Review of manuscript tc-2020-77: "Spatial distribution and post-depositional diffusion of stable water isotopes in East Antarctica" by Mahalinganathan Kanthanathan et al.

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Summary

The authors present new stable isotope and accumulation data retrieved from two spatial (coast to inland) transects in the East Antarctic regions of central Dronning Maud Land (cDML) and Princess Elizabeth Land (PEL). The data were obtained from drilling and analysing short (1 m) snow profiles at 25 and 21 positions along the respective transects, covering coastal, mountaineous (cDML) and plateau regions. Based on the data, the authors present standard analyses of the relationships between oxygen and hydrogen isotopic data as well as between isotopic data and local temperature and accumulation rate, and how these vary between the two transects. In addition, they present brief diffusion and back-trajectory analyses. While the paper presents new data, the overall quality and scientific originality of this work, as I will outline in my general comments below, does not meet the standards of The Cryosphere. Therefore, I rate this manuscript as being not accetable for final publication in this journal.

General comments

The major shortcomings of this work are its low quality and scientific originality.

Regarding quality, the writing suffers from many small grammatical errors, which make the manuscript hard to read. Additionally, the introduction is poorly structured, the methods incomplete, and the results read like a dry technical document using many repetitive phrases. Overall, I would strongly suggest the authors to consult a language editing service to improve grammar and style. In addition, there seem to be some inconsistencies between the data as given in the tables and presented in the figures (see my specific comments).

Furthermore, the paper lacks significant scientific originality. While the authors present an extensive data set from two expensive transect sampling campaigns, which in principle would offer the chance for an interesting study, they unfortunately fail to exploit this scientific potential. What the authors present is a set of analyses which are standard for isotope studies and which already have been conducted and shown many times before. This is especially sad since the authors seem to be well aware of the challenges and uncertainties related to the interpretation of (surface snow) isotopic data in Antarctica as it has been developed by the recent literature. However, by contrast, this study does not offer any new insights which could aid the community in gaining a deeper understanding of the involved processes shaping firn and ice isotopic records. The two following examples are symptomatic for this deficiency. The authors make use of state-of-the-art firn diffusion modelling to estimate the amount of diffusion which should have attenuated an isotopic profile over the course of time, but they miss to actually test this diffusion model for their chosen site by quantitatively comparing how the model prediction fits to the data. The second example is the discussion of the back-trajectory analysis. The most text of the respective discussion section actually presents additional analysis results that were not mentioned in the results section before, while the small portion of interpretation remains at a poorly speculative level, which leaves the reader guessing what we can learn from this analysis. An interesting aspect of the presented data set is the different importance of the spatial accumulation variability for the spatial isotope variability of the two transects. This would be an interesting candidate for an in-depth study, which the authors however do not pursue. Also, the transect data seems to comprise at least 1 m long isotope profiles for more than 40 positions, but the authors do not analyse or show any of these profiles, instead sticking to the analysis of only the mean values. Overall, the manuscript fails to meet the scientific criteria of The Cryosphere and is not in a state to be accepted for final publication.

Specific comments

Below I list some specific comments that could help to improve the manuscript, but I do not include the many occurrences of grammatical errors, missing specific articles, etc.

- L1: The abstract should briefly introduce the background of the study in a first sentence.
- L1: Please consider using the correct terminology "stable water isotopologues", or use phrases such as "stable isotopes" or "isotopic composition".
- L3: Please introduce δ^{18}O and δ^{2}H shortly here, or alternatively, rewrite this passage and introduce the notation in the introduction.
- L8: The term HYSPLIT is not explained anywhere here. Please also note in this regard that you do not explain how the back-trajectory analysis is actually conducted, whether in the Methods nor the results text.
- L18: Why is high resolution important in this context?
- L25: I do not see any obvious direct connection between the Town et al. study on atmosphere–surface snow exchange and wind-driven erosion and redistribution processes. You have to elaborate more on this, and consider adding more relevant recent literature, e.g. the stratigraphic noise studies by Münch et al. (2016, 2017) and the mega dune studies by Ekaykin et al. (e.g. Ekaykin et al., 2016).
- L27; sentence "The deuterium excess [...]": There is no logical link to the previous text.
- L36; "no net change [...] were observed": This statement is misleading. For normal firn diffusion and densification, we do not expect any net mean change, as a basic property of these processes. As it is written, one could interpret this as something which just has not been observed yet due to measurement uncertainty or such.
- L36: The statement that diffusion in ice is negligible has to be put into context. Ice diffusion is certainly not negligible for very deep ice cores = very old ice!
- L36; "and therefore gets preserved": Unclear from the context, what gets preserved?
- L27; sentence "Studies by...": There is no logical link to the previous text.
- L43 first sentence: This statement is in direct contradiction to the sentence about the studies by Steen-Larsen et al. (2014) and Ritter et al. (2016) a few sentences above.
- LL15-50: Overall, the introduction is poorly structured with missing logical links between sentences and paragraphs. You may consider rewriting it altogether.
- L67: What do you mean by external precision? To which isotope species do you refer here – δ^{18}O or δ^{2}H? Why do you measure samples also on the Los Gatos device, if all samples have been already analyzed on a mass spectrometer (cf. L65 " All samples...")?

- LL75-76: As you also have impurity data available from the same cores (as evident from previous publications), it would be probably more robust to conduct the dating using isotope and impurity species together, instead of setting an arbitrary threshold on the isotope peak heights.

- LL51-86: Descriptions of the HYSPLIT back-trajectory analysis and the used multiple regression model is missing.

- L95: It would be actually nice to see some example isotope profiles or even use them to further study the spatial variability and its drivers along the two transects.

- LL107-109: Using the data provided in Tables 1 and 2, I find for both transects a significant correlation between d-excess and temperature on the 0.1 significance level ($R = -0.4$, $p = 0.06$ for cDML; $R = 0.39$, $p = 0.08$ for PEL) – can you comment on this? In any case, concerning the opposite correlations and the significant scatter of the data (Fig. 4), I however ask myself what we can actually learn from regressing ‘d’ against temperature here and if there is actually any meaningful explanation behind the correlations?

- L137: What kind of multiple regression model do you use here? This could be added to the Methods section.

- L153: What do you mean here with “all the samples”? Did you use the combination of cDML and PEL samples for another $\delta^{18}O - \delta^{2}H$ regression?

- L163; “seems to be reasonable”: This is expressed the wrong way around and anyway a quite obvious result: Your slope results from two Antarctic subset regions scatter around the Antarctic-wide mean slope – as it is to be expected.

- L169-172: It should be mentioned that the relationship by contrast is positive for the PEL transect data at the same significance level (according to my estimate, see the previous comment above), so a more detailed discussion of the d-excess-to-temperature relationship is needed here.

- L188-189: What exactly are the initial and final depths here?

- L189: Münch et al. (2016) is not the correct reference; I guess you mean Münch et al. (2017), where a similar approach was taken.

- Section 4.2: What do we learn from this? Have you tried to forward-diffuse the younger record with the estimated diffusion length to see if the mismatch in seasonal amplitude is really only a result of diffusion? By which value did you shift the newer data downwards and how did you choose this value? If you really want to constrain the post-depositional changes at this site similar to the study by Münch et al. (2017), you also have to take into account the effects of densification and stratigraphic noise, but I also think that the extent of overlap your two records have is much too small to arrive at any meaningful conclusions.

- Section 4.3: This section effectively presents additional results from the back-trajectory analysis, which should be placed therefore into the respective results section. Beyond that, it is pretty much unclear to me what we learn from this exercise.

- Figure 1: Please explain the abbreviations ETOPO1, IBSCO and RAMP2, and provde a source for these map data.

- Figure 3: There are two more cDML points on this figure than given in Table 1 (there are two missing isotope values in the table). For PEL, I cannot reproduce this figure using the values given in Table 2: while the overall plot looks similar, there are many offsets between the different points and also a linear regression gives slightly different results. Please carefully check that the given data are consistent with the figures and that they always reflect the latest version of your work.

– Figure 4: While I can reproduce the cDML plot given the data in Table 1, the PEL plot using the data of Table 2 looks different than the one provided here. Please check the consistency of all your data and figures; see also my comment on Fig. 3.

– Table 1; caption: What kind of averages are the stable isotope values? Averaged over the 1 m snow cores?

– Table 3: How should one read this table? Since the table is not symmetric around the diagonal, I understand that the correlation numbers differentiate between the two regions, but it is unclear which region belongs to which column or row. Also, why are there no correlation values listed for d-excess, although you mention in the text and show in Fig. 4 that there is at least some correlation between 'd' and temperature for the cDML transect data?

References

Ekaykin, A., Eberlein, L., Lipenkov, V., Popov, S., Scheinert, M., Schröder, L. and Turkeev, A.: Non-climatic signal in ice core records: lessons from Antarctic megadunes, The Cryosphere, **10** (3), 1217–1227, DOI: 10.5194/tc-10-1217-2016, 2016.

Münch, T., Kipfstuhl, S., Freitag, J., Meyer, H. and Laepple, T.: Regional climate signal vs. local noise: a two-dimensional view of water isotopes in Antarctic firn at Kohnen Station, Dronning Maud Land, Clim. Past, **12** (7), 1565–1581, DOI: 10.5194/cp-12-1565-2016, 2016.

Münch, T., Kipfstuhl, S., Freitag, J., Meyer, H. and Laepple, T.: Constraints on post-depositional isotope modifications in East Antarctic firn from analysing temporal changes of isotope profiles, The Cryosphere, **11** (5), 2175–2188, DOI: 10.5194/tc-11-2175-2017, 2017.