The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-199-RC1, 2020

The Cryosphere
Discussions
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Interactive comment on "Two-Dimensional Liquid Water Flow through Snow at the Plot Scale in Continental Snowpacks: Simulations and Field Data Comparisons" *by* Ryan W. Webb et al.

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Received and published: 24 August 2020

The authors report on model simulations of lateral water movement in a sloping snowpack. The topic is highly relevant, as it is an open research question to what extend lateral flow in snow modifies the hydrological cycle in snow covered catchments. The coupling of a dedicated snow model with a dedicated water flow model is a unique and very interesting approach. The authors find a much larger lateral flow than vertical flow, suggesting that lateral flow is highly important, even in relatively flat terrain (10 degrees slope angle). Even though the concept of the study is very robust, the manuscript itself falls short at several aspects, and many issues are not, or only weakly discussed. It is

strongly encouraged that the authors thoroughly improve the manuscript. The writing style is of very high quality, with clear figures. However, I think the authors should avoid using the jet-type color scale used in some figures.

Main concerns:

The main drawback of the way the study is presented is the lack of validation with the available field data (as for example published by the authors as Webb et al, 2020), except for the observed dye tracer layers showing the lateral flow layers. There must be more validation data available to validate the results of, for example, the SNOWPACK simulations. This should be worked into the results and discussion sections. For example, does the field data confirm the degree of wetting of the SNOWPACK at the initiation of the field campaigns?

Also, it is reported that: "Initial conditions were provided through manual snow pit observations" (L92), while at the same time, it is reported that: "Within this domain, the iTOUGH2 numerical model simulated the flow of liquid water with time-varying snow layer properties provided by the SNOWPACK model." (L59). That seems contradictory. I have some difficulty reviewing the remainder of the manuscript, since this issue is so unclear. When only material properties are taken from SNOWPACK, without any consideration for the wetting of the snowpack, or the exact layer transitions that are simulated by SNOWPACK (which are a reflection of the driving data to run the simulations), while at the same time observations are used to initiate the iTOUGH2 simulations, then I think that the coupling introduces large errors, simply resulting from the mismatch between simulated and observed stratigraphy. I'm highly confused at this point what exactly has been coupled. In fact, material properties don't vary that much for the couple of days simulated with iTOUGH2, such that when iTOUGH2 can be initialized with observations, the SNOWPACK simulations may not add any information regarding the change in microstructural properties. Maybe it would be a sufficient approach then to have SNOWPACK inform iTOUGH2 about melt rates only, to prevent the problems with potential mismatches between modeled and observed stratigraphy?

All this remains very unclear now.

Furthermore, I struggle to relate earlier reported dye tracer levels to the ones shown here. For example, I assume that the "above treeline" site in Webb et al. (2020) corresponds to the alpine site in this study (it may be a good idea to use the same naming for the different sites as the previous study). Fig. 9(aii) in Webb et al. (2020) shows a dyed layer at the snow/soil interface and around 1.50m inside the snowpack (about 35 cm below the snow surface), both which I can find back in Fig. 3c. However, Fig. 9(aii) in Webb et al. (2020) also shows a layer around 1m above the soil, which seems to be indicated much lower in the snowpack in Fig. 3c. Also, for the treeline site the layers don't seem to correspond. Fig. 9(bii) shows a first dyed layer around 60cm below the snow surface, and another one 60cm above the soil/snow interface, with a third one in between. Fig. 3b shows the highest dyed layer much higher in the snowpack (about 30 cm below the surface) and the lowest about 20cm above the snow/soil interface.

In Webb et al. (2020), it is reported that the SNOWPACK simulations were run using the Richards equation water transport scheme. However, the liquid water content distributions shown in Fig. 2 in Webb et al. (2020) and Fig. 2a in this manuscript, are remarkably homogeneous, even though I found in multiple studies that using Richards equation in SNOWPACK leads to inhomogeneous water distributions. To me, it looks like results from the bucket water transport scheme. The SNOWPACK model also should indicate the hydraulic barriers, since those impede vertical flow, even though the SNOWPACK model does not consider lateral flow.

Many figure panels aren't discussed in the manuscript.

At several instances, it is important to repeat information from earlier published work. For example, the setup and driving data of SNOWPACK simulations should be explained in more detail. Some information is necessary to interpret the results, and only referring to earlier published work is then inadequate in my opinion. For example, it's

important to know what the source of precipitation is for SNOWPACK, to understand if the stratigraphy would match local conditions or not. Typically when SNOWPACK is driven with in-situ measured snow depth, it shows better agreement with local stratigraphy, compared to when it is run with rain gauge data. Since there is a large uncertainty in rain gauge data for solid precipitation, individual snow fall events can be severely over- or underestimated. Based on citations, I assume SNOWPACK was run with in-situ snow depth data, but I think it's important to repeat that kind of crucial information here. Similarly the use of the canopy module, or a better description of the field sites are important aspects to repeat.

Minor comments:

- I would recommend to use the same terminology as in Webb et al. (2020) to denote the field sites (i.e., above treeline, near treeline, below treeline).

- A discussion of boundary conditions upslope and downslope in iTOUGH2 needs to be added. It seems to be a zero flux boundary to the left and right, such that water accumulates at the downslope boundary. This may also explain the role of the drain mentioned in L91, to prevent water accumulating in the model because of zero flux boundary conditions.

- L77: "was calculated by subtracting theta_w from rho_s". Please reformulate, because this doesn't seem to be a calculation that makes sense if theta_w is not multiplied by the density of water. Furthermore, why not simply multiply theta_ice with the ice density of 917 kg/mˆ3 to get dry density?

- Eq. 1 and 2: These equations come from Yamaguchi et al. (2012), not Yamaguchi et al. (2010).

- L90: "10-30 cm deep soil". Please denote what kind of soil was prescribed and how this relates to the soil at the study plots. Related: in L138: the lateral flow at the snow-soil interface may be caused by the prescribed soil type. Thus, it is imperative to discuss this.

- L91: Please explain why deeper soil under deeper snow.

- L91: Please explain the role of the drain in the simulations and in the analysis.

- L102: Please specify if the north or south facing subalpine site was taken.

- L112-116: It's not clear where/how this data is used in the manuscript.

- In L139, it is not clear how the vertical water transport is calculated. It is obviously not homogeneous in the vertical direction, so does this concern the average vertical flow over all layers in the snowpack? I actually think that it is also interesting to relate this number to meltwater input, or water arriving at the snow/soil interface, or water infiltrating into the soil, to put it in broader perspective with respect to hydrological processes.

- L143: "liquid water continued to flow" Please specify if this is true for both SNOWPACK and iTHOUGH2, or that it only concerns the lateral flow in iTHOUGH2.

- L190: liquid water in the snow matrix is not necessarily evidence of flowing water, since the capillary forces will retain some liquid water (the irreducible liquid water content, or residual water content).

- Fig. 3: right panel is labeled "b) Alpine" which should be "c) Alpine"

Feel free to contact me in case anything is unclear in this review.

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