Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-236-RC2, 2018

Geoscientific Model Development Discussions

Open Access

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 #2

Received and published: 17 December 2018

Ali et al. make a manual calibration of the drought-deciduous PFT in CLM to represent rubber trees. The development of PFT parameterisations for such tree crops is important, as they are rarely explicitly represented in large-scale models. The basic concept of the work is fairly solid, but I have reservations around the justification for the calibrated values and the general applicability of the results. The major limitation is that some important parts of the parameterisation are highly site-specific, which will substantially limit the potential to easily apply this model to sites outside the low nutrient and climate envelope to which it has been parameterised. Furthermore, many of the

Printer-friendly version

Discussion paper

parameterisation choices are only loosely supported and may be implicitly incorporating other factors such as climate biases. I would have preferred to see a parameterisation which incorporated more flexibility for site conditions – particularly if there are any plans to incorporate this more routinely into a trunk version of CLM where users may be unaware of the caveats of this particular PFT. A clear statement in the conclusions and abstract would help avoid this. I suggest that major revisions are required before publication.

Main comments

The model should be run for more sites, either for the calibration or evaluation, or ideally both, in order to give confidence in its applicability, even within the relatively narrow window of low nutrients and phenology for which it has been parameterised. Several sites are mentioned in Table 1. Why not also run the model for these sites, rather than the current loose comparison to the Jambi results?

The paper as it currently stands does not convincingly demonstrate that the new PFT parameterisation performs better than the basic drought deciduous parameterisation for the Jambi site. This would be easily to solve by adding the results for the unparameterized version to all calibration and evaluation figures.

Expanding on the point made in the first paragraph, above, there are three examples of parameterisation which are highly site-specific and limit wider model application: 1) Parameterising leaf longevity to month (line 185) substantially reduces the wider scale applicability of the model. It is fine if it is only intended to apply the model in the location it has been parameterised for, but it is not appropriate if it is intended to apply the model in other locations or for climate change scenarios. Can the longevity not be linked to climate triggers instead? 2) Similarly, increasing root maintenance respiration to account for increased allocation of carbon to alleviate nutrient limitations constrains the model to only be used in these low-nutrient situations. Surely there are studies which have looked at how allocation or respiration varies with nutrient availability that could

Printer-friendly version

Discussion paper

be used to develop a more general parameterisation? 3) The Q10 tweaking is not very convincing. First off, the justification for the shift in the Q10 is that the rubber plantations used for calibration are 0.5°C hotter than forests. In this case the temperature input to the Q10 could logically be raised by 0.5°C, but this is no justification to change the Q10 itself. Secondly, there are other factors here that could lead to the lower soil carbon. Is litter fall lower for instance, because less productivity goes into growth due to the latex harvest? Or is erosion of soil following clear-cut of the original forest at fault?

I presume that the climate data used was large-scale, rather than a local weather station. In this case, how can the authors be clear that the differences between the observations and the model are down to the parameterisation of the vegetation and not differences between the real site climate and the large-scale climate dataset? There is a real danger that climate biases from a single site are being parameterised into the vegetation response here.

Please make clear whether the soil respiration measurements are total belowground respiration or pure soil respiration (i.e. using root exclusion cores). When reading how they are compared to the model I sometimes think the former and sometimes the latter. This is important for the justification of the parameterisations made.

The section on model projection under drought (lines 522-527) is a bit under-developed. Even setting aside whether the current parameterisation can be expected to perform well under conditions outside the range for which it was parameterised, this section currently doesn't tell us much. It would be better to elaborate mechanistically on how the different scenarios simulated link to different yield reductions – i.e. make some useful hypotheses. Otherwise, why make all these simulations?

Other comments

Line 193. Please explain a bit better the pools here. It is not immediately clear what a "growth pool" is. This might be a pool of labile carbon to be used for growth, or a pool of structural tissue carbon (e.g. wood). I think you mean the latter, but it is not immediately clear.

Line 228. Why is fcur_st defined as this value? If it is a guess, please say so and explain the logic used.

Line 227. Please provide a citation or explanation for tapping starting at six years.

Line 280. Please explain the principle aspects of the model spin-up and climate data used, so that the reader does not have to refer to another paper to get the basic idea of the simulation set-up.

Line 228. Did you also transfer below-ground carbon to litter following the clear-cut?

Line 333. Pers. comm. from who? Please also be more precise about how this guess was “educated”, explaining the logic used.

Line 334. “with a depth of the dip not so large in the dry season”, is not clear, please rephrase.

Line 337. The change of LAI in the dry season is not in Fig. 2.

Lines 337-341. Why even explain this experiment? It has not improved the model behaviour and there is no data to support it anyway.

Line 363. Citations or logic needed backing up why these are in the range of plausibility. The comparison to existing ranges in CLM PFTs is not convincing.

Line 490. This logic doesn’t hold. There could also be a systematic underestimation of leaf turnover rate.

Lines 554-557. Actually, the effect on the carbon cycle of not getting leaf life span correct is not shown. Please quantify this to support the statement made.

Lines 659-661. This is very tenuously supported by the work here. More confidence in the parameterisation and application of the model at these other sites would be required to support this.

Figure 7. Both panels can be combined into one for easier comparison and to save space.

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