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 thank very much the reviewer Michael Lombardo for the comments. Please find below our point-by-point replies in blue color.

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

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.

We agree with the reviewer and edited the introduction substantially. The revised version provide a clearer overview of the state of the art in wet snow studies and property estimation. We enriched the description of the current limitations and stated more clearly our contribution.

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".

The introduction of the revised manuscript was significantly modified to improve the state of the art on the estimation of the WRCs and effective properties of wet snow in current modeling. Especially, we highlight the following gaps: i) current regressions to estimate the shape parameters of the VG and MVG model for snow, used to predict the WRCs and the unsaturated hydraulic conductivity, were developed based on few drainage experiments using mostly large dense melt forms, so there is a gap in the knowledge of the WRCs and unsaturated hydraulic conductivity for the whole range of snow, ii) current regressions of the unsaturated thermal conductivity of snow are either extrapolations of thermal conductivity regressions for dry snow, or based on simple ponderation of the thermal conductivity of ice and of liquid water, neglecting the effect of the microstructure (which was shown to be important for the case of dry snow) or the fact that water conducts four times less than ice, and iii) effective diffusivity was simply not investigated for the wet snow case. To add more context, we added references for studies about water vapor diffusivity for unsaturated soils.

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.

We agree with the reviewer and we dedicated a new section in the revised manuscript ("4. Main limitations") to present the main limitations of the study. The new section synthesizes the following points:

- In the present work, the Pore Morphology Method (PMM) has been used. This method is valid in quasi static regime, and when capillary forces dominate in comparison to gravity and viscous forces. Dynamic effects than can occur in practice can not be captured as in two phase flow simulations (Vogel et al., 2005; Ahrenholz et al., 2008; Bhatta et al., 2024; Prodanović and Bryant, 2006; Jettestuen et al., 2013) or using a Pore Network Model (PNM) (Vogel et al., 2005; Joekar-Niasar and Hassanizadeh, 2012; Xiong et al., 2016). Despite such limitation, the PMM provides good estimations of the WRCs for porous media whose wetting phase shows generally spherical menisci, which is the case for snow (Hilpert and Miller, 2001; Vogel et al., 2005).

- Regarding the pore-scale simulations with Geodict, to get an idea of how well the simulation can be compared to reality, we would like to point out this video presented at the GeoDict User Meeting 2021, which shows the validation of the pore morphology model in advances in two-phase and single-phase flow simulations : https://www.youtube.com/watch?v=WMSorqm3B_g(from 11:000 to 14:30). However, the boundary conditions applied on the four sides of the 3D images not linked to the WP or NWP reservoir may play an important role on the residuals after drainage or imbibition. While little influence was reported for the case of drainage, the amount of residual air (or entrapped air) during imbibition may vary significantly depending on the chosen boundary conditions. This point which concerns all the methods (PMM, PNM, two-phases flow simulations) to describe two-phase flows has been little discussed in the literature, to the best of our knowledge, except in Galindo-Torres et al. (2016) and Zhang et al. (2025) in the case of lattice Boltzmann simulations. For our snow samples, preliminary tests showed that the maximum water saturation (θ_w^s) ranges from 45% to 90% of the porosity, depending on the applied boundary conditions (symmetry, wall or displaced fluid outled). More generally, knowledge on the residual air in snow seems limited. Estimates based on measurements remain an experimental challenge and show large differences (Yamaguchi et al., 2012; Katsushima et al., 2013; Adachi et al., 2020). Further studies would be required to validate the proposed approach through refined comparisons on experimental imbibition data coupling measurements of the microstructure by X-ray tomography and of the liquid water content by neutron radiography (see e.g., Tengattini et al., 2020). At this stage, given the uncertainty, the imbibition curves were simulated assuming that there is no air residuals, as in the Mercury Injection Capillary Pressure (MICP) experiments (e.g. Hilpert and Miller, 2001; Berg et al., 2016), thus $\theta_w^s = \phi$. We evaluated the impact of this simplification on the shape parameters of the VG model. Even with θ_w^s as low as 0.6ϕ, the shape parameters remain almost constant, with an effect only visible for α_{vg}, with differences around 10% of the value compared to the case $\theta_w^s = \phi$. This was also reported in the experiments of Likos et al. (2014) and Farooq et al. (2024), which showed that having θ_w^s smaller than ϕ generally implies greater α_{vg} values, but has no significant impact on the n_{vg} values.

- Since the PMM is applied on 3D images, uncertainties can arise from the size and resolution of the images under consideration. The effects of both parameters on the results are available in Hilpert and Miller (2001) and Vogel et al. (2005), and are assumed to be transferable to snow. In the present study, we checked that our snow images correspond to representative elementary volumes. Incertainties remain regarding the size of the melt forms images, for which the maximum available size was taken, but still present a limited number of heterogeneities (see Fig. 1).

- Our simulated WRCs were compared to WRCs measured during experiments of drainage and or imbibition. Such a comparison is not straightforward, as, in the experiments, the snow microstructure can evolve rapidly when in contact with liquid water, whereas, in the simulations, the ice skeleton is fixed and defined by the provided tomography image, always remaining in its initial stage. The comparison simulation-experiment was mainly done through the comparison of the shape parameter of the VG model derived from the WRCs. Experimental estimates remain, however, limited, often focusing either on imbibition or on drainage, or studying only a small range of snow types (Yamaguchi et al.,

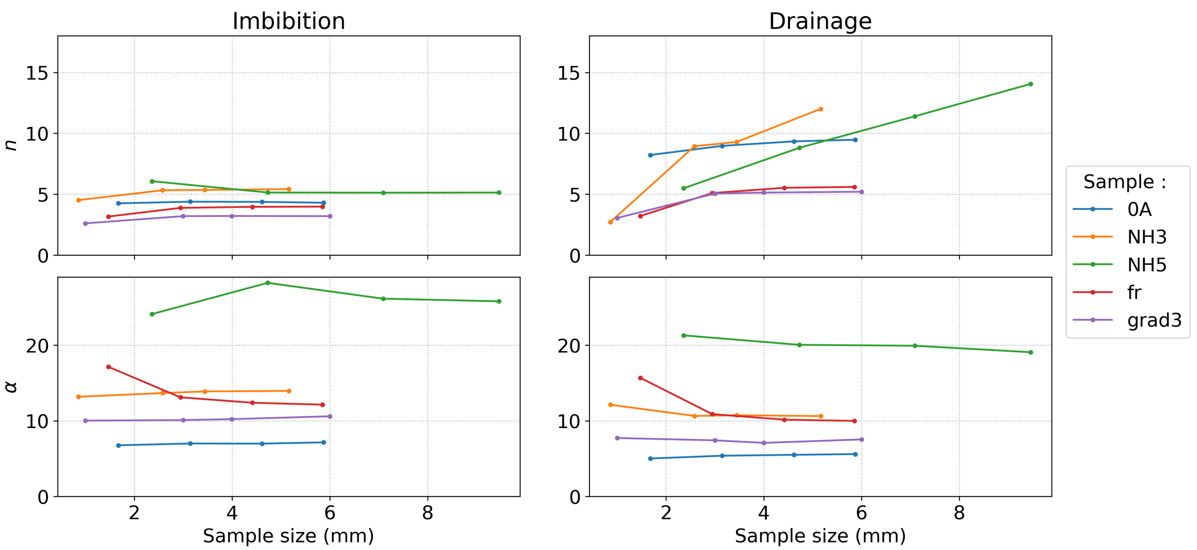

Figure 1. REV evaluation on the van Genuchten parameters for 5 snow samples.

2010, 2012; Katsushima et al., 2013; Adachi et al., 2012; Lombardo et al., 2025). While estimates of α_{vg} are rather consistent between all the measurements and our simulations for both imbibition and drainage (Fig. 2.a and b.), estimates of n_{vg} differ significantly (Fig. 2.c and d.). Hence, it is difficult to conclude on the evaluation of our simulations. Again, dedicated studies would be required to provide further experimental data.

– Finally, the uncertainties of the WRCs simulations are not necessarily transferred to the estimates of the effective transport properties of wet snow. The simulations provide the 3D skeleton of the air, ice, and liquid water, for which the distribution of each phase in space can contain errors, as discussed above. However, only the unsaturated hydraulic conductivity depends at the first order on both the volume fraction and the 3D distribution of the phases. Its predictions with the VGM model inherit from the uncertainties on the estimates of the VG parameters used to model the WRCs. In contrast, the unsaturated thermal conductivity and water vapor diffusivity of snow depend, at first order, mainly on the volume fraction of the phases, and the contribution of the phase distribution is secondary.

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.

We agree with the reviewer and added the standard deviation of each coefficient of the proposed regressions in Table 2 of the revised version of the manuscript.

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).

We agree with this comment. The grain diameter estimation of Yamaguchi et al. (2012) slightly differs from ours: our definition is based on 3D snow images while Yamaguchi et al. (2012) is based on its 2D counterpart obtained from outlines of disconnected ice grains. While the conceptual definition is quite similar for both of the methods, where an "equivalent diameter" is estimated, some typical differences between 2D and 3D are well-documented (e.g Brzoska et al. (1999), Cooperdock et al. (2019)):

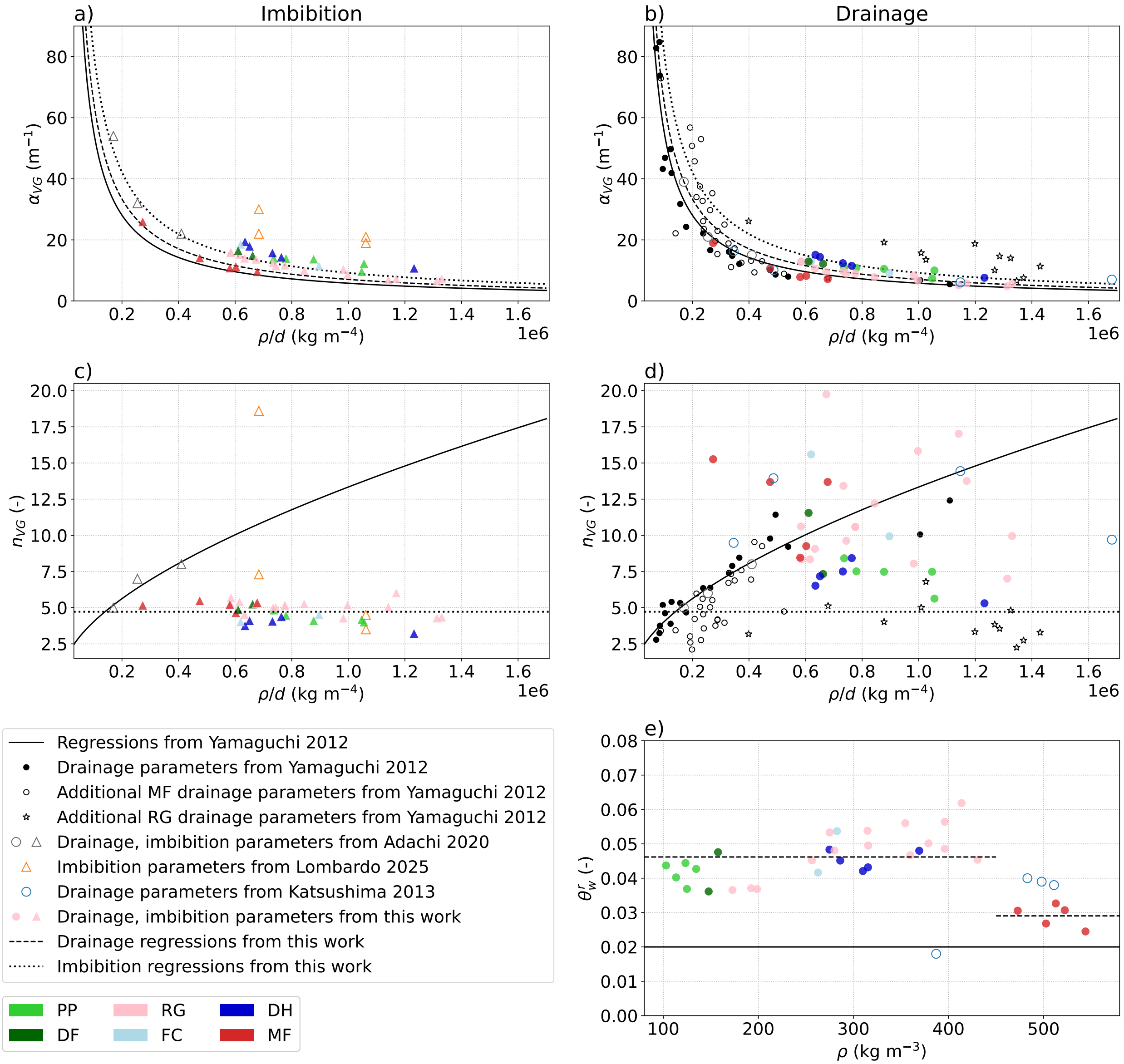

Figure 2. a) α_{vg}, b) n_{vg} and c) θ_w^r parameters of the VG model as a function of ρ/d or ρ for imbibition and drainage. The regressions of Yamaguchi et al. (2012) are shown by black lines, the values used to deduct those regressions are shown by black disks ("S-samples", composed of refrozen MF - see Yamaguchi et al. (2012)). The additional drainage measurements from Yamaguchi et al. (2012) are shown by circles (MF samples) and stars (RG samples). Measurements from Adachi et al. (2020) are shown by empty gray markers. Measurements from Lombardo et al. (2025) and Katsushima et al. (2013) are shown by empty orange triangles and blue circles. From this work, parameters from imbibition and drainage simulations are shown by colored disks and triangles, respectively, the colors showing the snow types. The proposed regressions based on our simulated data are shown by dashed and dotted lines (see Table 2).

– The 2D method tends to neglect all snow structures related to necks between large disconnected grains;

– Due to the combination of gravity and projective effects, grain diameters may appear different in 2D than they really are in 3D;

– Tiny air bubbles or holes inside ice structures are often overlooked in 2D outlines.

Depending on grain types and specific morphologies, these methodological differences may result in an overestimation or an underestimation in grain sizes so that an exact conversion between the two approaches cannot be reasonably achieved. As a consequence, comparing our results to those of Yamaguchi et al. (2012) in Fig. 2 should be realized by keeping in mind that estimation errors on the ρ/d values might exist, potentially as systematic errors (stretching of regression curves to the right or to the left) or as an increased dispersion of the dataset. Based on existing literature comparisons (e.g Brzoska et al. (1999), Cooperdock et al. (2019)), and on the figures obtained here (number and diversity of investigated snow samples and morphologies) we are quite confident that such errors are moderate and that overall comparisons between regressions obtained from the different methodologies are valid. Very precise comparisons are however more difficult and the difference between the regression of Yamaguchi et al. (2012) and our regression for α_{vg} in drainage could be e.g. interpreted as a consequence of this methodology difference. With this regard, our regression can be seen as an updated version of Yamaguchi et al. (2012)'s regression, proposing estimations that are based on systematic 3D diameter estimations, which are perfectly adapted to tomographic measurements.
Such explanations where added in the "3.1.3 Analysis of the VG parameters" subsection.

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 (Adachi et al., 2020; Leroux and Pomeroy, 2017). It would also be interesting to see if this ratio is dependent on grain type or another parameter/combination of parameters.

We agree with the Reviewer and included a paragraph on the quantification of the hysteresis based on the suggested ratio, in the revised paper Section 3.1.3. The paragraph reads: "We use the α_{vg} parameter to quantify the degree of hysteresis of the WRCs, based on the ratio α_{vg} from imbibition over α_{vg} from drainage. For our set of images, this ratio ranges from 1.19 to 1.42, with an average value of 1.28. It is consistent with the ratios measured by Adachi et al. (2012), for which values of 1.46, 1.52 et 1.38 are found for the S, M, and L samples, respectively. No correlation was found between this ratio and grain type, grain size, or density, and the values confirm a small range of hysteresis ratio around 1.5, which is lower than the classical value of 2 used for soils (Leroux and Pomeroy, 2017)."

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?

The selected volumes are the maximum size of the uCT scans by Calonne et al. (2012), for which we checked that they correspond to REVs for the determination of the n_{vg} and α_{vg} parameters. As shown in Figure 1, the REV is reached for all the images except for the n_{vg} parameter for the two melt forms samples (NH5 and NH3) in the case of drainage, for which larger uncertainties could thus exist. This uncertainty is provided in the manuscript and recalled in the revised Section 4 on the study limitations.

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?

We agree with the reviewer and chose to indicate the mean absolute errors (MAE) to characterize the nonlinear relationships, instead of the correlation coefficient R. The MAE values are for instead shown in the revised Figure 3 for each image.

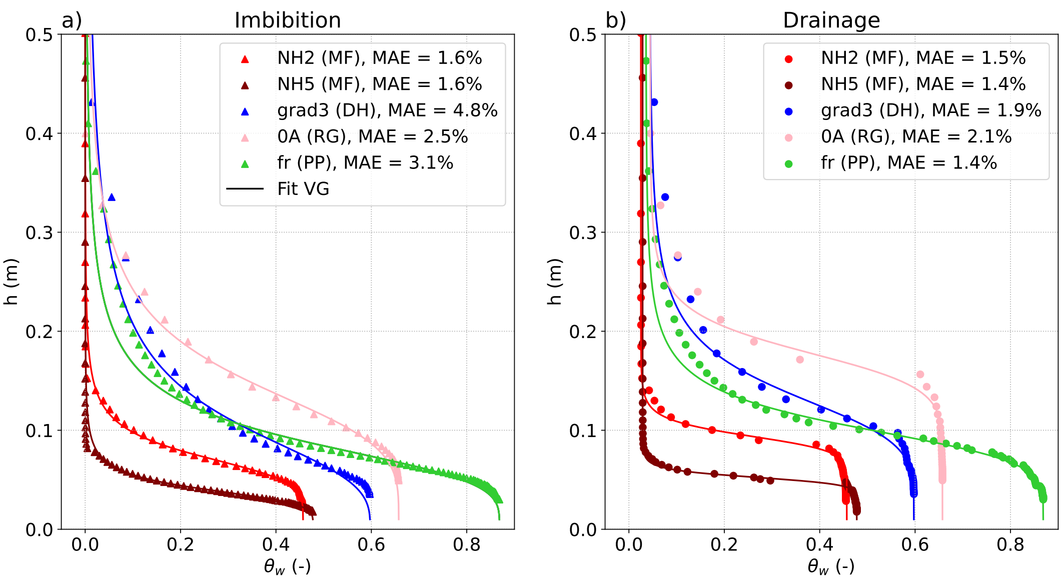

Figure 3. Numerical imbibition and drainage WRCs for different types of snow samples with the corresponding VG fits. The curve colors represent the different snow types, the MAE on θ_w of the fits are expressed in percent.

(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?

Mean curvature has been used for years as a grain size estimator (see e.g. Lesaffre et al. (1998) -section 4.2). It shows very good correlation to SSA (e.g. Flin et al. (2003) - Fig. 5) and its distribution is widely used to characterize snow microstructure and its time evolution (e.g. Calonne et al. (2014)). By just considering the complementary part of the ice grain image, the curvature distribution also gives pertinent information on the pore size distribution, as the curvature distribution inherently contains such type of information. We agree that thickness measurement might, potentially, be an interesting estimator, but we presently have no access to any appropriate numerical tool to confidently characterize this quantity. Actually, we have already tried many other parameters in order to find correlations to the n_{vg} value, but only some very particular curvature distribution related estimators gave convincing results.
We recently improved the n_{vg} parameterization by using the interquartile range (IQR) of the distribution, which is more suited to the shape of our distributions than the previously used standard deviation (see Fig. 4). This estimator also applies with the same success to the imbibition values, with much less amplitude on the n_{vg} variation.

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?

The model implemented in the software SatuDict is the Pore Morphology Method (PMM) (e.g., Hilpert and Miller, 2001; Silin and Patzek, 2006; Schulz et al., 2015; Berg et al., 2016; Liu et al., 2022; Arnold et al., 2023). The PMM does not require to solve any partial differential equations. Therefore, no physical boundary conditions, such as rate of drainage, pressure are applied. The PMM uses a sphere with a radius r as a probe to detect the pore space that is accessible by the non-wetting phase (NWP), here the air. This radius is computed from the Young–Laplace equation: $r = 2\gamma\cos(\psi)/p_c$ where p_c is the capillary

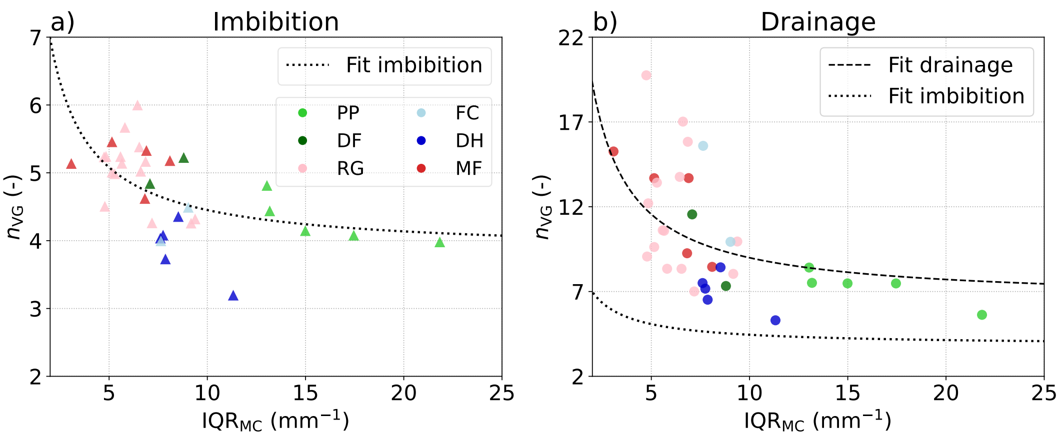

Figure 4. n_{vg} parameter as a function of the interquartile range $\mathrm{IQR}_{\mathrm{MC}}$ obtained for the mean curvature distribution computed on each 3D dry snow image. Regressions for imbibition and drainage are shown with dotted and dashed lines.

pressure, γ is the interfacial tension and ψ is the contact angle. Morphological operations, namely, erosion and /or dilation are used in the PMM (Hilpert and Miller, 2001). The algorithm of the PMM can be decomposed into several steps as follows:

- In drainage condition, the porous medium is initially saturated with the wetting phase (WP) ($p_c = 0$). The invading NWP is connected to the inlet, which is the NWP reservoir, and the WP can escape through to the outlet, the WP reservoir. (i) Then, p_c is increased incrementally, i.e. r is decreased incrementally. The solid phase is first dilated by a sphere with radius r (ii) All the pores connected to the NWP reservoir are labelled as NWP. (iii) The NWP is then dilated with the same sphere with radius r. The remaining pores are filled with the WP. The saturation can be then calculated. (iv) All the pores filled by the WP disconnected from the WP reservoir are considered as WP residual, and are no more considered in the next steps. All these steps are repeated by increasing the value of the pressure, i.e. decreasing the value of r. In the present case, the radius r has been decreased gradually with a step of 2 pixel size.

- In imbibition condition, the porous medium is initially saturated with the NWP. The invading WP is connected to the inlet, which is the WP reservoir, and the NWP can escape through the outlet, the NWP reservoir. (i) Then, p_c is decreased incrementally, i.e. r is increased incrementally. The solid phase is first dilated by a sphere with radius r. (ii) The NWP is then dilated with the same sphere with radius r. (iii) All the pores connected to the WP are now labelled as WP. The remaining pores are NWP. The saturation can then be calculated. (iv) All the pores filled by the NWP disconnected from the NWP reservoir are considered as NWP residual, and are no longer considered in the next steps. As for the drainage condition, all these steps are repeated for the next value of the pressure, i.e. the next value of r. In the present case, the radius r has been increased gradually with a step of 2 pixel size.

The PMM has been used to simulate the quasi-static Water Retention Curves (WRC) of porous media (glass beads, sands,..) in drainage or imbibition conditions (Hilpert and Miller, 2001; Liu et al., 2022). In imbibition conditions, if step (iv) is ignored, the PMM also allows for the computation of Mercury Injection Capillary Pressure curves (MICP), which are a commonly used technique for measuring porosity and pore throat size distribution, for instance (Hilpert and Miller, 2001; Berg et al., 2016). As underlined in Hilpert and Miller (2001), the accuracy of the PMM may depend on the resolution and size of the 3D images and of the definition of the structural element. Finally, it is worth mentioning that the boundary conditions applied on the four sides of the 3D images that are not connected to the NWP or WP reservoirs may also play a role in the WRC curve simulations, even if this point has not been discussed in the literature, to the best of our knowledge. In Vogel et al. (2005), impervious boundary conditions are applied, whereas other conditions, such as symmetry, displaced fluid outlet, or invading fluid inlet, can also be applied (Berg et al., 2016). These boundary conditions may lead to different values of the NWP (air) residuals during the imbibition process, since these residuals can be trapped or not at the boundary. Depending on the boundary conditions, the maximum water saturation (θ_w^s) can range from 45% to 90% of the porosity. Despite such large differences, the values of α_{vg} and n_{vg} in the van Genuchten (VG) model (van Genuchten, 1980) remain almost constant, with an effect only visible for α_{vg}, with differences around 10% of the value compared to the case $\theta_w^s = \phi$. This was also reported in the experiments of Likos et al. (2014) and Farooq et al. (2024), which showed that having θ_w^s smaller than ϕ generally implies greater α_{vg} values, but has no significant impact on the n_{vg} values. The impact of the boundary conditions is less pronounced in drainage conditions, since the water residuals are mainly located at the junction between grains.

In the present study, the PMM implemented in the SatudDict software was used to compute the WRCs of the 34 snow samples. We computed (i) a primary imbibition curve assuming that there is no air (NWP) residuals as in MICP experiments, thus $\theta_w^s = \phi$ in van Genuchten (VG) model, and then (ii) a primary drainage curve until to reach the water (WP) residuals (θ_w^r). In both cases, symmetric boundary conditions are applied on the four sides of the volumes.

In the revised version, the description of the pore morphology method was substantially modified and includes all the above considerations (see introduction and Section 2.3)

(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.

We agree with the Reviewer and deleted the explanation about the fit error in the revised version of the paper. We included more explanations about the parameterizations of Yamaguchi et al. (2012), and added the data points of Katsushima et al. (2013) and Lombardo et al. (2025). Also, as stated above, we recently improved the n_{vg} parameterization by using the interquartile range (IQR) of the distribution for both drainage and imbibition, which is more suited to the shape of our distributions than the previously used standard deviation (see Fig. 4). The paragraph in the revised paper reads: "For drainage, our n_{vg} values are spread and show little correlation with ρ/d. Looking at the experimental data, a large spread is also observed in Katsushima et al. (2013), Lombardo et al. (2025), and for the rounded grains samples of Yamaguchi et al. (2012). Our estimated n_{vg} values overall do not follow the regression of Yamaguchi et al. (2012), although the n_{vg} values of the melt forms samples are closer to this regression compared to the other snow types. Let us recall that Yamaguchi et al. (2012) presented a regression based on drainage experiments on sieved melt forms only (black filled dots in Fig. 2.d), which was in good agreement with n_{vg} values estimated on other samples of natural melt forms (black circles), but in poor agreement for samples of natural rounded grains (black stars), for which n_{vg} appear to be independent of ρ/d."

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

Based on our results, it is difficult to conclude if the separation is due to grain type or density. Most of the time, melt forms samples have high density, but snow types, such as rounded grains, can also feature high density. However, as seen in Fig. 2.e, the denser rounded grains sample (pink) has a density close to the less dense melt forms sample (red), while showing very different θ_w^r values, which suggest a stronger effect of snow type compared to density. We specify both effects in the revised text in the paragraph "θ_w^r **parameter**".

Why did you not use theta_s from the simulations? Or analyze theta_s as you did for theta_r in Figure 6?

As mentioned above, the boundary conditions on the four sides of the 3D images not linked to the WP or NWP reservoir may play a important role on the θ_w^s. This is one of the limitation of the PMM method (see our previous reply to a main comment, concerning the limitations of our study). In the simulations, we first computed a primary imbibition curve assuming that there is no air (NWP) residuals as in MICP experiments, thus $\theta_w^s = \phi$, and then a primary drainage curve until reaching the liquid water (WP) residuals θ_w^r. In both cases, symmetric boundary conditions are applied on the four sides of the volumes, a liquid water reservoir is located at the bottom and an air reservoir is located at the top of the sample.

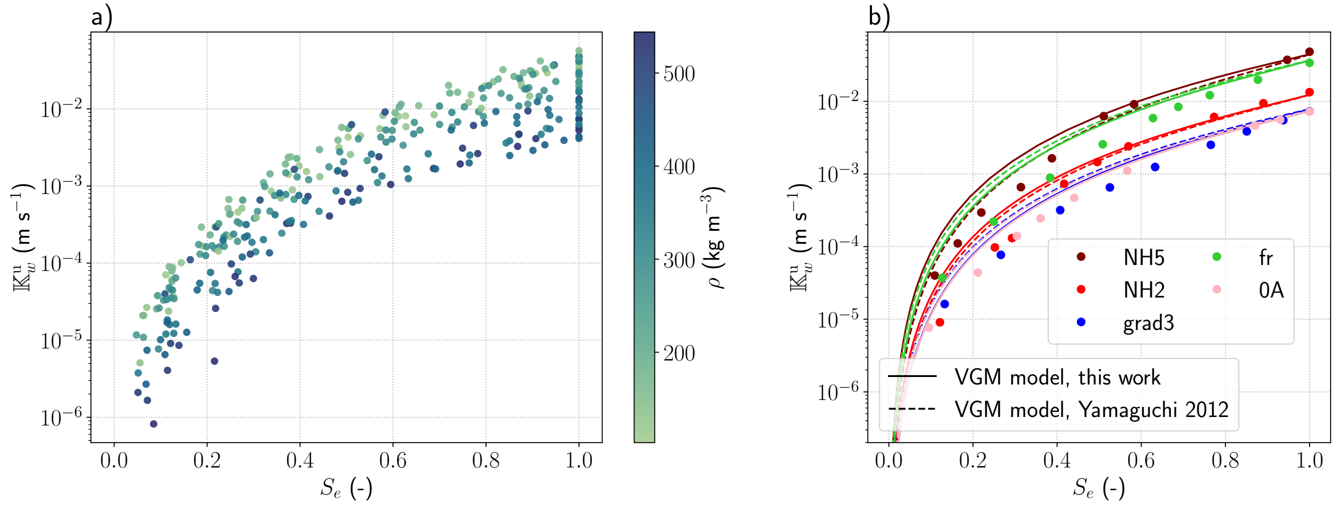

Figure 5. Unsaturated hydraulic conductivity $\mathbb{K}_w^{\mathrm{u}}$ as a function of the effective saturation for (a) the whole set of snow samples, and (b) the 5 selected samples. Computations were performed on the snow images from the drainage simulations only. The dry density of the snow samples is represented by the colorbar. The VGM model, using the values of intrinsic permeability from Calonne et al. (2012) and the VG parameters from the regressions of this work presented in Table 2 on each snow sample is shown with the solid lines. The VGM model, using the values of intrinsic permeability from Calonne et al. (2012) and the regression from Yamaguchi et al. (2012) is shown with the dashed lines.

(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.

We agree with the reviewer and added more elements to our analysis in the text. For the unsaturated hydraulic conductivity, as the log scale makes it somewhat hard to see in Fig. 5, we now provide the MAE values to compare quantitatively our regressions and the regression of Yamaguchi et al. (2012) against the estimated regressions based our numerical results (Table 2). For the melt forms samples, a slight improvement is found using our regressions, with MAE values around 10% for Yamaguchi et al. (2012) and around 9% for our model. This similarities can be related to the fact that, on one hand, the regression of (Yamaguchi et al., 2012) provides slightly better estimates of α_{vg} for this snow type, compared to our regression (Fig. 2.b); on the other hand, n_{vg} values are better estimates from our regression (Fig. 2.d and Fig. 4). For the other snow types, the VGM model using our shape parameter estimates provides better predictions of unsaturated hydraulic conductivity, showing MAE values around 17% compared to the VGM model using the estimates of Yamaguchi et al. (2012), showing MAEs around 22%. Indeed, for all the snow types excluding melt forms, both shape parameters α_{vg} and n_{vg} are overall better estimated using our regression (as optimized to best match our numerical simulations). This difference highlights the advantage of considering a high diversity of snow in the regressions, compared to the regression of Yamaguchi et al. (2012) based only on melt forms samples.

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.

The manuscript has been supplemented with more comparisons between our estimations of effective parameters and models of the literature. For the thermal conductivity, we added a self-consistent estimate that represents an arrangement of spheres with 3 connected phases (it is an implicit relationship where a polynomial equation needs to be solved) (Torquato, 2005). This

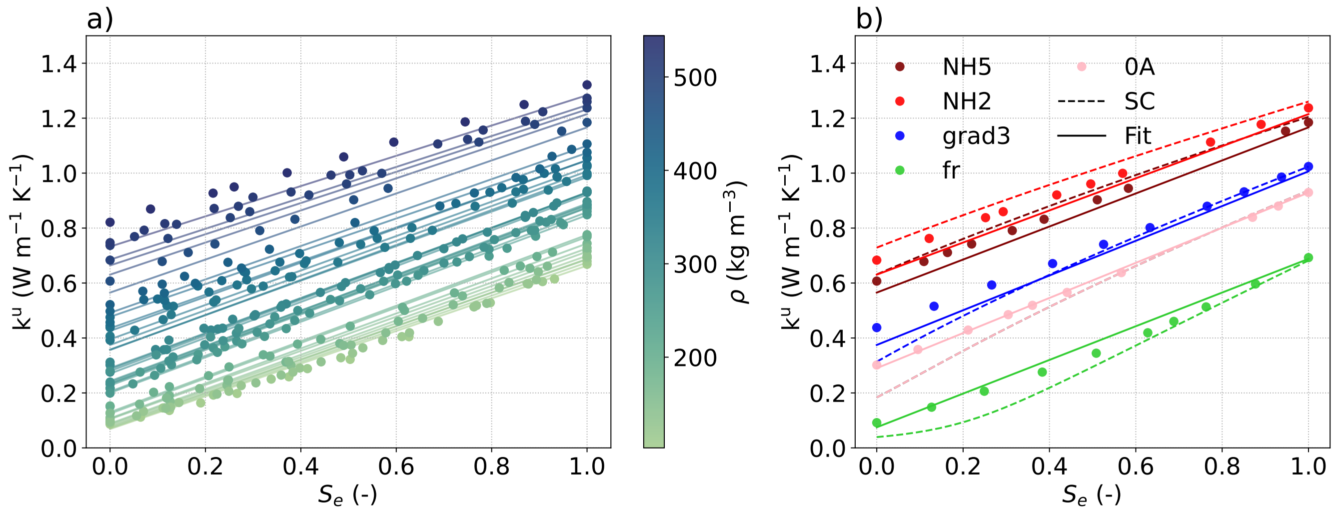

Figure 6. Unsaturated thermal conductivity k^{u} as a function of the effective saturation for (a) the whole set of snow samples, and (b) the 5 selected samples. Computations were performed on the snow images from the drainage simulations only. The dry density of the snow samples is represented by the colorbar. The suggested regression is shown by solid lines and the self-consistent estimate for 3 phases is shown by dashed lines.

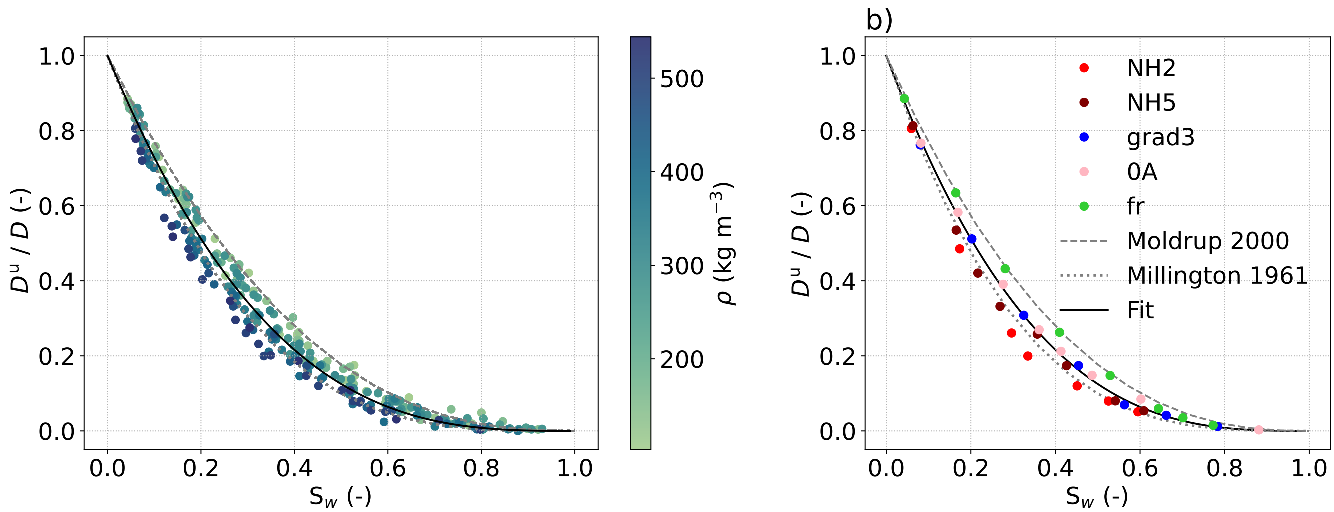

Figure 7. Relative water vapor diffusivity D^{u}/D as a function of the water saturation S_w for drainage simulations of (a) the whole set of snow samples and (b) the 5 selected samples. The dry density of the snow samples is given by the colorbar. The proposed regression of unsaturated diffusivity is shown by a black solid line, and the models of Millington and Quirk (1961) and Moldrup et al. (2000) are shown with gray dotted and dashed lines respectively.

estimate is shown in Figure 6 and is rather close to the proposed fit, which emphasizes the linear aspect of this property. For the water vapor diffusivity, we added the models for unsaturated structureless natural soils of Millington and Quirk (1961) and Moldrup et al. (2000) in Figure 7 (see Kristensen et al., 2010). They are derived from the same equation shape with a different exponent coefficient and show similar results. The relationship follow the general form of estimates: $D^{\mathrm{u}}/D_{\mathrm{dry}} = (1-\theta_w/\phi)^a$ classically used for soils (Kristensen et al., 2010) with $a = 3$ in our study, $a = 10/3$ in Millington and Quirk (1961) and $a = 5/2$ in Moldrup et al. (2000). For the hydraulic conductivity, instead of comparing the result using the shape parameters of Yamaguchi et al. (2012) with our shape parameters fitted on each image, we compared it with our shape parameters from the estimated regressions shown in Table 2, which allows a fairer comparison.

(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?

As clearly stated in the revised version of the introduction, in many models (Daanen and Nieber, 2009; Leroux and Pomeroy, 2017; Moure et al., 2023), the unsaturated thermal conductivity of snow k^u is derived based on an arithmetic mean, such as $\mathrm{k}^u = \mathrm{k}^{\mathrm{eff}}_{dry}(1-\theta_w)+\mathrm{k}_w\theta_w$, with $\mathrm{k}^{\mathrm{eff}}_{dry}$ the effective thermal conductivity of dry snow and k_w the intrinsic thermal conductivity of liquid water. By doing so, the impact of the microstructure and phase connectivity is not considered, although their importance was shown in dry snow (Calonne et al., 2011). In the operational Crocus model (Vionnet et al., 2012; Lafaysse et al., 2025), the parameterization of (Yen, 1981), only valid for dry snow, is extrapolated to wet snow, which leads to the assumption that ice and water have the same impact on the snow effective thermal conductivity, although ice conducts four times more than water. In contrast, the proposed regression is based on thermal conductivity simulations that account for the pore scale distribution of the different phases and consider the intrinsic thermal conductivity of each phase. This proposed regression is robust as it relies at the first order on the volume fractions of the different phases, which are easy to access, and does not rely on the shape parameters of the VG model (for which uncertainties could be large) as for the hydraulic conductivity for instance.

(Line 415) What is causing this bias?

We removed the sentence concerning those biases, as at this stage we do not have a physical explanation for it, and we wanted to keep a simple regression following the form of current regressions from the literature (Kristensen et al., 2010).

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

The water vapor diffusion and thermal conduction depend primarily on the volume fraction and connectivity of each phase, which are well described in our simulations and should be similar between drainage and imbibition. We tested the difference between drainage and imbibition simulations for the thermal conductivity, and a difference of 1.8 % was found (without obvious negative or positive bias), which is small compared to the other sources of error. Only the unsaturated hydraulic conductivity depends clearly on both the volumetric fraction and the 3D distribution of the phases and could thus show some difference between imbibition and drainage. However, as seen with Fig. 8, the difference between our results in imbibition and drainage is smaller than the difference with the results of Yamaguchi et al. (2012), which suggests that small changes could be also expected between both processes for $\mathbb{K}^{\mathrm{u}}_w$. We added more discussion about this point on the manuscript Sec. 4 "Main limitations".

Technical comments

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

The beginning of the paragraphs is indented in the revised manuscript.

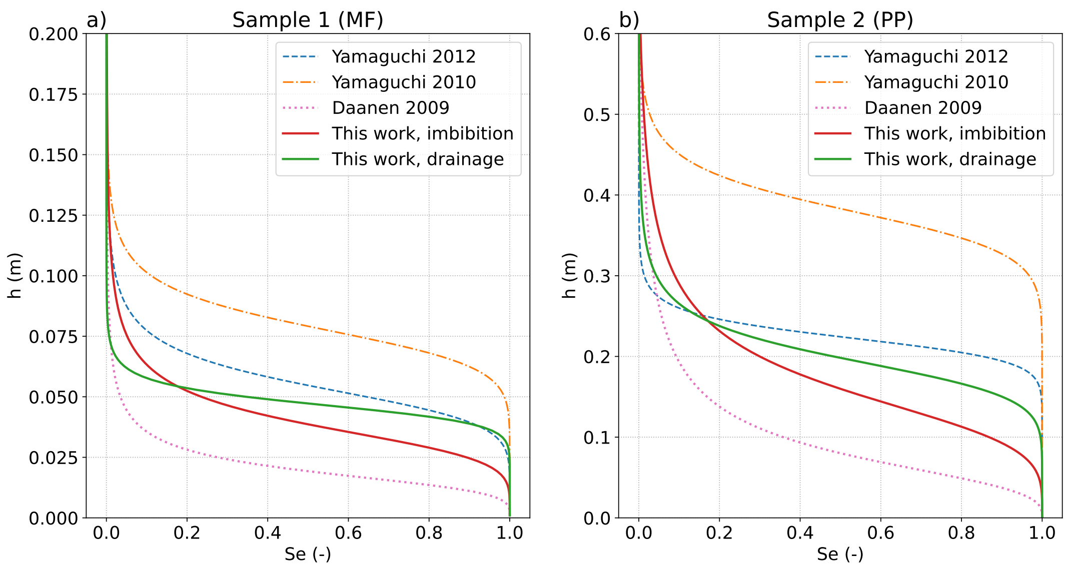

Figure 8. Illustration of the VG shape parameters regressions on the WRCs of 2 representative samples as a function of the effective saturation. The presented VG models are: imbibition and drainage models from this work and the models of Daanen and Nieber (2009), Yamaguchi et al. (2010) and Yamaguchi et al. (2012) (see Table 2).

Line 2: It is somewhat unclear if the "its" refers to snow or liquid water. Suggest to rephrase.
The sentence now reads: 'Liquid water flows by gravity and capillarity in snow and drastically modifies snow properties.'

Line 15: Grammar - I don't think "implies" can be used like this. Perhaps causes? Also, drastic changes of what? Microstructure?
We agree with the reviewer and modified the sentence, so it reads: 'Liquid water, introduced by rain or melt, flows into the snowpack and causes drastic changes in the microstructures and properties of the snow'.

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?
The introduction was improved substantially, as described in the general comments.

Line 21: Please add citations for the flow types.
We added the references of Gerdel (1954) and Colbeck (1976).

Line 24: Probably should cite SNOWPACK as well.
Yes, we included SNOWPACK in the revised version.

Line 24-26: The sentence starts with "recently" but then you cite Daanen and Nieber from 2009. Perhaps just remove "recently".
We agree and this sentence was modified in the revised introduction.

Line 27: Grammar - "little" is not correct.
We agree and this sentence was modified in the revised introduction.

Line 27: "effective wet snow properties" is not self-explanatory. Perhaps define or introduce the parameters you address in the paper.
Modified accordingly. The parameters are introduced in the revised introduction.

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.
We changed the sentence accordingly : "This relationship can be provided for a first imbibition, which corresponds to the wetting a dry porous medium by a liquid until it reaches saturation, or for a first drainage, which corresponds to the drainage of a fully saturated porous medium, and also for a drainage or imbibition from an intermediate state of saturation."

Line 41-43 : Please add citations for the soil examples
The soil examples were removed from the revised verison.

Line 43-44: For completeness, I would add that there are a few studies that did measure imbibition: Coléou et al. (1999), Adachi et al. (2020), Lombardo et al. (2025) [Disclaimer – I am an author on this paper.]
Thank you for pointing this out. We included these studies in the revised introduction.

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.
We agree. This paragraph was modified in the revised introduction.

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.
We agree. This paragraph was modified in the revised introduction.

Line 53: I think more precisely, Yamaguchi et al. (2025) 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.
Thank you for the comment. We include this information in the revised paper.

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

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.
We agree with the comment and modified the introduction accordingly. A paragraph on the unsaturated hydraulic conductivity of snow was added.

Line 65: Typo: literature
The correction was done.

Line 67: I think Yamaguchi et al. (2025) is also a relevant citation here
The citation was included.

Line 69 : This sentence on models seems out of place with respect to the topic of this paragraph.
We agree and, as the introduction was substantially modified, this sentence was deleted.

Line 78: Accounted "for" here
The correction was done.

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

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

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.

The following sentence was included: 'The imaged snow samples are composed of natural snow collected in the field as well as snow obtained from evolution under controlled environmental conditions in cold laboratory, which replicates natural snow evolution.'

Line 117-118: Add a citation for this

A paragraph on the Bond number, measuring the importance of gravitational forces compared to surface tension forces, was included in the revised paper and the following citations were added: Auriault (1987); Auriault et al. (2009).

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

The contact angle between liquid water and ice close to 0°C has been measured around 12° Knight (1967). The reference has been added to the manuscript.

Line 134: Typo: Ponds

The correction was done.

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.

We agree and removed these sentences.

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.

Modified accordingly.

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.

Thank you for the reference, which was included in the revised paper.

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?

Neglecting the diagonal components of the tensors is a classical operation that is for instance described in Calonne et al. (2011) or in Fourteau et al. (2021). Its means that the x, y ans z directions are the primary axes of the microstructure, which is the case if the snow has been sampled preserving its z axis. A reference with more details has been added to the sentence.

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

We removed the sentence which repeated elements already mentioned.

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".

We want to keep the absolute scale and not introduce S_e that soon in the paper as it allows a better appreciation of the different samples and their water contents. As seen in Fig. 3, the linear scale allows for a better observation of the WRCs of the different microstructures, thus from a qualitative point of view, comparisons can still be made. For instance, even with similar maximum saturation, NH2 (MF) and NH5 (MF) are very different from grad3 (DH) and show sharper curves. We changed the sentence to be more nuanced as it is hard to separate the effects of density and grain size on this effect: "More subtly, the WRCs tend to show sharper curves for high density and large grains samples. ".

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.

There is same amount of points for the imbibition and drainage curves. In the flat area of the curve, more points are visible for the imbibition curve as they are less steep.

Line 241 : Can you quantify this agreement?

The matching of the numerical estimates to the regression from Yamaguchi et al. (2012), and to the proposed regressions has been added to the text as MAEs.

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

We included this information in the revised paper.

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?

Yes we meant 30% less than the porosity, this part has been revised for more clarity.

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.

The sentence was reformulated to be clearer: "The relationship describes an exponential increase, which tends, for all samples, to merge into a single curve. This shows that the water permeability is at first order driven by the water content and the snow density, and that other dependencies with other microstructural parameters are, if any, of lesser strength."

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?

Both Fig. 11.a and Fig. 11.b illustrate the regressions. This was more clearly explained in the revised version of the manuscript. We think the density bias on the high-density and low-density can be a second order effect, and, as we did not want to go into this level of details, we removed the end of the sentence that can be misleading.

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

The term θ_w/ϕ is indeed the liquid water saturation S_w, we changed the Figure and the text accordingly for more clarity.

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.

We agree and define the close off earlier in the revised paper.

Table 1: Can you rename the samples to something more intuitive? I understand these are probably the names in Calonne et al. (2012) but for the reader here that doesn't really matter (of course you can correlate them to the Calonne et al. (2012) 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.

We keep the names as they refer to several previous papers (e.g. Flin et al., 2003; Calonne et al., 2014, 2012). However, we checked that the snow type is systematically provided in the paper together with the name sample.

Table 2: Is there a unit issue in the fits for alpha? There are two orders of magnitude difference in the coefficient between Yamaguchi et al. (2012) and your data (10^4 vs 10^6).

There is two order of magnitude difference in the coefficient because the exponent value is smaller. To avoid the fact that a large range of values for the two coefficients can describe almost the same curve, we chose to simplify our new regression by keeping the multiplicator coefficient of Yamaguchi et al. (2012) ($4.4\ 10^4$) and only fitting the exponent coefficient. This is motivated by the fact that the first coefficient has more impact on the small ρ/d values, where most of the measured values of Yamaguchi et al. (2012) are, and the exponent has more impact on the larger values of ρ/d, where most of our numerical estimates are.

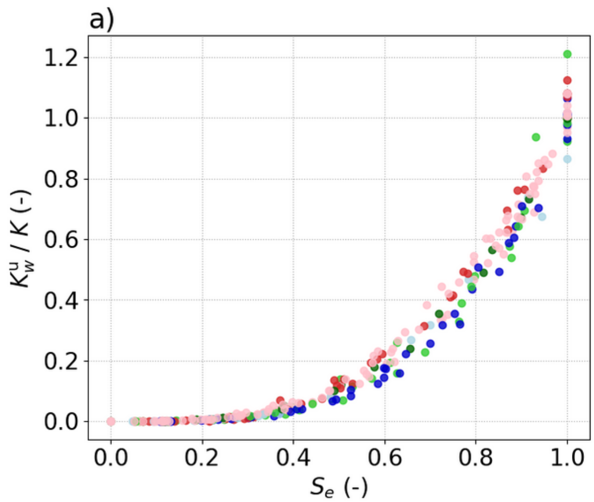

Figure 9. Effective relative water permeability as a function of the effective saturation for the whole set of snow samples. The colors represent the snow types (see Fig. 2 for colors legend).

Figure 5: It would be helpful to add the R2 values to the legends for each sample in Figure 5.
The mean absolute errors (MAE) have been added to each sample the Figure (see Fig. 3).

Figure 6: Something is weird with the caption formatting.
The correction was done.

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.
Based on this comment, we modified the figure, and added subplots for the n_{vg} and α_{vg} values separated for imbibition and for drainage, so that it enables a better vizualisation of the data points and the different regressions (see Fig. 2).

Figure 7: Please add the grain type legend
Changed accordingly.

Figure 8: This plot is very hard to read. Please improve the lines/marker differences to differentiate the datasets.
We simplified the Figure by changing the log scale of the figure to a linear scale. Besides, the comparison is now based on 2 'imaginary' snow samples, instead on 4 of our snow samples, for a more independent approach (see Fig. 8). The properties of the two samples have been chosen to be representative of melt forms (sample 1: d = 1.5 mm, ρ = 450 kg m^{-3}, IQR_{MC} = 5 mm^{-1}) and precipitation particles (sample 2: d = 0.1 mm, ρ = 130 kg m^{-3}, IQR_{MC} = 15 mm^{-1}).

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 9 shows Figure 9.a of the paper, where the density colorbar has been replaced by the snow types colors. To our perspective, this display does not bring more than Fig. 9.b, considering that the apparent clusters of the snow types colors are also clusters of density (PP and DF are mostly low-density samples, and RG and MF are more high-density samples - see Fig. 2.e to see the distribution of density of our different snow types).

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?
Changed accordingly.

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

The steps of the simulations are computed in terms of pore size and not in terms of saturation. Thus, depending on the microstructure, the x-coordinates are different for each sample.

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