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Interactive comment on "Solving Richards Equation for snow improves snowpack meltwater runoff estimations" *by* N. Wever et al.

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Reply to review #1 by S. Morin

General reply: We would like to thank Samuel Morin for his very positive and constructive comments about our study. Following his suggestion, we performed simulations for another alpine site (Col de Porte, CDP) with a seasonal snow cover. CDP is at a much lower altitude (1325m) than Weissfluhjoch (2540m) and as a consequence, the climate conditions at CDP are markedly different from Weisslfuhjoch (WFJ). CDP experiences extended periods of melt and a high frequency of rain on snow events. We state that wetting of the snow cover is occurring during the whole snow season, where at WFJ, wetting of the snow cover is concentrated in spring, and

in some years in the early start of the snow season.

The CDP simulations have shown that our assumption of a residual water content equal to zero ($\theta_\mathrm{r} = 0$), which was done to circumvent the singularities in the van Genuchten model, was not longer tenable. During wetting of the snow, our assumption of $\theta_\mathrm{r} = 0$ resulted in a stronger than observed outflow and too little amounts of water were held back in the snowcover. This became clear in the CDP simulations (when comparing runoff in the simulations versus the lysimeter) because of the extended melt periods at this site. For WFJ simulations, this effect is not so pronounced due to the shorter melt season.

We therefore introduced a different method to treat θ_r. We now define the residual water content on the current time step t ($\theta_\mathrm{r}^\mathrm{t}$) in the van Genuchten parameterization to be 0.5 times the current volumetric water content θ_w ($\mathrm{m}^3/\mathrm{m}^3$), limited between 0 and 0.02. So formally written:

$$\theta_\mathrm{r}^\mathrm{t} = \min\left\langle 0.02, \max\left\langle \theta_\mathrm{r}^{\mathrm{t}-1}, f \cdot \theta_\mathrm{w} \right\rangle \right\rangle, \tag{1}$$

where f is a tuning factor, currently taken as 0.5. The maximum value for θ_r of 0.02 was chosen to be equal to the residual water content determined in the experiments by *Yamaguchi et al.* (2010). It is possible, due to refreezing, that θ_w drops below $\theta_\mathrm{r}^{\mathrm{t}-1}$. In that case we choose a similar approach as when initializing dry snow layers (described in Appendix A3) by setting:

$$\theta_\mathrm{r}^\mathrm{t} = \theta_\mathrm{w} - \frac{\epsilon_\theta}{10}, \tag{2}$$

where ϵ_θ is the convergence criterion for θ_w.

We applied this method also for the Weissfluhjoch simulations. Also, we found the problem with the fast melt rates in spring when using measured albedo for WFJ. So now using the new approach to treat θ_r, we performed simulations for both WFJ and CDP with measured albedo, and achieve a reasonable agreement in measured and modelled snow height. With the current model setup, we think we optimize the use

of the available measurements in the datasets, thereby reducing uncertainties in parameterizations. The statistical measures we presented in the paper even improve marginally for WFJ.

For CDP, the general quality of the model results is lower than for WFJ, for example expressed by the root mean squared error in snow height. This was also noted in *Essery et al.* (2013), where it is discussed that in general, snow models have more difficulties with snow covers in locations where melting conditions occur for an extended period of time and for strong melt events during the accumulation season. Compared with WFJ, the results suggest that the differences between water balance schemes are smaller than for WFJ. This actually is in agreement with our findings for WFJ, where we found that water balance schemes produce very similar results during the melt season when the snow cover is isothermal. At CDP, these melting conditions are present for a much longer period of the snow season. However, most of the conclusions drawn from the WFJ simulations hold. For example: simulations with Richards equation show the highest r^2 value and Nash Sutcliffe model efficiency coefficient, especially on the hourly time scales. We will include those results in the revised version of the manuscript.

Please find below a detailed response to all other questions and issues raised by Samuel Morin.

- "Although I found the English pretty good, I encourage the authors to have their article proof-read by independent colleagues (not necessarily native English speaker) to remove the few awkard formulations that can be found in the manuscript, and which are not addressed below."
  Reply: We will make sure to have the manuscript proof-read by others to improve the text in this regard.

- "Conversely, it would be very appreciated by the snow modeling community that the 14 years data set used here to drive and evaluate various flavours of SNOWPACK are made available publicly for evaluating other models under the high-mountain conditions encountered at Weissfluhjoch."
  Reply: We are currently working on organizing and documenting the Weissfluhjoch dataset. It can be expected that the data will be publicly available within the next 6-12 months.

- "What could make it even stronger is to evaluate the performances of the Richards Equation implementation in SNOWPACK at other sites that Weissfluhjoch."
  Reply: Please see our response above.

- "Regarding bucket-approaches, have the authors attempted to study the impact of the model internal time step on the result of the bucket formulation ? Trying a few typical values may be useful the widen the generality range of the conclusions reached here."
  Reply: we did not check this. However, for the bucket approach as applied in the SNOWPACK model, this test would only make sense if the complete SNOWPACK model is run with a smaller time step. If the snowpack state does not change, the bucket model will route all liquid water in the first time step with no additional changes in subsequent time steps. However, if the complete SNOWPACK model is run at a smaller time step, all other modelled processes could produce slightly different results, making the effect on the bucket scheme difficult to detect.

- "Title: The title should better reflect the content of the article. In particular, it should be mentioned that the model was tested at one high-altitude site, which somewhat reduces the generality of the title taken as a firm statement. It could also be mentioned that the Richards Equation was implemented in a detailed multi-layer snowpack model, which is one possible approach among others to

model snow for hydrological purposes."
Reply: We will reword the title to be more specific in the revised manuscript.

- "Abstract:
  - Page 2374, line 4 : 'snowpack model' : I think it would be useful to state the name of model in the abstract.
  - Page 2374, line 5 : 'high alpine site' : better name the site in question."
  We will be more specific in the abstract for the revised manuscript.

- Introduction: "Page 2375, line 18 : 'small grains' : It could be precised which grain size metric must be employed for such processes ; later same line : replace 'course' by 'coarse' (?)"
  Reply: we refer to the SNOWPACK definition of radius as mean grain radius, which is defined according to the Fierz et al. (2009) classification. Indeed we mean coarse instead of course, thanks.

- "Page 2376, line 16 : it is unclear what means 'gravitational flow resulting from RE'. While it is true that the original publication describing Crocus (Brun et al., 1989) mentioned an equation explicitly describing the speed of downwards water migration, the bucket approach has been used in all further studies using Crocus and the current formalism (bucket) is described in Vionnet et al. (2012). I don't know whether such changes over time also occurred in SNTHERM, it may be worth checking with the current managers of the code."
  Reply: We were not aware that Vionnet (2012) describes a different water transport scheme for CROCUS than in the original paper. We think it is better to reflect on the current situation and we will revise the manuscript accordingly. When neglecting capillary forces, as discussed by Colbeck (1972), the resulting flow from Richards equation is sometimes referred to as gravity flow (Colbeck, 1972). We will improve the wording in the revised manuscript. The currently publicly available version of SNTHERM did not change since 1989 and the new version

SNTHERM04 is still in development. We will continue referring here to the original publication.

- "Page 2376, line 25 : the Hirsahima et al. (2010) scheme should be introduced either before or at least a concise explanation of what this scheme is should be placed here. Otherwise, the reader is left to referring to the original publication. Later on, this scheme is referred to as 'NIED' if I understood well ... I also think it should be mentioned here that two flavours (i.e., the use of 2 parameterizations of the van Genuchten model) of the full RE scheme are employed and compared."
  Reply: We will revise the manuscript according to your suggestion and make more clear that we use the term NIED scheme for the Hirashima et al. (2010) scheme. Hirashima et al. (2010) only used the Yamaguchi et al. (2010) parameterization, so the Daanen and Nieber (2009) parameterization of the van Genuchten model is only tested for the full RE.

- "'LWC production' is mentioned several times in the paper although I didn't find a precise description of how LWC production was computed, and with which model set-up. Since this term is used in the Results and Discussion section, I think it deserves a description in this section."
  Reply: We omitted to mention that we derive the LWC production from the Bucket simulations. First of all, this scheme is in the original SNOWPACK version and the model is tested most with this scheme. Furthermore, differences between simulations are rather small and the purpose of showing the LWC production is only to serve as a reference of the relation between LWC production and runoff.

- "Page 2378, line 25 : 'grain radius': please specify how is grain radius defined here."
  Reply: this is the defined the same as in Lehning et al. (2002): the mean grain radius, following Fierz et al. (2009).

- "Page 2379, line 15 : I would suggest to replace 'Yamaguchi' and 'Danen' by

'RE-Yamaguchi' and 'RE-Danen', respectively, to avoid confusion with the NIED scheme."
Reply: we will take this suggestion into account when revising the manuscript. However, we should also avoid misunderstanding in the fact that the NIED scheme is also using the Yamaguchi parameterization.

- "Page 2380, lines 20 to 23 : The compared impact of density and 'grain size' evolution of snow permeability is not necessarily always going in the same direction as stated by the authors. Field experiments and model runs recently reported by us (Domine et al., 2013) show that opposite evolutions can be found under the same meteorological conditions depending on the magnitude of the temperature gradient. Also, the use of the Shimizu (1970) regression curve for evaluating snow permeability from snow properties was recently challenged by new tomography-based experiments and new regressions were proposed which could be used here as an alternative to the Shimizu (1970) formalism (not all such studies are quoted here for the sake of brevity - see a description of some of these studies in Domine et al., 2013)." *Reply: Thanks for pointing us to this interesting study by Domine et al. (2013). The context of the remark in the manuscript is that in our simulations, the main trend shown by the model was in the direction described. We will revise the manuscript referencing this study and providing the background for our remark. Furthermore, it is interesting to see the progress regarding the understanding of the snow permeability. However, we would like to consider these developments in future studies and continue using the Shimizu (1970) formulation in this paper.*

- "Page 2381, line 18 : the range of maximum snow height is given with a high level of precision (1cm resolution), but at this stage it is not said over which time period this range is applicable. This needs some reformulation."
Reply: This range was reported from a climatological point of view. The range was determined for the full manual measurement series at Weissfluhjoch over

the period 1937-2012. This will be mentioned in the revised manuscript.

- "Page 2381, line 20 : I think it should be made more explicit what, within the dataset described, is used to run SNOWPACK, and what is used to evaluate it. There are so many ways to run SNOWPACK using inputs of various levels of complexity that giving such information is critical to the reader."
Reply: In the revised manuscript, we will amend this section with more information about how SNOWPACK was run.

- "Page 2382, line 10 : replace 'mm' by 'mm w.e.' (or, even better, kg m-2)"
Reply: Thanks, actually the lysimeter is measuring at a resolution of 0.8 liter, which implies a resolution of 0.16 *kg/m*^{2}*. We will correct the manuscript at this point.*

- "Page 2384, line 1 : replace 'depth' by 'thickness' (?)"
Reply: Thanks, we will revise the text here.

- "Page 2386, line 5 'winter snow season' : this needs to be better defined."
Reply: With the winter snow season, we meant the snow season as defined on p.2385, L7-11. We will make clearer at this point that we will use the term snow season with this definition throughout the rest of the manuscript.

- "Page 2389, line 15 : it may be mentioned that the initial publication describing Crocus was already comparing model ouptut with lysimeter data, essentially with the same conclusions (Brun et al., 1989)."
Reply: We indeed see that (Brun et al., 1989) shows the same result. We will amend the manuscript at this point.

- "Page 2389, line 27 : this sentence will become clearer once 'LWC production' is better described in the methods section."
Reply: The LWC production is taken from the simulations with the bucket scheme. We will revise the text regarding LWC production.

- "Page 2393, line 22 : errors originating from the model construction can also have varying effects depending on the season."
  Reply: We will mention this reason also in the revised manuscript.

- "Page 2394, line 22 : perhaps replace 'melt' by 'wet'"
  Reply: We agree that wet snow metamorphism is a better term

- "Page 2395, line 10 : the use of 'considerable' here is strange. Replace by 'deep' ?"
  Reply: We agree that deep is a better wording

- "Page 2395, line 21 : could you describe a little more how is determined Niter ?"
  Reply: N_{iter} is the amount of iterations needed in the Picard scheme to achieve convergence for a particular time step. The Picard scheme is considered globally convergent, although convergence can be very slow. In that case, it will help to reduce the time step. Then N_{iter} generally goes down, because convergence is achieved quicker. A balance should be found between time step (more time steps and thus more calculations needed to progress in time) versus number of iterations per time step (more matrix inversions per time step), both are numerically more demanding.

- "Figure 1: Since snow seasons are rather independent from each other, maybe the solid lines connecting the individual errors could be removed, and the Runoff sum for each year denoted with a different symbol (without connection from year to year) ?"
  Reply: It is true that the years are not correlated, as the snow cover disappears in summer. We will revise Figure 1 and Figure 4 and use a different plotting method.

- "Figure 2 : why is not LWC production included in panel (b) ?"
  Reply: We left out LWC production in order not to clutter the figure too much. We will add this in the revised manuscript, by possibly increasing the figure print size.

- "Figure 4 : is the figure representing one particular year, or all years of simulation ? The reference to this figure in the text is very brief and does not allow to understand exactly how the figure was produced and needs to be interpreted."
  Reply: This figure shows the average delays for all simulated years. We will revise the caption and the text to explain this.

- "Figure 4 : same comment as Figure 1"
  Reply: It is true that the years are not correlated, as the snow cover disappears in summer. We will revise Figure 1 and Figure 4 and use a different plotting method.

- "Figures 6 and 7: it may be mentioned in the caption what is the size of the bins that were used to produce the plot. Also, please replace 'mm' by 'mm w.e.' at least." *Reply: the data are always binned in 15 classes, with various bin size. We will revise the manuscript according to your suggestions.*

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