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Geoscientific Model Development Discussions

Interactive comment on "Addressing Biases in Arctic-Boreal Carbon Cycling in the Community Land Model Version 5" *by* Leah Birch et al.

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- This paper addresses a "high bias in photosynthesis or gross primary productivity (GPP) at high latitudes" apparent in CLM5.0 model simulations." The paper is geared towards identifying issues in the current standard model version and making recommendations for modifications aimed at improving model performance for simulating carbon fluxes in the arctic-boreal zone (ABZ).

- The focus on accurately simulating seasonal C exchange in the high latitudes is important as it strongly controls the seasonal cycle of atmospheric CO2 - an aspect of Earth system model predictions that has been found to be inaccurately simulated and thus warrants closer attention and calls for improvement of available models. The present paper tackles this issue and thus promises to be an important contribution.

- I considered it particularly useful that the authors applied point-based simulations for

direct comparison with observed C fluxed at eddy covariance sites.

- However, several aspects of this study limit the usefulness of the presented research. I list major points below. Given that I have to raise these (in my view) rather fundamental issues, I cannot recommend this paper for publication in its current form.

- However, I was also appealed, e.g., by the useful focus and separating effects by phenology (start and end of season) and factors determining photosynthetic rates (Vcmax, Jmax). This could be explored further. Part of the challenge for the present study is that the apparent high bias in simulated GPP in the ABZ is the outcome of multiple potential factors that probably feed back on each other. E.g., high photosynthetic efficiency (light use efficiency) during the summer leads to high C assimilation which should enable an expansion of total leaf area which, in turn, should increase photosynthesis by increasing the fraction of absorbed light. The complication is that this is sort of a "chicken-or-egg problem" (What's the root cause?). A rigorous way forward to address this would be to disentangle contributions by, for example, prescribing seasonal leaf area from observations and calibrate parameters determining light use efficiency first. If the phenology routine was decoupled from other parts of the model (which it is not, see below), it could also be calibrated separately (without having to run the entire model). Then, once light use efficiency and phenology are well calibrated, one may calibrate parameters determining leaf area (e.g., allocation factors). In my view, this would be a promising way forward here. I understand however, that this may not be easily achievable. A "middle ground" could be found, e.g., if the model evaluation focused on these separate factors (phenology, light use efficiency, leaf area index) and tried to identify their relative contributions to model-observation mismatch in original and revised model versions. Having said that, I also consider that the model revision itself warrants reconsideration. I do not consider the model modifications to be recommendable for adoption for global simulations, as I argue below.

Major

- Modifications to make the model fit ABZ observations does not assure that the model performance is not deteriorated outside these biomes. This may seriously undermine the usefulness of proposed changes for global model simulations. Restricting the model applicability to the ABZ makes little sense for mitigating this limitation in view of common (global) applications of this model (e.g. Global Carbon Project simulations, CMIP, etc.). This limited scope of observations for informing model structure and calibrating parameters is all the more disappointing as authors note themselves that the model's current implementation, e.g., of the phenology routine or the temperature acclimation of Vcmax and Jmax, is based on data from a limited climatic range (essentially just the temperate zone). In this view, the manuscript seems to repeat a practice that has apparently been at the heart of poor model performance of the currently available CLM version. One way to resolve similar issues has been to assign PFT-specific parameters and thus accommodate for different parameter values to take effect in different biomes (this works in combination with achieving a realistic simulation of the PFT distribution). However, what is proposed here, e.g., for the phenology module, is to apply not just different parameters to a ABZ-typical PFT, but to change the *model structure* (Sect. 2.4.1). If I understood it correctly that authors propose to apply this structure only to PFTs growing in the ABZ, I have to raise concern about the implications of such PFT-specific parametrizations. This may seriously complicate interpretation global model predictions in future applications and the calibration of the model.

- Proposed modifications are very model-specific, don't make systematic use of available observational data of the affected variables, have little potential for adoption into other modelling frameworks, and have little potential to improve the general understanding of how simulations of C cycling in the ABZ can be improved (see major points below). In addition, by its focus on evaluating the (essentially global) model only with observations from the ABZ, the paper does not make clear whether the proposed model modifications improve global model performance metrics. For example, a test against global iLamb benchmarks would have been useful to demonstrate the

usefulness of proposed changes. Let me clarify my concerns about the proposed modifications:

- Spring phenology: The proposed modification relies on internally simulated quantities as arguments to the phenology function (soil temperature, snow depth). This implies the risk of undesired effects on simulated phenology caused by modifications (possibly in the future) to the snow or soil temperature routines. Such "feedbacks" between different parts of the model complicate model development and the identification of root causes for model bias. The chosen formulation of spring phenology is all the more surprising since this complication is avoided by the use of growing-degree-day-based models that are standard and well-established (see e.g., Richardson et al., 2018; Hufkens et al.., 2018) for robust simulations of spring phenology (with some modifications like chilling requirement).

- Temperature acclimation of Vcmax and Jmax: Authors suggest to revert the formulation of temperature-acclimation from the currently implemented version designed following Kattge & Knorr (2007) to a previous version based on Leuning (2002). This happens to improve model performance in CLM5.0 and is justified here by reference to the limited representativity of the parametrisation proposed by Kattge & Knorr (2007). The manuscript does not clarify the structure of the parametrisations of the two versions. Either way, this change is hardly justifiable by improved process understanding. Authors also refer to Kumarathunge et al. (2019) who recently updated the analysis of Kattge & Knorr (2007) using a much extended dataset, now encompassing data from a wider climatic range. It remains elusive why the parametrisations proposed by Kumarathunge et al. (2019) were not used here. This would have been a potentially useful modification of the CLM model, based on improved understanding and a wider and more robust observational basis. [references given in the manuscript of Birch et al.]

- The modified initialisation of Vcmax and Jmax at the start of the season (Sect. 2.4.5) is specific to a particular module (LUNA) within CLM5.0 and thus has little relevance for adoption into modelling frameworks outside or for informing process understanding. In my view, this rather seems like a bugfix than a model improvement worth publication outside a CLM-specific technical report.

- Carbon allocation: Alternative choices (static allocation with different root:leaf allocation ratios, dynamic allocation, Sect. 2.4.6) were tested. However, as I understand it, the tests appear to be evaluated with respect to model performance in simulating GPP. Authors limit the justification for selecting a particular value by reference to a small number of references. This approach to model development makes no systematic use of relevant observational data on allocation patterns itself nor of calibration methods, and runs the risk of being affected by compensating errors between model performance in simulating allocation and GPP (authors do not demonstrate that the chosen modification of the allocation parametrisation actually improves simulated allocated patterns).

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