

Reviewer 2

This work is remarkable at least from three points of view: for the abundance of reliable data used (and unfortunately not provided to the public of researchers), the use of modern parameterisations of the snowpack evolution, using a new version of SNOWPACK 1d model which includes water flux moved by Richards equation and a new parameterisation of soil temperature, and, last but not least, the trial to get quantitative answer without parameters' calibration. The report of the results is detailed and honest. Some results, as stated by the same Authors, seem to be a byproduct of SNOWPACK being 1D, and this should probably discussed at a deeper level. Information about the SNOWPACK model availability and data availability are required, maybe in Acknowledgements or in a short dedicated section (entailed for instance: How to Replicate this Research). Future work could address the reliability of parameters and their sensitivity estimation. It seems clear to me, in fact, that some a-priori, parameterisations could not be the correct: indeed, a little qualitative discussion on these topics could be interesting also in this paper.

Overall the paper is excellent and worth to be published in TC, after some minor revision work. Please find below my detailed observations.

We would like to thank the reviewer for the positive feedback and the constructive comments. Regarding the parameters used in the SNOWPACK model: they mostly have been published before, as indicated by the references provided in the manuscript. We think it is interesting to combine a short discussion on possible future improvements of process representations in the outlook section, which was proposed by the first reviewer. We will discuss this on a process level, rather than a parameter level, because snow settling, for example, depends on several parameters and we think that it would be too detailed in the context of the manuscript to discuss individual parameters.

Please find our detailed response to the other issues raised by the reviewer below.

Detailed comments

Page 2659 - Introduction is well designed. Probably a short of literary review about model alternatives to SNOWPACK would help the general reader to have a more clear view of possibilities.

We will mention alternative models in the introduction, restricted to the multi-layer physics based models SNTHERM, CROCUS and SNOWPACK:

One-dimensional multi-layer physics based snowpack models, as for example SNTHERM89 (Jordan, 1991), CROCUS (Brun *et al.*, 1989; Vionnet *et al.*, 2012) and SNOWPACK (Lehning *et al.*, 2002a,b) are widely used to assess various aspects of the snow cover.

Page 2664 - The data collection is impressive. More information about data availability, needed.

It has been decided now that the operational data from WFJ (both the meteorological forcing, the snow lysimeter data as well as the biweekly snow profiles) will be made available on a repository with the publication of this manuscript, linked via DOIs. The SNOWPACK model is available under the GNU Lesser General Public License (LGPL) Version 3; we will include a link to the repository (<http://models.slf.ch>) in the revised manuscript. The data from the upward-looking ground penetrating radar in combination with snow profiles made in its vicinity, has been collected on a project-basis and is available on request from the authors. We will add this information in the Acknowledgements.

Page 2665 - Since the way SNOWPACK is initialised has strong impact on the results, a little more of explanation on how HS approach works, as opposed to Precept driven, could be useful.

Although the algorithm has already been described in Lehning et al. (1999), we agree with the reviewer that due to the importance of the snow height driven approach in the manuscript, it is helpful to expand on the approach. Basically, if the measured snow height exceeds the modelled one, the model will create as many layers of 2 cm as necessary to match the observed snow height again, if snowfall conditions are met. The snowfall conditions are: (i) measured air temperature $\leq 1.2^{\circ}\text{C}$, (ii) difference between measured air temperature and modelled snow temperature $\leq 3.0^{\circ}\text{C}$ and (iii) a relative humidity $\geq 70\%$. These conditions provide an estimation whether the atmospheric conditions are such that snowfall can be expected. For example, the second condition tests for cloudy conditions, when the increase in incoming longwave radiation compared to cloud-free conditions will cause the snow surface temperature to increase and become close to the air temperature. We will provide this information in the manuscript.

Page 2673 - line 22 - I think this is an improper use of the supplement. Figure S8 should be added to the main text. (What is IMO an appropriate use of the Supplement is shown in lines 2-3 of page 2675).

We understand that it is necessary to show Figure S8 in the main text. We will make this change in the revised manuscript.

Page 2674 - line 8 - The discussion about the NSE coefficients found should be more extensive. While most of them are good, some of the coefficient are really bad (NSE 1h bucket). Therefore, these performance should be discussed. I agree that NSE could not be the best test: but, in case, this also should be discussed.

To prevent being biased towards performance in terms of NSE coefficients, we also included r^2 statistics. Originally we decided not to discuss the results regarding runoff in too much detail, as this would then be a repetition of our previous work (Wever et al., 2014). The main problem with the performance of the bucket scheme on hourly time scales when looking at NSE coefficients is poor timing due to neglecting the travel time through the snowpack in the bucket scheme. We will add this in the manuscript, although we still think it is not necessary to provide an extensive discussion as the reference to the other paper is sufficient, in our opinion.

Page 2675 - line 13. "The latter influences the snow temperature through the thermal inertia of dense snow layers and through the strong dependence of density on thermal conductivity (e.g., Calonne et al., 2011). " I was tempted to say that is the thermal conductivity that depends upon the density, not vice-versa.

We are sorry to have caused confusion here, but it is indeed intended to say that the thermal conductivity is dependent on density. We revised this sentence as:

The latter influences the snow temperature through the thermal inertia of dense snow layers and through the strong density dependence of thermal conductivity (e.g., Calonne et al., 2011).

Page 2676 - line 25. p.2676: "The contrasting result suggests that the snow layers near the top of the snowpack have a too low density in the simulations." Why not incorrect estimation of incoming solar radiation ? Or of the thermal capacity, for reasons not depending on density ? I am not able to grasp the reasons for the unique interpretations the Authors give for this behaviour.

We agree that more explanation is required here. We add the possibility that it is not necessarily snow density, which influences thermal conductivity of the snowpack, but that snow density errors also introduce errors in thermal capacity. We will add that a closer inspection of the simulations revealed that the underestimation of snow surface temperature particularly happens at night (not shown in the paper), which excludes the possible influence of errors in diagnosing the net shortwave radiation. We will revise the text as follows:

"Interestingly the snow surface temperature is generally underestimated, whereas the temperature at the highest snow temperature sensor is overestimated in the simulations. The contrasting result suggests that the snow layers near the top of the snowpack have a too low density in the simulations, impacting both thermal conductivity and heat capacity of those layers, or the thermal conductivity is underestimated for typical snow densities found close to the surface. These effects provide a stronger isolation of the snowpack, causing heat from inside to escape at a slower rate and allowing the surface to cool more. This offers an explanation why the underestimation of the snow surface temperature particularly occurs at night (not shown). In contrast, errors in diagnosing the snowpack energy balance (i.e., in net shortwave or longwave radiation, or turbulent fluxes) would be expected to influence all temperature sensors in the same direction."

Page 2677 - line 13 and subsequents. ". . . suggesting a better timing of the movement of the meltwater front through the snowpack ..". How this is actually affected by the fact that SNOWPACK is 1D ? Is this a manifestation of 3D effects of water re-distribution ?

It is indeed true that the fact that SNOWPACK is only 1D, and assuming horizontal homogeneity is a simplification of reality that will be particularly important for simulating liquid water flow. We think this issue is clearly illustrated by the fact that although the timing of the movement of the meltwater front through the snowpack is improved, the runoff as measured by the snow lysimeter consistently starts earlier than simulated. To better reflect that the snow temperature may also rise to 0°C by heat advection or refreezing of liquid water infiltrating the layer, the section will be rewritten as:

"Although this suggests a better timing of the movement of the meltwater front through the snowpack and the associated temperature increase to 0°C, also heat advection through the ice matrix and preferential flow and subsequent refreezing inside the snowpack may increase the local snowpack temperature to 0°C. The reason why the results from the temperature series at 150 cm contrast those at 0, 50 and 100 cm depth remains unclear."

Page 2694 - Figure 1 - The Figure is actually not very clear because some lines superimpose. Maybe this can be explained in the caption.

We will add in the caption that apart from forcing with either snow height or precipitation measurements, differences between simulation setups cause only small differences in snow height simulations, resulting in overlapping lines in the figure.

Page 2695 - Figure 2. This Figure is very explicative respect to the quality of the drivers. Maybe some more explanation can be added to comment it in the text. The difference between the two drivers seems related to the SD and both of them seems to have close to null bias. However Precip driven simulations show strong seasonality with positive difference in the central months. Why ?

The snow height driven simulations are forced to closely follow the measured snow height, and overestimations in snow melt or snow settling can be compensated for, which explains good agreement. The precipitation driven simulations on the other hand, rely solely on measured precipitation. The seasonality in the difference with measured snow height stems from the fact that SNOWPACK seems to overestimate snow melt for WFJ, leading to an overestimated SWE depletion in spring. However, during the accumulation phase, it seems that particularly a few large snowfall events are overestimated by the rain gauge measurements, including undercatch correction (see Figure S2e in the Supplement for winter season 2011-2012 for a very illustrative example), whereas for typical snowfall events, the undercatch correction works well. Once events are overestimated, it will continue to bias the difference of simulated and measured snow height, as there is no mechanism to compensate for the error, in contrast with the snow height measurements. We will provide some more explanation regarding these issues in the text.

Page 2696 - Page 2697 - Figure 3 and 4 have mm in ordinate. Using cm would be homogeneous with the rest of the paper.

Although it is true that this seems an inconsistency, we prefer to keep the original notation, as it is very common in literature to express snow height in cm, and SWE in mm w.e.

Page 2698 - Figure 5 - The RE plot shows sharp variations of LWE on the vertical that move downward in time. This is fine with me. However, we also observe jumps in liquid water content from instant to instant. Are these jumps instantaneous just for representation problems or there are more detailed dynamics ?

These jumps are associated with the diurnal variation in LWC, as we explained on p2672, L24-26 and p6-7. We found correspondence of modelled diurnal variations using RE with those derived from field measurements using the upGPR (Heilig et al., 2015). As the figures are probably too small to convey these temporal and spatial variations, and to better illustrate the difference between the bucket scheme and RE, we have put a detail from the simulations inside the figures, see Figure 1 in this document.

Page 2702 - Figure 9b. Please use SD for Standard deviation instead of S.D.

We will adjust the figures accordingly.

Page 2703 - Same comment as in Figure 5

Please see my response there, considering the fact that snow density is also influenced by variations in water content and thereby can exhibit diurnal variations.

Page 2705 - Figure 132 - Grain size: does grain of relative large size move downwards due to compaction ? (Or there is also a metamorphism associated ?) Therefore does also liquid water content (in Figure 5b) move downward due to compaction ? (The same for density ?) Could, please clarify these aspects to me ?

Both processes play a role. First of all, the snowpack is constantly settling due to compaction and overload by snow falls. Furthermore, SNOWPACK considers observed enhanced settling with wetting of the snowpack. Grain size eventually evolves due to metamorphism processes and under the presence of liquid water. The implementation of these processes is detailed in the original SNOWPACK paper by Lehning et al. (2002a). As layers are moving closer to the ground due to settling, the properties keep attached to the layer in the simulations. We think this is an appropriate representation of reality. For this reason, liquid water accumulations inside the snowpack also move downward (as depicted in Figure 5b), although this is not a result of water flow. A note is added to the manuscript to briefly explain this behaviour:

These accumulations peak at around 10% LWC and occur during the first wetting of the snowpack and above capillary barriers inside the snowpack. The apparent slow downward movement of liquid water accumulations during the melt season results from snowpack settling, moving the specific layers with water accumulations closer to the ground.

Page 2707 - Figure 14. All the 14s figures are indeed interesting. However, putting all of them in the same page produces a quite unreadable result. There is certain complexity in these plots that should be probably better explained. It is true that page 2680 is dedicated to this Figure. However, the connection between the assertions in the text and what represented is not so clear to me.

We are a bit in doubt here what to do. On the one hand, we agree that putting all four figures on one page makes the page rather crowded. On the other hand, we think it is very illustrative to be able to compare the figures directly to each other, which is impossible when the figures are spread over several pages. In the final format, the figures can probably be enlarged when they are printed vertically below each other on two sheets of A4 paper. Another option may be to show the figures in landscape mode. We will discuss this with the typesetting department. Additionally, we labelled the four main observations mentioned on p2680, L28 to P2681, L7 with (i), (ii), (iii) and (iv), and reference them later in the text. We hope that this approach offers more clarity. The revised text:

From the four snow seasons presented in Fig. 14, the following observations can be made: (i) snowpack runoff measured by the snow lysimeter consistently starts earliest in the snow season. (ii) The progress of the meltwater front is always faster in the simulations with RE, compared to the bucket scheme. (iii) The radar-derived meltwater front progresses generally slower through the snowpack than in both water transport schemes in the model. (iv) The manual snow profiles mostly show melt forms in parts of the snowpack that have been wet according to the radar data, whereas the simulations often show larger parts of the snowpack becoming wet earlier than indicated by the profiles. These observations will now be discussed in more detail.

(i) Since preferential flow can route liquid water efficiently through the snowpack (Kattelmann, 1985; Waldner et al., 2004; Techel and Pielmeier, 2011), upGPR-determined depths of dry-wet transitions are not necessarily linked to the onset of measured snowpack runoff (Heilig et al., 2015). Studies by Katsushima et al. (2013) and Hirashima et al. (2014) found that ponding plays a crucial role in forming preferential flow in both laboratory experiments as well as model simulations. The ponding of liquid water in the simulations for WFJ (see Fig. 1) suggests that preferential flow may have developed. The amount of snowpack runoff measured before the arrival of the meltwater front is highly variable. From 1 until 8

April in snow season 2011, large amounts of snowpack runoff were observed, most likely due to lateral flow processes, whereas in snow season 2014, only marginal amounts were observed. In the latter snow season, there is a strong increase in observed snowpack runoff close to the time of the arrival of the radar-derived meltwater front at the snowpack base. This variability between years is not necessarily caused by different preferential flow path structures, but may also result from the limited capturing area of the snow lysimeter (Kattelmann, 2000).

(iii, iv) The vertical distribution of the melt forms in the observed snow profiles may be considered particularly representative for matrix flow and for the four presented years it generally corresponds well with the parts of the snowpack that may be considered wet from the upGPR signal. (ii) As the bucket scheme shows a higher correspondence with the upGPR data than RE, the convenient improvement in the accuracy of simulated snowpack runoff with RE, as found in Wever *et al.* (2014), seems to be partly caused by (unintentionally) mimicking some preferential flow effects. To what extent this is caused by parametrisations of the water retention curve or hydraulic conductivity, or by the specifics of the implementation of RE in SNOWPACK, remains unclear. (ii, iii) Although the bucket scheme may seem to better coincide with the meltwater front in the upGPR data, it may as well be argued that the differences between both water transport schemes are smaller than the discrepancies with the upGPR data. It is likely that the limits of one-dimensional models with a single water transport mechanism will prevent a correct simulation of both snowpack runoff as well as the internal snowpack structure at the same time.

In the beginning of the melt season, observations contrasting to the main melt phase discussed above can be made. The initial melt phase is characterized by a regularly disappearing meltwater front at night. In this period, the depth to which the liquid water infiltrates the snowpack is underestimated in the simulations. Here, the RE scheme shows larger infiltration depths, which are in better agreement with the upGPR data, although again differences between both simulations are smaller than the discrepancies with the upGPR data. This result is contradictory with the main melt phase, where the speed with which the meltwater front progresses through the snowpack is largely overestimated in the simulations. Furthermore, the distribution of melt forms in the snow profiles does not always coincide with the deeper infiltration depths detected by the upGPR.

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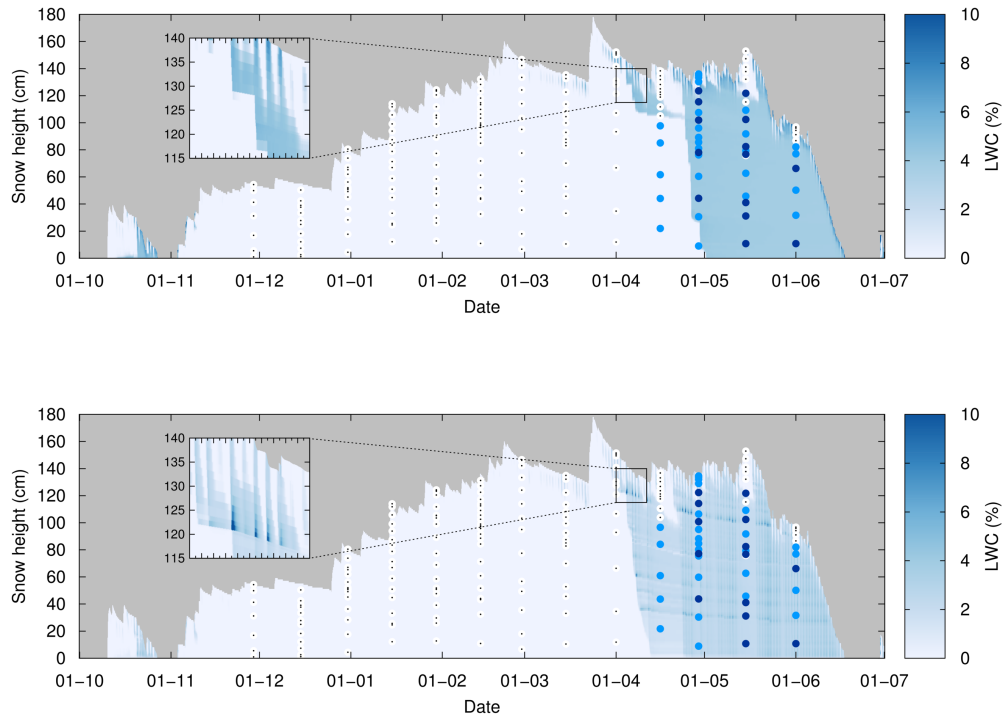


Figure 1: Snow LWC (%) for the snow height-driven simulation with the bucket scheme **(a)** and with Richards equation using the *Yamaguchi et al.* (2012) water retention curve and arithmetic mean for hydraulic conductivity (RE-Y2012AM, **(b)**), for the example snow season 2014. Dots denote layers that have been reported as dry (0% LWC, white with black center dot), moist (0-3% LWC, light blue) or wet, very wet or soaked ($\geq 3\%$ LWC, dark blue) from the biweekly snow profiles. When layers are reported as "1-2" (dry-moist), it is considered moist. In the zoom insert, major and minor x-axis ticks denote midnight and noon, respectively.