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Climate of the Past Discussions

Interactive comment on "Gas enclosure in polar firn follows universal law" *by* Christoph Florian Schaller et al.

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– Please also find this response attached as a pdf. –

We would like to thank Anonymous Referee $\#1$ for the willingness to act as a reviewer. Respectfully, however, we do not agree with several of the provided comments and the reviewer's general judgement of the manuscript. This response is an attempt to resolve these issues. We ask for this not to be seen as uncompromising - we are not unwilling to make changes and improvements to the manuscript, and have still incoorporated as many of the comments as possible - where we disagree we have explained our reasoning fully.

The referee comments are displayed in italics, followed by our responses in normal

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font. For the cases where we did not follow the reviewer's suggestions, we discuss the reasons for our decision.

General: The authors present closed porosity data of firn and ice samples from three different polar sites in Greenland and Antarctica using 3D X-ray tomography. They find a 'universal' critical closed porosity where bubbles are sealed. While the technical approach seems robust and data are very interesting the interpretation and conclusions are too simplistic. The authors give the impression of being much closer to the physical reality than previous investigations and even accusing researchers in this field of producing corrupt data and misinterpreting them; however important details are not fully elaborated in this paper. The most important is how the scale-dependent porosity affects parameters like D-age and total air content (details in the specific comments below). The paper needs crucial revisions before it should be considered for publication.

General comment regarding accusations:

We want to clarify that we did not intend to "accuse" other researchers of anything. In our study, we show that there is a systematic error source that was underestimated in previous studies, yielding "misleading" results. Hence, we use the term "corrupt" in order to emphasize that the data do contain a systematic error, which significantly changes the results. There is no accusation in that as we are certain that this error was not underestimated or ignored intentionally.

General comment regarding the cut-bubble effect:

As we will further elaborate in the specific comments, we prove a massive underestimation of the cut-bubble effect. We are able to do so because we determine the microstructure a larger volume of firn (4 cm height, 8–10 cm diameter) and then use a sub-volume (1 cm height, 6 cm diameter) for our analysis. This way, we can determine the total and closed porosity either

1. similar to previous research (where cut bubbles would be counted as open pores as the air gets sucked out of them) or

2. eliminating the cut-bubble effect (by tracing cut bubbles within the larger volume and thus being able to decide whether they are open or closed).

The result can be seen in our manuscript, Figure 2b. The effect does not only change the values by factors of up to more than two, but also significantly influences the shape of the curve. A main reason for that is that (especially near close-off) closed parts of the pore network can extend over several centimeters, which is a classical percolation phenomenon. Previous studies estimated that correction factors of up to 1.1 would be sufficient to correct for this effect.

General comment regarding the introduction:

Several of the referee's comments do refer to "vague", "not very informative" or "undefined" statements in the introduction. We do think that the introduction is not the place to go too far into detail, but rather give a general overview of the related literature and provide a motivation for the presented study.

We'll address the specific comments below. As this only requires minor changes to the manuscript, we do not agree with the referee's demand for "crucial revisions".

Specific comments: p. 1, l. 18: "direct record" seems not very informative. Probably direct records do not exist in the ice, but there is a large range of "indirectness". It should either be defined or reworded.

In the scope of an introduction, we consider it worth mentioning (especially to readers that are not from the field) that the gas record is still influenced during the entrapment process. In addition, right after saying it is not a "direct record", we do further elaborate on what we intend to say with that. Similar terminology was used in published studies, e.g. Mitchell et al., 2015 "The air trapped in ice sheets is not a direct record of the past atmospheric history" (Introduction). (see also "General comment regarding the introduction")

p. 2, l. 4: "direct measurement": the same as above.

Actually the context is different - here we are talking about measurement methods for firn microstructure and no longer the gas record. In contrast to previous researchers who relied on melting firn samples and deducing microstructural parameters from the results (= indirect method), we do conduct non-destructive 3D radioscopic imaging and thereby direct measurements of firn microstructure.

p. 2, l. 12: "statistically solid dataset". This is a much undefined statement. Each record contains certainly a large number of data, but what does statistically solid mean?

We conducted microstructure measurements throughout the lock-in zone for three cores, analyzing an unprecedented number of samples (each of a representative volume) per core. The results are very consistent and have been error-checked in various ways, amongst others comparison with other (independent) methods and repeat measurements. In contrast to previous methods, we are able to estimate uncertainties. In order to emphasize this in an introductory manner, we use the (admittedly vague) term "statistically solid". (see also "General comment regarding the introduction")

On the other hand universality of the critical porosity is deduced from only 3 records, which seem very marginal for such statement.

We used three cores that well reflect the temperature and accumulation rate range for polar ice cores. Previous literature would predict large differences in the critical porosity observed for these three sites, while we observe only marginal differences. Of course there is a certain probability that we by chance sampled anomalous sites / ice-cores, but we consider it highly unlikely to obtain the same anomalous value three times.

p. 2, l. 17: "The reduced coupling of proxies and surrounding conditions...will foster the development of new proxies, such as the air content as a marker of local insolation". This statement is somewhat unclear. I agree that it may help to put the interpretation of existing proxies on a more realistic basis, but to foster the development of new proxies is a very vague statement that calls for specific arguments.

As quoted, an example of a new proxy ("air content as a marker of local insolation") is provided.

In Chapter 5 (Implications), first paragraph, we do further elaborate on how our results will help understanding the air content and thus establishing a proxy and not only putting in on a more realistic basis. (see also "General comment regarding the introduction")

p. 2, l. 27-30: This section requires some elaboration: "each data point": of what?

Of the data sets presented in our study, see e.g. Table 1, Figure 1, Figure 2. We will add ".. each data point (as referred to e.g. in Table 1) ..." for clarity.

"the remaining cut bubbles were less than 0.1%". How was this value determined? As the "sample" volume (1cm x 6 cm diam.) has a similar surface/volume ratio as a typical sample for porosity or total gas measurement this low value seems very surprising. Values in the order of $5-10\%$ in the firn-ice transition zone would seem more realistic.

Context: "For each one meter core segment, we scanned a minimum number of five sections of approximately 4 cm height and the full core diameter (8–10 cm) with a focus on homogenous layers. [...] To eliminate the effect of cut bubbles at the surface of the sample (Martinerie et al., 1990), each data point corresponds to a layer of approximately 1 cm height and 6 cm diameter. Having the microstructure of the surrounding material in all directions at hand allows us to safely determine whether a pore is open or closed. For all measurements, the remaining cut bubbles were less than 0.1% of the pore volume."

In other words:

1. We have the three-dimensional microstructure of a larger volume (4 cm height, 8–10 cm diameter) at hand.
2. We take a smaller subset (1 cm height, 6 cm diameter).
3. For most pores cut by the subset boundaries, we can still deduce whether they are open or closed because we know how they continue in the surrounding material.
4. There are some (= the "remaining") bubbles, that are part of the smaller subset, but even within the larger volume it cannot be decided whether they are open or closed.

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5. We take the volume of these bubbles that lies within the subset and divide it by the total pore volume in the subset.

6. We obtain values smaller than 0.1% for each sample.

Indeed, knowledge of the surrounding material is one of the main advantages of our method over previous approaches. It allows us to determine the effect of cut bubbles and show it was crucially underestimated. The value of 0.1% refers to the volume fraction of pores for which we could not decide whether they were open or closed and not the volume fraction of cut bubbles.

In addition, the reviewer states that for the volume fraction of cut bubbles values in the order of $5 - 10\%$ would be realistic. However, we show (Figure 2b; Chapter "Discussion", second paragraph) that correction factors of up to 10% as applied in previous studies seriously underestimate the cut bubble effect. This is a key result of our study. (see also "General comment regarding the cut-bubble effect")

p. 3, l. 6: I suggest to replace "percentage" by "fraction" as the value is not given in percent

Will be replaced.

p. 4, l. 3: "Our estimation (Fig. 2b) proves a serious underestimation of the cut bubble effect and, in particular, confirms the existence of a critical porosity in contrast to recent assumptions of single-layer close-off occuring within a certain porosity range (Mitchell et al., 2015)". [occuring -> occurring]. I think there is a misunderstanding here. Mitchell et al. actually confirmed local density (or porosity) as a good predictor for bubble closure. They only introduce stochastic variability of local density (porosity), which is well documented by measurements, to better describe the layering. But indeed there is a difference in the shape of the closed porosity (or total gas) vs. density function. Although various researchers have carefully corrected for cut bubbles an underestimation of this effect cannot be excluded. A smooth transition toward 100% *closed pores as observed and still present after cut bubble correction contradicts your tomographic results an also simple percolation theory. This calls for further studies.*

[occuring -> occurring] will be corrected.

"Although various researchers have carefully corrected for cut bubbles an underestimation of this effect cannot be excluded." - See next-to-last answer. (see also "General comment regarding the cut-bubble effect")

Regarding Mitchell et al., 2015: We do not doubt that local density (or porosity) is a "good predictor" of bubble closure, indeed it is the determining factor. In addition, there is also nothing wrong with incorporating variability of local density to represent layering. However, they model the (local) critical porosity as a random variable, which does not seem to agree with our data. The study suffers the same problem as previously mentioned here and described in our manuscript – porosities are determined indirectly by melting/vacuumization and the cut bubble effect is only corrected for by a constant factor of 7%. Thus the closed porosity versus local density data presented show a smooth transition instead of an abrupt close-off. Mitchell et al. try to represent this in their model by making the critical porosity a random variable. However, as the data are corrupt, the model does not represent the behavior of polar firn.

p. 4, l. 5-10: This paragraph needs clarification. First, it is unfair to speak of corrupted datasets. All measured data have errors. Not all systematic errors may have been fully addressed, but therefore they are not corrupt. Then it is most confusing to mention 37% critical closed porosity without presenting its context. This value simply relates the total gas data to the equivalent density (or porosity, or closed porosity) assuming virtual instant close-off. This has not much to do with the local pore close-off discussed here. Instead of suggesting "avoidance of such concepts" the authors should rather carefully discuss that beside the local pore close-off (at 100% local closed porosity) other factors affect total gas content in the ice (comparison with Martinerie data; Fig 3) and the concept of non- (or low-) diffusivity below a certain depth with a bulk porosity significantly above 0.1, which is crucial for the ice age – gas age difference.

The cut bubble effect introduces errors larger than 50% near close-off (see Figure 2b). Using X-ray tomography on large volumes, we had the first opportunity to measure this effect. It turned out, it was underestimated by other researchers who did not have this opportunity. (see also "General comment regarding the cut-bubble effect" and "General comment regarding accusations")

Regarding other factors influencing the total air content - this is discussed in detail in Chapter 4, second-to-last paragraph: "Even though a single layer closes off at the same critical porosity, sealed layers may have variable air contents. Above the close-off depth, we determine average coefficients of variation for the total porosity of 1.3% for B53, 1.8% for B49 and 2.5% for RECAP_S2. Higher porosity variability will lead to a larger amount of shallowly trapped bubbles, thereby increasing the air content V (Stauffer et al., 1985). In our case, shallow trapping is characterized by the different slopes of the lock-in curves given in Fig. 2a, leading to increased air contents of about 2% for B49 and 8% for RECAP_S2 in comparison with B53. In addition, the lock-in zone extends over a depth range of approximately 7 m for B53, 9 m for B49 and 15 m for RECAP_S2. Larger lock-in zones are expected to cause enhanced sealing effects (i.e. permeable layers being sealed by impermeable ones above). This further increases the air content (Stauffer et al., 1985). The effect is hard to quantify as our measurements do not yield information about the spatial extent of horizontal layers. Nonetheless it may explain the 8% and 27% larger air contents for B49 and RECAP_S2 (compared to B53) respectively, that V measurements for deep ice cores would predict (Martinerie et al, 1992). "

p. 4, l. 13: "cannot resemble" -> "cannot fully reflect"

Will be adjusted.

p. 5, l. 16-23: As mentioned above the local pore-close off is not the parameter that determines delta-age and delta-depth. It is rather the depth where diffusivity approaches zero. Better knowledge of the local close-off mechanisms is certainly very interesting but does not help to resolve the discrepancies in a simple way as suggested here

Even though, as stated in our manuscript, critical porosity is neither the only nor the main parameter determining delta-age or delta-depth, its temperature dependence is used in the cited delta-age calculations. Furthermore, we are aware that the simple calculations conducted in our study are not how delta-age is modeled these days. It was not our intention to do a full delta-age model, but rather estimate the dimension of the influence that avoiding the temperature-dependence introduced by Martinerie et al., 1992, has.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2017-94/cp-2017-94-AC1-supplement.pdf

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2017-94, 2017.