The Cryosphere Discuss.,
doi:10.5194/tc-2016-55-RC2, 2016

The Cryosphere Discussions
Open Access
EGU

Interactive comment on "A 2D model for simulating heterogeneous mass and energy fluxes through melting snowpacks" *by* N. R. Leroux and J. W. Pomeroy

Anonymous Referee #2

Received and published: 28 April 2016

The paper describes a 2D snowpack model, that solves heat and mass equations (Richards equation), in order to assess the liquid water flow in snow, with a particular focus on preferential flow. The model can be regarded as an experimental model, as some important processes found in natural snow covers are not represented (i.e., snow settling or snow metamorphism), which is not an immediate problem for the study presented in the paper. The novelty in the model approach is a coupling between heat and mass equations (taking into account phase changes), whereas the principles behind, and description of, the formation of preferential flow in snow are mostly known from earlier studies. Regarding this point, it basically is a re-implementation of previous studies. This is not necessarily a problem, as independent verification of results

Printer-friendly version

Discussion paper

is a central part of science. However, my feeling is that the paper in some aspects just falls short of providing the important results that could be achieved by using the model described by the authors. Two things come in mind: first, some comparison with field data or laboratory data (there is a lot out there in datasets or publications of laboratory experiments) to validate the model results with "real" snow covers. A second alternative route is to better set up the sensitivity study, such that a connection with natural snow covers is made. I will provide some extra explanation regarding this point below. But to summarize, it is basically not clear whether the sensitivity study was supposed to span the typical variations of snow cover properties in natural snow covers, or is representing typical measurement errors. So in its current form, the manuscript is neither providing the validation with field data, nor those results from the sensitivity experiments that may serve other researchers. Furthermore, I do like concise papers, and this paper is generally concisely and pleasantly written, but in many crucial details, its lacking the necessary information to understand the work (see my comments below). Nevertheless, the study contains relevant results, and is also timely (the interest in liquid water flow in snow seems to have a new boost since a few years), potentially fitting well in ongoing studies and discussions. However, in my opinion, it requires a thorough revision in order to make the manuscript more mature for publication.

I actually think that the authors did do a very good job in constructing the numerical model, which was probably not easy to achieve! I can imagine that it involved some hard and dedicated work, and I think that the model is forming a solid framework for future studies. In this light, I would like to encourage the authors to release the source code as open source to the community. This would also further enhance the importance of the study.

Major comments:

- The authors apply random perturbations to the snow cover properties in the order of 1%. There is no motivation provided by the authors why this value was chosen. Also no information about the procedure to apply the perturbations is provided (is it

Gaussian?). It seems a significantly smaller perturbation than used by Hirashima et al. (2014) and, more importantly, Hirashima et al. (2014) found that the results are strongly dependent on the applied perturbation! Right now, this is not at all discussed in the manuscript.

- I have doubts whether Eq. 8 is correct. Actually, it seems to be very similar to Eq. 15 in Hirashima et al. (2014), where it is a modification of the equation found by (Baker and Hillel, 1990), see Katsushima et al. (2013) for details. So I wonder whether the provided citation (Rooji and Cho (1999)) by the authors is correct. Furthermore, given that diameter = 2*radius, the 2 in Eq. 8 should be in the denominator, following the equation provided by Hirashima et al. (2014).

- A similar problem is found in Eq. 7, where I think the factor 2 should be in the denominator. It is recommended that the authors verify their implementation in the code, as the simulations may change considerably for these type of errors.

- Section 3.2: please make a distinction between approximations and unknowns. For example: I think it is not justified to claim that changes in grain size were not simulated, because (L3) "due to the lack of complete understanding of the physics of these processes, ... The assumptions made in this model also indicate the current knowledge...", because the SNOWPACK model for example is simulating grain growth in the presence of water based on the results by Brun et al. 1989b (INVESTIGATION ON WET - SNOW METAMORPHISM IN RESPECT OF LIQUID- WATER CONTENT), Ann. of Glaciol. 13. So I do understand that in this version of the model, the authors neglect grain growth, but in my opinion, it is a misrepresentation to claim that it is necessary due to the lack of understanding. Similar for point 3. This assumption is made for convenience. I can agree with the assumption, but it should not be implied that this is due to the lack of understanding.

- Section 4.2 is for me a bit problematic. Colbeck (1979) indeed found that preferential flow paths in snow persisted after forming. This probably is due to changes in

Printer-friendly version

Discussion paper

snow microstructure. A counter example is provided by Schneebeli (1995), a reference which deserves citation here. Using dye tracer, he found that actually preferential flow paths are not (necessarily) constant in space and time. Right now, section 4.2 in the manuscript is actually missing a kind of concluding remark, but it sounds like the authors claim that their model reproduces persistence in the PFP. But in my opinion, the persistence of the preferential flow paths in their model is likely there, because the random perturbations did not change, as the model by the authors do not have a microstructure model in their snow model. Then it is a kind of: "getting the right results for the wrong reason". So I think the section may need to be removed from the manuscript, or else it should be much better defended why the model by the authors is congruent with the observations by Colbeck (1979), and not those by Schneebeli (1995). Which part of the physics in the model is confirming the result by Colbeck (1979) and not the result by Schneebeli (1995)? Reference: MARTIN SCHNEEBELI Biogeochemistry of Seasonally Snow-Covered Catchments (Proceedings of a Boulder Symposium July 1995). IAHSPubl.no. 228,1995. Development and stability of preferential flow paths in a layered snowpack.

Major comments regarding sensitivity study:

- It is not well motivated where the sensitivity study is based on. For example, snow density is varied by 10$\%$, which one can regard as the typical accuracy with which snow density can be determined in the field. However, the range of densities found in a natural snow cover range from roughly 100 kg/m^3 for new snow to 400-500 kg/m^3 for old snow and up to 600 kg/m^3 for firn. So here, the sensitivity study seems to capture measurement error rather than the range of values found in natural snow covers. On the contrary, the sensitivity study for temperature ranges over 10 degrees. This is the opposite, rather capturing the natural variability found in snow covers than measurement errors.

- A similar comment can be made about the sensitivity study for α and n. Where is the choice of a variation of +/- 10$\%$ based on? As α and n are coupled via ρ/d (see

Yamaguchi et al. 2012), it is doubtful whether it is an informative result to vary both coefficients separately. I think in the end it is important how much the water retention curve changes. When n is small (1-2), 10$\%$ causes a big change in water retention curve. When n is > 5, the effect is much smaller. Opposite with α. When α is large, a 10$\%$ has more influence then when α is small. So just modifying α and n independently, for just one value of ρ/d, is not so informative.

- Also grain size is varied over only a very small range. However, grain size has a very important effect on the area that is involved in preferential flow (see for example Katsushima et al. (2013) and Hirashima et al. (2014)). Can the SMPP model reproduce these results?

- I can understand the confusion with the irreducible water content. It is true that a similar term is used in the Marsh and Woo papers (1984a,b), although they use the term saturation. It is also true that they used a value of 0.07. However, I do not think that this value is comparable to the role of the residual water content in the water retention curve, where it basically is the lower asymptote of dry conditions. My interpretation of the value used by Marsh and Woo is that the irreducible water saturation is actually the value of S_w (the water saturation) in Equation 1 in the Marsh and Woo (1984a) paper. The saturation is defined between 0 and 1 where 0 is dry snow (or to be precise, snow at residual water content), and 1 is all pores are filled. That means that is should be scaled with the porosity to get the volumetric water content, which would be comparable to the residual water content as used by Yamaguchi et al. (2012). Assume a typical porosity of 0.6 for snow, the irreducible water saturation would translate into a irreducible water content (volumetric) of around 4$\%$. Furthermore, my interpretation of their definition of irreducible is more in a bucket type approach, i.e., a typical amount of liquid water that remains in the pores without significant amounts of water flowing, which is not necessarily equal to the dry limit of the van Genuchten water retention curve. In a bucket scheme, typically a value of 4$\%$ is used (see for example Wever et al. 2014). Note that field measurements of bulk liquid water content typically ranges from 0.02-0.04 (see Heilig et al. (2015), a reference that deserves citation in this manuscript).

Although I also think that the residual water content as used in the water retention curve is likely grain shape and/or grain size dependent, the range used in the sensitivity study (1-10$\%$) doesn't seems to be realistic, given the observational evidence in literature. This is again an example where the choice of range for the sensitivity study is not well motivated, and is actually much larger than for snow density, given the typical range for these properties you will find in nature.

As the reference list provided by the authors is not clear at this point, the 1984a paper is for me: Wetting Front Advance and Freezing of Meltwater Within a Snow Cover 1. Observations in the Canadian. Reference: Heilig, A., C. Mitterer, L. Schmid, N. Wever, J. Schweizer, H.-P. Marshall, and O. Eisen (2015), Seasonal and diurnal cycles of liquid water in snow - Measurements and modeling, J. Geophys. Res. Earth Surf., 120 (10), 2139-2154, doi: 10.1002/2015JF003593.

Minor comments:

- Figure 3 is not really informative, as it is not at all clear if the change in runoff with or without preferential flow has any correspondence with reality. Moreover, the choice to only show the result for density is somewhat arbitrary.

- Figure 7: the 4th column of graphs, the title is suddenly expressing the time in seconds, not in hours/minutes.

- P3L17: "A melting snow cover can be considered a moving boundary"

- P2L28. Should this not read "the minimum suction"? I guess it depends on how positive/negative suction is defined?

- Eq 3 is not Richards equation, but just mass conservation. Richards equation is combining mass conservation with Darcy-Buckingham's law. Eq. 3 is valid under many more definitions of the flux q, of which Richards equation is a special category.

- Note that Darcy's law is basically the formulation for saturated flow, where the Darcy-Buckingham's law is valid for variable saturation, by introducing a water contents dependence on the hydraulic conductivity.

- P4: to be precise: Calonne et al. (2012) developed a relationship for permeability, which can be translated in saturated hydraulic conductivity.

- P4, L20: "is" -> "describes"

- P4: Eq 6 should be placed after L20-21.

- P4L25-26 should move to another place, as first the Equations need to be introduced.

- P5L13-14: It seems that here, dry snow is defined when volumetric LWC is below residual water content, and wet when it is above. Yamaguchi's formulation (i.e., the van Genuchten water retention curve) as far as I know is not applicable at all when LWC is below residual water content. So I don't understand this sentence.

- P6L15: "optimum grid size". Please provide the value for optimum grid size here. I'm also confused why there is no mention about the time steps? Convergence is often determined by both the time step and the grid cell size. Maybe also mention on what type of computer the simulations are run, and how much CPU time is needed for certain simulations, to give the reader an idea of the computational requirements of the model.

- Section 3.1 is confusing. It sounds as if the snowpack could be considered as being on a slope, where the left-hand side is upslope, and the right hand side is downslope (thus the specific choice of boundary conditions), but this is not explicitly explained and is a bit a puzzle right now.

- P6L26: Actually, free drainage boundary conditions are a type of Neumann boundary conditions.

- P6L27: I'm confused about the slope angle. When I'm correct, no result is shown that depends on slope angle? All the results seem to be for a flat snowpack. Furthermore, Eq. 4 is only valid for flat conditions, or when the snowpack is considered vertically (which makes the description of the boundary conditions a bit more complicated). Often in snowpack simulations, the snowpack is considered slope-perpendicular, in which case Eq. 4 needs a modification for the slope angle. Of course, it all depends on definitions of for example the z-coordinate. In any case, the manuscript is confusing at this point and some more clarification is needed.

- The issue which is addressed on P13L25 seems to be linked to the numerical scheme, but is also not addressed in the appropriate section. So now this point comes out of the blue in the conclusions.

References:

- Please provide DOIs, should be standard nowadays!

- de Rooji: should read de Rooij.

- The difference between Marsh and Woo, 1984a and b is not made in the reference list. Which one is which?

- The paper describing SNOWPACK, part II, is having the wrong author list.

- At least one reference is missing, which is cited in the text (Wever et al. 2014).

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-55, 2016.

Printer-friendly version

Discussion paper