Second review of Horizontal Ice Flow Impacts the Firn Structure of Greenland's Percolation Zone by Leone et al.

B. Vandecrux

The new manuscript has significantly improved compared to the previous version. The model is properly presented, and the main text is now self-sufficient. I am grateful for the complete response provided by the authors which clarified some aspects of the study. Nevertheless, several rough points remain and should be clarified before it reaches the quality standard of the Cryosphere.

General comments:

Regarding the structure, I still believe that the splitting between results and discussion sections is confusing. For example, the two paragraphs in section 3.3. are very elusive and leave the reader wondering about the advection impact on MFP trends which only comes in the discussion section. I would strongly recommend switching to a "Results & Discussion" format, where, for each topic (Sensitivity analysis, transect comparison, MFP interpretation…), the results from the model can be presented and immediately discussed. Should the current structure be maintained, the addition of sentences guiding the reader through what is in the result and what is in the discussion would be highly beneficial.

Most of the plots lack clarity and readability:

- Y-axis in Figure 2 should be "Relative difference in air content (%)"
- Label for line colors should be added in the legends in figure 2, S2, S3.
- It took me, again, some time to interpret Figure 5b. A legend should be added to presenting each line. I also suggest making the axis label more explicit such as : "Depth in ice core" for the top axis and "Age of the firn/ice" for the bottom one.
- Figure 5a right label units should be m.
- In table 1, Why "approximate"? Check the journal's standard for abbreviations for "equiv". Also change "Speed" to "Surface velocity". Maybe use "Surface velocity" in Figure 1 and 2 for consistency,
- In Figure 1, 2 and Table 1, three formats are used for the unit of surface velocity please use one of them and check the whole manuscript for consistency.

While the method section presented a rather complete sensitivity analysis, the description and discussion of its results appear incomplete. For instance, even though it is deemed "interesting", only three lines are dedicated to the impact of meltwater infiltration and the reader is sent to the Supplementary Material. Figure S2 should be brought to the main text and each panel should be discussed properly. The impact of Meyer and Hewitt's scheme is not presented or discussed anywhere in the manuscript. Yet it represents the most physically-based scheme and a certain "middle way" between shallow percolation and deep percolation. It should be used as a baseline when assessing the impact of advection in the transects.

Specific comments:

l. 130:
I understand that the model is Lagrangian with regard to the horizontal movement. Is it as well for the vertical management of layers? I remember that Meyer and Hewitt's approach was originally implemented in an Eulerian fashion: model layers have a fixed volume and each time snow is added to the top of the model, firn is shifted downward through the model layer. This approach is known to smooth firn characteristics as firn gets buried because of the repetitive averaging it implies. Could you please clarify if it is the case here?

L.191:
I am missing information about the initialization of the models. Which density, temperature, grains size profiles were used at the inland model boundary?

l.231: Please add Porter and Mosley-Thompson (2014)

l.239-240: Not sure what is meant by "increases complexity to horizontal advection signal". Please rephrase.

l.267: do you mean " the inclusion of meltwater infiltration in the firn model"? Do you have a model run where meltwater percolation is not included at all? Or do you mean compared to the "shallow percolation" scheme. Here I am also missing a presentation of the impact that the scheme of Meyer and Hewitt has on the results (see general comment).

l.274: Please replace “differs” by “decreases”. Which density value is discussed here: the average for the whole column, the average above pore close off or the maximum density deviation?

l.275: replace “to pore close off” by “of pore close off”

l.311: What is the simulated pore close off depth at Crawford Point? In figure 3 it seems to be 45 m but how was it initialized? What depth was the pore close off in Higgins’ core? Not sure whether it is the right place for this model validation, but this information should be somewhere to assess the model’s reliability.

l.324: “unpredictable” is quite definitive, maybe fine a softer word

l.341-343: Very interesting finding. Could you specify for which variable: density, pore space, temperature or all of them? Maybe add to abstract?

l.347: replace “package” by “parcel”

l.365: This is also an interesting result. Maybe worth to be in the abstract? Please refer to figure 3 where the reader can appreciate how different the firn thickness is on the the EGIG and Jakobshavn transects.

l.390: It is unclear where does the one third value originate in Figure 5b. Could you clarify?
This part could be made more clear and impactful by referring properly to specific item in Figure 5b.

Since the contribution from advection to the MFP trend is period-dependent, it is maybe oversimplifying to give "one third" as a general contribution and it would be more useful to give the specific value for few periods:
"Over 1765-2007 period, a MFP trend of 0.08% yr-1 was found in the core compared to 0.04% yr-1 in our model that includes ice flow but no warming. This indicates that half of the trend in the core can be explained by advection. Over the 1900-2007, the observed MFP trend was 0.11% yr-1 among which X % yr-1 (please provide trend for same period) is due to advection according to our simulation. This indicates that over that period, warming contribution to MFP trend grew to X% and that the advection signal was minor."

L.395-405: This paragraph is rather confusing.
If advection is found to have a visible impact on the MFP present at Crawford Point, given that MFP is linked to firn ice content and therefore firn density, then advection should have a visible effect on firn density.

The fact that the upper EGIG transect is apparently "barely" impacted by advection is just because that impact is compared to lower areas where advection has a much higher impact.
I would therefore either remove the whole paragraph or rephrase to something such as:
"Figure 3 indicates that the firn at Crawford Point (located at the km 50 on the EGIG transect) is relatively less impacted in term of density and temperature than lower sites on the transects. Yet our results show that this relatively small impact is still sufficient to have a visible impact on MFP and on inferences regarding recent climate evolution at that site. Our work also indicates that interpretation of MFP at sites where advection has a stronger impact (lower accumulation, steeper topography or higher velocity than Crawford Point) should not be done without estimation of the advection component."

l.407-420: This paragraph seems out of place or insufficiently developed. If it aims at providing a simple way for evaluating the advection impact on MFP at a location, then it needs to be compared to ice core observations at Crawford Point and to the output of your more advanced model.
It cannot be left as "this should work". Otherwise I would recommend removing this section.

A nice addition would be to produce a map of advection-related MFP trend over the Greenland firn area. Given a 30 year period on which climate trend is usually evaluated, maps of velocity, accumulation and melt, one could use equation 5 and 6 to calculate that map. This would give a great opening to the study and definitely increase its impact.

L.429: Replace “failure” with softer word: "unsuitability", "shortcoming", "limitation"...