General Comments

This paper introduces a novel approach for determining the water retention curve of snow with simulations of uCT tomography images. The paper also calculates the effect of liquid water on several effective transport properties. Overall, the scientific approach is novel, interesting, and addresses a significant knowledge gap. Specifically, there are few existing data on the hysteresis of the water retention curve of snow, how these curves (and hysteresis) depend on snow microstructure, and how the presence of liquid water affects other snow properties. I think the approach is very exciting and promising, but that the value of the work is undercut by the quality of the writing. The science unquestionably warrants publication, but the manuscript needs significant revision in the organization and writing before it is ready for publication. I suggest the manuscript be reviewed in detail by a native speaker for typographical/grammatical errors as well as continuity/flow.

Specific Comments

Introduction: The flow of topics is sometimes strange, which makes it difficult to follow the train of thought. Because of this, I feel the introduction fails to succinctly indicate what knowledge gap is being addressed, which undercuts the value of the work. The relevant information is there and mostly needs to be edited/reorganized. A few specific comments to the introduction and smaller typos throughout are noted in the Technical Comments below.

Introduction: I think you need to elaborate on Lines 70-71. You need to include more information on how the effective transport properties are currently estimated and why this is not sufficient and/or why new water retention curves will improve on the current methods. This is a knowledge gap you are filling, but it is not very clear how the new water retention curves will achieve this better compared to previous methods. This would also support your statement in Line 193, where you state that effective thermal conductivity of snow and unsaturated effective water vapor diffusivity have been estimated "very little for the case of wet snow".

Limitations: The main limitation and question surrounding this work is how much the results can be trusted, since there is no ground truth or independent measurements with which the simulations were verified. Because of this, there should be some discussion addressing the uncertainties, why we can consider the simulations accurate, and how the reader should interpret the results in order to help them to decide to use (or not use) the proposed parameterizations (instead of the parameterization by Yamaguchi2012 which is based on measurements). It is not immediately clear if the system used by Hu2018 (the citation used) is transferable to snow given the pore size distributions used in that work compared to some of the larger pores found with certain grain types. In general, it would be good to discuss the accuracy, errors, and biases of the model that should be considered when applying the model to snow from whatever system it was validated with.

Parameterizations: I recommend adding prediction intervals and standard deviations for the fits of n and alpha. These would be helpful for quantifying the uncertainty of the fits if they are used in the future.

Discussion: You need to discuss the fact that optically equivalent radius derived from the SSA is not the same as the radius used by Yamaguchi2012. I think it is a very important factor since the grain diameter can vary a lot between methods and definition (here, optically equivalent radius vs equivalent sphere radius).

Hysteresis: I would suggest adding the alpha ratio (alpha_wetting / alpha_drainage) as a quantification of hysteresis (e.g. Line 209) and compare this to the literature values (Adachi2020, Leroux2017). It would also be interesting to see if this ratio is dependent on grain type or another parameter/combination of parameters.

REV: There is some discussion/description of the effect of the REV. However, I was still confused by how the REV was selected (Line 175). Is it just the maximum size of the uCT scan by Calonne2012? Given the range of voxel side lengths from 2.5 to 10mm, compared with typical grain sizes of MF which can be up to 1-2mm, it would seem to me that there would be some relationship between the REV size and the snow microstructure/relevant length scale which would have an effect on the simulations. Could you provide some clarification on this?

R2: Is R2 the correct metric for such a nonlinear function? Or perhaps another metric like a mean absolute error which can provide an error in physical units? Or something else?

Mean curvature as proxy for pore size distribution: (Line 262) Could you cite work demonstrating that this is a good proxy? I understand that mean curvature and mean pore size are correlated but perhaps other microstructural parameters (e.g. thickness) would be more suitable? Could you elaborate on why this makes sense for draining but not for imbibition? Did you look at the relationship for imbibition?

Residual liquid water content: What was the rate of drainage in the drainage experiments and/or what pressure did you set to induce suction out of the snow? Is it possible that this had an effect on the amount of residual water that was trapped and therefore led to the higher residual liquid water content values compared to the literature?

n: (Line 252) I think you can remove the explanation of the fit error. As you state, this is not likely to account for the discrepancy and I think is overreaching what you can really say with the small differences in R2. I would also suggest adding the fact that the Yamaguchi2012 parameterization was based on their S-samples, which were sieved. As seen in Fig.4 of Yamaguchi2012, the parameterization of n also doesn't fit the N-sample data but does seem to fit your data and your value of 4.7.

Residual liquid water content: (Line 275) Can you really conclude that the separation is due to the grain type as opposed to density based on the presented data?

Saturated liquid water content: Why did you not use theta_s from the simulations? Or analyze theta_s as you did for theta_r in Figure 6?

Effective wet snow transport properties: (Line 355) Can you add some text addressing the "so what?" of these results like you do for the thermal conductivity? Currently, the results are just presented, and the conclusion is just that the model seems to match the data. The bigger picture or effect is missing.

Effective wet snow transport properties: Is it possible to include a comparison to current parameterizations/models for hydraulic conductivity and diffusivity as was done for thermal conductivity. The comparison for thermal conductivity really shows how different the results can be, and this large difference demonstrates the importance of the approach provided here.

Effective wet snow transport properties: (Line 388) As presented, your regression matches your simulations MUCH better than the other methods, which is partially due to the fact that you are fitting your own simulation data. Are you capturing any physical effects that lead to better results that the other methods don't? And what limitations do we need to consider before simply adopting your regression?

Effective wet snow transport properties: (Line 415) What is causing this bias?

Effective wet snow transport properties: (Line 429): Can you provide some discussion about how different the results are if you use the imbibition simulations and why they are different?

<u>Technical comments</u>

Writing: Not having a space between paragraphs is confusing as some paragraph breaks are ambiguous since the document is full justified.

Line 2: It is somewhat unclear if the "its" refers to snow or liquid water. Suggest to rephrase.

Line 15: Grammar - I don't think "implies" can be used like this. Perhaps causes? Also, drastic changes of what? Microstructure?

Lines 17-24: This part of the Intro is confusing. Suggest rewording/reorganizing to make the topics in each sentence connect more clearly. E.g. Perhaps new paragraph at Line 20 and what does "to predict wet snow" mean?

Line 21: Please add citations for the flow types.

Line 24: Probably should cite SNOWPACK as well

Line 24-26: The sentence starts with "recently" but then you cite Daanen and Nieber from 2009. Perhaps just remove "recently".

Line 27: Grammar - "little" is not correct

Line 27: "effective wet snow properties" is not self-explanatory. Perhaps define or introduce the parameters you address in the paper.

Line 36-38: The way this definition of imbibition and drainage is written makes it seem like you always have to start or end at saturation, which I don't think is 100% correct or is, at a minimum, a bit confusing as written. Maybe consider rewording.

Line 41-43: Please add citations for the soil examples

Line 43-44: For completeness, I would add that there are a few studies that did measure imbibition: Coleou1999, Adachi2020, Lombardo2025 [Disclaimer – I am an author on this paper.]

Line 45: The brief discussion of Adachi2020's MRI method is a bit confusing since you seem to be transitioning to discuss hysteresis, but then go on to discuss how the VG parameters were fit in the other experiments. Since I think you should mention Adachi2020 in the comment above, perhaps you can just move that part and not discuss it here.

Lines 41-54: This paragraph generally needs rewriting. It is confusing and the message is unclear, likely because the topic changes several times and the transitions between the topics are not obvious/fluid.

Line 53: I think more precisely, Yamaguchit2025 provided a parameterization for sieved melt forms. There were also the "N-samples" which were natural snow samples, but they did not fit the parameterization.

Line 55: Grammar – "enable to capture" is incorrect and the commas are also incorrect.

Line 55: I think you need to discuss Hydraulic Conductivity before this paragraph since the "overall hydraulic behavior" is dependent on the not only the capillary forces described in the WRC but also the flow rate which is often done using the (saturated and relative) hydraulic conductivity, as you discuss later.

Line 65: Typo: literature

Line 67: I think Yamaguchi2025 is also a relevant citation here

Line 69: This sentence on models seems out of place with respect to the topic of this paragraph.

Line 78: Accounted "for" here

Line 76-79: I think you remove this sentence about wet snow metamorphism and just discuss this limitation later in the manuscript.

Line 94: Aren't the subscripts just the phase (ice, water, or air)? This sentence about the subscripts is confusing.

Line 102: Can you make a statement about whether the snow samples in Calonne2012 were natural, laboratory generated, sieved, etc.? Of course the information is in the original paper, but it would be easier for the reader if this was just briefly stated here.

Line 117-118: Add a citation for this

Line 119: Why did you choose 12 degrees? Cite?

Line 134: Typo: Ponds

Line 131-135: I don't think you need these sentences about the configuration. It is clear from the rest of the intro what imbibition and drainage are and the measurement of WRCs doesn't need to represent a situation that occurs in nature.

Line 161: Small formatting suggestion: in Eq 6, VG subscripts of alpha, n, and m look a bit weird to me. Perhaps increase their offset or decrease their size.

Line 167: I think this paper demonstrates nicely these relationships and how they are only roughly correct: van Lier and Pinheiro, 2018. Could be a nice citation to include.

Line 203: Not being particularly familiar with these tensors, I was wondering if the non-diagonal terms being negligible has a physical meaning and/or is standard. If it's important, perhaps provide a citation which contains this explanation so that a reader unfamiliar with this can inform themselves?

Line 213: It is unclear what this sentence is supposed to mean. It seems like you are just redescribing imbibition and drainage.

Line 222: I think you can't compare the shape when using liquid water content. If you want to compare the shape, you should plot saturation vs pressure as this normalizes the x-axis. I also don't really understand what is meant by "smoother inflections".

Line 228: It seems the point density for imbibition is higher than for drainage? i.e. there are more points on the imbibition curves in Fig 5. Is this true? That certainly would impact R2.

Line 241: Can you quantify this agreement?

Line 251: This makes sense since Yamaguchi2012 shows only drainage data – perhaps state this.

Line 283: I don't understand this sentence. As written, it seems like you are saying that the saturated liquid water content (LWC) can be up to 30% of the porosity. Do you mean that it can be 30% less than the porosity? i.e. Saturated LWC = 0.7xporosity?

Line 336: In order to make the statement in Line 336, I think you need to show the plots for other microstructural parameters such as ssa, curvature, etc.

Line 373: It is unclear if you mean Fig 11 a or b. Could you also postulate why there seems to be a density bias in your regression?

Line 396: This term is also the saturation, which might be better to use since saturation is used elsewhere in the paper already.

Line 400-401: For this statement, I think you need to include something about the close pore water content. As is, it doesn't make sense that the plot defines the domain of Du – as you state afterwards, it's theta_cp, so I would include that immediately in the sentence to make that clear.

Table 1: Can you rename the samples to something more intuitive? I understand these are probably the names in Calonne2012 but for the reader here that doesn't really matter (of course you can correlate them to the Calonne2012 paper). It would be easy to use the snow type as the name MF_l, MF_s, DH, etc. (or similar). This would allow the reader to immediately know what the snow properties are instead of having to memorize a random code since samples are grouped by grain type throughout the paper.

Table 2: Is there a unit issue in the fits for alpha? There are two orders of magnitude difference in the coefficient between Yamaguchi2012 and your data (10^4 vs 10^6).

Figure 5: It would be helpful to add the R2 values to the legends for each sample in Figure 5.

Figure 6: Something is weird with the caption formatting.

Figure 6: I think the (a)-plot as an overview of the data is nice, but could you also maybe replot only your data such that the reader can see the difference between imbibition and drainage for your data. Currently, the plot is too difficult to read to make out what is what.

Figure 7: Please add the grain type legend

Figure 8: This plot is very hard to read. Please improve the lines/marker differences to differentiate the datasets.

Figure 9a: Can you repeat this plot but with grain types and/or indicate grain types with the marker shape on the existing plot so that we can see how the grain type matches up?

Figure 13: It is really hard to tell that the solid line is blue. Just make it black since there are no other solid lines?

Figures 9b, 10b, 11b: Why are different saturation values (x coordinates) chosen for the different samples?

<u>References</u>

Adachi, S., Yamaguchi, S., Ozeki, T., and Kose, K.: Application of a magnetic resonance imaging method for nondestructive, three-dimensional, high-resolution measurement of the water content of wet snow samples, Frontiers in Earth Science, 8, https://doi.org/10.3389/feart.2020.00179, 2020.

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Coleou, C., Xu, K., Lesaffre, B., and Brzoska, J.-B.: Capillary rise in snow, Hydrological Processes, 13, 1721–1732, https://doi.org/10.1002/(SICI)1099-1085(199909)13:12/13<1721::AID-HYP852>3.0.CO;2-D, 1999.

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Yamaguchi, S., Adachi, S., and Sunako, S.: A novel method to visualize liquid distribution in snow: superimposition of MRI and X-ray CT images, Annals of Glaciology, 65, e11, https://doi.org/10.1017/aog.2023.77, 2025.