Review Response to Bennoit Montpetit

Dr. Montpetit,

Thank you for your thorough review of your manuscript, specifically regarding the parameterization of the snowpack model. We believe a distinction should be made between small scale field studies and hemispheric scale retrieval algorithms; the latter require substantial generalization in order to be applied broadly, which is the ultimate goal. The proposed algorithm is in a prototype phase to demonstrate its applicability for the estimation of snow density conditions from passive microwave remote sensing. In the future, we plan to build upon the prototype algorithm to better characterize the snowpack model across large areas and facilitate pan-Arctic snow density retrievals. Below are responses to the specific review comments.

1.1 Using weather station data to estimate TB with SMRT

We recognize the limitations of AWS data for capturing spatial variability, specifically with respect to snow depth. However, we do not think reanalysis data would necessarily be appropriate here - Cao et al. (2020) showed that ERA-5 demonstrates high biased snow depth in high latitudes. We limited our input datasets to seasons where the AWS SD measurements were similar to those measured over snow courses (i.e. CanSWE) – so they should be somewhat similar to spatially averaged depth (we can probably state this more clearly in the manuscript). Additionally, we are looking to revise the layer thickness ratio which will reduce the model sensitivity to the AWS snow depth (explained further under 1.6).

Regarding ground temperature, operational AWS do not measure ground temperature in the Arctic. The confidence in ground temperature estimates from reanalysis data is limited during the cold season (Herrington et al., 2024), and ERA-5 exhibits high biased estimates of ground temperature in high latitudes (Cao et al., 2020). The absolute accuracy of ground temperatures is less important when using a frequency difference approach – we believe our parameterization of ground temperatures relative to snow temperature is in the right ballpark to characterize the effect of thermal emission from the snowpack (specifically from the wind slab layer).

In terms of lake fraction, there should be minimal effect for the 3 northern most sites, though it is an important consideration for Cambridge Bay – we will mention this in the study site section and include lake fraction estimates for each site. Given the high latitude of Cambridge Bay, we expect the influence of lake fraction to be minimal later in the season. In the future, we will look into handling lake ice fraction more explicitly.

1.2 Choice of DMRT with non-sticky spheres

Your point about DMRT-QCA is valid and we will look into using IBA based on Meloche et al. (2024). However, we are not convinced that introducing stickiness into the microstructure representation is the best way forward, because it is an unmeasurable property and is essentially a tuning parameter. Roy et al. (2016) used a non-sticky case with a scaling factor (from Roy et al., 2013) to convert from optical to effective grain size. Further, Roy et al. (2013) concluded “even if the stickiness seems to be a pertinent physical explanation, in practice, its introduction poses difficulties because it is not a measurable quantity for natural snow and its optimization is not unequivocal, even in the simplified case that we considered here by using a constant value for the entire snowpack". Vargel et al. (2020) use stickiness to compensate for optical grain size (rather than effective grain size) – whereas we optimize for effective grain size directly removing the need for a stickiness compensation factor.

1.3 Choosing the (Kelly et al., 2003) grain growth to simulate TBs

Sturm et al.'s (1997) kinetic grain growth model is based on empirical observations. While that model estimates physical grain size (rather than effective radius), we argue it should be representative of the relative change in grain radius. We plan to modify the grain size optimization process to focus more on end of season conditions and compare them to values reported in the literature. Further, recent works (e.g. Wooley et al., 2024; Meloche et al., 2022) have shown inter-season variability in grain size (SSA) is low relative to changes in density conditions – therefore, we believe it is appropriate to use an optimized grain growth model to retrieve density parameters.

1.4 Using 2m air temperature to estimate depth hoar layer temperature

We agree that the 2m air temperature is not representative of depth hoar temperature, reflecting an oversimplification of our model, and will modify this parameter. We plan to replace the homogenous snowpack temperature with a linear temperature gradient. However, the temperature of the depth hoar layer will have a very minor impact on the simulated brightness temperatures (i.e. <1K), as emission contribution from that layer is much lower than that of the wind slab layer.

1.5 Using the (Dobson et al., 1985) model to estimate the frozen ground permittivity

Thank you for the suggestion, we will incorporate the findings from Meloche et al (2021). However, the effects of the substrate should be minimal using a frequency difference modelling approach and we do not think it will change the results by a considerable amount.

1.6 Using a static depth hoar ratio to simulate seasonally evolving tundra snowpacks

It is possible that we could incorporate the relationship between snow depth and depth hoar ratio from Meloche et al. (2022), however that work does not consider temporal evolution and is restricted to end of season conditions. Woolley et al. (2024) relies on stratigraphy from snow pits to properly segment their modelled density profiles for evaluation – while we acknowledge it is important to consider variable layer thickness ratios, that paper does not involve the prediction of layer ratios but instead relies on in situ measurements to interpret their modelled results. Currently, there is insufficient data available to properly parameterize the seasonal evolution of tundra snow density profiles, and we mention that is likely the cause for improved density retrievals towards the end of the season (on line 249). Moving forward, we are looking into including a maximum depth hoar thickness for each site (considering site characteristics like vegetation; Domine et al., 2016) , rather than a fixed ratio based on snow depth to reduce the sensitivity to the AWS snow depth measurements.

1.7 Not considering atmospheric contributions to the simulated TBs

We recognize that atmospheric contributions should be considered and reflects an oversimplification of our modeling approach. That being said, we do not think that a complex representation is strictly necessary in this case. You mention GlobSnow's handling of atmospheric corrections, yet the two references you provided do not mention any specifics about atmospheric contributions. Instead, GlobSnow uses an empirical atmospheric model that uses static parameters over space and time. We plan to incorporate the atmospheric model used in GlobSnow, parameterized with AWS data (rather than using a static air temperature of -5C used in GlobSnow).

1.8 Using a "brute-force" method to optimize the cost function

We are unsure of what you mean by "brute-force" in our optimization procedure – it does use assumed parameters but that does not necessarily make it brute-force. On the other hand, the MCMC methods which you referenced could be more accurately characterized as "brute-force" methods, by essentially trying every possible parameter combination. We do not think the MCMC approach is necessary here and would be difficult to apply broadly (which is our ultimate goal) because so many of the required parameters (i.e. distributions of grain sizes) are not well known.

The reviewer commented about the assumptions made in the model and the need to calibrate "many" of those assumptions. We would like to clarify that only the effective grain size parameter was calibrated (along with the H parameter) and the other parameters were forced by AWS observations or informed from the available literature.

1.9 Validating retrieved densities with CanSWE

We address the limitations of the CanSWE dataset in the manuscript (line 303), and we acknowledge that its bulk nature is not ideal to evaluate the two layered model. However, no other dataset covers the necessary spatiotemporal scales to evaluate the model on large scales. That being said, we are in the process of acquiring stratigraphic data for the Eureka site from Derksen et al. (2014) to better evaluate the estimated layer densities.

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