

The Cryosphere Discuss., referee comment RC2
<https://doi.org/10.5194/tc-2021-313-RC2>, 2021
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Comment on tc-2021-313 by referee#2

Anonymous Referee #2

Referee comment on "Impact of measured and simulated tundra snowpack properties on heat transfer" by Victoria R. Dutch et al., The Cryosphere Discuss.,
<https://doi.org/10.5194/tc-2021-313-RC2>, 2021

Review of Dutch et al. "Impact of measured and simulated tundra snowpack properties on heat transfer"

The paper presents an evaluation of CLM v5 1D "point" snow simulation at an Arctic site, Trail Valley Creek, where intensive snow observations have been carried out for 3 periods over the 2017-2018 and 2018-2019 winters, while in-situ observations provide an adequate forcing for the model. A novelty lies in the use of SMP profiles to infer snow density and then conductivity, allowing for a much higher number of observations than traditional snowpit density measurements. The evaluation focuses on snow characteristics and the (un)ability of the model to reproduce key features of the Arctic snowpack on the one side, and on the ground thermal regime on the other side, whereby the Snow Heat Transfer Metric (Slater et al., 2017) is used. A bias correction is proposed to help CLM better capture the insulating properties of the snowpack.

The paper addresses a very relevant topic to assess the role of snow and the impact of climate change and on Arctic ecosystems and the cryosphere, and is well in the scope of The Cryosphere. The methods used are scientifically sound and the results'analysis is relevant. However I have some general comments/questions regarding the methodological choices :

**** General Comments ****

- the CLM snow module seems from scratch inappropriate to simulate the properties of the Arctic snowpack. This finding is clearly not new ; previous studies, notably Barrere et al., 2017, and others well cited by the authors, have highlighted the deficiencies of this kind of

models in the context of Arctic snow. In the present version of the manuscript, the description of the snow module of CLM is so light, that it is hard to capture the key features and functioning of the model. Typically, are the modifications Van Kampenhout et al., 2017, used in the present study ? It is not clear to the reader. As the CLM snow model is at the core of this study, a more enhanced description of the functioning of this model is required in the Methods of the paper : presently the line "Each layer is parameterised using layer thickness, temperature and mass of water and ice, as per Anderson (1976), Dai and Zeng (1997) and Jordan (1991)." (L37 p4) is much too vague as each of these publications contain different variants of constitutive equations. In the present form, it is not possible for the reader to understand which one is used for which process/variable. The constitutive equations for the evolution of layer thickness (hence compaction) and ice/water content or density should be explicitated or at least explicitly referenced.

- CLM relies on the parametrization of snow thermal conductivity by Jordan, 1991, as recalled in eq 2. However, the authors chose to derive their conductivity profiles from observations using the Calonne and the Sturm parameterization, and not the one by Jordan, which would have allowed a much more direct comparison to CLM results (eg Fig 6). How do you justify this choice, how does the Jordan, 1991 parameterization compare to the others ? Is the effective thermal conductivity mentioned by Jordan et al., 1991 (p18) and accounting for heat transport through conduction and vapor diffusion, effectively used in CLM (and not just the k_{snow}) ? This should be explicitated. The following is more a minor comment : please also beware of the use of the term "effective conductivity". Following Calonne et al., 2011 and in a material science perspective, effective conductivity refers to the conductivity of the ice-air-liquid water mixture that constitutes the snow material, while individual components like ice or air, have just a thermal conductivity. In other studies, and often in land-surface modelling as in Jordan et al., 1991, "effective conductivity" refers to the additional inclusion of latent-heat exchange within the conductivity used in the Fourier heat transfer equation. So the use of Keff L149 p4 is appropriate, while its uses L151 p4 are not.

_ As a general question, is there any perspective to generalize the bias correction factor designed here, and have in the future global CLM runs using this usefull correction ? This would strongly enhance the impact of the work carried out in this study.

**** Minor comments ****

Please find also below a list of more minor comments which I hope will improve the manuscript :

P3 L110-113 : the way it is formulated gives the impression that the snowpack always entailed

the 4 distinct layers mentioned. I imagined that in practise, more complex layerings were sometimes encountered. Maybe just reformulating saying "Stratigraphic information profiled in each snowpit (n = 115) was used to assign layer types to the measured densities (Fierz et al., 2009), among four different layer types : surface snow, wind slab, indurated hoar and depth hoar. This was made to assess spatial variability in the thickness and properties of different snowpack layers."

P3 L112-119: It is hard to understand the nature and impact of these adjustment without being quite familiar with CLM. I would suggest, if this PTCLM version is not published/referenced anywhere else, to just mention "adjustements" here and maybe detail them a bit more explicitly in an Appendix, so that the curious reader may dig full, explicit information from there.

P4 L126 : was a debiasing of ERA5 or adaptation to the local conditions maybe required ?(see : <https://doi.org/10.5194/tc-2021-255>)

P5 L 171 : "slower rate of soil freezing" : a delay is visible, but not so clearly a slower rate. Could you justify this more with the observations ?

P5 L176-178 : "anomalously warm mid-winter air temperatures... had little influence on the soil temperature profile" : the March 2019 warming is visible though on the soil temperatures, isn't it ?

P6 L 211: "Temperature-gradient" metamorphism should be specified here (as wet-snow metamorphism would have different consequences !).

P6, paragraph 3.2 : I think that a line on the increase in density and effective thermal conductivity towards the bottom of the snowpack should be added, to complete the description of the profiles. I noted that possible reasons for this are given in the Discussion, and it could be referred to here.

P6 L237 : "Whilst the absolute number of simulated snow layers is plausible," : these layers have no physical meaning in the model, this is well explained in the methods. Therefore the remark is in my opinion inappropriate, or it should be specified w/r to what this is plausible.

P7 L250: " with the median thermal conductivity using the Calonne approximation still notably lower". The difference between simulations (0.3 Wm⁻¹ K⁻¹) and the median thermal conductivity using the Calonne approximation (0.25 Wm⁻¹ K⁻¹) is not so high,

given the uncertainties attached to thermal conductivity estimations, and their range of variations. I suggest to suppress "notably".

P7 L 264 : what is the "maximum duration of simulated snow cover" ? (the period with continuous snow cover on the model ?)

P7 L 266-272: wouldn't it be easier to say that density was multiplied by a corrective factor α prior to the calculation of K_{eff} ?

P7 L 274 : " between the interquartile range of observed values shown in Table 2" :

for a given snow type or for all ? If for all, the range should be specified as it is not in Table 2 if I am not mistaken.

P7 L 284-286 : It is really not clear to me how this linear regression was performed, could you give more details ? (also regarding the temporal sampling used)

P9 L337 and 361 : "vital" seems a bit strong and out of place in this context

P9 L 346-347: maybe also mention here that the insulating effect of snow somehow saturates after a certain snow height has been reached, implying a stronger sensitivity of soil thermal regime to snow depth in the early winter close to snow onset, when the snow cover is very thin.

P9 L356: Comparing this point simulation with adjusted Keff, to CMIP5 models evaluated globally (if I am not mistaken..) is actually very unfair to the CMIP5 models ! The string differences in setup should be mentioned to balance this statement.

P9 L354-358: in relation to one of my major comments : did you tests other parameterizations than Jordan, 1991, for Keff in CLM, for instance the Sturm ? (I do not know how it positions w/r to the Jordan, but this should be mentioned if likely to also yield improvements).

P9 L363-365: "Larger values of the correction factor are needed to replicate observed soil temperatures later in the winter season, as errors in simulating earlier season snow depth are additive, leading to larger discrepancies for both snow depth and soil temperatures" : Could you specify to which specific Figure or result statement this assessment relate ?

P9 L367 : "This bias compensation between underestimates of snow depth and overestimates of snow thermal conductivity": I had rather say bias compensation between underestimates of snow depth and UNDERestimates of snow thermal conductivity (?) please correct me if I am wrong.

P10 L 380 : " Reducing simulated thermal conductivity by 80 % ($\alpha = 0.3$) produces changes in soil temperatures approximately equivalent to the impact of changing depth hoar fraction from 0 to 60 % (Zhang et al., 1996), suggesting the inclusion of vapour transport in the snowpack is at least equally important as values of snow thermal conductivity in accurately simulating wintertime soil temperatures. ". The sentence is ambiguous and the message hard to understand (not sure I understood properly). The inclusion of vapour transport in the snowpack will change the thermal conductivity of the snowpack by two ways : i) because this will form depth hoar with lower Keff ; ii) because of vapour transport induced heat transport. Anycase the inclusion of vapour transport in the snowpack, means different values for snow thermal conductivity. I think the sentence should be rephrased.

P9 L381-384: Honestly, the explicit inclusion of vapour transport within the snowpack in the snow modules of land surface models is a (very) long way from now. I am unaware of very concrete plans in that direction for CLM, but please correct me if they exist. I am much more confident that physically representative approaches, (like Royer et al., 2021), though not physically explicit, will be first used, and this would be the short to mid term perspective.

P11 L448: Figure A2c does not explicitly show that the relationship between $F_{\text{I}}^{\text{net}}$ and L is not heteroscedastic; it is rather the combination of all figures from this panel (?)

Fig 1 is hard to read, maybe consider having a grid (making date correspondances easier to follow), use background color instead of the color of very small histograms for precipitation phase ; maybe also enhance y-axes sizes and line width. Plus, the caption contains some errors, please check.

Fig 6 : nice figure !

Fig 8 is mentionned in the text before Fig 7, they should be inverted.

Could you thicken the lines a little bit (should be feasible when reducing the y range) - the figure is quite hard to read.

Fig 9 :

please add the 1:1 line on Fig 9a. The caption should tell what the background color of Fig 9b represents (I assume it is the result of eq 4 ?)

**** Edits ****

P3 L95 : measured BY a SR50a ?

P3 L106 : the position of the coma is weird, please check

P4 L124: or less WERE filled (word missing)

P4 L 156 :) missing

P5 L 171 : Alonger

P5 L 200 : please add the number of the Appendix

P6 L 244: ofdepth

P7 L 279 : " $0.55 \leq a \leq 0.3$ " seems the wrong order

P8 L 315 : Required -> required

P10 L 398: show-> shows

P11 L 415 : A point is missing after A1.

P11 L 421: the sentence contains two "is"