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Interactive comment on "A continuum model for meltwater flow through compacting snow" *by* Colin R. Meyer and Ian J. Hewitt

Anonymous Referee #1

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The manuscript presents a one-dimensional continuum model describing the flow of meltwater in a compacting snowpack. Compaction of dry snowpacks, and meltwater flow through non-compacting snowpacks had been considered in the literature before separately, but the coupling between the two hasn't been addressed, as far as I know. This latter coupling is key to examine the dynamics of meltwater percolating in thick firn layers, such as those present in Greenland, and to develop an understanding of how these layers respond to a warming climate. The authors present a mathematical model for these coupled dynamics, for which they derive both analytical and numerical solutions. They identify four different scenarios depending on the intensity of the surface energy forcing, and also analyze how the transition among those scenarios is affected by changes in annual surface accumulation. An important result is that firn

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can switch from a sponge-like behavior, such that all the melt is stored within the firn, to the behaviour of an almost impermeable substrate that allows further meltwater to run off the ice sheet. Interestingly, saturation of the whole layer is not required to start surface runoff.

Most of my comments below are relatively minor. The main thing I would like to see is more focus on the physical processes that sit behind the response of firn to changes in the surface forcing (more detailed comments in this respect in the minor comments below). In this respect, I wonder whether the two simplified problems discussed in Sec. 3 could also be used to illustrate some of the fundamental aspects of the flow of meltwater through firn in isolation, thus helping to explain the physical basis of the more complex results presented in Sec.4. In particular, whether saturation is achieved or not seems to be key to understanding the two different types of percolation zones, and hence whether perennial water storage is possible in the firn layer. In my view it would be useful if the authors could place more constraints on the physics that controls this switch, which seems to be the most interesting result in the paper. Last, I think that a slightly more critical literature review in the introduction, in particular when it comes to comparing and contrasting previous approaches to the present work, would also be helpful.

Minor comments:

Introduction page 2, line 26: mention that the test problems in Sec. 3 are primarily used to benchmark the numerics. Also, I agree that the results of these test problems compare favorably with data, but I'd like to see a mention to the fact that no mechanical compaction is considered in obtaining such results.

Section 2

- eqs 1-3: the velocity vector u is undefined

- page 5, paragraph 2.2: I am not an expert on the subject, but I wonder whether the specific choice of the parameterization for the compaction rate produces any qualitative difference in the results. I would like to see more discussion on this.

- eq 11: w_i is undefined

- eq 24: not sure gamma is defined. Surface tension?

Section 3

General comments, expanding on my major points above:

- Sec 3.1: You demonstrate that refreezing happens at the front, and so porosity behind the front decreases. Can you form ice lenses with this mechanism, once you allow for a depth-dependent porosity profile? Along similar lines, on page 10, line 10, you state that the effect of refreezing is to slow the propagation of the front and to increase porosity as the front passes. How would this result change if you allowed for compaction/ a depth-dependent porosity profile? Does this have anything to do with the switch between the two qualitatively different percolation zones of fig. 6II and 6III? See next comment for a related point

- Sec. 3.2: I agree with the authors that the agreement between theory and observations is good. It seems to me though (considering your expression for \dot{Z}_f, eq. 38) that the necessity to fit two different front velocities highlights once more that propagation of a water front in cold snow is strongly affected by compaction/ the porosity stratification. I'd like to see more discussion on this.

Minor comments:

- line 19: replace understand with understood

- Figure 2: the color scheme is not explained. Panel a: the text T=T_m should not be placed below the front, it's confusing. Maybe on the side of \dot{Z}_f?

- eq. 28: what does the ‘ stand for?

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Section 4

- Pages 13-15: The explanation of the differences between the two percolation zones is very qualitative and a little bit vague as a result, in my opinion. It seems to me that the key point is that in one case (fig 6II) there is unsaturated flow, whereas in the other case (6III) saturation is attained. Your results seem to suggest that a saturated from propagates more slowly than an unsaturated one, and hence penetrates less in depth preventing the formation of the firn aquifer. Would you be able to comment on this?

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