Interactive comment on “Evaluating the performance of coupled snow-soil models in SURFEXv8 to simulate the permafrost thermal regime at a high Arctic site” by Mathieu Barrere et al.

-Reply to Anonymous Referee #2

Review of "Evaluating the performance of coupled snow-soil models in SURFEXv8 to simulate the permafrost thermal regime at a high Arctic site" by M. Barrere et al.

The authors present an evaluation of the ability of different combinations of snow and soil developments within the SURFEX model platform, to simulate the snowpack and soil thermal dynamics at a high Arctic site. For that purpose, they collected very detailed and well-thought observations related to both snow and soil. These data are mostly used to validate the snow and soil schemes. Such observations are very rare in Arctic environments, and of great value for model validation. The paper mainly diagnoses forces and weaknesses in the current modelling of snow and soil by schemes of different complexity, one of which is to be run within the CMIP6-experiment. This diagnostic is important for climate modellers in the context of permafrost-carbon climate feedback.

Thank you for these positive comments and for giving us suggestions to improve our paper. Our responses are embedded in blue italics.

However, first, there are a few shortcomings that undermine the scientific quality of the paper :

* Important literature related to the topic of snow and permafrost is missing. Before the present study, Langer et al. 2013 performed extensive sensitivity tests to assess the critical snow and soil parameters for the soil thermal modelling at a high Arctic permafrost site. Loewe et al. 2013 provided the first 2nd-order bounds for snow thermal conductivity, confirming the importance of anisotropy besides density for the estimation of the Ksnow tensor.

Thank you for these references. They have been included in our paper (Langer et al., 2013 on p16, l1 and p17, l18; Löwe et al., 2013 on p12, l16 and p18, l18).

* Together with Referee#1, I noticed that some hypotheses about the origin of biases assessed by the paper, are presented as facts and not hypotheses. Examples of that are : - the early melt erroneously simulated in May 2015 and supposedly caused by lacking zenithal angle dependency in the albedo parametrization (how about missparamatrized turbulent fluxes ? local temperature and wind conditions differences with respect to ERA-i ? ...) - the accelerated melt-out induced by albedo parametrization [p15 lines 24-25 : "Ultimately, the treatment of snow albedo in models is such that melt-out is accelerated, so that soil thawing is greatly accelerated in spring"] What is the exact parametrization set responsible for this acceleration, how does it differ from reality ?

Further investigations have been conducted using the radiative transfer scheme TARTES implemented in Crocus, resulting in improved simulated melting episodes. This is why we suggest that the early melt is caused by the too simple albedo parameterization, and in particular by the absence of solar zenith angle dependency. We chose not to use TARTES because, as stated on page 8 line 23: “[…]the lack of data on snow impurities (nature, deposition, light-absorbing spectroscopy) prevented us from using TARTES in Bylot Island simulations”, as it led to other biases, the discussion of which are beyond the scope of this paper. Also, as stated in our response to Reviewer 1, models rely on error optimizations *so that it may be difficult to attribute a given model shortcoming to a single cause. Reasonable suggestion can however be made. In any case, we changed the text as follows:*

(P11, L8): "We did not observe any signs of spring melt before 18 May during the field campaign. To test whether the inexact melt onset date simulated was caused by the lack of solar zenith angle consideration, we briefly tested Crocus coupled to TARTES, which includes treatment of SZA. With TARTES, the melt onset date was accurately simulated, which leads us to suggest that not accounting for SZA is the main cause of the inadequate melt onset date simulation. However, a full implementation of TARTES in Crocus for this study would have required data on snow impurities."

(P15, L14): "Ultimately, the treatment of snow albedo in models is such that melt-out is enhanced under high latitudes, so that soil thawing may be greatly accelerated in spring."

(P16, L23): "Further, downwelling shortwave absorption is lower in the Arctic because of the large zenith angle in late winter, and this is not accounted for in the original version of Crocus. Therefore, solar warming of the snowpack is exaggerated, resulting in incorrectly simulated melting episodes."

-the possible impact of vapour flux on the snowpack density could maybe be assessed by simple estimations (order of magnitude)

We added on page 16, line 17: "Domine et al. (2016a) estimated vertical water vapor fluxes in the snowpack, and came up with a mass loss of the basal layer of 2.6 kg m^{-2} over two months. It corresponds to a density decrease from 300 to 200 kg m^{-3} for a 3 cm-thick layer, in line with the model overestimation of the basal layer density. Although this estimation is approximate, it supports the suggestion that the vertical water vapor flux is the main cause of the model misrepresentation of the density profile."

* Some statements lack the scientific accuracy that should be expected : - a linear regression is used to make ERAi data consistent with original field data; such a regression usually relies on the ordinary least-square method which provides an unbiased estimate. Hence, the statement (p6 line1) that "The correction led to reduce these respective biases by 20%, 3.3% and 10%." is annoying. I believe that the authors meant a reduction in the standard deviation between the corrected ERAi and the observational data.

Indeed, it was corrected.

- the explanation of the experimental noise in the ksoil data is vague : is the method really appropriate for frozen soils ? Even though the reader is referred to Domine et al. 2016, a brief assessment of the uncertainty and reliability of the NP method in frozen soils would be welcome here.

We added on page 5 line 1: "Our instrumental methods impose to use the same heating power in the NPs for snow and soil. The heating power was optimized for snow to minimize heating and hence perturbation to metamorphism. Since soils have a higher thermal conductivity than snow, especially when frozen, the thermal signal of the NPs is low and noise for frozen soil thermal conductivity data is higher than what could be achieved using a higher heating power."

And restated (P14, L5): "As detailed above, the noise in the frozen soil data is due to the low power used to heat the NPs."

A second major point is that the description of the models and in-depth analyses, very much differ between the snow sections (where effects are well described and analysed), and the soil sections (where model description and analysis of phenomena are at times missing). Examples of that are (non exhaustively) listed below : The way organic carbon is included in the soil profile is not clearly described. In brief words, what are the thermal and hydrological consequences of organic carbon in the soils ? What are the thermal properties of the top organic layer featuring litter in ISBA ?

The parameterization of SOC in ISBA is fully described in Decharme et al., 2016. Soil thermal and hydraulic properties are a combination of organic properties with the standard mineral soil properties. Including SOC affects the vertical profile of soil hydraulic (water retention, matric potential and hydraulic conductivity at saturation) and thermal (thermal conductivity, heat capacity) properties, depending on the organic content in each soil layer (see Fig. 2 in Decharme et al., 2016). We added on page 7, line 9: "Briefly, depending on its content which decreases sharply with soil depth, including organic carbon reduces the dry soil thermal conductivity, increases its porosity and therefore its saturated hydraulic conductivity."

About the surface litter, ISBA assigns organic matter thermal properties to uppermost soil layers (P7, L1): "The thermal conductivity of organic matter is set to 0.05 W m^{-1} K^{-1} when dry, and to 0.25 W m^{-1} K^{-1} when wet."

Figure 9 shows differences in soil thermal conductivity and water content profiles before and after organic carbon additions (run litter, SOC and following).

Model parameter values (thermal conductivity of the mineral soil, of organic matter, of litter...) should be added to help the reader assess the relevance of the model experiments performed and the added value of the 'increased sophistication'.

Details and values were added on page 6 line 28: "The soil thermal properties, i.e. thermal conductivity and heat capacity, are computed as a combination of water, ice and soil properties, volumetric water content and soil porosity, following the parameterizations of Peters-Lidard et al. (1998). Hence, the soil thermal conductivity is expressed as a function of its saturation, porosity, quartz content, dry soil conductivity and phase of water (frozen or unfrozen), where the ice, water and quartz thermal conductivities are respectively 2.2, 0.57 and 7.7 W m^{-1} K^{-1}."

In the current context, having very fine metrics (Tables 1 and 2) to assess the increase in performance gained by added complexity in the snow and soil schemes, is almost disproportionate.

Indeed, the accuracy shown in Tables 2 and 3 (10^{-2}) could be judged as excessive regarding the current context. However, it helps to distinguish differences between experiments, to identify important processes involved when simulating the permafrost thermal regime.

As a result of poor description of the base model and its enhancements with respect to soil processes (litter, SOC), the Results and Discussion sections dedicated to soil are not always informative and omit (or too briefly go through) important potential causes of model errors : i) model parametrization ii) summer soil water content and its impact on the duration of the zero curtain iii) possible impact of the too early snow onset in 2013 on the soil cooling. A thorough improvement of these sections would considerably benefit the paper.

We tried to improve the description of soil model parameterizations, as you suggested. Concerning the impact of summer soil water content on the duration of the zero curtain, we added details on page 15 line 24: "The main difference with observation is the timing of freezing and thawing. This is clearly influenced by temperature, so that the errors in simulated temperature impact this timing. However, the duration of latent heat exchanges are also determined by the water content." Then on page 15 line 32: "[…] it seems that the simulation of the water dynamics in the first cm of the soil is erroneous, and is better reproduced below -10 cm. The low simulated VWC values therefore partly explain the too fast simulated soil freezing. However, as discussed by Langer et al., (2013), the inadequate thermal properties of the snow cover is most likely the main source of error in the ground thermal regime."

About the early snow onset in 2013, it accelerates soil cooling because of the simulated high basal k_{snow} values (P15, L9): "This may be attributed to the thermal conductivity of the basal snow layer which is too high in simulations, allowing the soil to cool rapidly."

Finally, the manuscript is at times redundant and very precise regarding numeric values, whereas explanations and discussions of the processes could be more developed: A better balance would increase the quality of the paper. I recommend publication after the mentioned issues have been addressed.

Specific Comments :

* about redundancies : - Whole manuscript : it is maybe not necessary to compare several detailed density or snow values from observations and simulations when the Figures obviously support the fact that the profiles severely differ. This of course belongs the author's choice but it results in an abundance of precision that tend to hide the key message conveyed. - lines 4-12 p 13 exhibit several redundancy and should be reformulated for more synthetic clarity

Done.

- Lines 6-8 p14 do not add new information w.r.t. preceding text and could be suppressed.

Done.

* others : p6 lines 8-10 : which model experiment is compared to snow observations to infer the precipitation correction ?

Any Crocus experiment can be used to infer the precipitation correction, as they are very slight differences in snow height (see Fig. 7). Added on p6 l.6: "we arbitrarily changed the ERAi precipitation data for Crocus to match the observed snow height at snow pits dug in the immediate vicinity of BylSta"

p9 sect 5.3.1. It should be stated that all experiments except ES are run with the snow model Crocus. I also agree with referee#1 regarding the presentation of these "iterative" experiments. Mind the fact that Fig 4 and 6 have 'Crocus 'instead of 'wind' in their legend (I guess).

Done. Fig. 4 and 6 corrected.

p10 line 1 : model-> models. p10 eq 5 and 6 : 'sim' do not appear (probably an edition problem)

Corrected.

p 11 line 14 : the mentioned bottom DH density range [150-200] is not supported by Fig 3 where 2 among 8 densities measured in DH are clearly above 200 kg/m3.

The given density range [150-200 kg m^{-3}] reflects the very bottom snow layer (or basal layer) only, the layer at the interface with the ground. On the 2 profiles shown it is about 5 cm-thick. The 2 densities measured above 200 kg m^{-3} were taken in the depth hoar layer situated above the basal indurated depth hoar layer.

Fig 5: Crocus and ES thermal parametrizations are illustrated here with the same symbol. However, they do not represent the same physical property : while Crocus' ksnow solely accounts for conductive processes, ES-ksnow includes the thermal effects of latent heat fluxes within the snowpack, which makes it an 'apparent' thermal conductivity. My non-expect view on the use of "apparent" vs "effective" for ksnow is the following: "effective" qualifies the 'representative' property of an heterogeneous medium (ref : Gomez-Munoz et al., 2008) while "apparent" qualifies the fact that thermal diffusion through non-conductive processes can sometimes be accounted for with the same law as Fourier diffusion, making it possible to include the relevant diffusion coefficient into the Fourier conductivity. Please correct me on that if I am wrong. Otherwise, I think this key difference in the conductivities of Crocus and ES should be better highlighted on Fig 5 by the use of different symbols and/or in the caption.

Yes indeed, ES calculates an apparent thermal conductivity. We followed your suggestion and changed the symbol for ES. We also added the following sentence in the caption: "Crocus computes k_{snow} from the density only following Yen's parameterization, while ES includes the thermal effects of latent heat fluxes within the snowpack."

References:

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