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Geoscientific Model Development Discussions

Interactive comment on "ORCHIDEE-MICT (revision 4126), a land surface model for the high-latitudes: model description and validation" *by* Matthieu Guimberteau et al.

Anonymous Referee #2

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The manuscript presents a new version of the global land surface model ORCHIDEE, which aims at a more realistic representation of hydrological processes and carbon fluxes at high latitudes and is called ORCHIDEE-MICT. To this end, several new components are introduced, such as a vertical soil carbon profile, influence of soil carbon on soil thermal properties, and a revised scheme for plant water stress. The new model is thoroughly evaluated by comparing multiple variables, such as snow properties, soil moisture and temperature, runoff and evapotranspiration, GPP, NPP, biomass and soil carbon. Explanations for mismatches between model estimates and observations are provided. The paper is well written and of good scientific quality. I therefore recommend to publish it with some minor revisions (see below).

General comments

(1) I agree with reviewer #1 that the paper is quite long, but I do not think it is necessary to split the manuscript. Instead, I suggest replacing the last paragraph on page 4 with a table or schematic, similar to a table of contents, which lists the sections of the paper. This might help the reader to get a better overview of the paper.

(2) The simulated soil temperatures show, in general, a cold bias, only for one soil depth in combination with the GSWP3 forcing data the temperature is overestimated. However, ALT is significantly overestimated, which does not make much sense to me. Even if soil thermal conductivity was overestimated, leading to a overestimation of ALT, this does not explain why soil temperature is underestimated. Could you please explain this a bit more? Furthermore, I do not understand why spatial heterogeneity should lead to underestimated ALT in the field measurements (page 22, line 14)?

(3) In the abstract, it is stated that the new processes put ORCHIDEE-MICT at the forefront of land surface modelling at high latitudes. However, I would expect more comparison at the process level to other models (e.g. JULES or JSBACH) to substantiate this statement, maybe through a short paragraph in the discussion. I would also like to know why the inclusion of an organic layer or a moss/lichen layer, which has been done in JULES (Chadburn et al, 2015, TC) and JSBACH (Porada et al, 2016, TC) was not considered? Could you please explain in this context the relation of ORCHIDEE-MICT to another ORCHIDEE version which is currently in review in GMD (Druel et al, 'Towards a more detailed representation of high-latitude vegetation in the global land surface model ORCHIDEE (ORC-HL-VEGv1.0)') ?

(4) I agree with the authors that the new processes implemented in ORCHIDEE-MICT should improve the model performance at high latitudes. However, I did not find in the manuscript any comparison with the previous ORCHIDEE version in this regard. Could you please show with 2 or 3 examples how ORCHIDEE-MICT represents an improvement over the previous version, e.g. with respect to simulation of runoff, snow

patterns, carbon fluxes etc.?

Specific comments

p.3,l.4: Is the correct buildup of soil carbon pools during the spin-up the only important factor for the correct short-term (100yr) prediction of soil carbon fluxes? I would argue that accurate representation of decomposition is at least equally important. Please explain shortly in the discussion why you did not revise the decomposition scheme.

p.4,l.3: If transpiration is calculated per PFT, some averaging has to take place in order to calculate the energy balance per grid cell; Please explain.

p.5,l.13: To what extent does soil water content fluctuate in 11th soil layer? If significant changes occur, these are transferred with unlimited speed through the whole soil column down to 38m. This may lead to unrealistic dynamics of thermal properties.

p.9,l.14: Why do you assume that the residual soil moisture and the Van Genuchten coefficients are independent of soil carbon content? Does soil carbon have no effect on soil texture? Please explain shortly.

p.24,l.10: It is suggested that low speed of infiltration is the reason for the underestimation of soil water content in the deep soil. In addition to the mentioned deficiencies in the representation of infiltration into frozen soils, I would like to know whether the sensitivity of deep soil moisture to the parametrisation of soil hydraulic conductivity was tested? I think the importance of the soil water deficit should be pointed out a bit more in sect. 7, since it has far-reaching effects such as reduced transpiration, increased surface temperature, and reduced productivity.

p.26,l.30 Why is peak GPP overestimated? Please explain.

p.27,l.4: NPP is underestimated (Fig 16) due to water stress and lack of nitrogen fertilisation. GPP, however, is overestimated, which should lead to an underestimation of CUE. However, CUE is overestimated, and this is explained with a lack of nutrient limitation ('too much' nutrients), which is inconsistent with the lack of nitrogen ('too little'

nutrients) mentioned above. Please explain.

p.28,l.30: The respiration could also originate from a moss/lichen layer which may show some activity at low temperatures and under snow.

p.29,l.6: Biomass is significantly overestimated (see also Fig S5), and, in my opinion, the difference between climate forcing data sets cannot explain this easily: Precipitation in CRUNCEP is lower than in GSWP3, but biomass is higher, which seems counterintuitive. Could you please explain?

p.32,l.23: I think biomass compares well to observations only for GSWP3 forcing (Fig 21).

p.33,l.18: The model already has a cold bias, so even lower soil temperatures would be required for a more realistic (higher) soil carbon content. Why do you not mention explicit simulation of cryoturbation as a potential missing process?

supplement: Please indicate if CRUNCEP is subtracted from GSWP3 or vice versa.

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