Review of the revised version of "Impact of measured and simulated tundra snowpack properties on heat transfer"

General evaluation

These general comments were written after the subsequent specific comments, I apologize for the few repetitions.

This paper in fact reports two somewhat independent topics. The first one is the determination of vertical thermal conductivity profiles of snow near TVC using density profiles determined from SMP profiling. Density is derived from SMP data using an algorithm developed from snow on sea ice, if my understanding is correct. The second one is to compare these thermal conductivity profiles to those simulated by CLM. As expected from any snow model used in the Arctic, CLM does not perform well. It simulates much too cold temperatures and the authors discuss various adjustements to make the model better fit the data.

The experimental part is interesting and there is some novelty to it. There are however a number of weaknesses. First, the thermal conductivity is derived from density using various equations that do not agree with each other. Second, while I am willing to believe that the algorithm used to convert SMP data to density is good, it seems to have been validated for snow on sea ice, which is somewhat different from snow at TVC where vegetation impacts snow properties. Third, in the discussion, the authors mention an artefact where the penetrating SMP rod may have damaged the fragile depth hoar, causing an artefact with lower intermediate density and increased lower density. This third issue is very important and must be mentioned in methods so that the reader has it in mind while evaluating the experimental part. I detail this point below. So, while SMP clearly allows obtaining much more data with high vertical resolution, its limitations and potential artefacts must be stressed, with the possible conclusion that it is not (very) suitable for snowpacks with very fragile basal layers. Or that the wind slab must be removed to measure the depth hoar? In any case, I feel the SMP data deserves publication if the caveats are clearly stated.

Another issue with the derivation of thermal conductivity (TC) profiles from SMP data is the choice of the density-TC relationship. First, the authors must remain aware that this is far from a bijective relationship and that there is enormous scatter in this relationship. For example, in (Sturm et al., 1997) there is sometimes a factor of 5 variation in TC for a given density value. The use of such an equation may therefore bear some implicit very high uncertainty and error. Second, there are large differences between the various relationships used, so which one is correct, if there is one, and what is the error induced by these relationships?

I would like to comment on the various relationships used. The equation of (Jordan, 1991) is based on that of (Yen, 1981) which is in fact a compilation of previous works using a variety of methods. (Sturm et al., 1997) use only the needle probe method, but there are doubts on its reliability (Riche and Schneebeli, 2013) and this is not just due to anisotropy issue as stated by the authors. (Fourteau et al., 2022) have very recently made a detailed theoretical and experimental study of the needle probe method and demonstrated that the aproximate equation used by (Sturm et al., 1997) to derive thermal conductivity from heated needle probe data is not correct, leading to a negative artifact. Briefly, it is not valid for the short heating times used as some neglected terms in the full equations are in fact not negligible. (Fourteau et al., 2022) proposed an algoritm to correct existing needle probe data. In any case, I think the equation of (Sturm et al., 1997) is incorrect, and the fact that it produces values lower that the other parameterizations confirms this negative artefact. I very strongly recommend to stop using this equation. The equation of (Calonne et al., 2011) is based calculations from tomographic images. It is rigorous, however, it minimizes water vapor effects because is postulates slow surface kinetics. In other words the accommodation coefficient of water vapor on ice takes here a low value, so that latent heat fluxes are minimized and probably underestimated. More recently (Fourteau et al., 2021) used a similar approach but postulated fast surface kinetics, leading to slightly higher values than (Calonne et al., 2011). In conclusion, the equation of (Sturm et al., 1997) is incorrect and should just not be used. The equation of (Jordan, 1991) is based on ancient measurements using a variety of poorly documented techniques and I feel it has limited reliability. The equation of (Calonne et al., 2011) is arguably correct but makes the debatable postulate that surface kinetics is slow, while (Fourteau et al., 2021) make the more reasonable (in my opinion, but I am a coauthor of that paper) postulate of fast surface kinetics. My biased recommendation is therefore to use the equation of (Fourteau et al., 2021) and in any case not to use the equation of (Sturm et al., 1997). I recommend not to use different equations as an error compensation trick in CLM.

In fact, I am not too thrilled by the modeling part. Essentially this just says that a simple snow routine from a more complex model just does not work in the Arctic. It is interesting to confirm that the CLM snow scheme is deficient, but I do not think all the error compensation tricks used by the authors, and the lengthy discussion on parameter adjustments, have much interest. Figure 7 shows that for the first year, α =0.4 must be used while for the second year α=0.6 is better. So, the adjustment parameter changes from year to year, and is probably different for other sites, so that CLM has no predictive value when it comes to Arctic snow and ground temperature. We already know, I as well as all the authors, that even the currently most sophisticated snow model do not work in the Arctic, so no one expects the simpler CLM scheme to perform any better. The authors just need to show Figure 7, demonstrating the lack of predictive value of CLM and therefore its much reduced interest for predicting snow properties and soil temperature in the Arctic. They then could just discuss that implementing the missing process, upward vapor transfer, is too complex, and that other approaches they wish to propose must be envisaged. I Therefore think Figures 1, 2, 3, and 7 are interesting. I strongly recommend that the authors consider removing the other Figures, or at least most of them, and reduce the modeling text by at least 50%, probably more. Please also consider my comment of Figure 5 below.

Another general comment is that there is a serious lack of attention to detail in the writing and presentation. This is surprising given that “All authors were involved in reviewing and editing prior to submission” and that the authors include a large number of high-profile esteemed and highly respected senior researchers. For example (just one, and I will not edit for typos, the authors can do it), all of these authors think it is fine to write “Snow has a low thermal conductivity, typically in the range 0.01 – 0.7 Wm^{-2} K^{-1}”. So snow can have a thermal conductivity less than half that of air? And for brevity I will refrain from commenting the 0.7

value. In any case, I will do my best to write a hopefully constructive review. Let the authors do their best to write a paper with attention to detail.

Specific comments

Line 44. I am not sure what the authors mean by “indurated depth hoar”. They seem to have a definition different from mine (Domine et al., 2016b) and from Sturm’s (Sturm et al., 2008). Sturm and I have the same definition, since Sturm introduced me to indurated depth hoar on the Alaska north slope in 2004. I would think the authors would have a similar definition since Chris Derksen has done much Arctic snow field work with Sturm. However their stratigraphy does not show any indurated depth hoar, but faceted crystals. This is very confusing. I would also think the lower layer, with densities reaching 300 kg m^{-3}, would often be actual indurated depth hoar, possibly formed from melt-freeze layers. In any case, I suggest the authors realize that the classification of (Fierz et al., 2009) was made by avalanche experts for avalanche motivations. It is almost exclusively based on observations in Alpine snow and is largely inadapted to Arctic snow. I discussed this with Charles Fierz and he had never seen indurated depth hoar. I think Arctic snow researchers should use symbols adapted to their problem. I have proposed symbols for indurated depth hoar and indurated faceted crystals (stage prior to indurated depth hoar) in (Domine et al., 2016b) and (Domine et al., 2018) (already cited by the authors) and in other papers. Why not popularize these symbols, adapted to Arctic snow, which by the way is much more important area-wise than alpine snow ? It would spare us these inevitable inconsistencies between text and Figures.

Line 150. What is h_{sl}?

Line 185-190 are not necessary. These are very well-known considerations. In general section 3.1 can be greatly condensed.

Figure 2: Faceted crystals? Columnar DH? Please clarify symbols and make them consistent with earlier parts of paper. Plus, again, Fierz 2009 inadapted to Arctic snow. How about showing fall 2018 data? Fall data are in general scarce amnd therefore valuable.

Table 2 lacks detail and does not correspond with stratigraphy of Figure 2. There seems to be a dense basal layer, perhaps indurated depth hoar, in the lower 10-20%. Then the next 20-60% seem to have a homogeneous low density typical of columnar depth hoar. Therefore, these 2 layers should be separated in Table 2. Just having one DH layer is not consistent with data.

Line 214. Please descrive indurated DH. Written as faceted crystals in Figure 2.

Line 221. Ice lenses not shown in stratigraphy.

Line 235. Please check grammar

Line 257. Please be consistent with snow layers. Figure 2c and d mention 5 layers. Here just 3.

Line 265. 0.344 is 4 times as large as 0.08, not 3. Sturm is not an approximation, but an equation, or a parameterization.

Line 268. Most of Sturm's measurements were in fact in the Boreal forest of interior Alaska, where the mostly depth hoar snowpack layers have very low thermal conductivities. Many measurements were also from tundra snow but what the authors say is incorrect.

Line 281. Probably unnecessary explanation.

Figure 5. The graphs may be easier to visualize if the grey and black colors were swapped. In any case, I am not sure about the utility of panels b-d and in fact I am surprised by how CLM seems to work. The origin seems to be the top rather than the base of the snowpack, even though the snowpack forms from the bottom up, as the authors know. Following density from the top then does not really correspond to anything physical, as a given snow layer is not monitored. I do not have time to get into the methods and architecture of CLM, which is totally unknown to me, but it seems very strange. Since it does not seem to take into account actual processes, it seems that having such a model fit data will result in mandatory adjustments on a case by case basis, with no hope of ever having any predictive value. From what little I understand, I therefore wonder whether there is actually any hope of ever getting any reliable snow simulations from CLM. I'll be more than happy to be proven wrong.

Line 293. Why just mention 2018-2019? Is not it interesting to realize that in 2017-2018, α =0.4 works best? A single value cannot simulate both years. And therefore, expectedly, different thermal conductivity parameterizations have to be used for each year. This may be stressed.

Line 296. I do not understand (0.3 ≥3≥ α ≥ 0.5555)

Line 316. Sturm's parameterisation works best, but that parameterization is wrong. This is just an error compensation game. The model is wrong, and this is compensated by a wrong thermal conductivity parameterization. And the compensation is different for each year, meaning that the model has no predictive value.

Lines 326-328. This comparison is very misleading. The authors compare their indirect estimation of thermal conductivity based on an indirect estimation of density to actual measurements of snow thermal conductivity. Furthermore, the measurements of (Domine et al., 2015; Domine et al., 2016b) and of (Morin et al., 2010) are continuous season-long time series, so that the focus of those papers are on time-variations, while the authors'work is on height variations. By the way, vertical profiles of thermal conductivity measured by (Gouttevin et al., 2018) and by (Domine et al., 2012; Domine et al., 2016a) is closer to 5 cm resolution than to 10 cm.

Lines 330-344. This discussion is interesting but I think it should already be stated in the methods section. I have been wondering about this high density basal layer, thinking it may reflect rain-on-snow in the fall but only now do I realise it is probably just an artifact! I have been misled in my understanding of the data all along! So please shift this up. And by the way, why did not the authors perform SMP measurements with the wind slab remove to test for the actual impact of this artefact, since they must have been aware of it while making the measurements.

Lines 345-352. Continuous vertical profiling is indeed very nice and is clearly a significant improvement over discrete layer sampling. However, is SMP suited to Arctic snowpacks, with a basal depth hoar layer that can be extremely fragile and collapse at the slightest touch? (see details in (Domine et al., 2016b). The authors seem aware of this problem, and this clearly limits the interest of SMP for some Arctic snowpacks. Not all, I agree, since very windy areas such as Barrow and polar deserts seldom have very fragile depth hoar. This should also be discussed. Furthermore, SMP is blind sampling, since a snowpit is not dug in most cases as this would cancel the benefit of the technique. Therefore, artefacts due to soft layers would be undetected. In conclusion, while I do see the benefit of SMP in some cases, the authors may wonder whether a low resolution reliable manual density profile is better than a high resolution profile with potential and unverifiable artefacts.

Line 355. Alpine snow does have a density profile as simulated by CLM, but not the taiga snow, which by the way is not discussed by the references cited. In the boreal forest (for some reason, I refrain from using Russian words these days…?) the profile may be as in CLM at the beginning of the season but by the end of winter it is either flat or with lower basal density because of the upward vapor flux. See e.g. (Taillandier et al., 2006).

Line 361-362. “such as the snowpack vapour kinetics necessary to form depth hoar.” The term “vapor kinetics” is unclear. Use flux, and more accurately upward flux.

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