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Interactive comment on "Towards a more detailed representation of high-latitude vegetation in the global land surface model ORCHIDEE (ORC-HL-VEGv1.0)" *by* Arsène Druel et al.

Anonymous Referee #2

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Druel et al. include a number of new processes and parametrizations into the land surface model ORCHIDEE that are thought to be important in high latitude ecosystems including âĂć parameter optimization of C3 grass, âĂć implementation of a new shrub PFT, and âĂć implementation of a new PFT representing lichens and bryophytes.

Several additional relationships and processes have been also included, such as âĂć shrub-snow interactions, âĂć vertical soil organic matter profile, âĂć moisture dependence of heterotrophic respiration and anoxic conditions, âĂć moss effects on thermal diffusivity.

In general, I fully agree with the importance to advance the LSMs in these respects and I would like to see such important model development published soon. The au-

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thors also use a number of site-level observations and a formal parameter calibration procedure for this model development. However, I have some serious concerns about this manuscript which should be addressed prior to publication.

Most importantly, there are too many different topics treated in this single manuscript which then are themselves mostly only superficially addressed and which even may not have any relation to each other (in the model). I strongly suggest to focus the paper on 1-2 research questions and a reduced amount of new processes added. I would agree with a presentation of new shrub, moss and C3 grass parametrizations. After a thorough model evaluation, some model application could be presented e.g. to understand the relation of their carbon balances to each other and to trees as well as their effects on soil temperature. Still, I believe individual papers for shrub and moss functions and effects would be more clear. If all topics should stay within one paper, then substantial additional text and figures/tables are required in order to i) explain the research question and importance of processes using literature, ii) evaluate new (and sometimes old if affected) model functions, iii) present and discuss results with recent literature, and maybe apply the model to address a research question.

Some detailed important issues:

0) It is unclear to me how the authors can neglect the recent publication by Porada et al. (2016) which presents a process-oriented and dynamic representation of bryophytes and lichens in the land surface model JSBACH in introduction and discussion.

1) Mosses have an important function in Boreal forests and the forest ground is usually covered by mosses and lichens. Usually we can expect a NVP cover of more than 50% in Boreal forests and more in tundra (Rapalee et al., 2001; Porada et al., 2016). The approach in this study is to treat NVPs as separate PFT with a separate tile results in minor coverage in most regions. (The color scale in fig 5 is not useful to evaluate the shrub and moss cover, please improve). Hence, there will be a strong bias in moss and lichen effects on the heat balance and biogeochemical ecosystem functions using

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such model. That limitation should be discussed in detail.

2) I agree with the authors that the global model can hardly cover small-scale variations in NPP and biomass of shrubs and mosses and lichens. Therefore, I suggest modify Fig 6 such that we see one dot for each climatic zone representing the model and data means but including error bars representing their std. Then one can discuss where the model fails to reproduce natural variance within one climatic zone and natural variance among zones. Fig 7 shows importantly that there is hardly any latitudinal variation in the measurements while the model shows a strong variation. Please, discuss in detail.

3) It seems, model calibration and evaluation at site level has been performed with the same data. If you have too little data to split the dataset into representative parts for calibration and evaluation, then please repeat the site-level model evaluation with a bootstrap method: iteratively remove data for calibration and evaluate respective model results at these sites.

4) I do not agree that LAI is a valid dataset from remote sensing data which is useful for process model evaluation (and if you like to use it please show in the fig ORCH13-GLASS and ORCH16-ORCH13 in order to understand the previous model bias and improvement). Possible maps for a landscape-scale model evaluation: fAPAR (JRC), GPP (Jung et al., 2011 or Beer et al., 2010), evapotranspiration (Jung et al., 2010), biomass (Thurner et al., 2014), and inventory-based NPP and biomass data (IIASA; Beer et al., 2006; Quegan et al., 2011). This is important as the fraction of tiles of all PFTs has been modified. In general, it would also really good to evaluate catchment runoff with freely available data of large Arctic rivers.

5) The reduction in tree cover results in a reduction of transpiration in your grid cell averages. However, interception loss and evaporation should increase with a layer of mosses and lichens. If the water and energy balance is a topic in your paper, then please show results for all components, not only transpiration in Fig 12.

6) In this model version, two modifications affect soil temperature: snow depth and

moss&lichen cover. First of all, the model version should be evaluated in terms of snow depth and soil temperature. For soil temperature, you can use GTN-P borehole data from Romanovsky et al. (2010) and Christiansen et al. (2010) available at PANGAEA, and maps of soil temperature and ALT even from your study region from Beer et al. (2013) at PANGAEA. I expect a cooling effect from mosses (Porada et al. 2016) due to higher insulation in summer, and a warming effect due to higher snow depth in areas of high shrub cover (still unclear to me at landscape scale as shrubs accumulate snow from lateral wind transport, so it is just relocated within the grid cell?). In Fig 13 both effects are combined. Is there a way to separate them? In Fig 13 it seems the model overestimates ALT and that is even higher in ORC16? In Fig 13b it seems all three grid cells show higher ALT (red) while in 13c one profile shows warmer temp (red) and the others show cooler temp? I generally suggest concentrating on soil temperature because ALT estimation from modelled temperature is not reliable.

7) Parameter estimation: Please show a priori and a posteriori parameter distributions in the appendix.

8) Please include a discussion section in which you interpret the results using literature in order to learn something. Parts of your summary section can be used if enhanced by literature. The conclusions and outlook section should be much reduced.

9) Several new methods are described but their importance, evaluation, and application is unclear: âĂć Section 2.2.6: anoxic conditions are not simulated, soil organic matter dynamics are no topic of the paper. Please remove. Or was the intension to evaluate GPP and NEE at eddy covariance sites? âĂć Why is shrub allometry important and why not only assume smaller trees? âĂć Shrub-snow interactions are not evaluated or analyzed. What do we learn from these additional functions? âĂć Effects on albedo: Has been albedo improved when comparing to satellite products?

Minor issues:

Fig 10: not used in results but only in summary and that there also the fig does not

support the sentence.

CO2 conductance in non-vascular plants depends strongly on its moisture and not on stomatal conductance. If that concept is not used here, then please discuss this limitation and related potential biases in detail.

Page 16, line 35: I do not understand.

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