Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

The authors provided an extensive response-to-review document, which is really appreciated, as well as a thoroughly revised manuscript. I'm very impressed with the response to the review provided by the authors, because it provided many insights into the new percolation scheme in CROCUS. And to many issues raised by the reviewers, the responses were very informative and clear. However, I'm a little bit disappointed that some of the interesting points in this document did not end up in the revised manuscript, although I think that by doing so, the significance of the manuscript would be enhanced and it would result into a much more interesting manuscript. Please find detailed comments below. I consider point 1, 2 and 5 are of particular importance to be addressed adequately, after which I can recommend publication.

Thank you for the second in-depth review. We have revised the manuscript considering the points brought to our attention. As a result of changing theta_r (point 5 of this review) most of the figures were updated.
Fig. 2 (histogram)
Fig. 3 (histogram)
Fig. 6 (C & D)
Fig. 8
Fig. 9
Fig. 10 (added see point 1 from this review)
Fig. C1 (Fig 10 of previous draft now in appendix C)

We have also updated the “code and data availability” section with doi references.

Some remarks about the response-to-review document:

1) Particularly, I think that the test of switching on/off the compaction routine is very insightful, and the comparison of Fig. A and B in the response document should end up in the final manuscript in my opinion. First, Fig. A suggests that the numerical implementation of Richards equation is behaving numerically stable, and second, the comparison of Fig. A and B shows clearly where the next developments should be. This is not only for the CROCUS team, but also for the snow community as a whole: how to accurately describe the effect of liquid water flow on compaction and wet snow metamorphism, as well as how to take care of feedback mechanisms between both. This discussion would just increase the group of researchers for which the manuscript is interesting.
We agree that these two figures (A and B from previous response document)

demonstrate that the Richards routine is behaving as expected and problems arise from the compaction routine. The figure below were added to the revised manuscript, and shows the simulated forcing with SNOWCROMETAMO and SNOWCROCOMPACT turned off. With the correction to theta_r (see point 5 of this document) running Crocus without the compaction and metamorphism routines requires an additional restriction on the time step t to be stable. The maximum time step length was restricted to a maximum of 30 seconds.

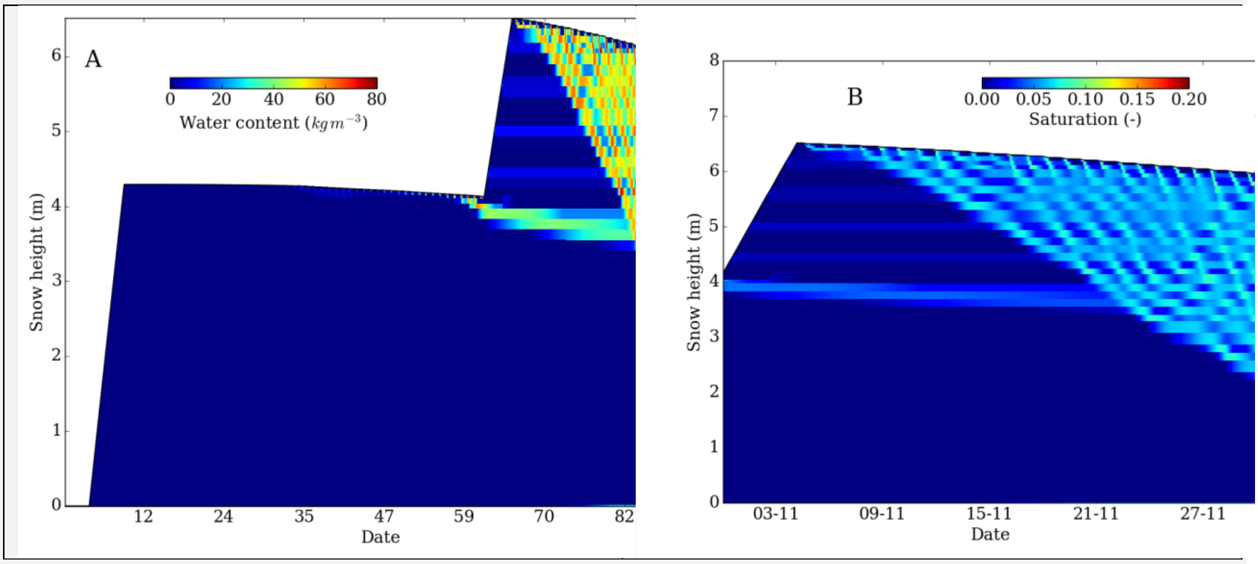


Figure shows results from the simulated data set with SNOWCROMETAMO and SNOWCROCOMPACT turned off. Time step t was held below 30 seconds.

Unfortunately running Crocus with SNOWCROMETAMO off and SNOWCROCOMPACT on is no longer stable, after updating theta_r. Hydraulic head magnitudes becomes very large (both positive and negative values occur) which results in errors when computing "CPSI" in the code (the derivative of the water retention curve). The implication of turning off the two routines corresponds to pushing the parameterizations of water retention and hydraulic conductivity beyond the range of physical plausibility. In other words to make these figures, water is passing through dendritic powder snow in Fig A, and through "compacted dendritic powder snow", which is not physically possible in Fig B (of previous review reply).

Sections 5.4 and 6.4 have been added to the revised manuscript to introduce the experiment of turning off feedback and discus the above figure.

2) Related to the previous issue: It's very clear now that the unrealistic alternating pattern arises from the interaction of the water percolation routine and the snow settling. As far as I now understand it, the problem is that higher values of LWC are typically associated with stronger settling rates. This leads to more snow compaction, higher densities, which in turn results into denser packing of the snow crystals, higher capillary suction, and thus, larger values of LWC, which in turn increases the snow

settling, etc. Am I correct here? If the authors agree with this reasoning, I think it is a good idea to explicitly explain it in the manuscript (it is now rather implicit). Actually, an obvious, and easy solution to this problem could be to limit the LWC used in the parameterizations for snow compaction and snow metamorphism to the 5% from the bucket scheme, with the motivation that this is done because the parameterizations for settling and metamorphism are typically developed and tested using the bucket scheme, and future studies may address the behaviour of snow with very high LWC. Did the authors tried this? I strongly recommend to try this out and report the results in the manuscript. If it doesn't solve the problem, it shows that the problems are of more substantial nature.

We believe that our message was not formulated clear enough and the feedback system was only partly understood. The feedback you described is correct but gets further complicated with the inclusion of hydraulic conductivity, the metamorphism and grid re-meshing when a layer melts.
A more compact snow layer will not always result in higher suction, which can be seen in Figure 2 of the revised manuscript. Compare the “decomposed” curve (200 kg/m3, 0.5 mm which closely follows the melt forms curve) with the “small round” curve (400 kg/m3, 0.5 mm). This shows that a layer with higher density has higher suction then less dense snow at low saturations (<12% saturation), but a lower suction (>12% saturation) at high saturations.

Similar behavior is seen in Figure 3 with the hydraulic conductivity curves. More dense snow has a larger hydraulic conductivity for low saturations (<~4% for small rounds & decomposed example), and at higher saturations less dense snow has a larger hydraulic conductivity.

The same behavior can be applied to grain size by using small round and melt forms which have the same density in figures 2 and 3 (revised manuscript).

These two feedback mechanisms alone cannot explain the striped pattern. The stripes suggest that there is a spatial aspect to the feedback, where one snow layer affects the neighboring layers. The method used for solving the Richards equation, where the average value of hydraulic conductivity and suction on the snow layers interfaces with iterations, can start to explain where the striped pattern arises. A single layer interface that has strong positive feedback will affect all the layer interfaces over the course of many iteration. However this issue is not yet fully understood.

We haven't been able to pinpoint the exact cause for the stripes, but through extensive testing outlined the mechanisms responsible for this behavior, i.e. turning off the compaction routine stops the stripes. Restricting the LWC to 5% for the compaction routine was unsuccessful at reducing the stripes, see figure below. Compaction is based on mass of overlying snow and viscosity. We imposed a 5% pore space LWC

limit on the wet deformation rate calculation leaving the total mass of the overlying snow with the water amount that the Richard routine uses (could be >5%).
Finding the exact mechanisms producing these undesired stripes and potential remedies will remain subject for future research.

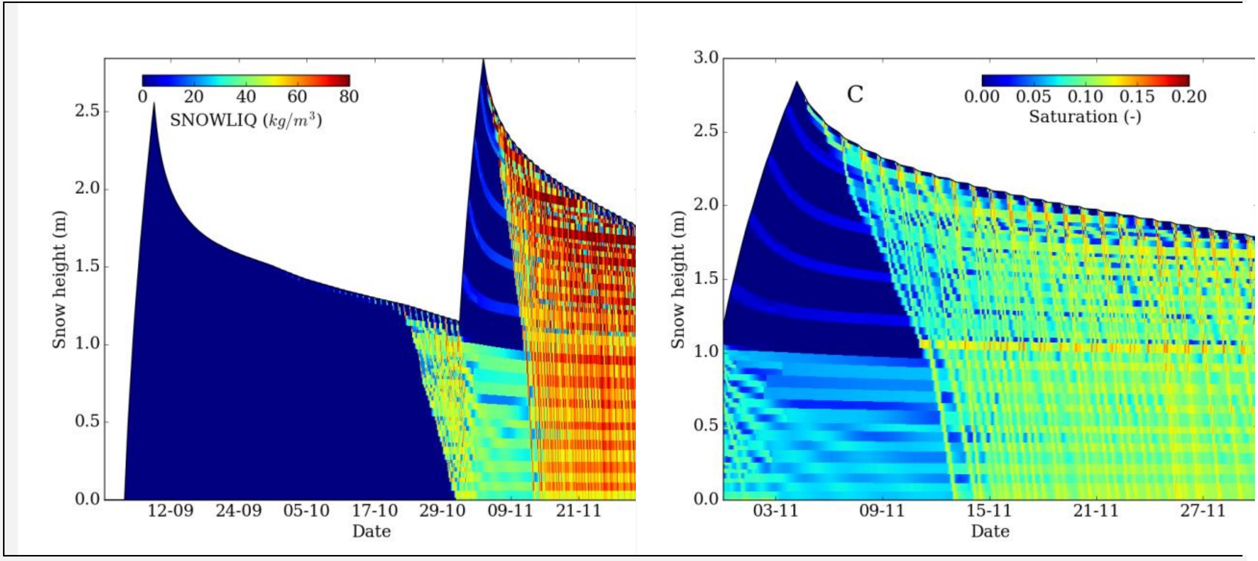


Figure shows results from restricting LWC at the C13 meatmorphsim routine the wet snow viscosity in the compaction routine to the bucket routines limit of 5% volume of pores space. Mass of water in compaction routine was not restricted to 5%.

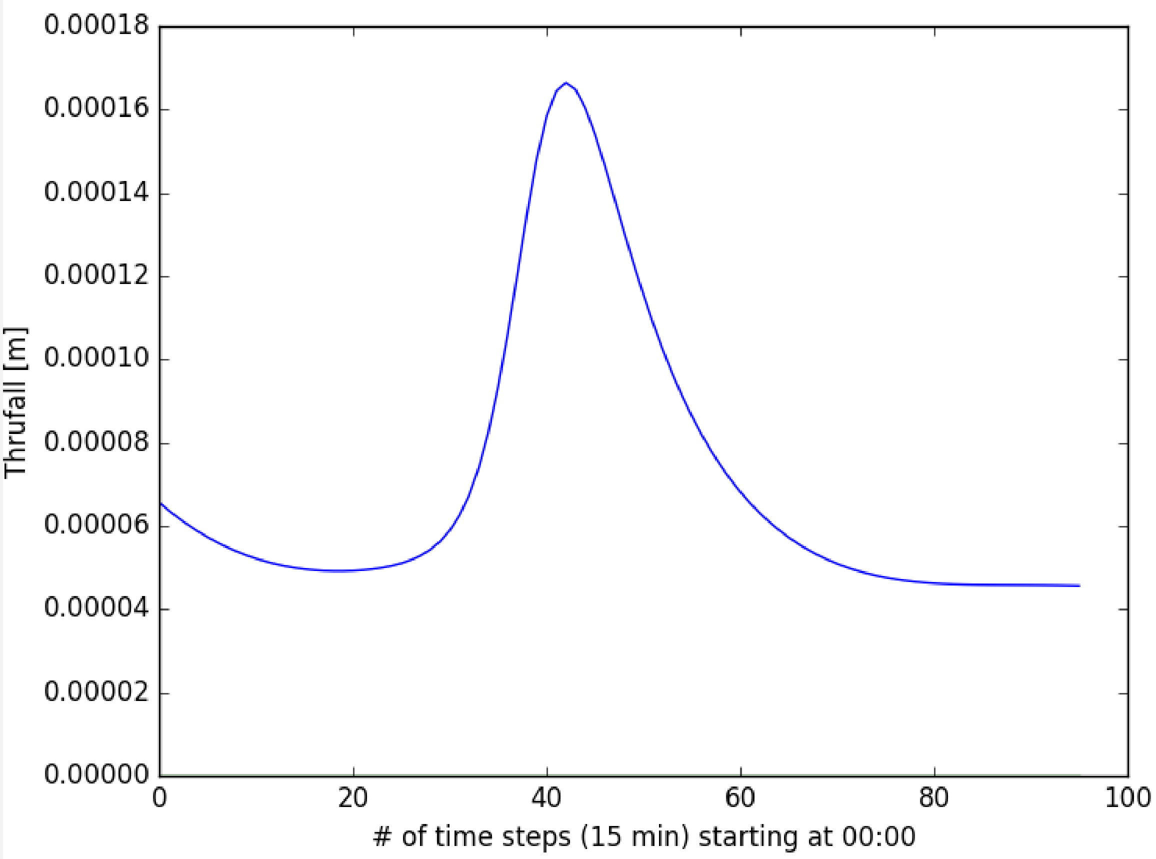
We have added section 5.4 and 6.4 on the feedbacks that exist between the parameterizations and the compaction and metamorphism routines.

3) I asked to mention the CPU time needed somewhere in the manuscript. I do understand the point of the authors that many factors influence the CPU time, as for example the file output resolution. However, I still think it is very important for such model/numerics description papers to give readers an idea of the computational burden of the proposed model/numerics improvements. So maybe the authors can just provide the relative extra CPU time to use the new Richards equation scheme over the old bucket scheme, or at least state that the CPU time needed for the Richards equation scheme is of the same order of magnitude as the bucket scheme, as it seems to be. Note that it is important to consider that a switch to 1-dimensional Richards equation is generally accompanied by a very acceptable increase in computational time, in contrast to the three-dimensional snow models recently proposed (for example by Hirashima et al. 2014, Leroux and Pomeroy (2017)), which are accompanied by such a large increase of computational time, that a useful application on seasonal time scales or large spatial scales on real natural snowpacks is not (yet) feasible.
We added a paragraph at the start of the discussion (section 6) that compares CPU time of Richards and the bucket routines.

4) I made the remark that a plot of snowpack runoff is very useful. The in response

provided plot of the soil moisture in the upper soil layer is also interesting, but not so informative when it comes to snowpack runoff. The reason I asked about runoff is that in snow modelling, many researchers are interested in the hydrological aspects, mostly snowpack runoff. It should just be easy to plot it, as it results directly from the free-drainage boundary condition (just translate gradient in pressure head to the flux). Two important aspects that I hope show up is a shift later in the day of the arrival of the meltwater at the bottom of the snowpack, and a recession curve at night. This aspect is not a show-stopper for me, but it would just enhance the impact of the manuscript.

Here is a plot on the flux from the bottom of the snowpack for day 89 of the simulated data set, which is the next to last day of the simulation. There is no flux to the soil on day 89 of the simulated data set for the bucket routine. The bucket routine only passes water on the last few time steps of the simulation, and therefore we do not have a good reference for soil flux with the bucket routine.



We have not included this to the manuscript because we do not have validation data to compare this with. We cannot assess if there is a shift in peak water flux from the plot above but Fig. 8 of the revised manuscript shows a delay in the diurnal water fronts through the snowpack. We do notice a decay of the water flux during the night.

5) Fig. A in the response-to-review document is very important and I strongly recommend to take it into the manuscript. However, Fig. A shows one confusing thing: after passing of the melt water front, a significant amount of liquid water (typically 2-3%) should be held in the capillaries, although the plot seems to suggest that the water content after passing of the melt water front falls back to almost 0 in some layers. One suspicion I have is that Eq. 7 is not implemented correctly. As far as I understand the code, Eq. 7 from the manuscript translates into the following source code line:

```
ZTHETA_R= MIN(ZTHETA*.75, 0.02)
```

However, this is not consistent with Eq. 7. It should translate into:

```
IF(ZTHETA.LT.0.02)THEN
ZTHETA_R=0.75*ZTHETA
ELSE
ZHETA_R=0.02
ENDIF
```

The latter approach is in my opinion the better one. Otherwise, a condition can occur that even when theta>0.02, theta_r is reduced below 0.02, after which theta gets smaller, after which theta_r is reduced, and this continues all the way to 0, although we know that a bulk liquid water content for a wet snowpack is typically more than 2%. Note that for numerical stability, it may be better to write something like IF(ZTHETA.LT.(0.02 + theta_min)), such that ZTHETA is always significantly larger than ZTHETA_R, which is required in the van Genuchten model.

This has been corrected and the figures in the manuscript have been updated to reflect this. This correction keeps snow layers above theta_r after the first substantial wetting. The conclusions drawn (about feedback) using “min theta_r” are not affected by this correction. We found that theta_min could be changed form 10^{-6} to 10^{-5} without changing the timing of the warming front (figure C1 and appendix C of the revised manuscript), so the default value has been changed, because 10^{-5} makes for a slightly faster CPU run time.

6) The discussion about the time steps inside the Richards equation solver in combination with the mass balance check, as provided in the response-to-review document, should be present in the manuscript I think, in a much more condensed form of course. It is important to note in the manuscript that the mass balance error is acceptable. Otherwise, I can imagine that other readers will also feel that the allowed mass balance error is large compared to the minimum allowed time step.

We added some sentences to Appendix A where the mass balance error is discussed.
Some remarks about the manuscript:

7) Section 6.2.2: I think the authors are too negative about the free-flow bottom boundary (p13,l10-11). First, in most snow models, the outflow from the snowpack is not at all constrained by the underlying soil. The bucket approach in CROCUS is probably also not taking into account the conditions of the underlying soil. One can always argue that the soil module of ISBA should take care of the incoming water flux from the snowpack, i.e., decide if it infiltrates into the soil, or creates overland flow. Note that in reality, a frozen, saturated, or extremely dry soil can have such a reduced infiltration capacity, that meltwater from the snowpack creates a significant amount of lateral overland flow, and thereby constitutes a significant flood risk. I agree with the authors that the approach of SNOWPACK to solve the snow-soil continuum at once has the advantage that these processes can be adequately captured, and with the SNOWPACK model, we are indeed able to reproduce melt pond formation from snowpack runoff in case the soil has limited infiltration capacity. On the other hand, one looses the sophisticated coupling some hydrological models have from the unsaturated zone to the aquifer and streamflow (I'm not sure how this is with ISBA). I think in section 6.2.2., the authors may want to discuss some of these aspects there.

Section 6.2.2 has been updated and is less negative, however one of the major motivations for this study by the Crocus team was to better couple the soil and snowpack routines.

8) Prewetting amount: If I understand correctly, Fig 8 A and D should be identical with Fig. 10 A and B, as both have the same prewetting amount of 10^-5? Yet, there is a clear difference, but it is not clear where this originates from. Is it a typo in the caption of Fig 8, that it should actually be 10^-6? Or what else changed between both simulations? Another issue here: sections 5.3 and 6.4 now fail to explain why the prewetting is so important, but I think the reason is that when it is set too high, hydraulic conductivity becomes already so significant, that water percolates, even when we should still consider the snowpack "dry". When more water is added after the prewetting and this is refrozen every time step, heat is advected. If authors agree with this explanation, they may consider adding it to the manuscript. Note that in SNOWPACK, we don't refreeze prewetting water every time step. We have hysteresis, i.e., the threshold for executing phase changes is a factor 10 larger than the prewetting amount. I think a similar approach in CROCUS would help to reduce the warming effect. But I consider this something for future work. In my opinion, the discussion of the sensitivity of the pre-wetting amount is not so interesting, and as I suggest to add figures describing the influence of switching on/off the compaction routines, this section and Fig. 10 could be removed to save space, if the authors wish to do so.

Yes there is a typo here. We did not want to include identical figures so the figures should be one order of magnitude above and below that what we pick as default. We think that the pre-wetting amount is still important as it is a major difference between solving Richards equation in soil and snow. Pre-wetting amount is also an unphysical parameter that was used so we feel that it is important to show a sensitivity test on it. Since the results of the sensitivity test do not show that the variable is sensitive below a threshold value, we chose to include this in Appendix C.

Technical corrections (line numbers refer to revised manuscript):
(Note that I think that some of the technical corrections should have been identified before submission by a proper proofreading by author and co-authors.)
- p1,l11: "thought" -> "through"
This has been changed in the revised manuscript.
- p1,l12: add gravity: "capillary suction, gravity and hydraulic conductivity"
This has been changed in the revised manuscript.
- p1,l15: "coved" -> "covered" (?)
This has been changed in the revised manuscript.
- p1,l22: "The parameterization ... crust layers." This is an assumption (although a well justified one) and not really supported by data in the manuscript, so I think this sentence is misplaced in the abstract.
This has been taken out of the abstract.
- p1,l30: either remove comma or write: "time consuming, and LWC"
This has been changed in the revised manuscript.
- p1, l30-31: "change over timescales that are"
This has been changed in the revised manuscript.
- p2, l2: remove comma: "rescue workers have reported"
This has been changed in the revised manuscript.
- p2, l26: "due to suction and sloping terrain and water pooling"
This has been changed in the revised manuscript.
- p2, l29: "and heterogeneous" -> "as well as heterogeneous"
This has been changed in the revised manuscript.
- p3, l22: "however" -> "although"
This has been changed in the revised manuscript.
- p5, l2: "found the speed"
This has been changed in the revised manuscript.
- p6, l12: note that Eq. 7 is not a continuous function, there is a break at theta=0.02. Either make the function continuous, or say "a piecewise function".
This has been changed in the revised manuscript.
- p6, l17: I suggest to write: "which corresponds to a minimum pressure head, for which it holds for every dry snow layer that the liquid water content is smaller than a prescribed minimum value \theta_min."
This has been changed in the revised manuscript.
- p6: Eq. 8 and 9 uses different symbols to indicate multiplication. The "x" is not adequate for scalar multiplication.
This has been changed in the revised manuscript.
- p6, l28: add comma "is a complex system, it is"

This has been changed in the revised manuscript.
- p7, l2: Eq 8 should refer to Eq. 10? (Occurs twice)
This has been changed in the revised manuscript.
- p7, l7: add comma "computations, the following"
This has been changed in the revised manuscript.
- p7, l8: I suggest "There must be a substantial snowpack: if there are less than 3 layers"
This has been changed in the revised manuscript.
- p7, l9: The sentence is incomplete.
This has been changed in the revised manuscript.
- p7, l26: "borders"
This has been changed in the revised manuscript.
- p8, l25: I would move the sentence "The lower boundary is the soil-snow interface." Before starting the paragraph: "There are two options". Otherwise it is not clear that the bottom boundary condition refers to the snowpack, and not the soil.
This has been changed in the revised manuscript.
- p9, l2: Eq. 10 should point to Eq. 11?
This has been changed in the revised manuscript.
- p9, l24: For clarity, I suggest: "The peak shortwave radiation"
This has been changed in the revised manuscript.
- p10, l20-21: This is not so clear. I suggest: "causes the surface layers to get wet, while deeper layers remain below freezing".
This has been changed in the revised manuscript.
- p10, l24: "shows the formation"
This has been changed in the revised manuscript.
- p11, l4: I would write: "pore space that is filled by water (i.e., the saturation) ...", as "saturation" is the term used in the figure.
This has been changed in the revised manuscript.
- p11, l21-22: This sentence is not grammatically correct. Should be something like ".. is drastically different .., although the timing ..."
This has been changed in the revised manuscript.
- p12, l4-5: "Routines such as for compaction and grain metamorphism were ..."
This has been changed in the revised manuscript.
- p12, l14-20: I guess the authors do not plan to print this in bold.
This has been changed in the revised manuscript.
- p12, l18: "modles"
This has been changed in the revised manuscript.
- p12, l25: I would start this sentence with: "When using the upper soil layer as lower boundary, the hydraulic conductivity" to make clear that this is in contrast to the free-flow boundary.
This has been changed in the revised manuscript.
- p13, l8: "there is there is"
This has been changed in the revised manuscript.
- p15, l6: add comma "snow layer, snow"

This has been changed in the revised manuscript.

- p15, l8: I think a nicer formulation would be: "are the only crystal type that needs to be described by a water retention curve."

This has been changed in the revised manuscript.

- p15, l13: note that a recent study shows the effect of hysteresis in snow, a study that I recommend citing here: Leroux and Pomeroy (2017).

This has been added to the revised manuscript.

- p16, l5: "The parameterizations"

This has been changed in the revised manuscript.

- p15, l7 and p16, l9. Note that the abbreviation MF is not introduced, but as it is only used twice, I would recommend to write "melt forms" in both cases and not use an abbreviation.

This has been changed in the revised manuscript.

Fig 9 and 10: Make the captions more in line with Fig. 8, for example: "Crocus output for Neverland forcing using different time steps ..."

This has been changed in the revised manuscript.

Fig 10: Subfigure B has different label, and wrongly formatted. I prefer the B style, that the labels show everyhwere "theta_min = XXXX"

This has been changed in the revised manuscript.

Fig 10: The caption only indicates what A and B refer to, but not C and D, and according to the figure itself B is also with pre-wetting 10^-5 and not 10^-7 as is written in the caption.

This has been changed in the revised manuscript.

References:

Hirashima, H., Yamaguchi, S., and Katsushima, T.: A multidimensional water transport model to reproduce preferential flow in the snowpack, Cold Reg. Sci. Technol., 108, 80-90, doi:10.1016/j.coldregions.2014.09.004, 2014.

Nicolas R. Leroux, John W. Pomeroy, Modelling capillary hysteresis effects on preferential flow through melting and cold layered snowpacks, Advances in Water Resources, Volume 107, 2017, Pages 250-264, ISSN 0309-1708, http://dx.doi.org/10.1016/j.advwatres.2017.06.024.