The Cryosphere
Discussions

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Interactive comment on "Verification of the multi-layer SNOWPACK model with different water transport schemes" *by* N. Wever et al.

Anonymous Referee #2

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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.

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.

Page 2660 - Part on water retention curves is interesting and informative.

Page 2662 - Part on soil freezing is OK

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

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.

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).

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.

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.

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.

Page 2677 - line 13 and subsequents. " . . . suggesting a better timing of the movement of the meltwater front though 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 ?

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

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 ?

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

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 ?

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

Page 2703 - Same comment as in Figure 5

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 ?

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.

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