementation did not produce results that compared well with measurements. Therefore, it remains unclear if their model is suitable to be applied to other regions and management systems.

R2: We appreciate this comment very much and thank the reviewer for raising his/her concern. Now, we have performed model evaluations at two contrasting climate zones (Thailand and Cambodia) and with presumably different management practices, high soil fertility. Please see the model evaluation section in the manuscript.

C3: Furthermore, I wonder why the authors initially chose to base their work on CLM which its necessarily (as a land surface scheme for an Earth System Model) crude representations of vegetation structure and processes given their focus on a single site.

R3: We would like to look at how carbon, energy and water fluxes vary spatially over the Jambi Province for different land use systems. We now present our land use map in the manuscript (see Figure S11). The reason why we are representing the processes for rubber in CLM is that in our previous studies, we have developed a sub-canopy module for oil-palm (Fan et al., 2015) in CLM. To be consistent, we would like to use the same "parent" model and run it on the Jambi province. Indeed, CLM has been used in many land use change studies.

More specific comments below. C4: p. 9, line 214: Please provide a more detailed explanation for your assumption about the effect of tapping on carbon allocation. How did you derive formula 3 from this assumption? Is there any reference literature on the proposed mechanism?

R4: We agree with the reviewer here. We have improved the description of tapping in the manuscript (see lines 231-239) and it is more realistic now. In order to address the effect of tapping on carbon allocation, we have performed the sensitivity analyses key measures of productivities to changes in latex tapping fraction (please see lines 401-407).

C5: p. 9, line 233: Is there a limit to amount of tapping before a tree wouldn't survive?

R5: Yes, in reality if the soil has a low fertility and trees are tapped with either a large amount of intensity or tapped continuously without resting days, then they could eventually die. Currently, we do not consider the "resting days" in the model. Rubber plantations typically have 1 to 2 resting days (Giambelluca et al., 2016), which can be dependent on the age of the plantation and the management.

C6: p. 11, line 288: Here you say that figure S1 shows values for simulations and measurements of tropical evergreen forest, but the caption of S1 says it shows values for rubber plantations.

R6: Thanks for pointing this out. We have corrected it now.

C7: p. 11, line 292: What do you mean by "we used the default parametrization of stress deciduous tropical PFT of CLM4.5, but the generic model performance was poor relative to some of the measurements"? Did you apply tapping to the unmodified PFT without the changes in allocation, phenology, and effects of soil water on stomatal



conductance described later in the paper?

R7: We have removed the text "we used the default parametrization of stress deciduous tropical PFT of CLM4.5, but the generic model performance was poor relative to some of the measurements" from the manuscript now. To facilitate better understanding of the model development and the effect of parameters, including tapping on carbon and water fluxes, we now show comparisons using the parameters of the drought deciduous PFT as it is and the simulations using the rubber parameters.

C8: p. 12, line 333: "pers. comm." with whom? R8: We have removed this text from the manuscript now.

C9: p. 13, line 335: Please define "brevi-deciduous" as this is no common language (drought-deciduous?). R9: We have also removed this text from the manuscript now, but note that previous studies (Giambelluca et al., 2016; e.g. Kotowska et al., 2016) have used this term to describe the phenology of rubber.

C10: p. 13, line 361: Please describe in more detail how you determined the revised values of the parameters smpso and smpsc. Here you cite Verhoef and Egea (2014), but this paper does not present a value for rubber or certain tree species. Did you use the model of Verhoef and Egea in CLM? In this case, how did you determine the required soil hydraulic parameters?

R10: We did not use the model from Verhoef and Egea (2014) and thus have removed this part from the paper. However, we now describe in the manuscript (in lines 380-386) the following: "...by modifying the default soil water potential for drought deciduous tropical PFT in the model for stomatal opening "smpso = -35000 mm" to "smpso = -8750 mm" and stomatal closing "smpsc = -224000 mm" to "smpsc = -56000 mm". To arrive at these values, we first reduced the stomatal slope from 9 to 5 in a step-wise fashion. We could not further reduce the transpiration so we started changing these two parameters. We reduced these two parameters also in a step-wise fashion until the transpiration started to be limiting."

C10: p. 14, line 381: How did you derive the assumption that the base rate of maintenance respiration for fine roots is 50% higher than in the standard model version. Doughty et al. (2018) find higher root respiration at lower fertility sites with lower cation exchange capacities, but don't quantify the size of the effect. However, they don't find a significant difference in maintenance between more and less fertile sites.

R10: We do not focus on calibrating the soil respiration processes so have also removed the text that focused on changing the base rate of maintenance respiration from the manuscript now.

C11: p. 14, line 391: Is there any physiological explanation/reference for the implemented additional increase in growth respiration of fine roots? Is this typical for your site or rubber trees in general?

R11: We decided not to focus anymore on improving the soil respiration and thus have also removed this part from the manuscript now.

C12: p. 15, line 406: I don't understand your reasoning to increase the Q10 value. Q10 describes the increase of soil respiration per 10_C increase in temperature. You argue that it is necessary to increase Q10, because "rubber plantations at our study sites are 0.5_C hotter than forests". That's not an argument to increase Q10, because higher temperatures will lead to higher soil respiration under the same Q10 value anyway. Do soil characteristics at your test site explain why it should be necessary to double Q10?

R12: We do not consider changing Q10 anymore and thus have removed this part from the manuscript now.

C13: p. 16, line 453: "The leaf life span is high in the wet than the dry season." Do you mean higher?

R13: We do not consider seasonal changes in specific leaf area with leaf age and so have removed this section from the manuscript now. Nevertheless, we think that this finding was quite novel, emphasizing the need for a dynamic seasonal component of



functional traits in models.

C14: p. 16, line 454: You state that there is no temporal data on rubber tree SLA. How did you derive the values for the month-dependent function?

R14: We have removed this section from the manuscript now.

C15: p. 17, line 466: Delete "the" in "in the Southeast Asia".

R15: We have eliminated this section from the manuscript.

C16: p. 17, line 477: You results don't show that CLM-rubber is able to simulate the dynamics of NPP and above-ground biomass, because you compare a simulated time-series with a single measurement. While the simulation agrees with the measurement this does not automatically imply that simulated changes up to this point are also representative of real dynamics. For soil organic carbon you compare two measurements with model results. While both data sources indicate a decline over time, it is unclear if this change is statistically significant. The same applies to figure S2.

R16: We agree with the reviewer and now we do not mention that the model was able to 'simulate the dynamics of productivity' but instead say that the model 'captured the measured value'; see lines 585-586. We have removed the soil organic figure from the manuscript. Previous studies (van Straaten et al., 2015) at our studied sites did show that the decline of soil organic carbon was significant.

C17: p. 17, line 482: "The modeled biomass of the fine roots and the annual latex yield were much closer to the measurements in the model validation case (Figure S3; a, b) than the model calibration case (Figure 3; a, b)." The big difference between the two figures is not visible.

R17: We agree with the reviewer. We would like to point out that the soil texture and the age of the plantation differs between the validation and calibration cases.

C18: p. 18, line 495: While Figure 5 shows a good agreement between modelled and measured temporal variability of transpiration, absolute value agree substantially. By around 40% in Fig. 5a and almost 50% in Fig. 5b. This is no close agreement.

R18: We don't quite understand what the reviewer is saying "absolute value agree substantially". We think now we show better agreements though. This was achieved via re-calibration.

C19: p. 18, line 510: Please provide a better explanation of the proposed mechanism and why you base the assumption that "long-lived rubber leaves could have a low massbased photosynthesis, and that rubber plants could spend carbon in the construction of other tissues such as those associated with protection against insects or prevention of leaf diseases." on model results. It seems you simulated photosynthesis using both fixed and dynamic SLA and then fit a function to simulated monthly average peak photosynthesis. The fit of the function is better (higher r square) for the dynamic SLA case. But how can you validate model results by a function fitted to these results?

R19: We have eliminated this section from the manuscript.

C20: p. 19, line 527: Are there no studies available that analyzed the effects of climatic parameters on rubber yields in Indonesia or South East Asia? Something similar to: https://www.sciencedirect.com/science/article/pii/S0168192398000513

R20: Yes, surely there are but to evaluate the performance of the model, we require other data-sets too, like climate data, for example.

C21: p. 19, line 538: Did you really conduct simulations at the ecosystem scale or simulations of the single rubber PFT?

R21: Yes, we conducted simulations of a single PFT. We did not consider understory vegetation. And, now, we do not compare ecosystem scale fluxes in Jambi.

C22: p. 19, line 541: "high" should probably be "higher"

R22: We have removed this section from the manuscript.



C23: p. 19, line 543: Please explain the data showed in Table 2 in more detail. Are these value for a specific year or average values over a longer timeframe?

R22: We have also removed this section from the manuscript.

C24: p. 21, line 575: Q10 is a soil parameter and has nothing to do with PFTs. Furthermore, the study of Meyer et al. (2018) is based on measurements in Germany and Belgium so it is questionable if their results are directly transferable to tropical conditions. Meyer et al. also found that Q10 varies with soil moisture, so their main finding is not that models generally use Q10 values that are too low, but using dynamic Q10 values that vary with soil moisture may be a more suitable approach.

R24: We do not focus on Q10 anymore so have also removed this section from the manuscript.

C25: p. 22, line 628: Please provide more evidence for your conclusion that can be used CLM-rubber to perform larger-scale simulations within Indonesia given that you calibrated the model to specific site-conditions. How representative are the simulated yield levels for other regions within Indonesia or other site conditions and management regimes? At least you could simulate yields at one other location with Indonesia that is different to your site.

R25: Yes, we have evaluated our model now and put it into our manuscript. Also now we state in the manuscript in lines 811-818 the following "In lowland areas of Jambi, with highly weathered mineral soils, rubber plantations are very rarely fertilized, and soil fertility (i.e. soil N availability, extractable P, and soil organic C) decreased with conversion of forest to rubber (Allen et al., 2015, 2016). Additionally, previous study at the rubber plantations in the Jambi province has shown that it did not differ by a large amount (Kotowska et al., 2015). On the basis that CLM-rubber predicted the rubber yields reasonably well, we think that CLM-rubber can simulate the dynamics of rubber plantations on the Jambi province (see Figure S11)."

C26: p.23, line 638: Do you plan to integrate rubber plantations also in CLM-FATES?
R26: Yes, we will certainly put it into CLM5 and CLM5-FATES. We would like to look at the aging processes for rubber in CLM5-FATES.

References Allen, K., Corre, M. D., Tjoa, A. and Veldkamp, E.: Soil Nitrogen-Cycling Responses to Conversion of Lowland Forests to Oil Palm and Rubber Plantations in Sumatra, Indonesia, PLOS ONE, 10(7), e0133325, doi:10.1371/journal.pone.0133325, 2015.

Allen, K., Corre, M. D., Kurniawan, S., Utami, S. R. and Veldkamp, E.: Spatial variability surpasses land-use change effects on soil biochemical properties of converted lowland landscapes in Sumatra, Indonesia, Geoderma, 284, 42–50, doi:10.1016/j.geoderma.2016.08.010, 2016.

Fan, Y., Roupsard, O., Bernoux, M., Le Maire, G., Panferov, O., Kotowska, M. M. and Knohl, A.: A sub-canopy structure for simulating oil palm in the Community Land Model (CLM-Palm): phenology, allocation and yield, Geosci. Model Dev., 8(11), 3785–3800, doi:https://doi.org/10.5194/gmd-8-3785-2015, 2015.

Giambelluca, T. W., Mudd, R. G., Liu, W., Ziegler, A. D., Kobayashi, N., Kumagai, T., Miyazawa, Y., Lim, T. K., Huang, M., Fox, J., Yin, S., Mak, S. V. and Kasemsap, P.: Evapotranspiration of rubber (Hevea brasiliensis) cultivated at two plantation sites in Southeast Asia, Water Resour. Res., 52(2), 660–679, doi:10.1002/2015WR017755, 2016.

Kotowska, M. M., Leuschner, C., Triadiati, T., Meriem, S. and Hertel, D.: Quantifying above- and belowground biomass carbon loss with forest conversion in tropical lowlands of Sumatra (Indonesia), Glob. Change Biol., 21(10), 3620–3634, doi:10.1111/gcb.12979, 2015.

Kotowska, M. M., Leuschner, C., Triadiati, T. and Hertel, D.: Conversion of tropical lowland forest reduces nutrient return through litterfall, and alters nutrient use



efficiency and seasonality of net primary production, Oecologia, 180(2), 601–618, doi:10.1007/s00442-015-3481-5, 2016.

---

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-236, 2018.