

***Interactive comment on* “Brief communication: Firn data compilation reveals the evolution of the firn air content on the Greenland ice sheet” by Baptiste Vandecrux et al.**

Anonymous Referee #2

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Review of “Brief communication: Firn data compilation reveals the evolution of the firn air content on the Greenland ice sheet” by Vandecrux et al.

The manuscript describes the firn air content of the Greenland ice sheet. The amount of air in the firn layer is a good measure for the amount of meltwater that can be buffered in the ice sheet and that therefore cannot contribute directly to sea level change. A total firn area is presented based on earlier work and a compilation of 344 firn cores is used to derive a spatial map of firn air content in the upper 10m (FAC10). The firn area is divided into 3 regions: dry snow (DSA), low-accumulation wet snow (LAWSA), and high-accumulation wet snow (HAWSA). For the DSA, no change over time has been

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found from 1953 to 2017, while LAWSA show a substantial decrease over the last two decades with a FAC loss of ~25%.

For me, the manuscript needs substantial revisions before it is suitable for publication in The Cryosphere. The current manuscript is in a sloppy state and would have benefited greatly from another review round by its co-authors. With sloppy, I refer to the lack of flow in the text due to typo's and bad sentence structure in general, but also things that should have been spotted by the author or co-authors before submission. I illustrate this with 3 examples, while all comments are listed in the rather long list of ‘minor comments’ at the end of this review: 1) Some numbers in the manuscript do not add up: the temporal decrease in LAWSA FAC10 is noted (P7, L5-7) to be 180 km³ (or 26%, or 150 Gt), while the absolute amounts presented are 690 km³ (1997-2008) and 520 km³ (2011-2017). This results in a difference of 170 km³, or 24.6%. In the conclusions section, even different numbers are presented (P9, L16-18): here, a 21% decrease from 1998-2008 (1997-2008 and 1998-2008 are used interchangeably, it seems) to 2011-2017 corresponds to 168 Gt of loss in meltwater retention capacity. Such juggling with numbers make the other results also less reliable. 2) There are two references to Fausto et al., 2018 used, but in the text they are not differentiated into Fausto et al., 2018a (snow density) and Fausto et al., 2018b (snow-line elevation). Fausto et al., 2018b is used as basis for one of the main conclusions of the manuscript (the firn area extent), but is not well-known -as it is an internal GEUS report- compared to the peer-reviewed Frontiers paper (Fausto et al., 2018a). It left me searching for a while in the Frontiers paper. . . 3) The figures need to be upgraded: Figure 1c and 1d are too small, while there is sufficient room for expansion; the colour scale used in Figure 2a and 2b does not show sufficient detail; Figure 3a is useless due to the colour scale used.

Next to the above points on the general state of the manuscript, I also have 3 major points that need to be addressed before the manuscript should be eligible for publication. Afterwards, a list of minor points is given on a line-by-line basis (where P and L

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refer to page and line, respectively).

Major Points:

1. I think the authors should rethink if this manuscript should be considered as a normal-size publication in TC or as brief communication (BC). To me, a normal-sized publication would fit better with the content of the manuscript. Currently, there are 7 supplementary figures in the Supplementary Material (SM), which to me is not fitting for a BC-style paper. This style has very strict limitations on pages and number of figures to keep the publication brief. If the authors feel the need to show more information with extra figures, it is better to switch to a normal style publication. This also gives the authors room to expand the methodology and include the accompanying figures in the text instead of the SM (where much less people will read them). Moreover, the text include three references to subjects that are "out of scope for this paper" (P3, L14; P7, L23; P8, L3), while I think it is very relevant to include them into the scope of this manuscript. If the publication is expanded to a normal-sized, these topics could be properly addressed. If the authors choose to keep the manuscript in the BC format, they should at least remove the SM figures.

2. For the three firn regions of the GrIS, the average FAC10 is given in the manuscript: DSA at 4.9 m³ m⁻² LAWSA at 4.3 m³ m⁻²; and HAWSA at 2.4 m³ m⁻². This does not at all agree with what I would expect. As a consequence, I have strong doubts about the empirical relations and method used to calculate the spatial FAC10 maps that lead to these average numbers. Based on the published knowledge of the GrIS firn layer, one would expect the FAC10-ratio between DSA:LAWSA:HAWSA to be in the order of 5:2:4. In the LAWSA, there is low accumulation and substantial surface melt (enough to be considered "wet snow"). Most surface melt is refrozen in the cold firn leading to many ice lenses and high densities, as observed by for example Harper et al., 2012 and Machguth et al., 2016. If the LAWSA covers the entire firn area between the DSA (FAC10 ~5 m³ m⁻²) and bare ice (FAC10 = 0 m³ m⁻²), one would expect the average FAC10 to be 2-3 m³ m⁻², and not 4.3 m³ m⁻² as reported here. For the HAWSA on the

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other hand, the reported FAC10 of 2.4 m³ m⁻² is much lower than one would expect. The HAWSA is mainly found in the south- and southeast of the GrIS and coincides quite well with locations where firn aquifers are found. At these locations, the high accumulation and relatively high firn temperatures cause less refreezing of meltwater near the surface resulting in deep percolation and recharge of the firn aquifer at depth. As a consequence, not many (thick) ice lenses are found in these regions. Due to the high accumulation, the firn in the upper 10m is relatively young (3-5 years old), resulting in less time to densify compared to low-accumulation regions. Considering this, it is to be expected that the average FAC10 of the HAWSA is higher than that of the LAWSA, while the opposite is reported in this manuscript. In the current manuscript, the above average FAC10 numbers are presented without much discussion. Only on P9, L1-6, a couple of sentences are used to discuss the HAWSA FAC10. I think it is very important that this is more elaborately discussed! If the average FAC10 numbers turn out to be true, this is a very important result as it would change our view on how firn (and FAC) is spatially distributed around the GrIS. However, I think it is more likely that these numbers show that the method used is not sufficient to describe the variations in FAC10. My guess is that either the number of firn cores (or spatial diversity in them) is not sufficient to constrain the empirical solution, or the atmospheric input of only average accumulation and temperature is not sufficient.

3. The results of Fausto et al., 2018 (snow-line extent) are heavily used to support one of the two main conclusions of the manuscript: the firn area extent of the GrIS. However, Fausto et al., 2018 is not a peer-reviewed publication, so their methodology is not tested nor reviewed. Here, the results of Fausto et al., 2018 are used without prudence, while some discussion on the methods used is needed. If the authors follow up on my suggestion to switch to a normal-sized publication, a short methodology can be included in this manuscript.

Minor Points:

P1, L25: "its characteristics are still little known" is better replaced by something along

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the lines of “still little is known about its characteristics”.

P1, L25: Remove space between 2000-2017.

P1, L26: Provide a percentage with the firn area extent

P1, L26: “We also present”

P1, L27-28: Presenting the results for the DSA (74%) and LAWSA (12%) leaves the casual abstract reader wondering what happened to the other 14%.

P1, L27-28: “warmest and driest 12%” is not true. Correct would be that it is the driest part of the warmest part of the firn area. Please rephrase.

P2, L5: “The FAC is the integrated volume”

P2, L12: firn temperature is also an important constrain the depth to which meltwater might percolate.

P2, L12-16: No mention here of firn aquifers while they are known to have very deep percolation (up to 20-30 m).

P2, L20: I find this a very crude and simple assumption. Both on the drier western side of GrIS (Humphery et al., 2012) and the wetter eastern side (Forster et al., 2014) are indications of percolation deeper than 10 meters. By only looking at the upper 10m a substantial amount of the retention capacity of the GrIS is missed!

P2, L21: The maximum volume that can be retained is much higher when the dry interior firn is included. An upper limit can be extracted from models (RCM or firn model) for example.

P2, L27: Fausto et al 2018a!

P3, L1: “From literature, we gathered ..”

P3, L13: Strange way of notation. Why is there a plus/minus sign in front of the 1, while 1 to 10 already indicates a range and therefore a lack of precision? And, why is the

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FAC10 range not given as “1 to 5”?

P3, L20: Similar to previous comment.

P4, L1: Why not use the latest model estimates (HIRHAM, RACMO, MAR), or use all 4 products to have some sort of best estimate.

P4, L4: “(3)” should be “(2)”.

P4, L8: it is stated that two patterns are evident in Figure 1, which is true. However, 1-2 sentences of explanation or analysis should be given after such a statement.

P4, L10: Figure 1c and 1d are so small that the variation in slopes is hard to see. Please increase these figures, or remove this statement.

P4, L14: $T_a = -16\text{C}$ is taken as the boundary between DSA and WSA, however how true is this in a changing climate. It is well documented that GrIS is warming and the ELA increases. Currently, the 1970-2014 average temperature is used, but it is likely that the spatial pattern of the boundary changes (a lot?) over time.

P4, L28: Interesting to see that the firn model equations of Arthern et al. 2010 are used, while 6 lines earlier (P4, L22) it is clearly stated that this manuscript attempts to construct a firn map without the use of RCM or firn models. . .

P4, L29: Why not use the 315 kg m⁻³ as reported by Fausto et al., 2018(a)?

P5, L1-2: Would be interesting to show or list how the various densification laws performed, and which ones were tested.

P5, L4: Figure S3 is very complex as they are 3-dimensional. When using multiple 3D figures it would help if they are all oriented similarly to make the figure more clear and less dizzying.

P5, L5: In the WSA, the characteristics are very complex and different depending on slight changes in climate forcing, as you also discuss in the introduction. It seems too

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simplistic to constrain this behavior only by average accumulation and temperature. The complex behavior is mainly caused by melt intensity and duration, which is not captured by using the average temperature. If RCM results would be included, surface melt could also be included in the empirical functions.

P5, L7: Here, the measurements from different years are grouped (likely to accommodate for climate change), so why was this not done for the boundary between DSA and WSA (see comment on P4, L14).

P5, L23: Due to lack of measurements in the HAWSA, the firn line (where FAC10 = 0) is used as an extra observation to better constrain the empirical functions. Would this also be a good addition for the LAWSA? It would at least be more consistent.

P6, L8: Should refer to Figure 1a, I think.

P6, L18: Should refer to Figure 2a.

P6, L18: Add comma between region and representing.

P6, L23-24: Here, conclusions are drawn about the temporal evolution of the FAC10 in the DSA. However, the FAC10 is calculated using the steady-state model solutions of Arthern et al., 2010, which makes it difficult to use them for temporal analysis. Steady state density profiles have no memory of previous climate and change directly based on the average climate input. From the text I cannot sense how much this would influence the results, but please add a discussion about this to the manuscript.

P7, L1: FAC10 should be FAC10.

P7, L5: Should refer to Figure 2d.

P7, L5-7: As referred to in the start of this review, the stated difference in FAC10 and the difference between the absolute values does not match.

P7, L8-9: Please rephrase "that had become unavailable by 2011-2017".

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P7, L11: Multiple references should be in chronological order.

P7, L12: I would remove "greatly". I agree that accumulation has a great and immediate effect on firn density, however, changes in accumulation over time are almost never substantial enough to give a significant effect in FAC. Especially not in places where surface melt is involved.

P7, L18: The influence of the extreme melt summer of 2010 and 2012 might be minimal at some locations with higher accumulation, as the 2010- and 2012-snow and refrozen meltwater might be buried below the 10m boundary used in this manuscript. Could you indicate for what locations this might be true?

P7, L26: Refer to Figure 2c and Figure 3c.

P7, L30-31: This is not really a hypothesis. The firn aquifer is studied by multiple papers and it is clear that meltwater percolates deeper than 10m and that the high snow accumulation insulates it from the winter cold. Possible references: Kuipers Munneke et al., 2015, Miller et al., 2017, Miller et al., 2018.

P8, L5: It is not the total FAC! The total FAC includes also all FAC below 10m, which is substantial in the DSA.

P8, L17: Add comma after Nonetheless.

P8, L21: The way this sentence is written implies that all variations in FAC10 can be explained by average accumulation and temperature. This is not the case, so please rephrase.

P8, L13-21: Here, model results are used to estimate the uncertainty in the generated firn maps. When comparing to model results, would it not be better to compare to firn model output directly. For example, Steger et al., 2016, Langen et al., 2017, and Ligtenberg et al., 2018 all present GrIS-wide firn model simulation from which FAC10 could be derived.

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P8, L24: "to be used in mapping FAC10."

P9, L5-6: These two options are listed as if they are equally likely. In my opinion, the hypothesized drastic decrease in FAC10 is much less likely.

P9, L9: Not true. Fausto et al., 2018 presents the first delineation of the firn area of GrIS. Please rephrase.

P9, L13: "on" should be "of".

P9, L16-18: As referred to in the start of this review, the numbers for LAWSA FAC do not match the numbers in the remainder of the text.

P9, L17: "FAC10" should be "FAC10".

P9, L18: add "between" before "1998-2008".

P9, L21: FAC10 might not only be insufficient to describes the retention capacity in the HAWSA, according to Humphery et al., 2012 there is also deep percolation observed in the LAWSA.

Figure 1: - Figure c) and d) should be much larger, while the axis label can be a bit smaller. Just use the figure area better. - In b), an interesting peak is visible in the firn area extent around $T=-11^{\circ}\text{C}$ and $b=150\text{ mm yr}^{-1}$. You would expect that the firn area is a smooth curve across the temperature-accumulation space. What area causes this peak and might it not be worthwhile to discuss it in the text.

Figure 2: - Due to the color scale, Figure 2a show little detail. - No need to show the core location again in Figure 2 as they are already shown in Figure 1. - The pattern of FAC10 in southwest Greenland on the boundaries from LAWSA to HAWSA looks very abnormal. Since you have a transfer-function to go from the DSA to LAWSA (P5, L12-13), why is there not transfer function between LAWSA-HAWSA?

Figure 3: - Due to the color scale, Figure 3a is useless. - Figure 3b also show very little detail for the same reason. Perhaps use an exponential scale.

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References: All other references are similar to the publications use in the manuscript.

Kuipers Munneke, P., S. R. M. Ligtenberg, E. A. Suder and M. R. van den Broeke. 2015. A model study of the response of dry and wet firn to climate change. *Annals of Glaciology*, 56, 70, 1-8. doi:10.3189/2015AoG70A994.

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