Final response on "Climatic signals from 76 shallow firn cores in Dronning Maud Land, East Antarctica" by S. Altnau et. al

To Anonymous Referee #1

AC: We thank all referees for their efforts and the constructive criticism.

The manuscript submitted by Altnau et al. present a compilation of surface mass balance (SMB) and water isotopic records from 76 shallow cores in the sector of Dronning Maud Land after a separation in different sectors. The main conclusion is that there is a clear difference in the relative variations of SMB and d18O of ice between the coast and the plateau, the plateau showing coherent variations of SMB and d18O since d18O is controlled by Rayleigh distillation and thermodynamic effects while on the coast, atmospheric circulation effects create strong unrelated variations of SMB and d18Oice. In general, this compilation is interesting and should be published. Still, there are several comments that should be taken into account to improve the manuscript that has too many figures and lacks from some clear conclusions:

- It seems that all data from this paper are already published. Still, it would also be nice to mention what is really new in this study compared to previous studies and what is the real novelty of this study. –

AC: We stated this in the paper and believe it was understandable, as Ref. #2 wrote: "The authors gave proper credit to related work and clearly indicated their own new/original contribution."

The main conclusions of the paper are not clear except the difference between the behaviors of d18O vs SMB in the plateau and on the coast.

AC: Those are the main conclusions, plus the reasons for it.

In particular, the link with the SAM is totally unclear

AC: True. It is not clear, that's what we wrote. We only discuss possible explanations for the relationship of SAM to SMB and $\delta^{18}O$.
If the referee meant that our explanations are unclear: We thought about giving a more detailed explanation of the meteorological conditions we discuss. However, although we fully support the requirement that a paper should be self-contained and a scientist, who works in the field, should be able to understand it without reading 5 other papers, we believe that ice core studies are highly interdisciplinary and we assume that the readers, who work with ice cores and climate, have (should have) some basic knowledge in meteorology. (We don't explain e.g. snow metamorphosis in each ice core paper either.) Thus we concluded that more detailed explanations would destroy the structure of the paper and deter from our main points. We re-wrote the discussion and conclusion section, but refrained from explaining basic meteorological terms. (see changes in a marked-up manuscript version)

and the conclusion part of the paper should be rewritten. It is not clear from what is written if the SAM has or not any influence on the ice d18O or SMB in coastal area since contradictory conclusions are presented. The authors also seem annoyed by a lack of clear signal with

sentences such as “The reasons are not yet entirely clear”. I do not see any problem to have a signal that is not clear or inexistent

AC: We never wrote anything that contradicts this. If we did not think the comparison was worthwhile we would not have included it in the paper. On the contrary, we wrote that the lack of correlation between SAM index, air temperature and $\delta^{18}O$ is highly interesting. We think the comment of Referee 1 is not very objective and we do not understand why Ref.1 should think/write that we are annoyed.

but the conclusion should be written more clearly to avoid a false take-home message.

AC: We re-wrote the discussion and conclusion section. (see marked-up manuscript version) However, we *discuss* different influences on and explanation possibilities for the non-existent correlation between SAM index and the other variables, but we cannot give a definite explanation that could be part of the *conclusion*.

- A discussion on how post-deposition effects also affect d18O of snow is missing (only post deposition noise on SMB is mentioned).

AC: We fully agree here and added some information and references about post-depositional processes in the discussion. (marked-up manuscript version l. 488 – l.494)

- Part 5.2.1 (and 5.2.2) could be rewritten for more clarity. More should be explained on the 11 cross-correlations and what are exactly the 3 cross-correlations of d18O that are significant. What does it mean? What conclusions can be driven for the different sites? For the meaning of the d18O signal in shallow ice cores?

AC: We rearranged this section since the paragraph with the cross-correlation makes more sense following the description of Fig. 6. Furthermore, we noticed that we had a typing error when we cross-checked the calculation for the cross-correlations. Instead of three statistically significant cross-correlations **nine** cross-correlations between the smoothed records (5 year running mean) are found to be statistically significant. This indicates that the temporal variability of $\delta^{18}O$ shows a relatively similar behaviour in most cores, which is in accordance to Figure 6.

p.5976 l. 1-10:
~~Only three of eleven cross-correlations between the detrended composite records of and also three cross-correlations between the smoothed records (5 year running mean) are found to be statistically significant at the 95 % confidence level according to t test. For the latter reduction in the number of degrees of freedom was taken into account for calculating the significance.~~
The stable isotope ratio for Ekström (Fig. 6a and b) and Fimbul (Fig. 6c) Ice Shelves is characterized by values generally lower than the multidecadal average during the periods 1950 to the mid-1960s and the 1980s, whereas the 1970s exhibits values above the mean. Ritscherflya (Fig. 6d) has only a short record, but agrees well with Ekström and Fimbul for the given period. For the last 20 years the smoothed record of $\delta^{18}O$ shows little variation. The $\delta^{18}O$ of the plateau cores (Fig. 6e) behaves similar to the ice shelf cores, with the exception of slightly higher values around 1960. The similar temporal variability between the different drilling sites is supported by the calculation of cross-correlations. Only three of eleven cross-correlations between the detrended composite records of $\delta^{18}O$, but nine cross-correlations between the smoothed records (5-year running mean) are found to be statistically significant

at the 95% confidence level according to Student's t test. ~~For the latter, reduction in the number of degrees of freedom was taken into account for calculating the significance.~~

There are too many figures in this part (and the following) that are only briefly mentioned and do not seem central for the final conclusion of the paper. Either some figures should be deleted or the text should be more explicit on what can
be learnt from these figures.

AC: We don't think there are too many figures. Particularly Fig. 5 and Fig. 7 give some valuable information to the reader at first glance and do not need more explicit explanation in the text. Therefore we prefer to keep these figures. We made some small changes in the description of Figure 6 in accordance to a comment of Referee #3.

- Some typing mistakes should be corrected (e.g. "Eat" instead of "East" on p. 5966).

AC: Done.