

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

Anonymous Referee #1

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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 discretised 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”.

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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.

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.
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.

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 decom-

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position starting too early. You talked about CO₂ trapping in the soil (P34 Line16) 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.

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.)

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.

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

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Figure 22, I think this statement should be more qualified e.g. 'to some extent' ! P28, line 27/8: just says "see 7" - should this be "see Figure 7"?

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