RESPONSES TO REVIEWER COMMENTS

Editor:
More specifically I would advice to certainly address the comments related to the reporting of the sensitivity analysis and the quality of the plots as highlighted by reviewer 2.

> Please see changes and responses as outline below.

Reviewer 1:

-There is a sign error in equation (4): the advection term should be negative as written.

This correction has been made in the revised manuscript.

Reviewer 2:

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.

While we understand the argument, we disagree with this opinion. Our opinion is that if we were to combine results and discussion, this would actually make the material *more*

difficult to follow for the majority of readers; and certainly more tedious to wade through repeated 'this is a result' and 'this is an interpretation' clauses. Furthermore, this approach would orphan some material reserved for discussion sections such as our treatment of uncertainties. And, with regards to Section 3.3. we already have words to inform the reader that we will come back to interpretation of this topic in the discussion section.

Most of the plots lack clarity and readability:
-Y-axis in Figure 2 should be "Relative difference in air content (%)"

Relative difference is (difference/original) where we calculate the percent difference (difference/average). This is described in the paper's methods as such; no action taken.

-Label for line colors should be added in the legends in figure 2, S2, S3.

Understand the opinion, but we feel that it is important to minimize the clutter in the figure. In fact, many journals have explicit directions to put as much information in the caption as possible to reduce clutter in the figures. In this case, we do not believe that extracting the information from the caption is onerous.

-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.

Suggested label changes have been done. As above, we believe the merits of avoiding clutter outweigh the burden relying on the caption for this information.

-Figure 5a right label units should be m.

Fixed.

-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,

Table fixed.
We label the figures with 'Speed' because the entity here is magnitude (velocity = magnitude + direction). Text and figures are consistent with our use of speed.

-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.

Done; consistent.

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.

Again, we understand the argument behind this opinion, but our opinion is that the paper must balance presentation of results with readability. Documentation of all details of the sensitivity analysis would detract from the most important points of the manuscript, and Figures 2 and 4 already present comparisons between different melt infiltration schemes for the synthetic and real world scenarios.

We do, however recognize that results presented in the figures should be discussed in the text. We have therefore added text to the results section describing the Meyer and Hewitt output from the sensitivity testing and transect modeling.

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?
This has been reworded to clarify.

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?
This information has been added.

l.231: Please add Porter and Mosley-Thompson (2014)
We are confused by this request as this paper has nothing to do with ice surface velocity. No action taken.

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

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).
Sentence reworded to clarify.

In addition, we have included a paragraph explicitly describing sensitivity testing results from the suite of infiltration schemes. This includes the continuum scheme from Meyer and Hewitt.

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?
Done.

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

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.

Fixed. This is a misunderstanding by the reviewer, as Figure 3 only presents the lowest 50 km of each transect, and therefore does not include Crawford Point. We have changed the transect extents in Figure 3 and supplemental Figures S4 and S5 to show the full transect results.

We do indeed include a statement concerning the model validation in Section 4.3. Our model age is within 7% of that presented by Higgins.

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

l.341-343: Very interesting finding. Could you specify for which variable: density, pore space, temperature or all of them? Maybe add to abstract?
Done. And, this was already in the conclusions.

l.347: replace “package” by “parcel”
Done.

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.
Indeed, the paragraph opens with a reference to Figure 3, two sentences prior. A second call out here would be repetitive and unnecessary; no action taken.

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."

This section has been reworded to accommodate this suggestion, and to improve the overall clarity.

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."

We have reworded this paragraph to clarify, including some of the wording suggested. However, we feel it is important to take the further step of explaining how this non-intuitive result can be explained. Indeed, the first sentence of this response seems to question the result. The second part of the paragraph provides discussion. We have also reworded this section for clarity.

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.

While producing such a map is technically trivial, we feel this would violate our personal standards for scientific quality. Regional climate models have substantial and non-uniform uncertainty, and the velocity datasets for much of the percolation zone, especially at higher elevations, are not up to the task. For example, velocity products for the featureless snow covered regions contain voids and artifacts; in fact, some products are actually a computed balance velocity over these areas. So our position is that using these data for such an analysis could suffer from garbage-in/garbage-out. Instead, we provide an analytical solution to the problem so that any future researcher can apply this with the opportunity to make their own assessment of data quality for their region of interest.

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