

The authors examine the large-scale effects of a bias affecting the subcanopy LW radiation simulation in the CLM4.5 coupled model. One of these effects is the lengthening of the snow cover duration in the most of the Northern Hemisphere boreal forested regions, which is related to a severe night-time negative bias in subcanopy LW that dominates during the winter period prior to equinox. The authors show that partial correction of this bias, that intends to reproduce the behavior of a two-layer canopy model, enhances the energy delivered to the subcanopy snowpack and therefore also enhances snowmelt.

The paper is rather well written, and tackles an issue of clear significance for global climate modelling.

However, in my opinion, it suffers from two weaknesses, that currently undermines its potential impact in the climate community :

1.

Technically, the demonstration is not thoroughly made, that single-layer canopy formulations generate melt delay in the NH with respect to « real world » (that here would be observations). Indeed, the demonstration of this effect is only made with respect to an approximation of a 2-layer model, that may itself be heavily biased. This is all the more worrying as Todt et al., 2018, illustrate an increase in a subcanopy LW positive bias upon the use of a 2-layer model at the Seehornwald conifer site (Todt et al., 2018, Figure 5). The melt delay associated with 1-layer canopy models claimed by the authors, may clearly be real, but the demonstration should be improved, for instance by confrontation of simulation results to (i) in-situ data at field sites and (ii) satellite estimate of NH snow disappearance dates. Comparison of simulation results to observed snow-cover fractions is briefly mentioned in the Discussion while it should be an important part of the Result section (as it is already quite well advertised as a rationale for this study in the Introduction). Thackeray et al., 2015 (their Figure 3 for instance) provides a good baseline for such comparisons.

2.

Secondly, for the evaluation of the delay effect against in-situ data, simulation errors coming from the meteorological forcing should be minimized. Evergreen forest sites used in Todt et al., 2018, provide appropriate observed meteorological data. They could be used in the place of erroneous large-scale meteorological forcing data (for e.g. Figure 4 and the associated results analysis).

Additionally, please find below some other, more minor comments on this work :

p1L10 : the last sentence of the abstract associates « boreal forests » and « warm winters » where « snowmelt occurs early ». This is not very intuitive

p2L3 : cite Krinner et al., 2018

p3L9-12 : « emissivity » is inappropriately used. What the authors call « emissivity », is an unexplained combination of emissivity and sky view factor. Please explicit and justify the approximations that you make here.

p3L9-12 : please explain briefly how T_v is calculated.

P6L11-13 : In the current structure of the paper, calculating and exhibiting correction factors for deciduous forest regions makes in my opinion little sense, as no use is made of them, and very little analysis of their difference w/r to coefficients calculated for evergreen forests is made. If Cherskii's coefficients are similar to those from evergreen sites, then what is the added value of including this site in the calibration intended for evergreen sites ?! To me, it just undermines the calibration approach.

Calculating correction coefficients for deciduous forests stands may be interesting, though, providing their difference (w/r evergreen sites) and impact (on e.g. snowmelt) is discussed in the paper.

p6L20 : please add 'CLM4.5' before 'grid cell' for more clarity

P6paragraph4.1 : I suggest to carry this analysis using an observed forcing to evidence the effect of going from 1-layer to 2-layer w/r to observations in an « ideal » case. Also, it should be mentioned

more clearly in the text that the selected period corresponds to the snowmelt season at Alptal and hence is relevant to assess the effect of subcanopy LW on snowmelt.

P7Paragraph4.2 : Maybe other forcings than CRUNCEP, exhibit less bias with respect to spatio-temporal in sky emissivity. Brun et al., 2013, concluded that ERA-i generally leads to improved simulations (w/r to other forcings) over large areas of the N high latitudes.

P8L3 : I suggest to outline the regions with $\text{frac_PFT} > 0.5$ on these maps, for better understanding of the effects of CORR and their magnitude.

P8L22: maybe the glaciated areas should be masked out as they are not the focus of this study.

Otherwise, the question arises as to whether cold content for these areas refers to the snow, or to the whole snow+ice columns.

P8L30 : this is not true to my understanding as melt out date is largely determined by the energy required to melt the snow (which is often higher than the one needed to raise snow temperature to 0°C)

P9L1 : maybe the delay between melt-out-date, and equinox, could be an interesting additional explanatory variable in Fig8. Also, an illustration of daily cycle changes before and after equinox for Southeastern regions would be a great complement to the explanations.

P9L15 : specify « over the study region »

P9L30 : Illustrations of comparisons to observations like in Thackeray et al. 2014, 2015, is exactly what is missing in your study, and would add great value to it.

Overall, there is a lack of proper illustration of the competing effects of CORR on the daily subcanopy LW radiation before vs after the equinox, and how this governs the effects of CORR at the global scale.

References

Brun, E., Vionnet, V., Boone, A., Decharme, B., Peings, Y., Valette, R., ... & Morin, S. (2013). Simulation of northern eurasian local snow depth, mass, and density using a detailed snowpack model and meteorological reanalyses. *Journal of Hydrometeorology*, 14(1), 203-219.

Krinner, G., Derksen, C., Essery, R., Flanner, M., Hagemann, S., Clark, M., ... & Ménard, C. B. (2018). ESM-SnowMIP: assessing snow models and quantifying snow-related climate feedbacks. *Geoscientific Model Development*, 11, 5027-5049