Geoscientific

Model Development

Discussions

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comment

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.

Anonymous Referee #2

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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 diag-

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[Discussion paper]

noses 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.

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.

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

* 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 re-

spective 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. - 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.

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 ? 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'. 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. 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.

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

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

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).

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

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.

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.

References: Gómez-Muñoz, J. L., & Bravo-Castillero, J. (2008). Calculation of effective conductivity of 2D and 3D composite materials with anisotropic constituents and different inclusion shapes in Mathematica. Computer Physics Communications, 179(4), 275-287.

Langer, M., Westermann, S., Heikenfeld, M., Dorn, W., & Boike, J. (2013). Satellite-based modeling of permafrost temperatures in a tundra lowland landscape. Remote Sensing of Environment, 135, 12-24.

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