RESPONSE TO REFEREE 2, cp-2019-89:

We are thankful to the referee for their useful and constructive comments.
We will first answer to the comments about the mismatch between air content measurements and modeling. A point by point response to the review is provided after. The text in blue is the text of the review, and the corresponding responses follow in black. The typos pointed-out by the referee will be corrected in the article, and are not specifically addressed in this response.

As pointed out by Mitchell et al 2015 and the referee, there is a difference between applying the density-closed porosity relationship to the a smooth density profile (called scenario A in the review) and applying it to a high-resolution profile before smoothing (scenario B). In the article we used scenario A. As pointed-out later, this will be clearly explained in the text (P14L22).

We tested scenario B and its influence on the modeled air content. For this, we created a heterogeneous high-resolution density profile by applying Gaussian variability to the smooth density profile. The firn was divided into two-centimeter-thick homogeneous layers, and each layer was given a random density anomaly. The Gaussian distribution of density anomalies was parametrized to reproduce the features of the high-resolution density measurements (standard deviation = 7.5kg/m3, see Figure 1 below). We then applied the local density-closed porosity relationship to derive a high-resolution closed porosity profile, and smoothed this closed porosity profile (because the Rommelare et al 1997 calculations requires monotonous profile). As pointed out by Mitchell et al 2015 and the referee this shifts a part of the closure profile towards higher-density (see Figure 2 below). However, using this methodology did not lead to a significant change in the final modeled air content (0.1016 cm3/g instead of the 0.102 cm3/g in the article), and does not explain the model-data mismatch. We also performed a sensitivity analysis: altering the thickness of the homogeneous layers and/or the standard deviation of the density anomalies did not improve the model-data mismatch.

Moreover, we would like to point that if all firn layers close in the same fashion (same density-closed porosity relationship at the centimeter-scale), we should expect similar air content in them, despite their difference in closure depth. Indeed, assuming that sealing effects can be neglected, the bubbles in the various layers form at similar local porosities, and with similar temperature and pressure in the open porosity, meaning that the same amount of air is trapped. This is consistent with our air content measurements of an early closure layer reported in Figure 10 of the article. We should therefore expect scenario A and B to result in similar air content values. In our understanding, this is what explains the low air content variability in the mature ice of WAIS (Figures 3 and 4C of Mitchell et al 2015).

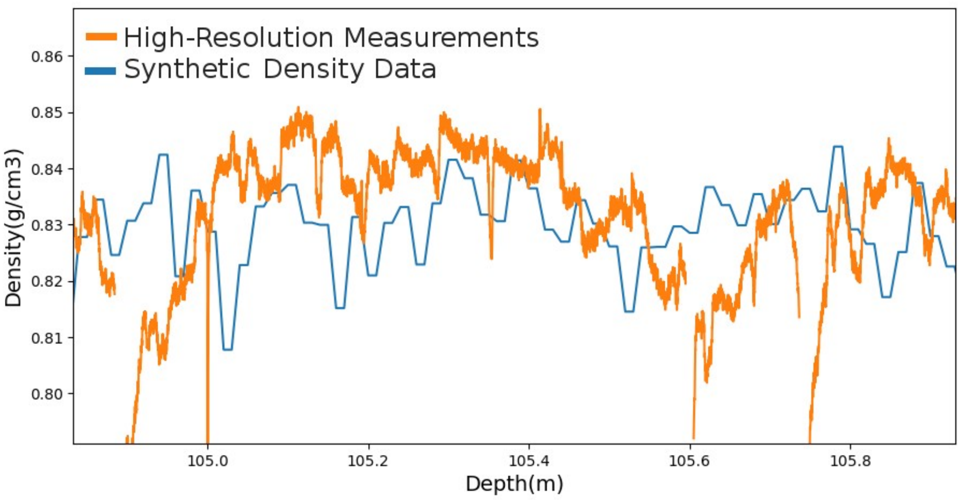

Figure 1. In orange: high-resolution density measurements of the "Lock-In" firn core. In blue: synthetic density profile reconstructed from smooth data with added random centimeter-scale variability.

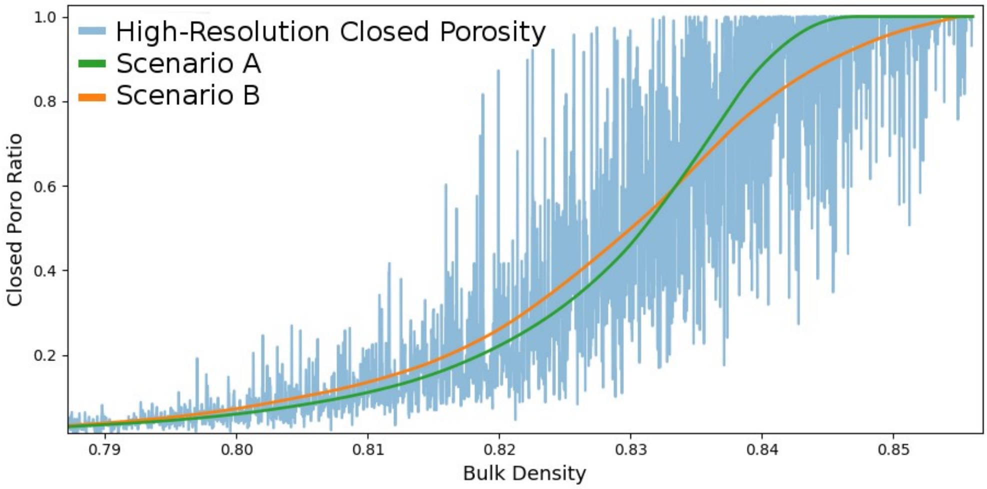

Figure 2. Closed porosity ratio profiles. In green: centimeter-scale density-closed porosity relationship used with the smooth density profile (scenario A). In blue: centimeter-scale density-closed porosity relationship used with the synthetic density profile. In orange: smoothed version of the high-resolution closed porosity data in blue (scenario B).

We also tested the closed porosity parametrization proposed by Mitchell et al 2015. The parameters can be adjusted to reproduce the measured closed porosity data, but the air content calculated with the Rommelaere et al 1997 method is then similar to the one the article (0.099 cm3/g instead of 0.102cm3/g). To predict a lower air content value, the closed porosity parametrization would have to be shifted toward higher densities, in a way not consistent with the tomography and pycnometry data (see also the last paragraph P14 of the discussion paper).

The other comments of the reviewer are addressed below. Typos are not specifically addressed but will be corrected in the article.

P2 L10: Witrant et al. investigate the thickness of the lock-in zone, which is technically speaking not the same as the trapping zone.
Indeed, we will provide a more relevant reference (Schwander et al 1993)

P2 L23: Note that this phenomenon was also recently observed at Styx Glacier by a Korean team (Jang et al., https://doi.org/10.5194/tc-2019-17).
We will add the Jang et al 2019 article to the list of layered gas trapping observations.

P8L1-5: Consider citing the original papers on this method, such as Chappllaz et al 2013 and Stowasser et al. 2012
We will modify the article to cite the original CFA papers with *"During the drilling operation, about 100m of mature ice was retrieved and later analyzed using gas continuous flow analysis (gas CFA, first developped by Stowasser et al 2012 and Chappellaz et al 2013). Methane concentration in enclosed bubbles was measured using the gas CFA system of IGE coupled with a laser spectrometer SARA..."*

P9L19: consider citing Freitag et al. 2002 here
We will cite Freitag et al 2002 on using CT to distinguish between open and closed pores.

Figure 2, caption. Perhaps you can emphasize that the rho-CP relationship applies at

the cm-scale, and cannot be applied to bulk density data.
The remark that our data apply at the centimeter-scale will be made in the text of the article P9L9 *"As pointed-out by Mitchell et al 2015, the relationship between density and closed porosity displayed in Figure2 is valid at the centimeter-scale, but not necessarily at larger scales."*

Figure 2: Could you add a panel where you compare your new data to the various rho-CP parameterizations that are found in the literature? Barnola, Schwander, Mitchell, etc?
The Schwander and Mitchell parametrization will be added to Figure 5 alongside the Barnola and Schaller parametrizations. We will also introduce them in the text P19L16 *"We also displayed in Figure 5 two other closed porosity parametrizations proposed in the literature, namely the Schwander et al (1989) and Mitchell et al (2015) parametrizations. The Schwander et al (1989) parametrization closes at lower density than the "Lock-In" data. This is to be expected as this parametrization was proposed to represent relatively warm and high-accumulation sites. The Mitchell et al (2015) parametrization has been adjusted to fit the "Lock-In" data (ρ_co = 840 kg/m3 and σ_co = 2 kg/m3 in Equations 5 of Mitchell et al , 2015). It shows an overall good agreement with the closed porosity data, and results in a 0.099 cm3/g simulated air content in the Rommelaere et al. (1997) model."*

Figure 2: It is very interesting to see that applying the cut-bubble correction correctly (the way you do) makes the transition at CP=1 much sharper/abrupt. That is an interesting finding. The prediction from percolation theory is indeed that the transition should be abrupt – however, previous data sets often made this transition appear somewhat smooth. This difference has long puzzled me, and so the smoothness may have always been an artifact of the cut-bubble effect. Please discuss this briefly. In Mitchell et al. we introduced the parameter sigma_co to fit the smooth transition in the available porosity data (page 2566); based on your observation, we may have overestimated the value of this parameter (could you suggest a better value?!).
Indeed application of the cut-bubble correction leads to a sharper transition at CP=1. We will point it out in the article (P12L20) *"It is interesting to note that applying the cut-bubble correction leads to a more abrupt transition at CP=1. This observation is consistent with the results of Schaller et al 2017."*
To reproduce our data a parameter σ_co = 2kg/m3 in the Mitchell et al 2015 seems appropriate.

Equation 3,4: it is common in physics to use single (not multiple) letters to denote variables – this avoids confusion when multiplication is involved. Why not use f instead of frac and R in stead of CP, and V instead of AC (or similar). "AC" in an equation makes one think you're multiplying "A" and "C".
We will modify the variable names to be more readable.

P13: Can you estimate the gas ages at 122 and 145 m depth, for our estimation?
We will put the information will be put P7L14, with the description of the air content samples: *"Based on synchronization between the "Lock-In" and WAIS Divide ice core methane measurements (Mitchell et al 2013), the gas ages at 122 and 145m have been respectively estimated to be 1500CE and 1000CE."*

A dome site like EDC probably has more stable accumulation than a flank site like vostok or lock-in – as the site moves over basal topography the surface slope (and thereby snow redeposition by winds) can change.
It is indeed possible that the variations of air content observed in the Vostok and "Lock-In" ice might be due to changes in accumulation. We however do not have observations to support or reject this hypothesis. Based on radar data (Figure 8 of Verfaillie et al 2012), it appears that "Lock-In" did not experience strong variations of accumulation in the last centuries, but it is not clear that this

observation can extrapolated back in time.

P14L1: the “effective” total porous volume of air isolation
We will change the name of Vi to “effective porous volume”, which is easier to read than effective total porous volume.

P14 L22: see Mitchell et al. (2015) Figure 4, who also found that models overestimate air content when using standard parameterizations
We will add the sentence *“Such a discrepancy has also been reported by Mitchell et al 2015”*.

P14 L18-22: please be explicit about the order of calculating CP from rho and performing the smoothing. So did you use scenario A or B from my example above
As explained before, we used scenario A to compute the air content. It will be made clear in the text with the sentence *“By applying the local closed porosity and density relationship to the smooth density profile, we obtained a smooth closed porosity profile. Using it in the model leads to an estimated air content...”*.
Using scenario B only has a minimal effect on the final air content. This will be pointed out P14L22 *“Mitchell et al 2015 argue that a better representation of gas trapping would be to apply the local closed porosity and density relationship to the stratified density profile, in order to first obtain a high-resolution and stratified closed porosity profile, and to subsequently smooth it. However, application of this methodology did not improve the model/data mismatch.”*

P16L23: I agree with this reasoning. Maybe state also that the driving overburden hydrostatic pressure is much much larger than these small differences in bubble air pressure, and therefore alpha=1 is very much expected.
This point will be made clearer in the text with the sentences *“The driver of pore compression in the firn is ΔP , the difference between the overburden pressure of the ice and the air pressure of the pores (Lipenkov et al., 1997). As reported by Martinerie et al. (1992), the ΔP of open and closed pores differ by less than 8%, and it is not clear how this ΔP difference translates in terms of bubble compressibility difference.”* . Due to the non-linearity of ice and the complex micro structure of firn, it is not clear what precise value is expected for alpha.

P23 bottom line: Maybe also add Freitag et al. 2013
We will add P22L23 *“Using this softening ions hypothesis, Freitag et al 2013 improved the ability of firn models to predict the stratification of deep firn”*.
We will also add a citation of Freitag et al 2013 at the bottom of P23.

Data availability: what about the CH4 data?
The methane data will be made available alongside the rest of the dataset. The data availability section will be modified accordingly.

Best Regards,
Kévin Fourteau on behalf of all co-authors