Dear Editor and Reviewers,

We are grateful for your constructive review of our manuscript. We made our best to address all the suggestions and provide an improved and fully revised manuscript. A response to each of the reviewers' comments is given below but we would like to highlight the most important updates of manuscript:

- We now present a research article with improved visuals and more in-depth discussion.
- We compare our FAC dataset and maps to three regional climate models.
- The construction of empirical functions is slightly updated, simplified and presented in the main text.

We thank the reviewers for improving significantly the study.
Sincerely,

Baptiste Vandecrux on behalf of the co-authors

Review #1 by Sergey Marchenko

Reviewer's comment	Authors' response
General comments Physical geography. Authors use the mean annual air temperature and net surface accumulation as arguments in functions describing the spatial distribution of FAC10. The functions are fitted to minimize the misfit with empirical estimates of FAC10 from cores. One important thing that is missing in the text is a detailed description of the physical (or may be practical) motivation for the choice of the above mentioned arguments. Both characteristics (net annual surface accumulation and mean annual air temperature) integrate the effects of processes occurring during the cold and warm parts of a year.	
Net annual surface accumulation is the result of mass accumulation in winter and surface melt in summer. While the first one can be expected to be positively linked with FAC (more accumulation in winter -> more pores), the second one can be expected to be negatively linked with FAC (more melt -> thinner snow layer by the end of summer with less pores, more water available for refreezing).	**In our study $\bar{\dot{b}}$ is defined as "net snow accumulation" (snowfall + deposition – sublimation) and is not "Net annual surface accumulation". It therefor already corresponds to the "precipitation rate" that is recommended. We now give more explanation in the text.**
Mean annual air temperature can be also separated in two parts: temperature in winter and in summer. The principal difference between the two is the likely range of values: significantly negative in winter and close to melt point in summer. High winter temperatures can be expected to result in a lesser cold content of the subsurface profile, leading to a less active refreezing during	**We now state that our motivation for using long term mean annual temperature are 1) its control on firn temperature and dry firn compaction 2) its control on melt amount in the summer. Both temperature-dependent processes have a densifying effect on the**

consecutive summer and larger FAC values. Air temperature during the warm part of a year is commonly used as a proxy for melt rate (e. g. Ohmura, 2001). High air temperatures in summer lead to faster melt and larger potential for refreezing as there is more water available with the effect of smaller FAC values. As noted above the melt rate (as a contribution to the net annual accumulation) and air temperature in summer (as a contribution to the mean annual air temperature) are closely correlated and probably interchangeable for the purposes of FAC parameterization. There are, thus, 3 proxies left: precipitation rate, winter air temperature and summer air temperature (or melt rate). Along with gravitational settling liquid water refreezing is one of the two contributors to the density increase over time. It can be limited by one of the three parameters: availability of liquid water, pore space or cold content. Subsurface temperature and density, defining the FAC, are heavily dependent on the relation between the three parameters.	**firn and therefore act similarly on the FAC.** **We do not aim at quantifying the cold content and therefore do not need to include the winter temperature.** **Also we believe that our dataset does not offer the possibility to constrain empirical functions taking more than two input variables.** **Eventually the amount of meltwater effectively retained in the firn indeed depends on the "availability of liquid water, pore space or cold content". However, this is out of the scope of our study and we choose to focus on the retention capacity of the firn. Future work will need to address how this capacity is effectively being used under different conditions.**
In the course of a temporal or spatial transition towards a warmer climate, air temperature increases. The associated rise in melt rates will deliver more water. Depending on whether the potential of pore space or cold content will be exhausted first, two different scenarios can be applied to a subfreezing firn profile: transition towards superimposed ice nourishment or development of a warm firn pack, possibly, with perennial firn aquifers in case runoff is impeded. This is exactly what happens in Greenland and what the authors of the manuscript, probably,	**For a matter of conciseness and because we do not question or discuss facies definition, we choose to cite Shumskii and Benson's work rather than paraphrasing it. The reader is left free to investigate the original references for more information.** **We also added a reference to Braithwaite**

attempted to reproduce by introducing three different domains: DSA, LAWSA and HAWSA. The above presented logic goes back to the theory of glacier zones presented in (Shumskii 1955). English translation was published in 1964 (see ch. 18 and 20). Definitions of glacier zones are also given in Cogley et al. (2011). One can also address the project report Marchenko (2012) and the phd thesis (2018) for a detailed description of the logic and Braithwaite et al. (1994) of some aspect thereof. The approach was applied by Pfeffer et al. (1991, see appendix there) and Janssens and Huybrechts, (2000) for estimating refreezing rates in Greenland. The idea of geographical patterns in Greenland firn pack development was recently expressed by Michael MacFerrin the his PhD thesis (see ch. 5.2.3), perhaps, worth citing in ch. 2.4 along with the other above published sources.	**et al. 1994 for their observation of meltwater refreezing “within a wetted layer of thickness 2-4 m”.**
One option is to use the three above mentioned parameters as arguments in functions for extrapolating and interpolating observed FAC values. That could be precipitation rate and mean temperatures during summer and winter months. The latter two can be replaced by either the annual sums of positive and negative degree-days or mean annual temperature and some continentality index. It is also possible that precipitation expresses continentality to some extent with higher values associated with more maritime climates. It is impossible to say without testing, but it may be possible to adequately describe the FAC10 values from cores around all of the Greenland ice sheet by a sum of three piecewise-linear functions of the earlier mentioned three parameters. These were just some suggestions and authors are, of course, free to choose the logic used for FAC10 estimates. In any case choice of arguments used for the spatial distribution of the empirical FAC10 values has to be motivated.	**As mentioned above, we already have one predictor (net snow accumulation) for all the processes that replenish the FAC and another predictor (mean air temperature) for all the processes depleting the FAC (firn compaction and melt). We do not believe that our dataset allow any higher degree of complexity.**

Comparing results with earlier published data I suggest a more extensive referencing of published FAC estimates for the Greenland Ice Sheet. There is, apparently, a considerable spread in values of both FAC10 and total FAC. This is noted in ch. 3.5 of the manuscript, but should, preferably, appear much earlier, already in the Introduction chapter. An overview of the published values would provide one important motivation point for undertaking this kind of studies. Furthermore, comparisons of results with published estimates could make an interesting discussion as the present study suggests an alternative approach to calculation of the firn air content. For example, Ligtenberg at al. (2018) make a reference to the dataset containing results of simulations on which the publication is build - https://doi.org/10.1594/PANGAEA.884617. A rough calculation of the total FAC in Greenland gives the value of 26300 Gt (please see the code used for the exercise in the appendix of the review), That is 20 times more than value from Harper et al. (2012) referenced in ch. 3.5, p. 8, ln. 8 of the manuscript. Full simulation results are available from Ligtenberg et al. and FAC10 value can be also calculated. The earlier study by van Angelen et al. (2013) is not referenced at all. It would also be interesting to compare the FAC10 values presented in the manuscript with corresponding output from the subsurface component CROCUS of the regional climate model MAR, surface data from which is used in the manuscript. Steger et al. (2017) also have figures showing FAC estimates for different areas in Greenland derived using the another layered model – SNOWPACK.	**We now compare our FAC10 dataset to existing RCM.** **van Angelen et al. (2013) is now cited in the introduction.**
Scale of the manuscript	**We now changed to a research article**

One of the shortcomings of the manuscript is that the reader is forced to refer to supplementary material while going through the methods chapter. At the same time the suggested approach to deriving distributed FAC values is elegant, novel and promising. In case authors decide to introduce a more extensive discussion based on comparison of the results with earlier published values and relocate the “methods” figure from the supplementary material (S3) to the main paper text, the paper can be reclassified to a “research article” instead of “brief communication”, which it is now. In case authors will prefer to keep the manuscript as “brief communication”, the number of references has to be greatly reduced. The list of references now contains 55 entries, while only 20 are allowed for this type of manuscripts according to the The Cryosphere protocol (https://www.the-cryosphere.net/about/manuscript_types.html). I would also suggest to: – transfer the table from the main manuscript to the supplement, – reduce the number of panels in fig 2 and 3 – merge panes from fig 3 in fig. 2, add the “methods” figure in the main text.	**format.** **Figures were updated.**

Specific comments

N	address	Comment	Authors’ response
1	Abstract	Include the estimate of the total FAC in Greenland in Gigatonns. The reader see the firn area, the absolute and relative values of FAC10 decrease in LAWSA, but both values would be more informative if the	**We added the loss of retention capacity in the LAWSA (in Gt) to the abstract.**

		Gt estimate would be found somewhere not very far.	
2	Ch. 1, p. 2, ln. 5-6	The phrase “for that depth range” seems to be out of place.	**Changed to “It indicates, for a specified depth range, the maximum volume…”**
3	Ch. 1, p. 2, ln. 9-11	Add the quantitative estimates of FAC from Ligtenberg et al., 2018, van Angelen et al., 2013, Steger et al., 2017	**We now compare our dataset to the output from three RCMs**
4	Ch. 1, p. 2, ln. 12-21	Bring the sentence on deep water percolation evidences from Humphrey et al., 2012 earlier, so that it appears second in the paragraph. This will group together the evidences of shallow percolation from Machguth et al. (2016) and Heilig et al., (2018).	**Here our intention was to show that 1) Braithwaite et al. and Heilig et al. give evidence of shallow percolation 2) Humphrey et al. give evidence of deep percolation when sufficient melt is present 3) Machguth show that in some conditions, even when sufficient surface melt is available, deep percolation does not occur because of ice layers.** **We tried to make it clearer and rephrased the paragraph.**
5	Ch. 1, p. 2, ln. 13	Heilig et al., (2018) had their installation at 2120 m asl, not at 2300 m.	**Updated**
6	Ch. 1, p. 2, ln. 23	How does this collection of core data relate to the data from Fausto et al., 2018 in Frontiers? They at least partly overlap, as is seen on the maps of core locations.	**They use partly the same sources (e.g. PARCA, Sumup…) but Fausto et al. focuses on the average density of the top 10, 20 and 50cm of the snow. As a consequence they also use snow pits that we do not use.**
7	Ch. 2.2, p. 3, ln. 2	Same as above	
8	Ch. 2.2., p. 3, ln. 3	”...as part of the FirnCover campaigns...” It is not obvious what is “FirnCover campaigns”, are these field activities affiliated with a University or some other organization? Either a reference or a	**We removed the name of the fieldwork and refer to Machguth et al. (2016) for the field procedure.**

		description of the routines applied in the field has to be given.	
9	Ch. 2.2, p. 3	I encourage a more extensive use of density data. FAC values are secondary with respect to the density-depth profiles. • Instead of extrapolating FAC values from too shallow cores, one can extrapolate the density profiles. This will make it possible to include the description of the extrapolation technique (ch. 2.3, p. 3, ln. 15-19) in ch. 2,2, right after the first sentence, which seems more logical.	**We consider that extrapolating FAC profiles is more straightforward as it allows later to evaluate the uncertainty associated to our extrapolation method directly in terms of FAC10. Also, we do not believe that extrapolating density instead of FAC10 would lead to substantial improvement to the final extrapolated values.**
		• Describe the "upwards extrapolation" technique (the 315 kg m^-3 value) before describing how gap filling is done.	**Updated.**
		• I guess that the data from all cores was resampled to a common grid. If yes, then what is the spacing between neighboring nodes? Do not let readers guess!)	**Indeed when comparing two FAC10 profiles they need to be resampled on the same grid (in our case every cm). We believe it is the only method possible and therefore do not need to be specified.** **The scripts supporting the article are available on GitHub and advertised in our acknowledgement for the readers who are curious about our sampling strategies.**
10	Ch. 2.3, p. 3	I recommend more descriptive explanation of what FAC is. That also includes reformulation of equation [1]. A few tips: • Use references! FAC values were calculated earlier.	**We now define the FAC as:** **"The FAC is the integrated volume of air contained in the firn from the surface to a certain depth per unit area (van Angelen et al., 2012; Ligtenberg et al., 2018). It is a measure of the firn porosity and indicative, for a specified depth range, of the maximum volume available to store percolating meltwater either in liquid or refrozen form (Harper et al., 2012; van Angelen et al. 2012)."**

			As a comparison, the only description of FAC in Ligtenberg et al. (2018) is: "The firn air content (FAC) is used as an integrated measure for the amount of pore space present in a firn column and is defined as the vertically integrated difference of the firn density and the ice density (taken to be 917 kg/m3). "
		• Express FAC values through porosity, which is a widely applies and more basic concept – that will make it more understandable for an unprepared reader	**We believe there has been more work done on firn air content recently (van Angelen et al. 2013; Ligtenberg et al. 2018) than on porosity.** **We do not believe using porosity would lead to a significant improvement of the study.**
		• Use [m] for units! It is more straightforward than [m^2 m^-3] and more descriptive.	**Updated**
		• Using the threshold of 873 kg m^-3 for FAC calculation contradicts the very definition of FAC as firn AIR content and also the below stated scope of the manuscript (ch. 2.3., p. 3, ln. 13-14). I assume that authors prefer to avoid the discussion of permeability of firn to water, if this is the case, in has to be stated.	**Indeed it was an error on our side. We now use 917 kg/m3.** **Discussion of whether it is filled by infiltration ice or liquid water is brought up again in section 3.5.**
		The value from Machguth et al. (2016) is a result of study in western Greenland. In this manuscript geographical differences in the firn pack are one of the main points and using the value seems not logical.	**This point is now discussed in Section 3.5.**
		Ligtenberg et al. 2018 used the physically motivated value of pure ice density, 917 kg m^-3, in their FAC assessment for the entire Greenland. One can even argue that the value of 1000 kg m^-3 is	**We now differentiate the FAC (calculated 917 kg/m3) and the retention capacity (calculated by filling the FAC with ice until it reaches infiltration ice density, Harper et al. 2012).**

		valid: water fills all the pores and then expands, increasing the bulk volume. That is known as frost heave and is widely spread in permafrost areas. Pingos can be higher than 50 m suggesting that lifting 10 m of firn is well possible for frost heave action.	**Firn frost heaves are out of the scope of our study.**
11	Ch. 2.3, p. 3, ln. 23	What is "sites" here? Is that 1*1 km spatial domains, or "clusters" with core data? It also remains not clear why are cores grouped according to the original publication? Would you not unite in one group cores that are close by (less than 1 km) but come from different publications?	**This paragraph was rephrased.**
12	Ch. 2.4, p. 4, ln. 1	"all locations": what is the grid spacing for FAC10 extra- and interpolation and, consequently, for bn and Ta?	**We now specify:** **"To put our FAC10 measurements in their climatic context, we extract the long-term (1979-2014) average net snow accumulation $\dot{b}^{-}$ (snowfall – sublimation) and air temperature (T_a)̅ for each FAC10 measurement location from the nearest cell in the Modèle Atmosphérique Régional (MARv3.5.2; Fettweis et al., 2017) available at 5 × 5 km horizontal resolution."**
13	Ch. 2.4, p. 4, ln. 10	The slope of FAC10 against Ta is not much different between HAWSA and DSA as it is evidenced by Fig. 1d.	**Indeed the slope was the same. We modified our method accordingly.**
14	Ch. 2.5.1., p. 4, ln. 28	What does the Arthern et al., 2010 model take as arguments?	**We now avoid using dry compaction laws and use a linear regression on Ta to describe FAC10 in the DSA.**
15	Ch.	Perhaps, a better place to describe the uncertainty quantification logic	**Updated**

	2.5.1., p. 5, ln. 2	(UQ) for the DSA is here, not in ch. 3.2. At least for other domains UQ is described in ch. 2.5.	
16	Ch. 2.5.2., p. 5, ln. 6	What is the spacing between Ta bins in the “decreasing piecewise-linear function”?	**We updated the construction of empirical functions and replaced this piecewise linear function by a more simple bilinear interpolation.**
16	Ch. 2.5.2., p. 5, ln. 7	“to resolve the FAC10 distribution each year”: is this expected at all? Reader likely does not expect that, since earlier in ch. 2.5.1. data from different years was lumped together.	**Removed.**
17	Ch. 2.5.3., p. 5, ln. 25-27	From Fig. 1b it is obvious that Ta and bn are strongly correlated. It is most probable that this fact above and not the amount of measurements explains the poor correlation between bn and residuals of the air temperature fit. In other words, adding more data will, likely, not help.	**In the revised version we revised our protocol and updated these parts.**
18	Ch. 2.5.3., p. 6, ln. 5	Are any routines applied to ensure a smooth transition of the FAC10 model between HAWSA and DSA? Earlier in ch. 2.5.2. such a routine is described for LAWSA-DSA transition.	
19	Ch. 3.1., p.6, ln. 13	“...average from 18 years of data” – comparing this with what is given in ch. 2.1. suggests that “average” is not a valid word here.	**Removed.**
20	Ch. 3.1., p.6, ln. 15-16	“...we do not believe that...” is not a valid expression. The low significance of the FAC in patchy firn just above the equilibrium line can be motivated by its likely small thickness.	**Changed to “Owing to the likely thinness of the accumulation area lower boundary, we expect the boundary does not play a negligible role in the overall retention capacity of the firn area.”**

21	Ch. 3.2., p.6, ln. 22	"...absence of temporal trend...": it would have been good to show that in a figure.	**Now showed in Figure 2b.**
22	Ch. 3.3., p.7, ln. 6	Where is 180 +-78 km^3 coming from? 690-520 =170...	**Updated**
		How is the uncertainty value of the difference (+-78) calculated?	**We now state how we calculate uncertainty:** **"The uncertainty applying on our estimated FAC_{10} and FAC_{tot} at a location cannot be considered independent because all estimates are made using the same functions of $\overline{T_a}$ and $\dot{\bar{b}}$. Consequently, we consider that the uncertainty of the mean of several FAC values is the mean of each value's uncertainty and that the uncertainty of a sum or difference of FAC values is the sum of the uncertainty applying on these FAC values."**
23	Ch. 3.3., p.7, ln. 7-8	I assume that 150 +-68 Gt comes from multiplying 180 km^3 by the assumed ice density (843 kg m^-3) and dividing by the density of water (1000 kg m^-3). If that is the case, it needs to be explicitly said. This logic is in direct contradiction with the phrase "...if we assume that all the air content can be used to store meltwater...". Also see the comment n. 9 above.	**We updated the way we calculate the firn retention capacity from infiltration ice (density 843kgm-3) filling the air content, to "the amount of water that needs to be added to the firn to bring its density to 843 kg m^{-3}" more in accordance with Harper et al. 2012.**
24	Ch. 3.3., p.7, ln.	Perhaps, residuals of fits, widely used in this manuscript, could be of help here as well...? Are the differences between the empirical fit and	**The inclusion of RCM now allows to discuss the temporal evolution of the FAC (Section 3.6).**

	17-19	FAC10 from cores drilled after high melt seasons in 2010 and 2012 show larger values than other cores?	
25	Ch. 3.4., p. 7, ln. 25	An observation: the stated mean FAC10 value in HAWSA of 2.4 m seems rather low, when visually comparing panels b and c in Fig. 2. It is considerably lower than in LAWSA for both periods. Check the value!	**We updated the number.**
26	Ch. 3.5, p. 8, ln. 5-11	As mentioned higher up a more extensive comparison of results of the manuscript with previously published FAC values is expected here. The fact that Harper et al., 2012 report Greenland-wide FAC10 value that is 17 times less than presented here deserves a wider discussion. It is claimed that their data had a lesser spatial coverage. But from that it does not follow that the FAC10 value should necessarily be less.	**We now extract the total firn air content from our estimate over Harper et al.'s considered area and compare the two.**
		Then again, results from van Angelen et al., 2013, Steger et al., 2017 and Ligtenberg et al., 2018 are of high relevance for the discussion. The authors are also using MAR data, which, most probably was run alongside with the subsurface model CROCUS. What FAC10 values does these simulation yield?	**We now compare our work to the output of HIRHAM, RACMO and MAR.**
27	Ch. 3.7., p. 8, ln. 26	Who measured the FAC10 in 2006-2007?	**This part was removed.**
28	Ch. 4, p.	"...21% decrease of FAC10...": in ch. 3.3, p. 7., ln. 2 an increase of 23%	**Updated**

	9, ln. 16	was reported	
29	Ch. 4, p. 9, ln. 21-25	“FAC10 observations also indicated that meltwater may percolate deeper than 10 m from the surface making FAC10 insufficient to describe the retention capacity of the firn there.”: is this a result of the present manuscript?	**This discussion point was removed.**
		“*In a similar way*, Machguth et al. (2016) showed that *under conditions not completely understood*, ice formation may prevent meltwater from accessing the entire top 10 m of firn.”: there is no similarity between this statement and the preceding one, rather opposition. What conditions are not completely understood here? It looks like authors intend to say here that depending on the subsurface conditions (temperature, density, stratigraphy, water permeability, slope of the impermeable layers with respect to horizontal) a different fraction of the FAC may be effectively used for storing the melt water. So, FAC10 is good, but, perhaps, not good enough and more research is needed to close the question here...	**We removed this point from the conclusion as it was more of a discussion point.**
	Fig. 1	Few suggestions: • Panel a: It is possible to show not only the spatial but also the temporal distribution of the core data by color-coding the year	**This was made impossible with the high clustering of the observation sites. We also consider that it does not bring any crucial information that is discussed in the text.**

		individual cores were drilled.	
30		• Panel b: may be do not use white-centered markers. Use color shading right from the center and add a white border around for higher contrast with the background. Try a different color bar, white-blue for example: more intuitive and in larger contrast with the background.	**Updated**
		In the caption add description so that it is more obvious that the black line is the domain of the Greenland Ice Sheet firn area in the Ta-bn domain.	
		• Panels c and d: combine the two panels and show LAWSA and HAWSA cores using different colors for the markers.	
31	Fig. 2	It is possible to combine some panels. Panel a and panel c can be combined. Panel b (when considered together with c) and panel d essentially overlap. When the temporal difference is shown (panel d) the significance of panel b drops and, perhaps, the panel can be left out.	**We applied your suggestions.**
32	Fig. 3	Combine panels a and c	
33	Fig. S3	3D graphs give a poor representation of the 3D reality. Try contour plots for the fitted surfaces with contour lines color-coded in the same fashion as empirical markers – FAC10 value. Or may be try 2D plots with one parameter on the horizontal and	**We now use 2d plots.**

		FAC10 on the vertical axis. Several sets of fit curves plus empirical FAC10 values for different ranges of bn will give an understanding of how the fit relates to empirical data.	

Technical corrections

N	address	Comment	Authors' response
1	Literature list	Distinguish between Fausto at al. 2018 in Frontiers (6) and in Geol. Surv. Denmark Greenland Bull. (41) by introducing "a" and "b" in the year of publication. Ambiguity in interpretation of short references along the text is now possible as it is (Fausto at al. 2018) in both cases.	**Updated**
2	p. 1, ln. 35	"...contribute to THE sea-level rise..."	**Not applied (see https://www.nature.com/articles/nature11566)**
3	p. 2, ln. 3	"...end-of-summer snowlineS but did..."	**Updated**
4	p. 2, ln. 4	"simple" is not a valid term here	**Removed**
5	p. 2, ln. 8	"...in spite of the diversity *of firn structures* across the ice sheet...": replace italic by "in characteristics/properties of the firn profile"	**We replaced "structures" by "characteristics"**
6	p. 2, ln. 23	We then calculate the FAC10 using a set of 344 firn cores collected between 1953 and 2017. We finally present the spatial distribution and where possible the temporal evolution of FAC10. Rephrased: Using a set of 344 firn cores collected between 1953 and 2017 we calculate the spatially distributed FAC10 and where possible present the its temporal evolution.	**Updated** **We replaced "spatially distributed FAC10" by " spatial distribution of FAC10"**
7	p. 2, ln.	Rephrase: "Using these data, we *determine* the firn area, *defined* as the region	**Removed**

	29	where only snow has been detected during the entire 2000-2017 period.”	
8	Ch. 2.2., p. 3, ln. 3	”...as part of the FirnCover campaigns...” It is not obvious what is “FirnCover campaigns”, are these field activities affiliated with a University or some other organization? Either a reference or a description of the routines applied in the field has to be given.	**We removed the name of the fieldwork and refer to Machguth et al. (2016) for the field procedure.**
9	p. 3, ln. 10	Replace ”section” by ”layer”	**We changed to “depth interval”.**
10	p. 3, ln. 16, 18	”10+ m core” is not a valid expression. Use ”deeper than”.	**Updated**
11	p. 3, ln. 16	”...with THE lowest Root...”	
12	p. 3, ln. 16	Rephrase: ”We therefore *attach* to any...”	**Changed to “associate”**
13	p. 3, ln. 17	Replace ”masurement” by ”estimate”. FAC10 is not measured directly.	**Since, just like firn density, FAC10 can be determined by simple calculations using observations of the mass of in a firn core, we would like as much as possible to preserve the appellation “observation”. It is also opposed to the FAC10 value predicted by our empirical functions (which are then estimations).**
14	p. 3, ln. 28	Shorten the sentence to have: “We extract each core site’s long-term (1970-2014) average net snow accumulation (bn) and air temperature (Ta)...”	**Updated**
15	p. 4, ln. 3-	Avoid double referencing to the color and figure number (“amber area in Figure	**We believe it is clearer if we can guide the reader**

	11	1a”). Is amber=yellow?	**to the appropriate coloured area in the relevant figure. Amber was changed to yellow.**
16	p. 6, ln. 6	What are the ”well-known dry-firn compaction equations”? References are needed here.	**We now avoid using dry firn compaction schemes and use a linear function of Ta.**
17	p. 6, ln. 7	TowardS	**Updated**
18	p. 4, ln. 20	Replace: ”from our” -> “using the”	
		“observations” -> “firn cores”	**This part was rephrased.**
19	p. 4, ln. 21	Replace: “to predict FAC10 anywhere in the firn area” -> “to interpolate and extrapolate FAC10 for the whole firn area”	**Updated**
20	p. 4, ln. 24	Form of the functions is not *arbitrary*. The authors make an attempt to bring in physics in the extrapolation of the empirical FAC10 estimates.	**This statement was removed.**
21	p. 4, ln. 28	Remove “we” before “tuned the surface snow density”	**Removed.**
22	p. 4, ln. 29	Add “a” after the reference to Figure S3.	
23	p. 5, ln. 4	Add “b” after the reference to Figure S3.	
24	p. 5, ln. 23	Replace ”...as additional measurements where FAC...” by ”...as an additional proxy of FAC...”	**This sentence was rephrased.**
25	p. 5, ln. 26	Replace: “meaning” -> “suggesting”	**Removed.**
26	p. 5, ln.	Replace: “We can make three estimates...” -> “Three principal assumptions are	

	28	possible ..."	
27	p. 6, ln. 12	Replace: "Spatial heterogeneity in melt and snowfall leave..." -> "Spatial heterogeneity in snowfall and melt leave..."	
28	p. 6, ln. 13	Replace: "missed by the method of Fausto et al. (2018)." -> "missed by the method applied by Fausto et al. (2018)."	
29	p. 6, ln. 19-20	Remove the unnecessary paragraph	
30	p. 6, ln. 21	Replace "Assuming a normal distribution of errors, 95% of..." -> "Assuming a normal distribution of errors with zero mean, 95% of..."	**Removed**
31	p. 7, ln. 1	Subscript in FAC10 symbol	**Updated**
32	p. 7, ln. 5	"Summing the FAC10 and its uncertainty indicates that..." I assume that lateral integration across the domain covering the Greenland Ice Sheet is meant here. The phrase, as it is now, can be misinterpreted, one might think that you are summing actual values and their assumed uncertainties.	**Now rephrased in Section 2.6.**
33	p. 7, ln. 5-6	Replace: "...of air is contained within..." -> "...of air was contained within..."	**Updated**
34	p. 7, ln. 28	Add "b" after "Figure 1"	
35	p. 8, ln. 2	Rephrase: "...occur at deeper than 10 m" -> "...occur below the depth of 10 m"	**Remove.**
36	p. 8, ln. 13	Rephrase: "...impactS our FAC10 maps..." -> "...impact our FAC10 maps..."	**Updated**

37	p. 8, ln. 16	Rephrase: "...Since Box et al. (2013) giveS 2 m air temperature..." -> "...Since Box et al. (2013) give 2 m air temperature..."	**Removed**
38	p. 8 ln. 24	Rephrase: "...provide insight on *how* the FAC10 *might have been* at a given place and time.". For example "what were the properties of...". Also add either "an" before or "s" after "insight" – "an insight" or "insights", but not just "insight".	**This paragraph was removed**
39	p. 9 ln. 2	"...systematically different *than* our calculated FAC10..." -> "...systematically different FROM our calculated FAC10...".	
40	p. 9, ln. 3	"*A last* measurement raises questions..." -> "One more measurement raises questions..."	
41	p. 9, ln. 12-13	"...to 10 m depth (FAC10) *could be* calculated" -> "to 10 m depth (FAC10) WAS calculated"	**We rephrased the conclusion according to our new findings.**
42	p. 9, ln. 13	"...three regions *on* the firn area *in which FAC10* where we *could* fit empirical..." -> "...three regions WITHIN the firn area where we fit empirical..."	
43	p. 9, ln. 17	"This decreas*ing* FAC*10* translates into the loss of..." -> "This decreasED FAC_{10} translates into the loss of..."	
44	p. 9, ln. 18	"...of meltwater retention capacity 1998-2008 and 2011-2017." -> "...of meltwater retention capacity BETWEEN 1998-2008 and 2011-2017."	

Reviewer #2

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 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:

Reviewer's comments	Authors' response
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.	**We apologize for these mistakes** **We now updated the numbers throughout the manuscript.** **Both our data and scripts will be made available to unsure reproducibility.**
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	**We now differentiate between the two sources.**

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.	**We changed the color scale according to the suggestions of Reviewer #1.**
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 publica- tion. Afterwards, a list of minor points is given on a line-by-line basis (where P and L 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	**We now present a full size research article, provide better discussion and reduce our use of the supplementary materials.**

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 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	**We updated these numbers and now "calculate an average FAC_{10} of 5.1± 0.3 m in the DSA, an average FAC_{10} of 2.8 ± 0.3 m in the HAWSA during the 2010-2017 period and an average FAC_{10} of 3.9 ± 0.3 m in the LAWSA during the 1998-2008 period, which decreased to 2.6 ± 0.3 m in the 2010-2017 period."** **We would like to remind that the HAWSA does not characterize only the aquifer region but also stretches down glacier to the firn line where no air content is available. We therefore expect the FAC_{10} to be much lower than in the DSA for example.** **Nevertheless it is true that the average FAC_{10} calculated in the HAWSA remains rather low, potentially explaining also the overestimation of RCMs in the HAWSA compared to our estimation (Figure 8d). It is now discussed in Section 3.6.**

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.	**The GEUS bulletin is a peer-reviewed journal (https://portal.issn.org/resource/issn/1904-4666). We also now refer to Fausto et al. 2007 which presented the method that Fausto et al. 2018 applied on the more recent MODIS data.**
Minor Points:	
P1, L25: “its characteristics are still little known” is better replaced by something along 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%.	**We rewrote the abstract.**

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”	**Updated**
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 (Humphrey 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.	**We now use a simple method to estimate FAC_{tot} (the FAC of the whole firn layer) from FAC_{10} and do not rely on the assumption that meltwater retention only happens in the top 10 m.**
P2, L27: Fausto et al 2018a! P3, L1: “From literature, we gathered ...”	**Updated**
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 FAC10 range not given as “1 to 5”? P3, L20: Similar to previous comment.	**We now give absolute uncertainty.**
P4, L1: Why not use the latest model estimates (HIRHAM, RACMO, MAR), or use all 4	**We now use MAR3.5.2 as the principal source for our Ta**

products to have some sort of best estimate.	**and ba. In the discussion, we show that we can fit equally well our FAC10 dataset with the older products from Box et al. (2013). We therefore believe that it is necessary to apply other sources which will probably fit equally well.**
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.	**Updated**
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.	**Sentence removed.**
P4, L14: Ta = -16C 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.	**The boundary between DSA and WSA is only defined by the inflection of the FAC10 curve in Figure 1c. Our dataset does not allow us to describe the evolution of this inflection point, although it is expected in a changing climate. We therefore cannot address this discussion point with our dataset and need to work with static Snow Areas within which FAC may change through 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. . .	**Following your suggestion, we replaced the densification equation from Arthern et al. 2010 by a linear function of Ta, making our approach fully empirical and avoiding the use of firn models that anyway do not fit our dataset.**
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 per- formed, 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 clearer and less dizzying.	**We now opted for 2D plots instead.**

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 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.	**We now show that our empirical functions of average temperature and accumulation fit our FAC10 dataset better than current state of the art RCM.** **However, we do not believe that our dataset allows us to use more than two input variables.**
P5, L7: Here, the measurements from different years are grouped (likely to accommo- date for climate change), so why was this not done for the boundary between DSA and WSA (see comment on P4, L14).	**We now present the deviation between observed and estimated FAC10 for each decade in the DSA.**
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.	**The use of remotely sensed firn line is judged less reliable than direct FAC10 measurements and is only used when insufficient in-situ measurements are available. It is made clear that in the LAWSA enough cores are available.**
P6, L8: Should refer to Figure 1a, I think. P6, L18: Should refer to Figure 2a. P6, L18: Add comma between region and representing.	**Updated**
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.	**Even though we now use a linear function of Ta in the DSA, I believe your question still applies.** **If the FAC had been decreasing in the DSA, the fitted time-independent linear function would normally underestimate older FAC10 measurement and overestimate recent FAC10 measurement. In other words, the temporal evolution would appear within the residuals presented in Figure 2b.**

	Since it is not the case, we consider that time cannot explain any variance in our FAC10 dataset or in other words that there has not been detectable temporal changes of FAC10 in the DSA.
P7, L1: FAC10 should be FAC10. P7, L5: Should refer to Figure 2d.	**Updated**
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.	**Indeed there was a mistake from our side. We now updated the numbers.**
P7, L8-9: Please rephrase "that had become unavailable by 2011-2017". 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.	**Updated**
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?	**Given a density of 400kgm-3, the upper 10m of firn contains 4000kgm-2 of water and for the 2012 horizon being buried in 2017 would require 5 years of at least 800 mm weq yr-1. These areas of high accumulation are mainly located in the HAWSA. Since we describe changes in the LAWSA, we do not believe such discussion is needed.**
P7, L26: Refer to Figure 2c and Figure 3c.	**Figures were rearranged.**
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	**We removed this discussion point**

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.	**We now use either spatially summed FAC10.**
P8, L17: Add comma after Nonetheless.	**Updated**
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.	**We do not mean "all variation" and specify the "spatial pattern" so we would like to pursue with the current phrasing.**
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.	**We now compare the output of three RCMs.**
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.	**This paragraph was removed.**
P9, L9: Not true. Fausto et al., 2018 presents the first delineation of the firn area of GrIS. Please rephrase.	**Although we believe that the firn line is a product of our study and that Fausto et al. 2018 only provided yearly snow lines while never even mentioning the word "firn", we now do not stress this result anymore.**
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".	**Updated**

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.	**Removed**
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=-11C and b=150 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.	**We updated Figure 1 according to the suggestions from reviewer #1. The remotely-sensed firn line is not as smooth as one would think when plotted in the Ta-ba space. There are many non-climatic factors that can affect the position of the firn line: topography, wind-driven snow transport, surface roughness, shading from surrounding topography…**
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?	**We modified the colour scale according to the suggestions of reviewer #1. We keep the location of FAC10 observations so that the reader is reminded that the FAC10 map is constrained by them and that areas with few observations are subject to greater uncertainty.** **With our new empirical approach there is a smooth transition between LAWSA and 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 a exponential scale.	**We updated that figure, kept the same colours but adapted the scale.**

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.

Miller, O. L., D. K. Solomon, C. Miège, L. Koenig, R.R. Forster, L. N. Montgomery, N. Schmerr, S. R. M. Ligtenberg, A. Legchenko and L. Brucker. 2017. Hydraulic conduc- tivity of a firn aquifer system in southeast Greenland. Frontiers in Earth Science, doi: 10.3389/feart.2017.00038.

Miller O. L., D. K. Solomon, C. Miege , L. Koenig , R. Forster, N. Schmerr, S. R. M. Ligtenberg and L. Montgomery. 2018. Direct evidence of meltwater flow within a firn aquifer in Southeast Greenland, Geophysical Research Letters, doi:10.1002/2017GL075707.