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The Cryosphere Discussions

Interactive comment on "Ice layer formation in the snowpack due to preferential water flow: case study at an alpine site" *by* Louis Quéno et al.

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This study combines a valuable dataset made by snow-pit, SnowMicroPen, upGPR, lysimeter, and weather-snow measurements with improvements to the liquid-water-transport scheme of SNOWPACK to study the important, but still poorly understood topic of deep ice-layer formation due to preferential flow. Results are promising, in that they show increased realism of the dual-domain scheme of SNOWPACK in reproducing ice-layer formation. Another interesting aspect of the paper is the overview of insights from the observational dataset and in particular Section 3.1.3.

My assessment is that the paper is interesting for readers of TC and certainly moves our understanding of ice layers forward. Yet, I do have several comments that may

be useful to enhance the clarity and analyses of the manuscript (see below), so I recommend the Editor accept the manuscript after minor revisions.

GENERAL COMMENTS

1. The manuscript sometimes reads like a case study (Title, Abstract), and sometimes as an incremental contribution to Wever et al. 2016. I do believe that there are several novel points in paper, some of which are discussed at lines 49ff, while some others emerge in Section 2.2.2. Overall, however, these novel points remain somewhat implicit for general readers. I encourage authors to elaborate on framing both in the Introduction and in the Discussion to better highlight these points of novelty.

2. Relatedly, novelty could also be better streamlined by explicitly identifying a few research questions to be reported at the end of the Introduction. These research questions should be broad enough to be interesting for the general public and could dramatically increase the impact of the paper.

3. In Section 3.2.3, authors chose to restrict results in Table 1 to only simulated ice layers, without considering instances when observed ice layers were not simulated. Did I understand correctly? If so, I would suggest authors to also include these 'missed' ice layers in Table 1 to gain further insights.

SPECIFIC COMMENTS

- I would recommend authors avoid wording like 'case study' in the Title and elsewhere in the manuscript.

- In the abstract, I would report that the new parametrization allowed authors to significantly reduce overestimations of melt-freeze crusts (see Section 3.2). More generally, to me the abstract seems a little imbalanced toward explaining data and methods rather than findings and insights.

- Line 49: Between the previous paragraph (lines 23-48) and this one about your study (lines 49ff), I would add one more paragraph to specifically discuss previous attempts

of modeling ice layers and why more research on this topic is needed. What are the knowledge gaps that you are trying to fill with this impressive field campaign and your new parametrization of ice layers? This paragraph should help formulate research questions and therefore better make the case of this paper, especially since the discussion about snow models between lines 40 and 48 is general and not really focused on ice layers.

- Line 68: how are measurements along these three corridors managed? Do you use one corridor every other week, or simply use the same corridor every other week as long as you reach the end of it?

- Line 74: maybe cite Wever et al. 2014 when you introduce the snow lysimeter since Wever et al. 2014 also described the specific design of this instrument.

- Line 79: get rid of -> remove

- Line 100: why not initializing the simulation on a day with no snow on the ground?

- Line 106: did you still allow for mixed rain-snow or all-rain events? If yes, using which phase-partitioning method? I assume these are rare at WFJ, but still worth discussing given that the paper focuses on liquid-water-transport schemes

- Line 119: are carried out –> were carried out

- Line 125: could you add more details about how water flowing back to the matrix domain depends on the freezing capacity of the matrix domain? As a general note about this Section, I am missing some discussion on whether the ice reservoir affects hydraulic properties of the snow layer. This is important given that ice lenses often represent a hydraulic barrier.

- Line 136-137: It is a little unclear why the the highest saturation should be reached more likely at the layer above, where no water transfer occurred.

- Line 178-179: In Katushima et al. 2013 and then Avanzi et al. 2016, it is showed that

isothermal conditions may coexist with preferential flow. Given that snow pits showed vertical structural heterogeneity after April 9 (see Figure 3) and Hirashima et al. 2019 have showed that matrix flow is coupled with structurally homogeneous conditions, I would assume that matrix flow started well after April 9 at this site.

- Figure 6: maybe add the same black boxes as in Fig. 3 to denote location of ice layers? Also, the color scale is a little unclear to me: Figure 7 shows that resistance is locally well above 2N. Are all values above 2 N reported with the same color as 2 N?

- Line 198: how does this manual picking selection works and what is its uncertainty?

- Line 205: may this also suggest that the ice layer was subjected to periodical melt and freeze?

- Line 275: Could authors elaborate on reasons why the simulated ice formation date is in average 22 days earlier than the observation interval for the RE/PF scheme and 4 days earlier 275 for the RE/PF/IceR scheme?

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