Review 1

Review for *Combining traditional and novel techniques to increase our understanding of the lock-in depth of atmospheric gases in polar ice cores - results from the EastGRIP region* by Westhoff et al.

General Comments:

The authors present and interpret new optical data to investigate the LID and bubble close-off in an EastGRIP ice core. The method is novel, and the data could be the basis for a strong paper. Nonetheless, I have significant concerns about both the organization and the strength/clarity of the scientific reasoning presented in the manuscript.

One major concern is that the manuscript focuses primarily on the OLID, but the scientific motivation for determining a specific OLID is not clear. The results plotted in Figure 2d and f and Figure 7 have more importance in the broader scientific context of understanding bubble close-off and delta age, given the obvious shortcomings of the Goujon/Barnola parameterization. If the OLID is not the actual motivation, the authors might consider broadening the focus to the depth-range of bubble close-off and the associated implications for delta age.

A second major concern is that the exact methodology for finding the OLID and the "bubble proxy" is poorly explained. As the authors state, this is a new methodology, and it needs to be very clear to the reader. Sections 3.2-3.3 are spent describing and interpreting data from a new method, which the reader has no way of understanding. The information in section 3.4 and Appendices A-D should be given before the data is presented and interpreted.

Thank you for the review of our manuscript. You have pointed out many valuable points that will help improve the manuscript. We will implement your review into the new version of the manuscript.

The major objective of the manuscript is to describe and evaluate a new experimental method to locate firn bubble close-off. New direct experimental constraints are especially useful for a complex site that challenges model limitations. The EGRIP site is affected by strong horizontal ice flow influencing firn densification and structure, as well as climate change further inducing time variations in firn temperature and snow accumulation. Such processes are not represented in a 1D steady-state model. In this non-steady state firn context, delta-age is likely to have changed in the recent past, however, estimates of delta-age and age distributions in ice will be provided. An alternative to the Goujon/Barnola parameterization (Mitchell et al., 2015) has been tested as suggested, and results will be provided in Figures 2 and 7.

We will restructure section 3, as mentioned here and in the specific comments. We thereby hope to make the structure of the manuscript better.

Specific Comments:

Section 1: The scientific motivation and larger context for this study are poorly defined. Namely, why pursue an optical method for determining the LID? Or, stated another way, what do we learn from 58.3 m that we didn't learn from 58-61 m? Does it provide more precise information about the delta age? If so, more time should be spent discussing the importance of delta age and the physical site characteristics that control it. Section 1.3 is labeled "Motivation," but it summarizes the paper rather than providing scientific motivation.

We will rework the introduction and the motivation. We will lay a larger focus on the delta age and what we can gain from the different methods.

Section 2.1: This section should be combined with 1.2 and should probably come after the Introduction.
We will rearrange the beginning of the paper. Motivation will be moved forward as the new section 1.2. Then Site locations and variations between cores will be combined thereafter.

It's not clear which sites are affected by compaction due to flow.
We will clarify that all cores are affected by flow, except NEEM.

An additional note: Here and throughout the paper, EastGRIP and S6 seem to be used interchangeably. (For example, Figure 2e is labeled EastGRIP open porosity, but I assume it is the same firn air pumping campaign from S6 that is plotted in 2a and 2b). Please clarify throughout the paper
That is a very valid point and will be addressed throughout the paper. In the discussion section, where we assume similar features for both cores, we will clarify this.

Figure 2: please comment on the data gap between 72 and 75 m in 2c and 2d.
We will include that the ice quality was too poor on that section for the gas CFA methodology, with 7 to 8 breaks along a CFA stick.

Section 2.3.4: Did the authors consider trying the Mitchell et al., 2015 parameterization, which has a more gradual bubble close-off? How realistic is the modified porosity profile relative to other measurements and parameterizations? The tracers used to tune the model should also be clarified.
Thank you for suggesting the use of Mitchell et al., 2015 parameterization. We implemented it in its "bulk" form (Equations 6 to 9) and it led to an improved match with the upper part of the methane data in the closed porosity. The modified porosity tested in the manuscript was not meant to be optimal but to illustrate the impact of the chosen parameterization. Figures 2 and 7 will be modified to include these results. The tracers used to tune the diffusivity in the open porosity are: CH_4, SF_6, CFC-12, CFC-113, CH_3CCl_3, and HFC-134a. Using only methane leads to nearly the same results. It will be clarified in the manuscript.

Section 3: Overall, the clarity of the scientific reasoning in this section needs improvement.
We will restructure the section, add a figure for clarification, and increase scientific reasoning.

Section 3.1: It is not clear whether these measurements were made as a part of this research or if they are previously published. If they are previously published measurements, they should be cited.
They were not made in direct connection to this research, but have also not been published before. We will include the information.

Section 3.2.1-3.3: The information in section 3.4 and Appendices B-C is necessary to understand these sections. Please reorganize.
We agree that these sections need reorganization. Section 3.4. will be moved forward to better introduce the visual effect of bubbles on the line scan images. A short version of appendix B and C will be added to the main text and the reference to more details in the appendix will be made clear.

The geometry of the 1x5 cm2 relative to the 165 cm slab is not clear. A figure similar to 2h in Westhoff et al. (2020) would be clarifying.
The "bubble proxy" is not explained:
1) Does one bright spot correspond to one bubble?
2) How is a "bright spot?" defined? Why use one pixel cutoff value versus another?
3) Can a single bright spot be more than one pixel?
4) Can a bubble be more than one pixel?
5) Is the proxy qualitative or quantitative?
6) Is the basis of the proxy empirical or theoretical? If theoretical, section 3.4 needs additional details.
Thank you for pointing this out. We will create a figure accommodating the comments above. We will also make sure to elaborate on this clearly in the text.

Figure 5b: please explain the different pixel cutoff values. It appears that the results are quite sensitive to the choice of 60.
The choice of 60 is arbitrary and based on what appears bright by the eye. Similar results were obtained for values of 50 or 70, but not shown here. The values of 150 and 250 show similar trends, yet with a much lower amplitude, not making them feasible for a visually appealing plot. This will be explained in the manuscript and also implemented into the figure from the comment above.

3.3 is labeled "Density and Visual Stratigraphy derived lock-in depth," and the authors seem to infer that the density measurements suggest the LID is the 58.3 m layer but L162 states: "Between 55 and approximately 67 m depth, density values lie between 790kg/m3 and 830kg/m3, suggesting this to be the LIZ." Please clarify.
This is indeed misleading. The density part will be removed from the title. The LIZ ranges over the depth from 55 to 67 m. The specific depth derived from the visual stratigraphy is 58.3 m. Which is also confirmed by the density measurements. This will be clarified in the next version of the manuscript.

Section 3.4: This section is the crux of the methodology/proxy and therefore needs more scientific justification. It also needs citations. It is not enough to draw a picture of what may be happening without explaining the underlying optical physics:
1) Why is it only closed spherical bubbles that make bright spots? What about mostly spherical pores that aren't completely closed off? It seems like any curved air/ice interface could potentially act as focusing lens if it is oriented correctly?
2) Section 3.2.1 describes light "reflection, refraction, and scattering" but Figure 6 shows light refracting and focusing. Please clarify.
3) What is happening with the closed pores between 50 and 58.3 m? Are the closed pores in that depth interval "odd shaped" and they ultimately evolve towards spherical?
4) What is the evidence for closed pores at 50 m? It does not appear to be from the line scan data.
Citations will be added to better the underlying optical physics. We will refer to the shapes and their effect on the line scan images separately and also clarify the wording of reflection, focussing, etc. The questions from 1) and 3) will be addressed in the manuscript. 4) is answered in the appendix and will be included into the main section of the manuscript. The entire section 3.4 will be moved forward to better explain the visual stratigraphy in sections 3.2 and 3.3.

It seems like a melt layer such as the one mentioned in 3.1 would make an effective impermeable layer, potentially preventing diffusion without producing any bright spots. Please address this.
This is very true and will be included in the discussion. The melt layer could create an impermeable layer and thus induce the lock-in, yet this one does not coincide with the appearance of bubbles and results from the firn air pumping. We will elaborate on this in the discussion.

Section 3.5:
The relationship between layering, bubble close-off, and the LID Is not clearly explained. Please clarify.
The text implies that the presence of a single bright spot is evidence that a layer is impermeable. But, if a single bright spot corresponds to a single bubble, that does not make sense. Please clarify.
"Some layers have bright spots, while others appear darker (d), going hand in hand with the number of bubbles we find (e)."- e) only shows number of bright spots… please explain what is meant by "number of bubbles you find" Is it just the number of bright spots? Do the maximums in pixel value correlate to maximums in bright spots? If so, please make a plot that shows the covariance or do a statistical test because it is not obvious from Figure 5d-e. Please clarify.
Thank you for pointing this out. It is indeed not clear and we will include a figure for clarification and address the points you mentioned.

Section 3.6.1- The information in appendix D is necessary to understand this section. Please reorganize.
It needs to be mentioned these calculations are done using closed porosity from parameterizations, not data here.
We will mention that this is calculated from closed porosity parametrization.
For a better understanding of the section, we will include appendix D into the text. As also mentioned in the comments further above, restructuring of section 3 is necessary and will be done.

Section 3.6.2
This section is potentially useful for improving understanding of delta age and the age distribution of air trapped in polar ice. I recommend a more detailed discussion here.
Can you show the Mitchell parameterization of Figure 7? Or even better implement it in the firn air model?
The discussion will be extended to increase the focus on delta age and age distribution. It will also be mentioned more clearly in the motivation. Delta age distributions and mean values obtained with all tested parameterizations, including Mitchell et al. 2015, will be provided.

Section 3.7
The authors state that there is a correlation between closed pore space and image brightness, but it is not clear where the information about closed pore-space is coming from unless it is the image brightness. Please clarify.
We will elaborate further on this, as it is solely from the image brightness.

Section 4

This should just be “10-50” years, not “± 10-50 years.” Moreover, the mixing delay should be easy to calculate with the firn air model. Why not use that instead of Schwander’s “typical number?” Additionally, the authors state earlier in the paper that some closed porosity forms as shallow as 50 m. This is not accounted for in paragraph 2. Please address.

We will remove the plus-minus, clarify the ”as shallow as 50m”, and add the following:
From the porosity parametrization, we have three delta-age probability distributions: blue 344 years, green 315 years, and black 337 years (fig. X). The results are dependent on the model limitations, e.g. steady-state 1D model with fixed accumulation rate and temperature, no layering, no flow-related thinning, etc.).

Section 5- please rephrase this first sentence “It is important to…”
Other stylistic notes:
Phrases like “hand in hand” (L243) and “has been around for a long time” (L230) are not appropriate for a scientific manuscript. I suggest something like “Layering in the LIZ can influence bubble closure (Blunier et al., 2000; Fourteau et al., 2019)” and “The maximum pixel brightness covaries with the number of bright spots” at those lines.
In general, please carefully proofread the grammar and give some careful thought to phrasing. There are many opportunities to make the writing clearer and more concise. For example, L296-297 could be revised to:
“All the data presented in this work indicate that the transition between the diffusive and non-diffusive zone in the EastGRIP area occurs between 58 m and 61 m.”
And line 283-284
“Density measurements and visual stratigraphy data can reveal more details about the firn-ice transition.”
Thank you for the suggestions, they will be implemented. We will also review the manuscript for clearer and more concise language.

Please clean up figure axes and axes labels. Some labels are left justified, and some are centered. Centered is best throughout.
Axes and labels will be cleaned up and centered in a consistent manner.