Authors' reply to referee comments RC1 of the paper egusphere-2025-2903 entitled "Simulating liquid water distribution at the pore scale in snow: water retention curves and effective transport properties" by Bouvet et al.

We thank the Reviewer 1 for the comment and positive feedback on the revised version of the manuscript. Please find below our point-by-point reply to the comments in blue.

I would like to thank the authors for the improvements made in the manuscript. It is much more clear now. Below are a few minor comments arising from the new text. Due to the introduction of a significant amount of new text, there are quite a few English issues. I have noted a few in the Intro but not the remainder of the manuscript – this should be addressed before publication.

Line 6 : I feel like it is a little misleading to describe the simulations like this since the software used a radius probe to find pores as opposed to an experiment where water is actually introduced and removed.
To clarify our method we replaced the sentence by: "Using the Young-Laplace equation and pore radius as a probe, liquid water is gradually introduced and then removed during wetting (imbibition) and drying (drainage) simulations, respectively."

Line 10 : "describe" instead of "reproduce"?
Changed accordingly.

Line 10 : remove "and" after "drainage"
Corrected accordingly.

Line 28 : hysteretic instead of hysteresis?
The correction was done.

Line 31 : "have been" instead of "were"
Changed accordingly.

Line 33 : such as
Changed accordingly.

Line 55 : sizes
Modified accordingly.

Line 63 : Defining the saturated and residual water contents based on drainage and imbibition processes is a little confusing in my opinion because these values depend on the experimental conditions. Maybe just adjust with what you have in Lines 245?
We prefer to introduce the general physical meaning of the variable in the introduction, and defer specifying modeling choices—such as the one given in line 245—to a later stage in the paper.

Line 71 : "To date, no estimates of the shape parameters of the VG model have been presented for imbibition in snow." I think it may be more accurate to say that no parameterization specific to imbibition exists. While no one has provided a complementary study such as Yamaguchi et al. (2012) for imbibition, Adachi et al. (2020) showed that n did not change between wetting and drainage, and captured hysteresis with the alpha ratio. The sentence has been modified to read: "To date, no regression of the shape parameters of the VG model have been presented specifically for imbibition in snow."

Lines 108-110 : this sentence has too many commas and is confusing
The sentence have been rewritten for more clarity.

Line 115 : of instead of in
Changed accordingly.

Line 117 : remove the
Changed accordingly.

Line 135 : between commas is wrong/weird
Clarified.

Line 189 : dash formatting seems wrong
We agree and corrected the reference formatting.

Line 194 : I am confused by step (ii). Should this be the same as for imbibition but with reversed NWP and WP?
As it can be seen in Fig. 2 of Arnold et al. (2023) the pore morphology method is not symmetric for the imbibition and the drainage processes, as the connectivity check of the phases is done at different steps of the algorithm.

Eq 9 : This was already defined?
We agree and removed the equation and added a reference to the equation in the introduction.

Line 249 : effect of
The REV is associated to the WRC, thus we prefer keeping it written this way.

Section 3.1.1 : Maybe discuss this later where you quantify the hysteresis in 3.1.3? or at least make it clear that there will be more discussion below.
We agree that the structure was a bit unclear, we merged the section 3.1.1 about hysteresis with the section below.

Fig 8 : it is a little confusing that both fits aren't in both plots.
We enlarged the y range of the first plot to see both regressions in both plots.

Line 327 : care to propose a reason why the ratio is smaller for snow?
From the work of Likos et al. (2013) we find consistent results with cohesive soils, considering that our material features much higher porosities. We added the reference to Likos et al. (2013) in the revised version of the manuscript. We believe further investigations should be done to understand physical meaning of the ratio value, as the degree of hysteresis seems to depend on complex processes linked to the material microstructure.

Line 367 : I disagree with this statement: "This division is probably due to the fact that the denser snow samples, composed of MF grown under conditions of liquid water saturation, show large pores which can hold little water by capillarity." I think it is due to the fact that it is easier for pores to be cut off during drainage in the less dense samples.
To better describe the physics, we changed the phrase to : "This division is probably due to the fact that the denser snow samples are composed of MF showing large and uniform pores compared to the other samples. The latter could be subjected to more disconnections during drainage due to the small and complex throats, resulting in higher residual water content."

Fig 9 : it is interesting that slope of your imbibition curve matches the drainage data more than your drainage curve for Sample 1 MF.
We believe this effect does not represent the general picture (not for all regressions, and not for all snow samples), and we prefer not highlighting it as it could be confusing for the reader.

Eq 11 and 12 were already defined?
We agree and replaced them by a reference to the equations already mentioned in the introduction.

Fig. 11 : why do the models overestimate the hydraulic conductivity?
The overestimation of the model compared to many of the numerical values is believed to come from the parameterization of the intrinsic permeability K from Calonne et al. (2012). We also think the chosen form of the model constrain the curves, for instance we might had better results if we chose to also fit the tortuosity tau (see eq. 4) on the numerical data.

Fig. 14 : add (a) to the left plot.

Modified accordingly.

Line 620 : none of these headings are shown.

The headings have been added.

References

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Authors' reply to referee comments RC2 of the paper egusphere-2025-2903 entitled "Simulating liquid water distribution at the pore scale in snow: water retention curves and effective transport properties" by Bouvet et al.

We warmly thank the reviewer for the helpful comments. The main comment raised in RC2 concerns the simulation of the imbibition process with the PMM method applied in the present paper to snow. We believe that there is no sufficient arguments that justify the removal of this part of the paper as suggested in RC2. Please find below our detailed arguments regarding this specific point in blue.

General comments

The authors made a big effort to revise the manuscript and to address the comments of the reviewers. The detailed reply made it possible to assess the quality of the simulations and the conclusions that can be deduced from them. The main shortcoming of the previous manuscript was the missing direct comparison between measured and simulated water retention curves (of the very same snow material) and that the quality of the simulations (and thus the estimation of the van Genuchten parameter values) remained unknown. To address this, the authors compared their simulations with measurements obtained with magnetic resonance imaging (Adachi et al., 2020) for two snow samples (M and S in Adachi et al., NH5 and NH2 in the submitted paper) that were similar with respect to snow type, density and particle size. This comparison is shown in the reply letter in Figure 4 on document page 31 (page 5 of the reply to comments of reviewer 3). I interpret this figure as follows: the shape of the drainage curve simulated with the morphological pore network model is fine (in the left figure, even the absolute values are matched), but the shape of the imbibition curve differs from the measurements. This figure in the reply letter shows that the simulated imbibition curve is not in parallel to the simulated drainage curve (or the measured one) but has a different shape (see figure on the right). The water invasion is too gradual for a wide range of capillary head values (see the figure at the right, with the gradual increase of water saturation by decreasing h from 0.150 to 0.075 m). I don't think that this shape is correct, and I don't trust conclusions from the wetting curve (also, the assumption that air phase is not entrapped during the wetting process is not realistic and its effect on phase distribution remains unknown). In short: I strongly recommend dropping the presentation of the imbibition analysis.

We believe that there is no sufficient evidence to invalidate the imbibition simulations presented in the paper, as:

(1) In snow, we compared our simulations with the 2 dataset available to date for imbibition: Adachi et al. (2020) and Lombardo et al. (2025). Concerning the first study, we agree with the reviewer that the imbibition curves given by the pore morphology method (PMM) do not give the same values of n_{vg} as it was measured by Adachi et al. (2020). It must be noted that the results of Adachi et al. (2020) are given for only three samples and haven't been repeated. Concerning the second study, the measurements of Lombardo et al. (2025) are consistent with our n_{vg} data (measurements on 4 samples). So no final conclusion can be made about the validity of our simulations based on these comparisons.

(2) In soils, many studies such as the one of Likos et al. (2013) shows that the exponent of n_{vg} is similar in drainage and imbibition, as suggested for snow by the reviewer. However, it is important to note that other experimental and numerical studies on granular materials (see for example Cheng et al. (2012) or Sweijen et al. (2016) show that the values of n_{vg} in imbibition can be much smaller than the one in drainage, similarly to our results for snow. All the soils under consideration in Likos et al. (2013) have, in a certain extent, similar granular microstructure and a porosity within the range 0.38 and 0.50. In the case of the snow, the shape and size of the grains strongly varies from one type of snow to another one, and the porosity ranges typically from 0.5 to 0.95. Such strong differences in term microstructure could explain that the values of n differs from imbibition to drainage.

(3) We investigated whether the difference of shape between the WRCs in drainage and imbibition could be explained by the fact that we do not simulate entrapped air during the imbibition process, as suggested by the reviewer. We show that taking into account entrapped air does not change our conclusions, as little impact is observed on the shape parameters of the VG model. To support this comment, we present a new Figure 1 in the revised version of the manuscript that presents the results of WRC simulations taking into account both air and water residuals for wetting and drainage, respectively. Please see the Figure below and the associated comments.

According to the above points, we believe that our imbibition results are reasonable at this stage. The uncertainties that remain on this, and more generally, on our simulations, are stated in the paper, in particular in the limitations section of the paper. A

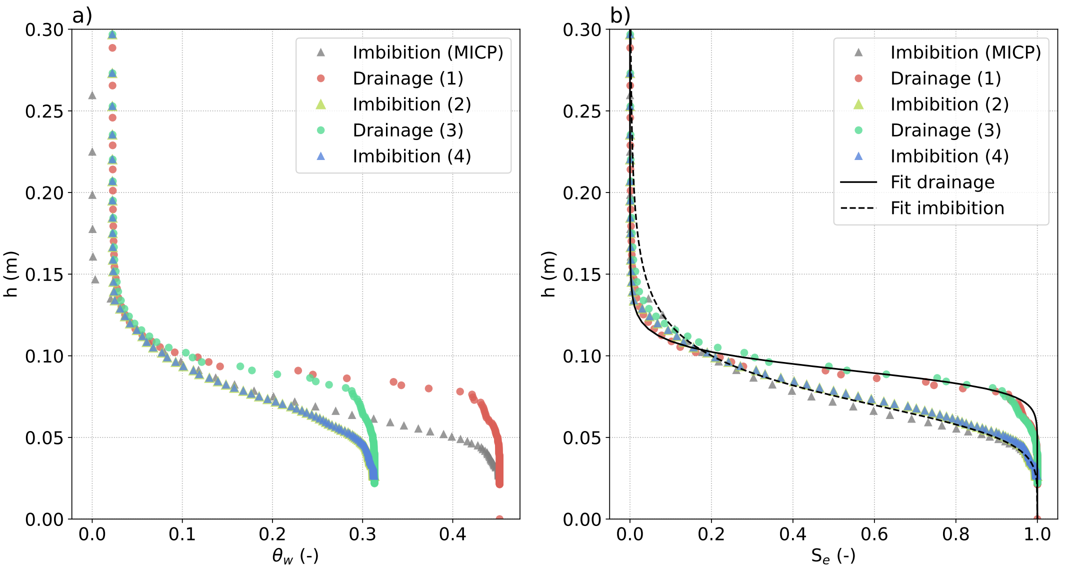

Figure 1. Example of two hysteresis cycles applied to the sample NH2, with primary drainage (1), primary imbibition (2), secondary drainage (3) and secondary imbibition (4). The boundary conditions enable entrapped residual air during imbibition and residual water during drainage. The gray points represent a first imbibition with no entrapped air enabled (MICP). (a) Liquid pressure head as a function of the water content. (b) Liquid pressure head as a function of the effective saturation. The solid and dashed black lines represent the VG fits used in the article for drainage and imbibition, respectively.

paragraph was included in the manuscript to improve the discussion on the unexpected hysteresis of the n_{vg} parameter, which reads: "The n_{vg} values for drainage are overall much higher compared to imbibition, as opposed to the findings of Adachi et al. (2020) or many soils observations (Likos et al., 2013), which mention similar values of n_{vg} for drainage and imbibition. Our results are more consistent with the ones of Lombardo et al. (2025) or with granular materials (Cheng et al., 2012; Sweijen et al., 2016) for which n_{vg} values in imbibition can be much smaller than the ones in drainage."

Comment of Figure 1 *Figure 1 presents a comparison of the WRCs with and without taking into account entrapped air during imbibition for the sample NH2. Hysteretic cycles were applied with boundary conditions (displaced fluid outlet) applied on the 4 faces of the volume not linked to the reservoirs, enabling residual air during imbibition and residual water during drainage. For these hysteretic cycles, the sample is initially fully saturated, then the sample is submitted to drainage, imbibition, drainage, and imbibition. Fig. 1.a shows the WRCs of each of these steps (Drainage (1), Imbibition (2), Drainage (3), and Imbibition (4)) as a function of the water content, as well as the WRC of imbibition assuming no air residuals (MICP). We can observe that, at the end of Imbibition (1) and (2), the residual air content is about 0.14, so the water saturation does not exceed 70%. As mentioned, this value can vary depending on the boundary conditions applied to the 4 lateral faces of the volume. Fig. 1.b shows the same data as Fig. 1.a, but with respect to the effective saturation. The continuous lines represent the fitted VG model proposed in this study, with the shape parameters from Table 2, so derived from our simulated WRCs of drainage and of imbibition without entrapped air. These two fitted VG models provide a good description of the entire drainage-imbibition process, regardless of whether air residuals are accounted for or not. Indeed, entrapped air has almost no impact on the value of n_{vg}, and a slight impact of around 10% on the value of α_{vg}.*

Additional general comments

1) Regarding the simulation of the drainage curve (see Figure 6b in the new version of the manuscript) the fit of the van Genuchten model is not very good and especially in the dry range the pore network simulations drain at higher h-values compared to the van Genuchten fit (see green, blue and pink lines and symbols). I think that this mismatch stems from draining different pore structures in the wet and dry part: in the wet part, pore bodies of decreasing size are drained with increasing h; in the dry part, the water structures in the crevices in the pore space become thinner with increasing h and no additional pore size classes are drained (from a conceptual point of view, the curve looks like the transition in soil physics from draining water bound by capillary forces to draining water bound by adsorption). The van Genuchten model captures well the drainage of pores with decreasing size but not the 'thinning' of water structures. Could the authors please comment on that?

The effect of a regime shift between decreasing pore sizes and the thinning of water structures may hold for images NH2 and NH5, which exhibit relatively uniform and large pores (and therefore few small-pore classes). However, it appears less plausible for images such as grad3, 0A, and fr, which are those affected by the mismatch in the dry range. Indeed, as shown in Fig. 5, even at 10% saturation, a significant number of smaller pores still seem to remain to be drained. Compared with the MF snow types (NH2 and NH5), these images display a broader distribution of pore sizes, including very small pores. Consequently, while the interplay between these two processes may explain parts of the WRCs, it remains uncertain whether the model can fully capture their behavior.

2) For the fitting: it seems that there are much more data point in the very wet range; did the authors assign less weight to these values to balance the relevance of wet and dry range?

It is true that the data points are not evenly distributed as the simulation steps are made with the pore radius and not the water content. However we dit not add weight to the data points, which can partly explain why the fit seems to match best in certain areas where there are more data points. To clarify this points we added a sentence in the figure description : "The model fits the data points better in the wet part compared to the dry part for snow types such as RG and PP. This might be due to (i) the fact the $m_{\mathrm{vg}} = 1 - 1/n_{\mathrm{vg}}$ constrains the shape of the fit, and that (ii) the data points are not evenly distributed in this θ_w, as the simulation step is the pore radius and not the water content. In future studies, the VG formulation could be refined using more parameters, such as m_{vg}, to enable fitting both the left and right inflection points of the WRCs, especially for drainage. "

3) Figures 10 and 11: I suggest that the authors fit the tortuosity/connectivity parameter (set now to $\frac{1}{2}$) to improve the fitting and to see how the parameter value change with snow type.

Investigating the impact of the tortuosity parameter τ_{vg} in the Mualem van Genuchten expression of hydraulic conductivity is out of the scope of our paper. We use the default value of $\tau_{\mathrm{vg}} = 1/2$. The evolution of τ_{vg} with snow microstructure will be studied in further works. To emphasize that point, the following was included in the paper (line 418): "In addition, the VGM model includes the parameter τ_{vg}, which describes the effects of the connectivity and tortuosity of the flow paths. Here we assume that $\tau_{\mathrm{vg}} = 1/2$, which is the default value suggested in Mualem (1976). Note that this parameter can vary depending on the porous material (e.g., Vereecken et al., 2010) and its value for snow could be refined in future works. "

Specific comments

Eq. 2,4,5 : The van Genuchten model equation and the Mualem van Genuchten model equation are listed twice (2, 4 and 5; and again 9, 11 and 12); this should have been captured
We removed the equations which where already described in the introduction.

Line 3 : Which 3D-data are scarce? Dry snow or wet snow?
The wet snow 3D data are scarce compared to dry snow, for which many 3D data exists.

Line 35 : state that here the formulation is for vertical direction with z pointing upwards
This was included in the revised paper.

Line 42 : mathematically it is the other way around (water content changes as a function of capillary head)
Modified accordingly.

Lines 55 : The sequence of the sentences is a bit confusing because it seems that the conclusion "WRC depends on snow density and grain size" requires additional studies. Maybe just drop the first sentence.
The sentence was removed.

Line 60 : the formulation should be different (as presented in text after eq. (9)): van Genuchten is used to describe the WRC (not to predict it) and the term prediction can only be used if you predict the parameter values in a first step.
We replaced the term "predicted" by "described".

Line 79 : I propose to use the term relative hydraulic conductivity that is typically used in soil physics
We used the double bar capital K to represent the hydraulic conductivity and the simple capital K for the permeability.

Line 85 : here you should explain and define S_e
The term S_e is now described in the introduction and its expression is provided.

Line 87 : equation 5 is from van Genuchten, not from Mualem
We corrected the citation accordingly.

Line 88 : This is a very strong assumption. In Mualem, the value ½ was the best match for 45 porous media (soils and a few glass beads) when forcing the model to have a constant value for all media. In the reference you cite (Vereecken), it is stated "Therefore, many studies have revisited the value ..... For example, Vereecken (1995) found values strongly deviating from 0.5, including many negative values, using a set of 185 measured hydraulic conductivity data sets." This should be rephrased and checked in the analysis (see point 2) in the main comments)
A comment on the tortuosity value has been added in the revised manuscript, see the response to the additional general comment 3).

Captions Figure 1 : explain colors, Omega and indices
The elements have been added to the figure.

Captions Table 1 : explain what the acronyms (snow type) stand for. What is meant with 'Image size'? The length of the side of the cube in voxels?
The title of the figure has been changed to : "Main characteristics of the 5 selected 3D images. The image size is the side length of the cubic image. Snow types are given according to the international classification (Fierz et al., 2009)."

Line 176 : "much more important", not "much important"
Changed accordingly.

Line 182 : The authors should state that this is only valid for small Reynolds and capillary numbers.
We agree and added those elements to the text.

Line 215 : It should be the maximum water content, not maximum water saturation (symbol θ is used)
Corrected accordingly.

Line 251 : do the authors mean van Genuchten parameter values? How did the values change in Fig. 4a with resolution?
The evaluated values for the size of REV are directly the values of WRC, with curves getting "sharper" in the wet area as the size of the volume increases. We changed the sentence in the manuscript to be more explicit.

Figure 10a : what does it mean that relative water permeability is larger than 1 in some samples? Please explain.
Thank you for your remark, we erroneously used the parameterization of Calonne et al. (2012) for K instead of the simulated value for the saturated media. We corrected the figure accordingly.

Figure 10 : Plot y-axis on log10 scale to see the deviation between eq. 11 and simulation results

We prefer keeping the linear axis, as the hydraulic conductivity is shown below with a logarithmic scale, which allow for a visual comparison with the VGM model and our numerical results.

Captions Figure 13 : you should refer to Crocus model (not only Yen)

Changed accordingly.

References

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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