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# *Interactive comment on* "Implementation of a physically based water percolation routine in the Crocus (V7) snowpack model" *by* Christopher J. L. D'Amboise et al.

**N. Wever (Referee)**

nander.wever@slf.ch

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## 1 General comments

The manuscript by d'Amboise et al. discusses the implementation of a solver for Richards equation into the detailed, multi-layer Crocus snow model. This equation describes water flow in porous media, such as snow, and previous studies have already shown that snowpack models can benefit from implementing this equation. It generally seems to provide a better representation of liquid water content (LWC) distributions and snowpack runoff behaviour. In that sense, the study represents an important step for the Crocus snow model. Also the study can be considered an important indepen-



dent verification of the results achieved with the SNOWPACK model, where the solver for Richards equation has been found to considerably improve the description of liquid water flow in snow in several aspects. I found the manuscript well written and pleasant to read, but there are also some language and grammar issues (see technical corrections below). I value and appreciate the effort undertaken by the Crocus team. Based on my experience, I know that introducing new routines to a snowpack model with such a big impact as a new water percolation routine is a serious and difficult piece of work.

My first main concern with the study presented by the authors is that the results presented here are not convincingly showing that the model behaves numerically stable. Distributions of liquid water content look different from distributions achieved with the SNOWPACK model. The absence of a comparison with field data of profiles of liquid water content or snowpack runoff makes it impossible to judge the validity of these results. I will do a few suggestions for additional verification of the numerical scheme, which I hope will provide convincing evidence that the model behaves numerically stable. My second main concern is that the general message of the manuscript is not clear and very open and may potentially confuse readers (see below).

## 2 Specific comments

I have the following remarks related to the numerical scheme:

1. Especially the alternating wet and dry snow layers shown in Fig. 9 and 11, and discussed in p10,l27, are very suspicious. It looks like a numerically oscillating solution. If it is a true LWC distribution, it is recommended to have a higher vertical resolution in the simulations (i.e., more snow layers) in order to better represent the strong gradients between the wet and dry snow layers. But the simulated values of 10%-15% seem unrealistic. Such high values may occur oc-

casionally above capillary barriers, as shown in the work by *Avanzi et al.* (2016), but I'm not convinced that it should happen so regularly in the simulations shown in Fig. 9 and 11. Particularly because the artificial large snow falls create a very homogeneous stratification, such that ponding is not expected to occur. So actually I wonder if this is not a representation of the fact that the Crank-Nicolson scheme can be prone to spurious oscillations? As far as I know, Crank-Nicolson schemes are generally considered globally stable, but irregular initial conditions may lead to oscillatory behaviour. The current simulations are done with only the optimal time step for Richards equation. But are the model results sensitive to the time step inside the Richards solver? If the model is forced to run with much smaller time steps, are the results different? Or if the model is run with higher grid resolution, or by switching of remeshing, are the results different? As far as I know, the oscillations from the Crank-Nicolson scheme can be reduced by smaller time steps and/or higher grid resolution. Also stability criteria for Crank Nicolson schemes exists, which could be discussed by the authors. Note that I also have a suggestion to initialize the model more stable, see point 6 below, which may also improve numerical stability. Maybe if possible also provide additional motivation for the choice of a semi-implicit scheme instead of a fully implicit one. Is the discretization (Crank-Nicolson + Picard iteration for Richards equation) used here newly developed in this work or has it been applied before? If so, please add the references.

2. The mass balance is verified in the Picard scheme with a threshold of $10^{-4}$. The authors should add units here, but for now I assume it is the mass balance error in $m^3/m^3$ or $kg/m^2$. I think that this value is set too large to judge mass conserving behaviour of the model. The minimum time step in the solver is $10^{-10}$ s (p16,l28). If the solver has a mass balance error of $10^{-4}$ with a time step of $10^{-10}$ s, this implies a mass balance error of $1^6$ $m^3/m^{-3}/s$ or $1^{10}$ $kg/m^2/s$, or $1^6$ $kg/m^2/s$, depending on the units. But this is a potentially large mass balance error! This



means that if bugs in the numerical scheme or in the implementation of boundary conditions exist, the solver can "cheat" upon the mass balance check by choosing small time steps. Note that in the current version of SNOWPACK, this is also possible. During development of the solver we were particularly paying attention to the smallest time step in combination with the mass balance check. However, we relaxed the condition, by setting a low minimum time step, also allowing the solver to "cheat" the mass balance check. The motivation is to have a more robust solver for end-users. For this review, I analysed the time step distribution for running 15 years of Weissfluhjoch simulations using Richards equation with SNOWPACK, and the smallest time step during this period is about $2 \times 10^{-5}$ s. The maximum allowed mass balance error in SNOWPACK is $1 \times 10^{-10}$ $kg/m^2$ for the entire model domain. The combination of smallest time step found in this simulation setup, together with the mass balance criterion gives $5 \times 10^{-6}$ $kg/m^2/s$. As can be seen in the Fig. 1 below, these small time steps happen very seldom for 15 years of simulations.

3. A check of the second norm of the deficit vector could help to verify the correct implementation of the matrix inversion to solve the equation. Given: A . b = x, where x is the new solution of the pressure head, then the deficit vector is defined as d = A . b - x, where d is the deficit vector. It should hold that the second norm of the vector, in case of correct implementation, is (close to) 0. Note that in an attempt to optimize the execution time of the SNOWPACK model, we removed the deficit norm check from the code, after using it first to successfully verify a correct implementation of the solver using this check.

4. An overall report for the Crocus model as a whole of the mass balance may also be necessary to verify a correct implementation: Delta SWE = evaporation/condensation + sublimation/deposition + snow/rain fall + runoff?

5. With the numerical scheme for Richards equation in the SNOWPACK model, we

found that an improved stability was achieved by initializing dry snow layers (the authors call it "prewetting") based on pressure head instead of LWC. The chosen form of Richards equation uses gradients in pressure head, thus it may be better to reduce gradients in pressure head when initializing the dry snow layers. Therefore, we used the procedure to determine for the whole domain the lowest pressure head in a layer corresponding to a prescribed minimum LWC. This value of the pressure head was then used to initialize dry snow layers, such that only gradients due to gravity are present. It ensures that no snow layer is initialized with a LWC above the prescribed minimum value, while at the same time starting the simulation with a numerically stable pressure head distribution.

6. I noticed in the source code that C (dtheta/dh) is limited to -$1^{-15}$. For what reason? C is supposed to be the exact derivative of the water retention curve. An artificial cut-off seems unnecessary and may introduce mass balance errors?

7. Section 6.2 and p16,l5-6: why not implement a free-drainage boundary condition at the bottom of the snowpack, instead of all the trouble it seems to give to use the SURFEX upper soil layer? We recently modified the SNOWPACK model such that it can run Richards equation without soil layers, and using a free drainage boundary condition seemed to work well. In SNOWPACK, we implemented free-drainage by setting the flux at the bottom of the lowest snow element similar to the flux at the top of the element, while only allowing downward flux (otherwise setting flux to 0). In case of only one snow element, we set the flux equal to the hydraulic conductivity in this element.

I have the following remarks regarding the manuscript itself:

1. The message of the manuscript is ultimately very unclear and open. The authors apparently don't trust the new water percolation scheme enough to use it for validation (p16,l9). That basically indicates to readers that this publication is not



intended to encourage users of the Crocus model to use the new percolation scheme. But what is then the main message of the manuscript? What are the next steps to improve the trust in the validity of the new routine? Are there any further developments needed or planned? The authors should also provide clear instructions of how to repeat the experiments. I was able to download the source code, but did not find any manual or readme to compile nor did it seem to contain the necessary files to run the test cases.

2. I don't agree with the last sentence of the abstract. First, the absence of validation limits the value of any comment about applicability, but basically the same uncertainties in water retention curves for different snow types and for high density crusts, and also many of the feedback mechanisms are present in the SNOWPACK model too. Nevertheless, we have now demonstrated several times that solving Richards Equation is having usefull applications for the SNOWPACK model, in spite of all the uncertainties. For example for assessing wet snow stability (*Wever et al.*, 2016a; *Vera Valero et al.*, 2016) as well as in a detailed analysis of rain-on-snow events (*Würzer et al.*, 2017) and for reproducing ice layers (*Wever et al.*, 2016b). As shown in Table 1 in *Wever et al.* (2015), different water retention curves or different methods to determine the hydraulic conductivity at the interface nodes (arithmetic vs geometric) have limited influence on the statistics for runoff, whereas the statistics clearly improve over the bucket scheme. Therefore, I don't agree with the statement of "limited applicability" with the reasons provided in the rest of the sentence.

3. The discussion section is nicely written and provides an interesting introspective discussion about the uncertainties and potential feedback mechanisms in water percolation modelling. Note that, however, many of the feedbacks are hypothesized or based on results of other studies. The manuscript itself does not present material supporting or quantifying the strength of those feedbacks (no validation or sensitivity study). It would be good if the discussion could be made stronger.

For example, the authors may want to discuss how water retention curves for crusts potentially look like, and how strong this influences LWC distributions or snowpack runoff?

4. p4, section 2.1: Maybe explicitly state that the working of the bucket scheme in Crocus is very similar to the SNOWPACK model. p4,l9: Is the bucket size always fixed to 5% of pore space? The sentence following this sentence is a bit confusing, as if there are additional constraints. Should the sentence "For Crocus, ..." not better read "This makes the holding capacity proportional to the density of the snow layer, *but* independent of snow grain type or surrounding environment." It it also not clear what is meant by "surrounding environment" and how it could potentially influence the holding capacity?

5. p8,l14: please provide a bit more detail on how the amount of evaporation is determined. Atmospheric forcing only provides the latent heat flux, which needs to be partitioned in evaporation and sublimation. How is Crocus doing it? Note that a reason for numerical problems with Richards equation can be when the prescribe evaporation flux exceeds the available water. For this reason SNOWPACK employs a system where the evaporation cannot exceed the amount of water available in the upper element plus the amount of water that can be advected from below given the hydraulic conductivity there. Similar for influx, although typically unrealistic large rainfall rates ($>>$ 200 mm/hr) are necessary to exceed the absorption limit in snow of liquid water. In reality melt ponds form in snow only when liquid water cannot leave the snowpack below, not because the water input rate exceeds the snow absorption capacity.

6. p13,l20: "such that the criteria to enter the percolation routine has been met." This is very confusing at it is for the first time mentioned that there are criteria whether or not to enter the percolation routine. Which criteria are meant here?

7. p14,l13: Many examples can be found to show that preferential flow has a much



smaller typical spatial scale. See for example Fig. 2 in Techel and Pielmeier (2011), or Fig. 1a in *Würzer et al.* (2017). Many other examples can be found in literature.

8. p15,l2-3: "this claim needs validation": I think it depends on the application. During the first wetting, grain shape will probably play a very important role. New snow getting wet probably retains much more liquid water initially than the water retention curve developed for melt forms will provide. For wet snow avalanche prediction, the first-wetting is often considered of crucial importance and I think improvements in the description of water flow in new snow and faceted snow (generally less shear strength) are required. On the other hand, for many hydrological applications, often the runoff behaviour during a melt season is important, for which the assumption of melt forms is justified.

9. p15,l19: This is a bit confusing wording, as principally, I would say that snow layers are initialized with the "pre-wetting" amount. But here, it is probably meant initialization when the routine is being called during a Crocus time-step.

10. p15,l26-28: Although I agree with the statement, it cannot be considered a conclusion of *this* work. It has not been demonstrated that the water flux over the crust is over- or underestimated (no validation done), neither has it been shown that the simulations are sensitive to the hydraulic parameters for crusts (no sensitivity study done).

11. Fig. 5: This figure is only mentioned once in the manuscript, and is not discussed at all. Please discuss the agreement between model and observation, or remove the snow profile.

12. I think the manuscript should not only show results for LWC distributions inside the snowpack, but also snowpack runoff.

## 3 Technical corrections / minor comments:

- General: note that *Calonne et al.* (2012) determined permeability, which can be related to conductivity. But in principle they did not publish conductivity experiments.
- General: please mention somewhere the CPU time needed to run the simulations, compared to the bucket scheme.
- General: there is a change of tense sometimes, compare for example section 4.3 with 3.4.
- General: sometimes "Figure" and sometimes "Fig." is used to refer to figures.
- Abstract: "this routine is based on". Why the wording "based on"? I would write here: "this routine solves Richards equation".
- p2,l2-3: note that simulations using Richards Equation have already been used for the assessment of wet snow avalanche activity (*Wever et al.*, 2016a) and for determining the initial conditions for wet snow avalanche dynamics simulations (*Vera Valero et al.*, 2016). Given that the authors discuss this topic in particular, they may consider citing these studies here.
- p2,l16: This would not be the way I would explain the precondition for flow fingering, but I also have not the evidence to object against it. Maybe verify with *DiCarlo* (2013)? I think this is a more up-to-date citation that may be cited here aswell.
- p3,l3: "Greenland Ice Sheet" (capitalized)
- p3,l8: "ice crusts": in line with the International Classification (*Fierz et al.*, 2009), this should be "layers". The mentioned study concerned more with ice layers than



with ice crusts. Similar p3,l4: lenses are discontinuous ice layers. In this case, I think it should be layers rather than lenses.

- p6,l19: "kr" should be $k_r$.
- p7,l17: Here and elsewhere: I prefer "conductivity of snow"
- p7,eq 13: I assume the second equation should read $\Delta z_{bot}$
- p7,l14: I assume the reference should point to Eq. 13 instead of 11.
- p7,l24: citation style of Celia et al. is wrong (without parenthesis)
- p8,l3: "computation step" is a vague term. I think this refers to the iteration level k+1? Maybe write: "the pressure head h at iteration level k+1.".
- p8,l29: maybe specify: "Air temperature become as cold ..."
- p9,l14-15: please rewrite sentence
- p10,l2: wrong figure reference
- p11,l10: "there is a complicated one-to-many relationship ..." Actually it seems to be very simple: below 2x10$^{-5}$ there seems to be almost no effect, so the prewetting should just be below this value...
- p11,l14-15: I understand what is meant here, but it may be unclear for readers without a strong snow modelling background. I would explain the remeshing procedure in the model description.
- p12,l10: typo: "witch"
- p12,l21: "simulated data sets simulation" I suggest "synthetic data sets simulations"

- p13,l17: "and but does not have" please reformulate
- Appendix A: This time stepping is method is very similar to the one I used, and I based it on the work by Paniconi and Putti (1994). Maybe give them credits by citing their work?
- p14,l19: "on visual grain size measurements"
- p15,l7: "where, " —> ", where"
- p15,l11: "grain" —> "grains"
- p16,19: "is used to deal"
- p16,l24: "criteria are met"
- p16,l28: "within"
- p17: eq. A1 is not numbered as such
- p17,l13: "lower density snow that found": please reformulate.
- p17,l17: "density not included"
- p17,l19-20: please reformulate. This sentence cannot start with "while".
- Appendix B.3: Note that it should read "Daanen" and not "Dannen".
- References: a few still point to discussion papers, where final papers have already been accepted and published, for example: Avanzi et al. (2015) and Wever et al. (2016). Please provide DOIs consistently when available.
- Fig. 5: Specify here also from which date the snow profile is.
- Fig. 7: subfigure B is wrongly labelled C



## 4 References

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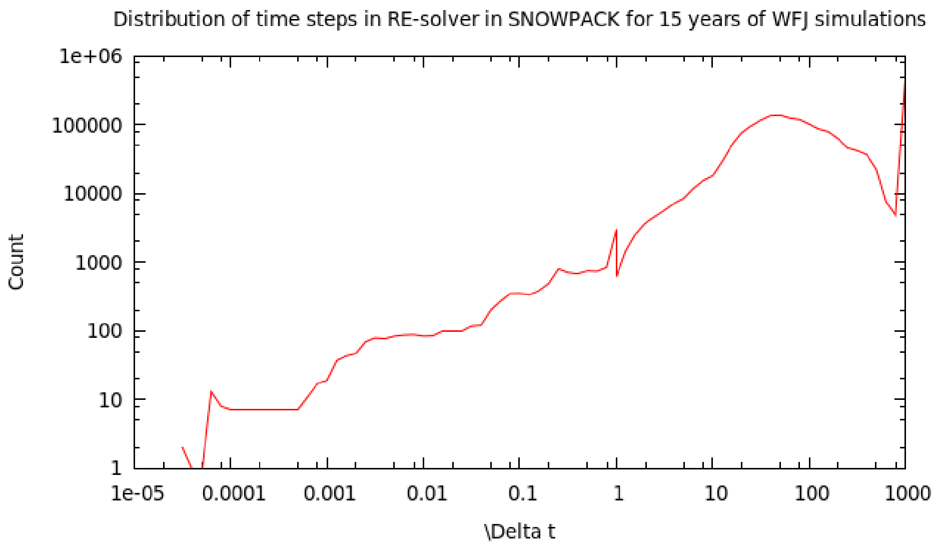


**Fig. 1.** Time step histogram in Richards equation solver in the SNOWPACK model for 15 years of WFJ simulations.