Review for "A Maxwell-Based Dual-Morphology Wet Snow Microstructure Model for Liquid Water Amount Retrieval in Greenland's Percolation Zone Using SMAP L-Band Radiometry" by Hui Jiang et al.

General comments

This study presents a framework for liquid water amout (LWA) retrieval in Greenland using spaceborne L-band radiometry and a dual-morphology implementation of wet snow microstructure. This dual-morphology description of wet snow is the main contribution of this work. It is used to simulate snow microstructures and infer an effective permittivity database with varying liquid water content and morphology, itself used to train an emulator facilitating the retrieval from satellite data. The theoretical framework and the observed strong dependence of wet snow permittivity on the morphology bring an interesting and new perspective to the monitoring of snow wetness from space. However, I have concerns regarding the readability and physical interpretation of the work, which are synthetized in the specific comments below.

My main interrogation is whether the general purpose of the manuscript should be more sensitivity-oriented, including a complete sensitivity investigation of the dual-morphology framework to support future LWA retrieval. First, the proposed framework includes fixed microstructure parameters whose effects are not assessed and barely justified (snow density, thin-coated and Tri-C geometries), which counterbalances the interest of a more physical representation of snow microstructure. The sensitivity of modeled TB and LWC with respect to these parameters should at least be evaluated in depth. Second, the retrieval-oriented perspective lacks robustness. For instance, there is no evidence that this new representation of the wet snow microstructure yields more accurate estimates of the LWA than previous works carried out by the co-authors. The retrieval is furthermore limited by the assumed 2-layer radiative transfer model, and by the uncharacterized sensitivity of SMAP observations to the wet snow thickness for different ranges of LWC and morphology.

Specific comments

1. A subsection dedicated to the presentation of SMAP data is lacking. Right now, the information is spread in the entire methodology, such as the (non-justified) use of morning passes only that appears for the first time in the results section (L465-466).
2. A subsection dedicated to the presentation of firn models used for validation is also lacking. The informations given in the results section make it harder to read and raise questions about the models' parameterizations.
3. The limitations of classical wet snow permittivity formulations, used to introduce the interest of the Bi-C, Tri-C and further dual-morphology representations, are well described in the introduction. However, there is a remainder paragraph in almost every subsection (some examples: section 2.4, L173-176; section 2.5, L206-219; section 2.6, L297-306; section 2.7, L320-331). It is still useful to remind the main novelty of a paper, but this is too much and it really complicates the reading. I suggest some reorganiation of the paper to go with my previous comments, that will also leave space for other technical issues. All the rationale of the study should disappear from the methods and results to be discussed in depth in the introduction and discussion.
4. The fixed snow density of 400 kg/m³ is a strong assumption, not justified (the Hossan 2025 paper that is cited L374 presents a previous version of the LWA retrieval, not measurements or any plausible physical constraint on the density). Of course, ice-sheet scale retrieval is facing too many unknown parameters that need to be fixed to solve the problem. However, given that you are only running simulations at selected PROMICE sites, why don't you use

the measured densities as input? The minimum would be to provide a sensitivity experiment showing the impact of density variations on: i) the effective permittivity, and ii) the retrieved parameters (LWA, d_wet, α).

5. The two-step inversion framework using time series retrieval is well described in the supplementary. However, in the main paper, section 3.3 writing is in my opinion not as clear. I recommend adopting the same structure as the supplementary: i) single-pass estimation of candidates LWA, d_wet, α; ii) time-series optimization with the different regularization terms. It should be clear from the first sentences what the general structure of the retrieval is.
6. What is the sensitivity to the assumed Tri-C and thin-coated geometries on simulated TB and LWC? It might be high, especially for the thin-coated due to the changing "interfacial area-to-volume ratio" (L273). Given that you propose a new microstructure representation, with strong impacts on the retrieval of LWC, this sensitivity must be evaluated in depth. I suggest varying the range of thin-coated and Tri-C thicknesses. I also wonder if the shape of Tri-C inclusions (cubic) can have an impact and if this could be tested with other geometries, or at least discussed.
7. I would like to read more about the performance of the emulator in reproducing the numerical permittivity database, and the selection/parameterization of the model to do so. This is treated as a secondary point with no reason.
8. The d_wet parameter is key to retrieve the total liquid water amount. However, I am not sure that even L-band measurements are enough sensitive to d_wet variations, especially for the upper LWC range close to 6%. In Fig6 and Fig7, I am quite surprised that every PROMICE site reaches the same range of maximum LWC and d_wet values, despite differences in altitude and melt rates. Results from KAN-U look particularly suspect with maximum LWA lower than at other sites, and the LWC<6% assumption does not hold there (slush was observed with subsurface flowing water, see for instance Clerx et al., 2022; https://doi.org/10.5194/tc-16-4379-2022). To understand in depth the capability of the retrieval, I would like to see sensitivity experiments showing the TB response to variations in d_wet across different values of the other parameters (LWC, α).
9. You actually treat the end of the season as active melting – as is represented in the two-layer radiative transfer model by the wet snow layer on top. I disagree with this statement that is repeated several times. The brightness temperature time series in Fig4 and Fig5 show a strong exponential decrease in September in Dye-2 and KAN-U (earlier at other sites), which likely indicates the propagation of the refreezing front downwards. There is here no active melting, only remnant liquid water at depth (more likely to be in Tri-C microstructure), which is not represented by the simple 2-layer assumption. The proposed model will anyway fail at representing realistic end-of-season conditions as the influence of refreezing and upper firn scattering become prominent.
10. Why did you use only one melt season to apply the methodology (2023), and how was it chosen? Longer time series would be much more informative to compare liquid water processes in high and low melt years.
11. All the graphs should have the same y scale (either LWA, d_wet…) to allow for inter-site comparison.
12. The discussion is currently quite poor, and is mostly written as a summary of already known information, before the conclusion itself. It is here necessary to discuss the methodological limitations of the proposed model, especially: i) the unknowns/assumptions in the dual-morphology description (shape and size of water inclusions, snow density), and ii) the limitations of the assumed 2-layer framework, especially at the end of the melting season when the surface has refrozen.

Technical comments

L11: liquid water not only percolates in seasonal snow, also firn, especially in the lower percolation zone.

L11-12: “The retrieval of LWA…”. It would be useful to add a sentence to state that this retrieval emerges as a new field of study, allowed by satellite L-band radiometers, in order to better observe/constrain surface hydrology processes that are not fairly represented by models.

L13-15: “Classical wet snow…”. It would be useful to remind that microstructure controls the permittivity.

L29-32: the first sentence of this introduction is a vague statement, before shifting to PMW retrieval. This is fine in the abstract but physics should be better described here. How does LWA governs retention, refreezing and runoff? How do these terms impact mass balance? What are the “broader climate impacts”?

L33: “over large areas”, and at low resolution.

L37-38: “retrieving subsurface liquid water and total LWA”. You provide only references for total LWA, I suggest adding references about the subsurface liquid water part (e.g. Vandecrux et al., 2026: https://doi.org/10.1016/j.rse.2025.115197

L50: funicular and pendular regimes were not introduced.

L72-74: “*For the Greenland percolation zone, however, the radiometrically relevant regime for L-band retrieval is typically a low-LWC regime, with liquid water volume fractions on the order of about 0 %–6 % (Hossan et al., 2025a)*”. I rather disagree with this statement. Slush has been observed many times in the lower percolation zone, such as the KAN-U region, and contributes actively to runoff. It is definitely not a <6% LWC. My guess is that Hossan 2025 was limited to these values because of: i) site sampling, ii) the impossibility of retrieving high LWC with a simple radiative transfer model because it is too absorbant. This assumption of the model (L87-89) is wrong to me – it is adapted to areas with LWC <6%, which KAN-U is not (see for instance Clerx et al., 2022; https://doi.org/10.5194/tc-16-4379-2022), and results there should be evaluated cautiously.

L120: It seems that the Tri-C medium here slightly differs from the one in Huang et al., 2024, which is further explicited. Is there any risk of confusion between those two representations? Is this new Tri-C a more robust, definitive version to be adopted in the literature?

L134: LWC us → is

L141: the subscript *film* is confusing to me. I guess you refer to the thin-coated fraction. However, the subscript *thin-coated* is used elsewhere, including in the definition of alpha L139. I recommand checking throughout and harmonizing the notation.

L149: vertical water depth → water column?

L151: “Its role is to preserve the total LWC”. I don’t really get the concept here, as both the LWC and alpha vary in the retrieval. I also don’t really understand the difference tou make between “effective morphology parameter” and “observed geometric fraction”.

L155-161: this section 2.3 should be removed as it only repeats concepts already explained. The illustration is useful but could be referenced earlier in the text.

L173-176: this was already stated many times.

L206 to L222: a large part of section 2.5 could be removed as this only (and again) states why the dual-morphology representation is more realistic compared to classical microstructures, which belongs either to introduction (already well described there) or discussion. The only useful part in that section is to describe how Maxwell’s equations are solved, whick lacks details.

L222: … using the software Ansys HFSS?

L228: “The resulting scattering and absorption responses of each realization”. How does FEM computes scattering and absorption? How many realizations are needed?

L255 and L262: all the terms in eq6 and eq7 are not explicited.

L259 to L306: “A useful physical intuition…”. This is interesting of course but it is long and does not read well in this specific section. I would consider moving what is discussion to the discussion section.

L311-312: Not necessary to repeat again this.

L320-322: these two sentences give only one same information, merge them.

L323-331: Always repeating the same information.

L357: In eq8, when the wet snow layer is fully absorbant, it only remains the reflection at the air-snow interface right? This likely occurs for high LWC where there is in this case no sensitivity to d_wet. Also the 2-layer model does not consider the reflection at the wet-dry snow interface, while in fact it becomes reflective in the summer.

L359-360: do you use measured Ts at the surface? If no, at which depth?

L373-374: see my technical comment regarding the snow density assumption.

L380: what is the LWC sampling?

L390-394: this is useless to state, remove. I don’t see how SMAP TB could be anyway used as a validation dataset, even without this regularization. First, it is the only observation that your retrieval is based on. Second, the retrieved geophysical quantity is LWC, not TB. The good match between the shapes of seasonal TB and LWC variations is just logical – and the consequence of the retrieval algorithm.

L424-426: there is not only dry or active melt periods. In the end of the season, there is surface refreezing with liquid water buried in the firn (see for instance Vandecrux et al., 2026: https://doi.org/10.1016/j.rse.2025.115197). Your model cannot reproduce realistically this period, where the refreezing firn scatters a lot the signal from the underlying wet layers.

Fig4: what is the cause of TB spikes in SDM time series? You should use the same TB scale across the subplots.

L436-445: these two paragraphs duplicate section 2.7.

L456: “independent model based references” which were never described before the results.

L465-466: Maybe I missed something but this is the first time where you state that only morning passes are used. This is i) too late, and ii) to be justified.

Fig5. I’m surprised the model is capable of reproducing the observed spikes in SDM which look like noise. Why?

L482-483: “Because a weak version…”: useless, remove.

Fig6: this would be easier to visualize with the same scale. I’m surprised by the low LWA retrieved in KAN-U (lower peak than at any other site), although it is where the most melt is recorded. I suspect this can be due to the retrieval’s limitations, including the maximum of 6% LWC allowed, and the uncertain sensitivity to d_wet. More general, it is suspect to me that the maximum seasonal LWC is close across all sites, and that only the duration of the melting season reflects the temperature gradient.

L493: this is not “sustained strong melt conditions” but remnant liquid water buried in the firn in September/October. This remnant liquid water information could be extracted from the firn models when the top layer becomes dry.

L499: Heilig et al., 2018 (https://doi.org/10.5194/tc-12-1851-2018) published a ground truth dataset at Dye-2 that could be used.

L500-502: this is a vague statement that should be precise. A dedicated subsection to present the model's parameterizations would help for that.

L505: so you perform "validation", then you state that the firn models are not consistent references, and because SMAP TB looks like SMAP retrieved LWC, this is ok? I think these comparisons should feed a high-level discussion section.

L508: "d_wet is not compared here with an independent reference". Why? You could use thermisor string data that are available at PROMICE sites to infer the depth of liquid water. It would be a strong confirmation if there was a good match between these reference data and your retrieved d_wet.

L513: drainage is unlikely in the percolation zone, except in the very lower and saturated part where subsurface water flow was observed.

L513-514 and Fig7: I'm surprised here also by the very similar range of wet snow depth reached across all the sites.

L527-531: to me this is evident and could be deleted.

L539: "This interpretation is supported by additional wet-snow studies.". This should typically feed the discussion.

L546: thin-coated amount or volumetric fraction?

L555: "the larger" repeated twice

L570-571: about Forster et al., 2014. This is not the same location, firn aquifers were identified primarily in Southeast Greenland. Furthermore, liquid water retention in firn aquifers is meters to tens of meters deep, while here we just have a shallow gradual refreezing of the firn. I would rather cite references that evidenced this gradual refreezing.

L571-573: this seems the logical explanation. However, I don't believe to the superposition of two effects, as this is a clear refreezing phase where there should be no active melt. I'm afraid you should have to consider this as an end-of-season artefact due to your model unability to represent the refreezing.

L585: Waer → Water

L586-592: there is not any new information to discuss in this paragraph, it is a summary.

Table 2: Late Season "Small" → refreezing = None

L607-609: yes but what do you propose to deal with the strongly underdetermined inversion problem with multi-layered models?

L610: is there any opportunity/methodology/plans to collect such validation data representing the wet snow microstructure?

Fig9: the thin-coated fraction in Dye-2, KAN-U and CP1 has spikes in a few passes, otherwise it is on the same scale as other sites. This looks a bit like artefacts in the time series.

L617: dual wet snow → morphology

L627, "reproduces…" I still don't get the interest of this obvious validation-like statement, especially in the conclusion.

L631-632: there is no demonstration of this point as no comparison is performed with any other traditional wet snow microstructure-based retrieval. Hossan et al., 2025 also found good matches at these 6 sites. We have a methodological improvement but how do we know if this valuable in terms of melt description?