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Interactive comment on "Microbial dormancy and its impacts on Arctic terrestrial ecosystem carbon budget" *by* Junrong Zha and Qianlai Zhuang

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Although, I have some concerns with the model formulation, I will focus on the calibration and validation. For the model formulation some more background and motivation of the used formulation would be appreciated. Currently, I need to read the referenced He paper to understand it.

I was happy to read about the model validation at independent sites and data. However, the calibration/validation it is not yet presented in a way to fully understand it and uncertainties are lacking. Uncertainties are necessary to answer my most important concern: Is the introduction of several more degrees of freedom, i.e. free parameters, justified? Comparing predictions based on a single optimal parameter set is not sufficient to justify a more complex model.

Fig. 3 shows that many parameters are poorly constrained (non-experts should be provided to compare the prior in Fig. 3 to the posterior see this). The problem of more degrees of freedom with more details models is equifinality: several combination of parameters match the data similarly well. That needs to be incorporated in forward simulations, which usually become more uncertain with equifinality.

The simulations need to be repeated for both model versions with a larger number of viable parameters sets - and in the validation need to include also the variation of parameters that were not part of the current calibration. Then the 95% confidence bounds can be displayed in the comparison to the data - at least for a few sites. I suspect that adding too much detail will increase the confidence bounds until 2100 so wide, that the dormancy model (and maybe even the TEM-MIC) cannot generate conclusive trajectories. But the authors may prove me wrong.

Detailed comments

Fig 3: It is not clearly stated, how many parameters were calibrated and Fig. 3 is barely readable because of display quality. Are there only 3 out of the 6 sites displayed? For the moment, I assume that the parameters of Table 1 were calibrated. I am also confused that you show distributions separately by site. As I understand, you need a cross-site parameter set. Which parameters (also of the non-optimized ones) differ by stratum (site/pixel/vegetation type/soil type)?

Cost function (17): Why did you not consider uncertainty of observed NEE? Usually, you need this to determine, which parameter sets are viable. If you have larger NEE confidence bounds, also more different parameter sets will generate predictions that are still compatible with the calibration NEE. For my main concern above it is important to keep also the slightly less optimal but compatible parameter sets.

L 294: Taking the mean of parameter sets is not recommended. If the parameter sets belong to the same limiting distribution, I'd rather pick one random sample if I forward runs with with a larger set are too expensive. For a proper analysis you need to run forward the model with several different parameter sets and then compute the mean and confidence bounds of the predictions. If parameters converged to a few several limiting distributions, i.e. clusters in the parameter sets, you need to report also follow ups for these alternative optima. If parameters did not converge, you have not yet successfully optimized the model.

The conclusion that a model of more degrees of freedom and re-calibration fits the observations better is plain to me, and not enough for a justification for using this model.

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