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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.

Anonymous Referee #1

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

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.

The major drawback of this implementation is that the model has been calibrated to

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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. 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.

More specific comments below.

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?

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

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.

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?

p. 12, line 333: "pers. comm." with whom?

p. 13, line 335: Please define "brevi-deciduous" as this is no common language (drought-deciduous?).

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?

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.

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?

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?

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

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?

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

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.

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.

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.

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 mass-based 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?

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

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

p. 19, line 541: “high” should probably be “higher”

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?

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.

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.

p.23, line 638: Do you plan to integrate rubber plantations also in CLM-FATES?

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