RESPONSE TO THE REFEREE #2 COMMENTS

Title: ORCHIDEE-MICT (revision 4126), a land surface model for the high-latitudes: model description and validation
Author(s): Matthieu Guimberteau et al.
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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 #2

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.

Thank you for this good suggestion. We will remove the last paragraph on page 4 and replace it by a table of content between the abstract and the introduction. Moreover, Figure 4 is moved to the supplementary section, as suggested by the Reviewer #1. We propose also to put the description of the evaluation dataset (sections 5.2 to 5.4) into an appendix section after the conclusion for a better clarity 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)?

Simulated ALT is indeed generally overestimated compared to the site observations from CALM network and the empirically-derived map for Yakutia (Beer et al., 2013). This seems inconsistent with the cold bias in soil temperature compared to the Russian meteorological stations' observations as shown in Fig. 6. However, we would like to clarify several points:

First, there is a mismatch for the locations of CALM sites for ALT and of Russian stations for soil temperature, the former mostly within the Arctic regions near the coast (for the Eurasian sites), and the latter more scattered throughout Russia. Therefore, the slight overestimation in peak summer-time soil temperature (and even underestimation with the CRUNCEP forcing) shown in Fig. 6cd for the Russian sites does not necessarily contradict with the significantly overestimated ALT compared to the CALM sites.
As for the regional maps for soil temperature and ALT, we acknowledge that Fig. 6ab may be a bit misleading if readers connect this figure to Fig. S3 which shows on the contrary a generally deeper ALT compared to Beer et al., (2013). This is because Fig. 6ab shows the annual mean temperature at 0.2 m depth, in which the larger cold bias in winter outweigh the warm bias in summer. To complement Fig. 6ab, we added maps for the maximum monthly soil temperature for the four depths in the Supplementary, also shown below, to facilitate a link between bias in soil temperature and bias in ALT. This figure shows an overestimation in maximum soil temperature below 0.8 m in the Lena basin, consistent with the deeper ALT for the same region shown in Fig. S3.

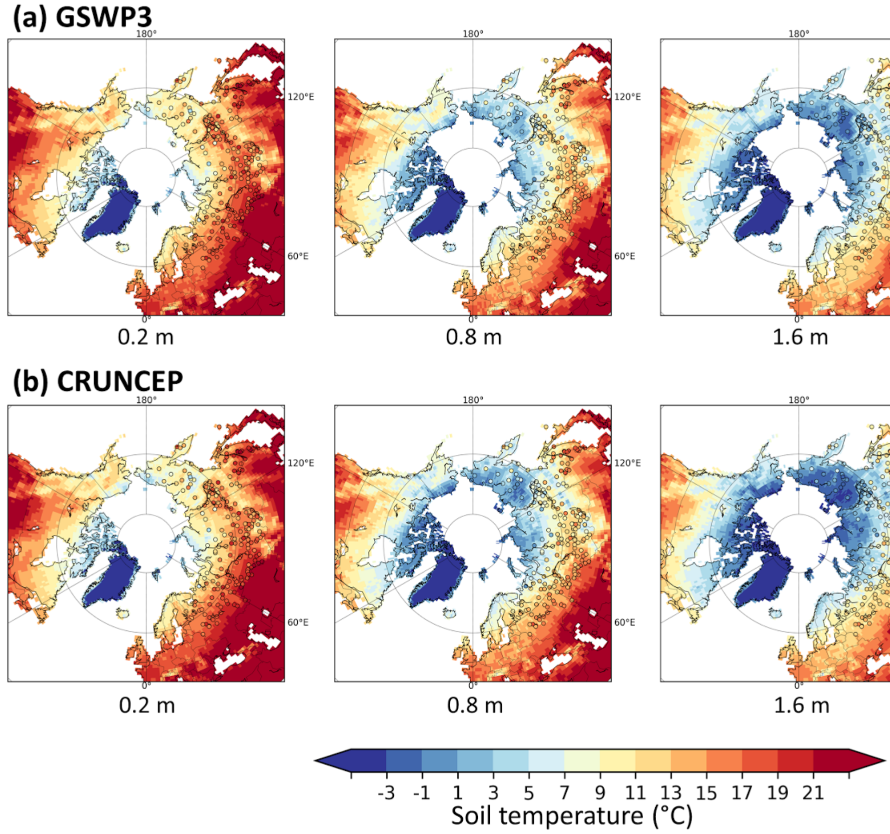

Figure S4. Maximum monthly soil temperature at different depths in **(a)** GSWP3 and **(b)** CRUNCEP-forced simulation (background maps), compared to the site observations (color filled circles), averaged over the period 1981-2000.

To clarify it, we revised the following sentences on Page 21 Lines 32-33: “Summer soil temperatures are higher in the GSWP3-forced simulation relative to those of CRUNCEP, and warmer than observations from the Russian meteorological stations in continuous permafrost region by 1~2 °C on average at 0.8 and 1.6 m depths (Fig. 5c,d). Spatially however, the bias in peak summer soil temperature varies for different regions, with a large warm bias for the Lena basin below 0.8 m, and some cold bias for the further eastern sites (Fig. S4). This is consistent with the overestimation of ALT for Yakutia (Fig. S5) compared to the empirically-derived map by Beer et al., (2013) (see Section 6.4). Differences between the two simulations…” And added one sentence at the end of the caption for Fig. 6: “…over the period 1981-2000. The spatial patterns of maximum monthly soil temperature are also shown in Fig. S4.”

Second, the mismatch between the local-scale ALT measurements at the CALM sites and the modeled ALT could be partly explained by the fact that we did not use the site information of the soil organic layer thickness in the calculation of soil physical properties, but used the gridded soil carbon database from NCSCD (Hugelius et al., 2013) for permafrost regions (as mentioned on Page 8 Lines 15-16), which was upscaled in the model to match the spatial resolution for each simulation (here 1° by 1°).

To further explore the impact of the site-specific organic layer thickness on modeled ALT, we conducted additional runs at the CALM sites, in which we assumed $f_{i,soc}$ in Eq. 9 equaling to one for the soil layers above the organic layer thickness at each site. While the other model inputs including climate and soil texture are the same as the previous northern hemisphere simulation. There are in total 69 sites that provide explicit organic layer thickness (https://www2.gwu.edu/~calm/data/north.html). Some sites, e.g. the Dot Lake in Alaska, also have a thick sphagnum layer above the decomposing organic soil; in such cases, we summed up their depths to derive a total organic layer thickness. The figure below shows the result, which is now added to the Supplementary.

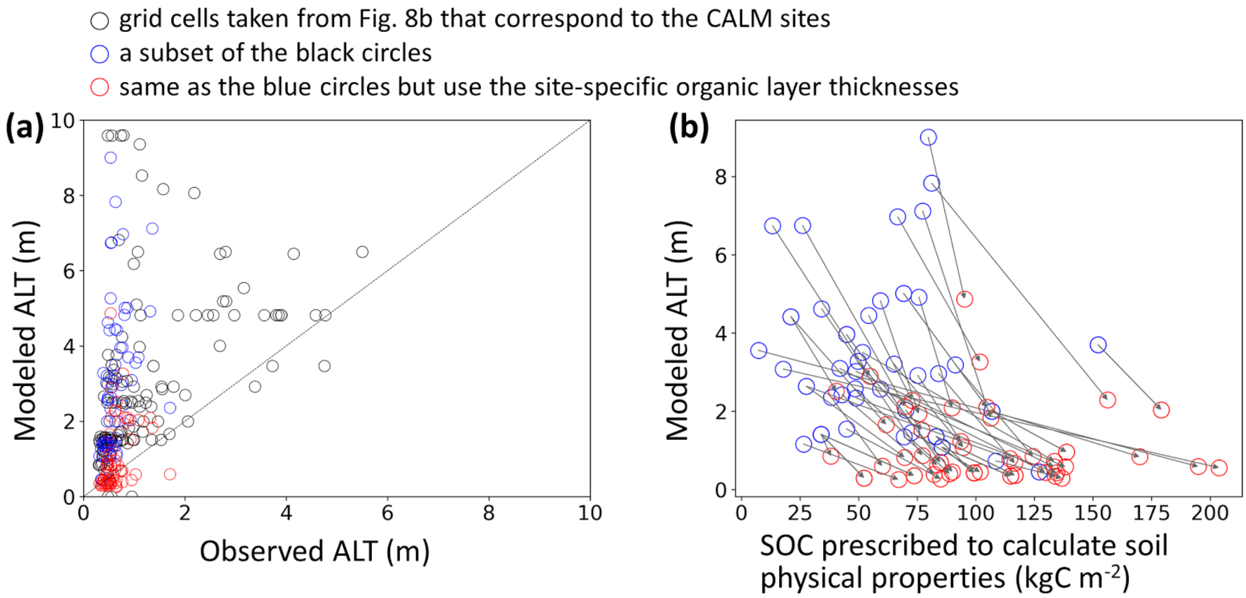

Figure S6. (a) Scatter plot of modeled ALT forced by CRUNCEP compared with observed ALT from the CALM network, averaged over the period 1990-2007. The black circles represent the grid cells taken from the regional simulation (shown in Fig. 8b). The blue circles represent a subset of the CALM sites for which we performed additional runs using site-specific organic layer thicknesses, with the result shown by the red circles. **(b)** Illustration for the difference of the additional site simulations. Each grey arrow connects the same site, pointing from the blue circles using soil carbon content from NCSCD (upscaled at 1° by 1° resolution) to calculate soil physical properties, to the red circles using the organic layer thickness provided by the sites. See text for further information.

Accordingly, the following discussions were added on Page 22 Line 10: "…whereas CRUNCEP-forced output shows relatively better agreement with the observations. Apart from the uncertainty induced by climate forcing, the model-data mismatch may also arise from scale differences for the organic carbon content that is used to calculate soil physical properties for each grid cell. As mentioned in Section 4.2, the empirical SOC map from NCSCD (Hugelius et al., 2013) is prescribed for permafrost regions in the soil thermal and hydrological modules, which is upscaled by the model to the target spatial resolution (1° by 1° in this study). These SOC values thus do not represent the site-level soil conditions, aside from the uncertainty of the NCSCD database itself. To further investigate this impact, we conducted additional simulations for the sites that provide explicit organic layer thicknesses (in total 69 sites), forced by CRUNCEP. In these runs, we assumed pure organic soil, i.e. $f_{i,SOC}$ in Eq. 9 equaling to one, for the soil layers above the site-specific organic layer thickness, while kept the SOC concentration unchanged below this thickness, i.e. from NCSCD. Note that the moss layer, vegetation mat, and/or organic root zone as described in some sites were all summed to derive a total organic layer thickness. The other configurations including climate forcing and soil texture were the same as the regional simulation. The result is displayed in Fig. S6, showing significantly shallower ALTs simulated by these site runs which better match the observations (Fig. S6a), with different magnitudes of ALT reductions among the sites (Fig. S6b)."

(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)') ?

The current model described in this study indeed lacks explicit representation of moss and lichen growth. For an inclusion of organic layer, however, the multi-layer structure of the model enables it to approximate the effect of an organic layer by assuming 100% organic soil above the prescribed organic layer thickness, as we did for the additional CALM site simulations. Actually, this method could also approximate the effect of moss/lichen on soil thermodynamics, assuming similar thermal properties of the moss/lichen layer to that of the organic layer, as the implementation in JULES (Chadburn et al., 2015, GMD). Therefore, the current ORCHIDEE-MICT is able to represent the insulating effect of moss/lichen and organic layer in a simplified way, given an input of their thickness.

However, for large-scale simulations to account for moss/lichen layer, what is indeed lacking in current ORCHIDEE-MICT is a prognostic modeling of moss/lichen surface cover, considering the lack of a gridded map for moss/lichen coverage, especially in the boreal forest understory (Chadburn et al., 2015, TC; Porada et al, 2016, TC). Chadburn et al. (2015) used functions of temperature, moisture, light and snow to empirically determine the ground cover of moss to be used in the soil thermal module, but did not simulate the carbon cycle of mosses. Porada et al., (2016) modeled the productivity and expansion of moss/lichen, which then, combined with fire disturbance, determined the dynamic surface coverage of moss/lichen.

Druel et al. (2017, GMDD) implemented an explicit representation of the high-latitude vegetation types including shrubs, boreal grasses, and non-vascular plants that are missing in standard ORCHIDEE. Processes and parameters regarding the growth of these new PFTs were defined, and the main biogeochemical results were evaluated. At the moment, their work is in parallel with the recent developments in ORCHIDEE-MICT described in this study, but could be merged within ORCHIDE-MICT in the next step.

To discuss these issues, we added a paragraph at the end of Section 10.2, Page 32 Line 15: "Previous land surface modeling studies have shown the critical role of organic matter in soil thermodynamics in permafrost regions (e.g. Lawrence et al., 2008; Chadburn et al., 2015), while different parameterizations of such effects are implemented in different models. Most of the recent models, like CLM (Lawrence et al., 2008), JULES (Chadburn et al., 2015), ISBA (Decharme et al., 2016), and ORCHIDEE-MICT in this study, assume weighted combinations of organic soil and mineral soil in the calculation of soil physical parameters for each soil layer in the model. This structure is more flexible than a fixed thickness of organic layer or moss layer as the implementation in JSBACH (Ekici et al., 2014), since the former could approximate the latter by assuming 100% organic soil above the prescribed thickness. Note that for the insulating effect of moss/lichen layer, the same values of thermal properties to that of the organic soil are usually used in recent models (Chadburn et al., 2015; Porada et al., 2016). In this study, however, we did not apply a fixed moss layer in the thermal module for the regional simulations, due to the lack of a gridded map for moss/lichen ground covers especially on the boreal forest floor, and to the lack of a representation for dynamic moss/lichen coverage as in JULES (Chadburn et al., 2015) and JSBACH (Porada et al., 2016). This could partly explain the regionally overestimated ALT compared to the empirical map for Yakutia (Fig. S5). An explicit representation for non-vascular plants in ORCHIDEE (Druel et al., 2017) has been worked in parallel with this study at the moment, but would be incorporated in ORCHIDEE-MICT in the future developments."

(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.?

ORCHIDEE-MICT is a new branch building on several separated former works with important processes for high latitudes for both the physical processes (e.g. Gouttevin et al., 2012a; Wang et al., 2013) and the carbon cycle (e.g. Koven et al., 2009). This paper is thus like the birth certificate of this new branch, demonstrating the effectiveness of all these combined processes in reproducing important observed variables. A comparison with the TRUNK, which does not yet incorporate all of these processes like the soil carbon discretization, would be unfair. An outcome of this paper will indeed be the integration of these high-latitude processes in the standard TRUNK version.

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.

We fully agree with the reviewer that a good representation of SOC is highly important to represent the carbon fluxes to the atmosphere. At some points we wondered how we could improve the scheme in ORCHIDEE-MICT, and from a short literature review (well summarized in Manzoni and Porporato, 2009, SBB, Wutzler and Reichstein 2008, BG or more recently in Luo et al. 2016 GBC) there is still no consensus in the soil science community, and the different approach with their own underlying assumptions can hardly impact the model outputs. The representation of SOC decomposition in land surface models is still under debate and within this debate we choose to be conservative and keep the scheme used for decades now based on CENTURY (Parton et al., 1988). Nevertheless, some actions are ongoing in our group to improve the SOC decomposition scheme including the impact of priming, dissolved organic carbon, etc.
We add these sentences in the discussion on Page 33 Line 18: "Other studies (Koven et al., 2013; Burke et al., 2017a) further limited the rate of decomposition of SOC at depth, to reproduce the lack of oxygen inhibiting decomposition. It should be noted also that the decomposition scheme of SOC is still based on Parton et al., (1988) as classically done in land surface models (Friedlingstein et al., 2006). Different approaches were proposed in models focusing only on SOC decomposition (Manzoni and Porporato, 2009; Wutlzer and Reichstein, 2008) based on different assumptions (substrate driven, decomposer driven, etc.), but no clear consensus emerged up to date to revise the SOC decomposition scheme in land surface models (Luo et al., 2016)."

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.

A transpiration flux is calculated for each PFT in each tile (forest and short vegetation). In each tile, a weighted spatial average of the different PFTs is performed. Then, the model calculates a weighted spatial average across the soil tiles to obtain a total representative flux of the grid-cell.

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.

The soil water content fluctuates in a variation range given by the hydrodynamic parameters of the different soils prescribed in the model. Thus it can vary from 0.034 to 0.460 m^3/m^3 (residual and saturated water content for a silt soil respectively). This corresponds to a maximum variation range of 68 to 920 mm for the 2-meter depth soil of the model. Yet, the most important variations of water content occurs in the superficial layers of the soil column that are submitted to precipitation events and infiltration. The non-linear decrease of the hydraulic conductivity with soil water content leads to reasonable values of gravitational drainage at the bottom and thus cannot lead to unlimited speed through the whole soil column.

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.

Soil organic matter may indeed contribute to the variation of the Van Genuchten equation coefficients (except for α) (Wang et al., CLEAN-Soil Air Water, 2014). However, we chose as a first step to keep the coefficients unchanged, and the resulted relationship of field capacity/wilting point versus SOC (Fig. S2) could capture the first-order characters in the observations by Hudson (1994). Modifications of the Van Genuchten equation coefficients would require an in-depth sensitivity study of soil hydrology in the model. This could be considered for further developments of the model.

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 parameterization 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.

In our parameterization of SOM in the model, the soil hydraulic conductivity can be affected by SOM through the increase of the porosity. However, we didn't perform any sensitivity tests of deep soil moisture to the parameterization of soil hydraulic conductivity. We point out the importance of the soil water deficit at the beginning of the paragraph "In the root zone" of sub-section 7.3 "Soil moisture", Page 24 Line 16: "The soil water deficit is of primary importance during spring and summer in the high latitudes because of its potential impacts on the vegetation transpiration, leading to a surface temperature increase and a reduction in the productivity. Yet, a soil water comparison between GLEAM and ORCHIDEE-MICT is difficult because ..."

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

The CO_2 fertilization seems indeed too important in ORCHIDEE. We have at least two leads to improve this behavior. Kuppel et al. (2012) used FLUXNET daily observations to optimize several photosynthetic parameters like the maximum carboxylation rate Vcmax. Given all the modifications brought to the model since this first FLUXNET optimization, mainly regarding the physics, it is probably time to recalibrate these photosynthetic parameters to get a more realistic GPP. Second, plants grown under elevated CO_2 show a photosynthetic downregulation (Sellers et al., 1996 ; Bounoua et al., 1999 ; Bounoua et al., 2010). This downregulation calibration is coded in the model to rectify Vcmax under different atmospheric CO_2 levels to empirically limit the CO_2 fertilization effect, but this option was not activated in these simulations.

- Kuppel, S., Peylin, P., Chevallier, F., Bacour, C., Maignan, F., and Richardson, A. D.: Constraining a global ecosystem model with multi-site eddy-covariance data, Biogeosciences, 9, 3757-3776, DOI 10.5194/bg-9-3757-2012, 2012.
- Sellers, P. J., Bounoua, L., Collatz, G. J., Randall, D. A., Dazlich, D. A., Los, S. O., Berry, J. A., Fung, I., Tucker, C. J., Field, C. B., and Jensen, T. G.: Comparison of radiative and physiological effects of doubled atmospheric co2 on climate, Science, 271, 1402-1406, DOI 10.1126/science.271.5254.1402, 1996.
- Bounoua, L., Collatz, G. J., Sellers, P. J., Randall, D. A., Dazlich, D. A., Los, S. O., Berry, J. A., Fung, I., Tucker, C. J., Field, C. B., and Jensen, T. G.: Interactions between vegetation and climate: Radiative and physiological effects of doubled atmospheric co2, Journal of Climate, 12, 309-324, Doi 10.1175/1520-0442(1999)012<0309:Ibvacr>2.0.Co;2, 1999.
- Bounoua, L., Hall, F. G., Sellers, P. J., Kumar, A., Collatz, G. J., Tucker, C. J., and Imhoff, M. L.: Quantifying the negative feedback of vegetation to greenhouse warming: A modeling approach, Geophysical Research Letters, 37, Artn L23701, Doi 10.1029/2010gl045338, 2010.

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.

GPP is overestimated at the regional scale largely for BONA, slightly for BOEU and is correct for BOAS (see Fig. 15), but GPP is underestimated over the Campioli sites (see Fig. 17), where the CUE is computed. We can thus infer that the Campioli database is not a representative sample of our domain, however this database offers the advantage of having collocated GPP and NPP observations, and thus access to realistic CUE estimates.
Nevertheless the reviewer is right, as we use the nutrients argument in opposite manners to explain a too low NPP and a too high CUE over the Campioli sites. We thus removed the following sentences in section 8.2.2 on Page 27 Line 6: "... or due to a lack of N-deposition combined with soil fertility effects in modeled NPP" and on Page 27 Lines 18-19 "This high CUE bias can be expected, as ORCHIDEE-MICT omits the effects of low nutrient availability on CUE (Vicca et al. 2012)". We now just stick to our water stress hypothesis, to explain these low NPP and GPP, resulting in a high CUE.

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.

Yes. We added these sentences in the text:

- at Line 27 P.28: "...(ii) insufficient snow insulation of soils (See Fig. 7); and (iii) the lack of the carbon cycle of mosses and lichens which could have respiration under winter low temperatures (Atanasiu, 1971)."
- at Lines 29-31 p.28: "...improve the snow insulating, prescribe an organic layer of insulating topsoil (e.g. mosses, O-horizons observed in boreal forests, see O'Donnell et al., 2011) into the thermal module, or explicitly represent the moss/lichen plants including their carbon cycle and physical effects (Porada et al., 2016; Druel et al., 2017)."

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?

The higher biomass by CRUNCEP than by GSWP3 could be partly explained by the higher GPP (Fig. 15). Indeed, precipitation in CRUNCEP is lower than in GSWP3; however, the specific air humidity in CRUNCEP during summer is higher than in GSWP3 (Fig. S14). A lower air humidity leads to a higher atmospheric vapour pressure deficit (VPD). ORCHIDEE being VPD-dependent, the stomatal conductance of the vegetation decreases to prevent excessive water loss when VPD is high, and consequently reduces photosynthetic rates. To address it, we add the following paragraph on Page 26, below Line 31: "Interestingly, comparing GPP forced by the two climate datasets shows higher values by CRUNCEP than by GSWP3 (Fig. 14), despite a generally lower precipitation in CRUNCEP (Fig. S17). This could be explained by the higher specific air humidity during summer in CRUNCEP than in GSWP3 (Fig. S14). A low air humidity increases the atmospheric vapor pressure deficit (VPD) and the leaf to air vapour pressure difference; plants then partially close the stomata to constrain a potentially fast transpiration (Oren et al., 1999; McAdam et al., 2015), which leads to reduced photosynthetic rate. The photosynthesis module in ORCHIDEE largely follows Yin and Struik (2009) in which stomata conductance decreases with an increasing VPD, thus is able to simulate a lower GPP under dry air conditions. A recent study (Novick et al., 2016) showed that, between the two factors that impact plant water stress, i.e. soil moisture supply and atmospheric demand for water (reflected by VPD), the latter limits evapotranspiration to a greater extent than the former in relatively wet forested ecosystems. In spite of its importance, the effect of VPD on vegetation productivity has been far less studied than soil water availability (Konings et al., 2017), warranting further investigations in both observations and land surface models."

Apart from a higher GPP, the higher biomass by CRUNCEP could also be because of the allocation scheme in ORCHIDEE. As detailed in Krinner et al., (2005), if LAI is above a PFT-specific maximum value (about 4 for boreal tree PFTs), carbon will not be allocated to leaves but to the sapwood which slowly converts to heartwood. Therefore, a higher LAI simulated by CRUNCEP leads to more carbon allocation to the wood for some areas, the turnover time of which is much longer than leaves. To address it, the paragraph on Page 29, Lines 13-14 is revised as: "Both model output and observation data are subject to the spatial uncertainties introduced by the use of satellite-derived land cover maps. We thus used the forest cover map as prescribed in the model for both data sets, and calculated latitudinal averages to compare with model results (Fig. S8). GSWP3-forced model output agrees well with observations averaged over the whole study region, while CRUNCEP-forced output overestimates biomass at all latitudes (Fig. S8a). For the sub-regions, the overestimation of biomass in BONA is consistent with that of GPP (Fig. 14), while biomass is more overestimated in BOEU compared with GPP, probably because of the lack of forest management and forest age structure for Europe. Comparing the two model results, CRUNCEP-forced biomass is much higher than GSWP3-forced biomass, which cannot solely be explained by the higher GPP by CRUNCEP (Fig. 14), indicating a bias in the allocation scheme in ORCHIDEE. As detailed in Krinner et al., (2005), the photosynthates are partitioned into leaves, roots, sapwood, and carbohydrate reserve, dependent on soil moisture etc. If LAI is above a PFT-specific maximum value, carbon will not be allocated to leaves but to the sapwood, which slowly converts to heartwood. Therefore, a higher LAI forced by CRUNCEP leads to more carbon allocation to the wood for some areas, the turnover time of which is much longer than leaves. A new allocation scheme based on the pipe model was implemented in another branch of ORCHIDEE (Naudts et al., 2015), which provides a physiologically meaningful relationship between foliage, roots, and wood. This would be incorporated in ORCHIDEE-MICT in the future developments. Simulated total forest biomass for the whole domain is 95 PgC under GSWP3 forcing (165 PgC under CRUNCEP), close to estimates from forest inventory data in Pan et al. (2011) of 92.1 PgC. Somewhat lower estimates are derived by Avitabile et al. (2016) and Thurner et al. (2014), of 73 and 84 PgC, respectively."

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

Yes, you are right. This is now corrected in the text: "ORCHIDEE-MICT performs generally well for biomass with GSWP3 forcing, including the latitudinal profile, ..."

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?

Actually, we take into account the cryoturbation effect in ORCHIDEE-MICT using a diffusion term (see Eq. 2 in Section 4.1).

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

This is added now in the title of the section of supplementary Figures and in the caption of Figure S9