RESPONSE TO THE REFEREE #1 COMMENTS

Title: ORCHIDEE-MICT (revision 4126), a land surface model for the high-latitudes: model description and validation
Author(s): Matthieu Guimberteau et al.
MS No.: gmd-2017-122
MS Type: Model evaluation paper
Iteration: Revised Submission

We would like to express our gratitude towards the two anonymous Referees for their constructive comments. We value very much their help in our effort toward a revised version of the manuscript which is at the end of this document. In the following, we write our point by point response.

- Reviewer's comments are in bold
- Modifications done in the new submitted version of the manuscript are in blue
- Figure and Table numbers, line numbers and pages all correspond to the initial manuscript version

REFEREE #1

This paper presents and evaluates the ORCHIDEE "high latitude" model version which is known as ORCHIDEE-MICT. The difference compared with the trunk version of ORCHIDEE is a vertically discretized soil carbon scheme and the coupling of this scheme to the soil thermal/hydrological properties, along with a representation of fire. What I really like about the paper is the coincident evaluation of so many variables (in order to correctly interpret interrelated biases) and the use of multiple datasets for the same variable when those are available (in order to give an idea of observational uncertainties). It is also great that two different forcing datasets were used, and I think it is a valuable conclusion that the uncertainty in forcing datasets needs to be taken into account to avoid "over-calibrating".
Clearly, this paper is extremely long. I think a suggestion of splitting it could be rightly met with the argument that all of the components are interacting so it would be difficult to split. However, depending on what the other reviewers or editors think, there could be a reasonable split into two linked papers that cover thermal/hydrological processes (in one paper) and carbon cycle processes (in another). Given the size of the work, it is well written so that it does not become too confusing to the reader. So for now (aside from the idea of splitting into two papers) I suggest only minor changes.

We agree that the paper is long but we think that splitting it into two linked papers would not be necessary, as mentioned by the reviewer #2 in his first general comment. Following the comments of this reviewer, we propose to remove the last paragraph on page 4 and replace it by a table of content between the abstract and the introduction. Moreover, Figure 4 will be moved to the supplementary section, as you suggested in point 3. We propose also to put the description of the evaluation datasets (sections 5.2 to 5.4, P.12 to 20) into an appendix section after the conclusions for a better clarity of the paper.

1. A theme that runs through the paper is the late response of LAI in the spring. The reason that is suggested (several times) in the paper is that this could be linked to the late persistence of snow cover. However, from my experience of such land surface models, they often don't incorporate a direct influence of snow on vegetation - perhaps this is incorporated in ORCHIDEE? But if so, can you make it more clear in the paper how the snow cover influences the vegetation in the model? The late LAI in spring also occurs in ORCHIDEE simulations where the snow does not stay too late (Chadburn et al., 2017), and I have heard that bud burst is simply triggered by the number of growing degree days and requires a rather large number to initiate bud burst. Therefore, I suggest you consider this alternative (possibly, more likely?) explanation, and recommend further study on the phenology scheme.

The reviewer is right, the snow cover is not directly taken into account in the vegetation phenology models used for the different PFTs of ORCHIDEE. After the sentences on Page 26, Lines 13-15: "In all the basins, the LAI simulated by ORCHIDEE-MICT has a phase delay of up to one month compared to both products. This is due to a delay in the start of the growing season, which may be related to excessive persistence of the snow cover (Fig. 10)" in section 8.1, we thus detailed the arguments to support our hypothesis: "The phenological models in ORCHIDEE (detailed in MacBean et al., 2015, Appendix A) do not explicitly take into account this influence, unlike what is done in Van Wijk et al. (2003), who model the link of the start of the tussock tundra growing season to the soil thaw at 10-cm depth. However there is a first indirect link between the snow cover and the vegetation phenology through air temperature, which influences both the start of the growing season, determined in ORCHIDEE using growing degree days (GDD)-based phenological models for deciduous species, and the start of the snowmelt season. There is a second indirect link through snowmelt. While there is still a large amount of snow, the soil surface temperature is kept at zero degree Celsius or below and the soil cannot thaw. Only when snowmelt occurs and when the snow fraction is small enough, will the soil start thawing, thus increasing soil liquid water content. This impacts the start of the growing season for grasses and crops, which use both a GDD and a soil moisture thresholds, and also reduces water stress, thus favoring photosynthesis for all PFTs."

In the discussion part, section 10.3, after the sentences Lines 32-33 P.33: "The phase of simulated LAI in spring lags satellite observations, in particular for BONA and BOAS sub-regions (Fig. 14).", we nevertheless acknowledge the reference to Chadburn et al. (2017), cited by the reviewer: "We argued that this lag is related to the late persistence of the snow cover. However a recent work (Chadburn et al., 2017) shows a late onset even in the absence of snow persistence on site simulations by ORCHIDEE. This calls for a revisit of the phenology-related thresholds in the high-latitudes, perhaps by introducing new PFTs (arctic C3 grass and shrub, and non-vascular plants), with their separate set of parameters calibrated, to better represent Arctic vegetation and their phenology (Druel et al., 2017).".

2. Another bias is the deep active layer, which really suggests that the soil properties should be better representative of the organic carbon content. The high organic content at the surface is quite well simulated (Figure 22), and this should have a great impact on the soil temperature. I would suggest that the problem might be the use of the linear weighted average for thermal conductivity. In terms of water and ice, the geometric mean is used - Equation 4 - so it might make sense to use that form for weighting the soil thermal conductivity as well, particularly the dry soil thermal conductivity which can be very low for an organic soil.

We would like to note that the soil carbon concentrations used in the thermal and hydrological modules were prescribed from the empirical soil databases of NCSCD and HWSD (as mentioned on Page 8 Lines 15-16), similar to the treatment in other land surface models like CLM (Lawrence and Slater, 2008) and JULES (Chadburn et al., 2015). So the modeled SOC by the carbon cycle module did not yet feed back on the soil physical properties. Although the coupling between soil thermodynamics and the prognostically modeled SOC is readily achievable by changing a configuration in the model set-up, we tend to exclude the bias of the carbon cycle module in the physical processes in this study as a first step.

The simulated ALT is indeed generally overestimated compared to the site observations from CALM network and the regional ALT map for Yakutia. However, apart from the bias in the parameterization of soil thermal properties, the fact that we did not use the site-specific organic layer thickness for the CALM sites, and that we did not prescribe regionally a fixed thickness for organic layer (to mimic the insulating effect of moss layer), also contributed to the bias in modeled ALT. Now we conducted, following the comment by Reviewer #2, additional CALM site simulations in which site-specific organic layer thicknesses were prescribed. These site runs then produced significantly shallower ALTs compared to the previous regional simulation. Please refer to our second response to Reviewer #2 for details.

As for the averaging method for soil thermal conductivity, it makes sense indeed to use geometric mean, which will be tested for the next steps of model development. We added the following sentence after the sentence on Page 9 Line 2: "Note that here we followed Lawrence and Slater (2008) to use linear weighting organic and mineral soil properties, while in some other models like JULES (Chadburn et al., 2015) and ISBA (Decharme et al., 2016), soil thermal conductivities are calculated as geometric averages of organic and mineral soils, consistent with the treatment for soil water and ice (Eq. 4). The geometric averaging method increases the effect of the organic fraction compared to arithmetic averages, and would be tested in ORCHIDEE-MICT in future developments."

In the seasonal cycle of NEE is an unrealistic peak of emissions in spring, which can be partly due to the late LAI already discussed, but also partly because of soil decomposition starting too early. You talked about CO2 trapping in the soil (P34 Line 16) but in fact it may be much more simply that the ground is thawing too quickly - again, due to the lack of thermal insulation from the organic layer. Although when the seasonal cycle of soil temperature is studied (Figure 6), this is not obviously the case, I think there might be a bias in the Russian dataset as it seemed to behave differently from other in-situ data. The problem is potentially with the removal of vegetation from the surfaces and site disturbance, which can result in the insulation of the 'organic layer' being removed (Frauenfeld et al., 2004). Certainly when comparing with in situ data in Chadburn et al. (2017), the soil in ORCHIDEE is thawing too early and likewise the soil respiration starts a bit too early in the spring.
I suggest adding discussion of the above points relating to the link between soil carbon and soil thermal properties.

We would like first to note that the Russian dataset for soil temperatures between 0.2 and 3.2 m depths (that we used in this study) have been measured under natural vegetation (mainly grass) and undisturbed snow; although the grasses were regularly mowed to keep their height below 20 cm (as described in http://nsidc.org/data/docs/fgdc/ggd251_soiltemp_fsu/), the main insulating effect of the organic matter near the ground surface has been kept. Therefore, bias of these measurements induced by human disturbance are generally very small.
Figure 6cd indeed shows a cold bias of soil temperature during spring; however, this figure is the average for all sites located in continuous permafrost region, while spatially, there is a warm bias in the Lena basin and a cold bias for the further eastern sites (please also see our second response to Reviewer #2), which partly offset each other in Fig. 6cd.
We agree that the spring peak in NEE may be partly explained by a too big soil respiration. But also note that the spring GPP is underestimated, especially in BOAS and BOEU (Fig. 15). To

address these issues, we modified the text on Page 34 Lines 15-17: “Interestingly the reasonable seasonal phase of simulated GPP (Fig. 14) can be contrasted with the larger lag of spring NEE uptake compared to inversions results (Fig. 19). This may be partly explained by the underestimated GPP at the beginning of the growing season (Fig. 14), and also by a possible too big soil respiration in spring. The moss/lichen surface coverage could be over 70% under the vast boreal forests (Porada et al., 2016), but we did not prescribe an additional moss layer in the regional simulations, which could lead to a too early thawing of the soil in spring.”

3. These two issues that I have mentioned: The phenology and the organic soil thermal properties, both seem quite important to me, and worthy of being mentioned in the conclusion, along with the issue of snow thermal conductivity which is certainly too high. These issues are extensively discussed in the text but not mentioned in the conclusions. (In particular, the organic soil properties are the ‘new’ process that is included in the paper so it seems important to include them in the conclusion.) It seems to me that the other processes are appropriately discussed (at least, as far as my expertise goes: I can’t comment on fires or say much about forests.)

We add these sentences in the conclusion:

- P. 34 L.27 “... in the ORCHIDEE-MICT land surface model. The effects of soil organic matter on soil thermal and hydraulic properties are incorporated.”
- P. 34 L.30 “Naturally, there remains significant room for improvement. The model appears to underestimate evapotranspiration and overestimate surface temperature, particularly in the southern portion of the boreal zone. Simulated phenology shows generally a delay in the onset of growing season. And the snow module underestimates the thermal insulation of snow.”

I would like to suggest some kind of reduction in the text, as the same points are sometimes made a few times, but it is hard to envisage how to do this- I’m sure you have thought about the same thing! However, to shorten I suggest at least moving Figure 4 to the supplementary as it doesn’t contain observations and doesn’t seem so informative as the others.

Yes, Figure 4 is moved to the supplementary section. We also propose to remove the last paragraph on page 4 and replace it by a table of content between the abstract and the introduction. We put the description of the evaluation datasets (sections 5.2 to 5.4) into an appendix section after the conclusion for a better clarity of the paper.

Small comments:

P29 Lines 25: "SOC stocks simulated by the model fit the spatial pattern from observed inventory data" - this does not seem convincing to me looking at Figure 22, I think this statement should be more qualified e.g. 'to some extent' !

Yes, we revised it as : "SOC stocks simulated by the model fit to some extent the spatial pattern from observed inventory data ...".

P28, line 27/8: just says "see 7" - should this be "see Figure 7"?

Corrected now in the text.