Reply to the Review Comments of Anonymous Referee #2

on the manuscript

TC-2017-35: Constraints on post-depositional isotope modifications in East Antarctic firn from analysing temporal changes of isotope profiles

by Thomas Münch et al.

We thank the referee for carefully reviewing our manuscript and for the constructive comments that will help to improve it. Below there is a point-by point response to both the general and the specific comments raised by the referee. The original referee comments are set in normal font, our answers (author comment, AC) are typeset in blue.

The manuscript "Constraints on post-depositional isotope modifications in East Antarctic firn from analysing temporal changes of isotope profiles" by Thomas Munch and co-authors is devoted to the study of post-depositional changes of snow isotope composition in central Antarctica using the huge dataset of recently obtained data. The authors clearly demonstrated, using robust statistical methods, that the observed evolution of the vertical profiles of snow isotopic composition can be explained without significant influence of the post-depositional processes. In general, I enjoyed reading the manuscript and suggest that it may be published as it is, or with minor corrections.

I think the authors could slightly modify the main idea of the conclusion of the manuscript. In the current version they state "no evidence for substantial additional post-depositional modification", meaning that they do not expect post-depositional modifications stronger than 1 per mil for oxygen 18. Indeed, 1 per mil is a very small value comparing to the spatial variability due to the stratigraphic noise. But on other hand, if considering the post-depositional modifications of the whole annual snow layer, 1 per mil is rather big value – it's an equivalent of about 1.25 *C of air temperature change! Thus, the obtained results still give some room for the post-depositional modifications of the snow isotopic composition, although they are less than 3 per mil as expected from the modeling (Page 14).

AC:

We totally agree with the reviewer that our results make post-depositional changes in addition to diffusion and densification unlikely at our study stite, but still leave room for such effects of the order of <1 ‰, and also very close to the surface where our data are insufficient. But we have to bear in mind that this limit of 1 ‰ refers to the root mean square deviation of the T15 and T13 profiles (after accounting for diffusion and densification) calculated over the entire record's overlap, thus on the seasonal scale. If we consider annual means, this value should be much smaller. However, still we will tone down our conclusions by stating that additional post-depositional changes appear unlikely, but that we can only constrain this to changes down to the order of less than ~1 ‰ RMSD on a seasonal basis.

Other comments or corrections:

Figure 1 would be more informative if you add a wind rose, or just an arrow showing the prevailing wind direction.

AC:

(We assume that Referee #2 refers to Figure 2 here). We will add a wind rose and an arrow indicating the prevailing wind direction (57°) to this plot.

Page 14, line 11, "Sublimation led in lab studies. . ." – the sentence looks somewhat awkward, please consider revision.

AC:

We apologize for the fact that this sentence was poorly formulated. We will rephrase it as follows: "In lab studies it was shown that sublimation leads to isotopic enrichment (Sokratov and Golubev, 2009); the modelling of post-depositional modification as a result of wind-driven firn ventilation by Town et al. (2008) yielded overall annual-mean enrichment from the enrichment of isotopic winter layers."

Page 16, line 8: averaging
AC:
This typo will be corrected.

Page 16, line 10: did you want to say that the spatial separation should be well above the spatial decorrelation length?
AC:
No, indeed we mean well *below* the decorrelation length of the stratigraphic noise. If you compare two isotope profiles that are spaced above the decorrelation length, the contribution of stratigrahic noise to the overall variability of the profiles will be different between the profiles since the noise is spatially no longer correlated. As a consequence, the resulting spatial variability between the profiles will likely mask any temporal changes you want to detect. By contrast, for a spacing below the decorrelation length, the noise contributions will show high similarity and therefore it will be easier to discriminate temporal and spatial variability. The downside of such an approach is that you have to make the second measurement as close as possible to the first one, making disturbances or contaminations of the second profile by the previous measurement(s) more likely. In the manuscript, for the sake of clarity, we will amend the cited sentence as follows: "Alternatively, single records can only be compared faithfully for temporal changes when their spatial separation is well below the spatial decorrelation length of the stratigraphic noise, minimising the amount of spatial variability between the records."