Reviewer #2

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, mountainous (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 acceptable for final publication in this journal.

We are grateful to the reviewer for providing their constructive comments. We have addressed (in blue) all the reviewer comments to improve the overall quality of this manuscript to fulfill The Cryosphere standards.

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).

The quality of the manuscript is being improved significantly with changes in the Introduction section, including missing details in the methodology section and undertaking more data analysis and discussion as suggested by both reviewers. The inconsistency in data and figures / tables are also addressed. English language improvement would be made as suggested in order to improve the quality of the manuscript.

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 1m 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.

Scientifically, we have now included the profiles of isotopes from the representative regions and will provide the complete depth profiles of isotopes as a supplement and discuss the major features in the text. We have included the discussion on how the diffusion model fits to the actual ice core data as suggested by the reviewer. We have also included the details of the back-trajectory analysis in the results section and improved the discussion on the same in order to understand the possible moisture sources for the study region. We also attempt to improve the discussion on the spatial variability of accumulation and stable isotopes in the two transects as noted by the reviewer. We are confident that these measures undertaken would substantially enhance the quality of the manuscript to the standards of The Cryosphere.

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.

The background of the study is included in the beginning of the abstract as suggested.

L1: Please consider using the correct terminology “stable water isotopologues”, or use phrases such as “stable isotopes” or “isotopic composition”.

The correct terminology, “stable isotopes”, is being used throughout in the revised manuscript.

L3: Please introduce $\delta^{18}O$ and $\delta^{2}H$ shortly here, or alternatively, rewrite this passage and introduce the notation in the introduction.

We have introduced the $\delta^{18}O$ and $\delta^{2}H$ in the abstract as suggested.

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.

HYSPLIT exercise along with the details of back-trajectories are now explained in the methodology section of the revised manuscript.

L18: Why is high resolution important in this context?

The high accumulation in the coastal Antarctic region permit us to have snow cores representing detailed changes that occurred over a complete year. As a result, we have over 40 snow core depth profiles representing the spatial changes over central Dronning Maud Land and Princess Elizabeth Land, with a sampling resolution in terms of months (or large precipitation events), which typically lacks in short ice cores. The combination of areal coverage with high temporal resolution aids in the interpretation of regional temperature and climate while giving insights on the ice core records.

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. L27; sentence "Studies by...": 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? 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.

The introduction section is now rewritten with more focus, clarity and citing more recent relevant literature while maintaining logical flow as suggested in the specific comments L15-50. Statements on firn diffusion and densification with respect to interpretation has been clarified in the revised manuscript.

L67: What do you mean by external precision? To which isotope species do you refer here – $\delta^{18}O$ or $\delta^{2}H$? 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...")?

Conventionally, the external precision on stable isotopes of water are estimated based on the repeated analysis of international standards like Standard Mean Oceanic Water (SMOW). Since such standards are limited and precious, all water isotope measurements are made with reference to a laboratory standard calibrated with SMOW. In current case, the precision determined by repeatedly measuring a laboratory standard called Central Dronning Maud Land snow (cDML) is used for estimating external precision. Here we run the cDML standard 20 times – where this standard is passed through the autosampler and all the lines that a sample would be going through, and arrive in the precision value. All samples were first analysed using IRMS. We freshly sub-sampled and some of these samples were reanalysed with the newly acquired LGR Triple Isotope Water Analyzer that uses Off-Axis Integrated Cavity Output Spectroscopy (OA-ICOS) technique to measure $\delta^{18}O$ or $\delta^{2}H$ after 9 years. Since data generated through this method also revealed similar amplitude, we demonstrate that there exist no diffusion in stable isotopes while storage. Such completely different methods used in this study confirms the reliability of the stable isotope data presented here.

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.

We have already used the ionic concentrations along with stable isotopes in order to date the snow cores as cited in L77. However, since this is not properly conveyed in the manuscript, representative examples of the isotope profiles along with the impurity species will be included in the revised manuscript.

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

The HYSPLIT description and details of the multiple regression model are now included in the revised manuscript.

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.

Isotope profiles from representative sections of topography will be included in the revised manuscript.

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?

All the parameters except the d exhibited a strong correlation between each other at a higher significance level (0.01) in both cDML and PEL. In the revised manuscript, we include the d-excess temperature relationship in Table 3, and also include a more-detailed discussion on this relationship as suggested.

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

We use the multiple linear regression model and we clarify this in the revised manuscript.

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?

We intended to convey that we used all the sub-samples rather than mean values at every core location in cDML and PEL transects. This is clarified in the revised manuscript.

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.

We agree. We have now revised and compare our work with the compilation of Antarctic wide dataset here and we have changed this sentence.

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.

A more detailed discussion explaining the data from the PEL transect shall be included in this section as suggested by the reviewer.

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

This equation was intended to calculate the differential diffusion signal between $\delta^{18}O$ and $\delta^{2}H$ in order to reconstruct the temperature, however was not utilized in the manuscript. In the revised manuscript, we will include a discussion on the temperature reconstruction from the firn core as suggested by the reviewer 1.

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

Correct citation is provided in the revised manuscript.

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.

In this section, we aim to understand the process of diffusion over time, by comparing the snow core record with a firn core that was drilled 5 years after the snow core. The exercise was a direct comparison of the amplitude diffusion between the snow core and the firn core. In order to improve the discussion here, we shall include the forward-diffusion of the snow core record to test the diffusion length. Though the record may be small, the higher resolution of the snow core record would help us understand the diffusion process in the high accumulation region.

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.

In order to provide more clarity, we shall quantify the trajectory clusters instead of the frequency map, as suggested by the reviewer. We also have improved the discussion in this section to understand the origin of moisture sources.

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

The abbreviations and map sources are provided in the revised manuscript.

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.

We apologize for this error. The temperatures provided in Table 2 was of a different model, while the figure used the RACMO temperature. This inconsistency in table 1 will be fixed in the revised manuscript.

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

The averages mentioned in Table 1 are annual average values and not 1 meter average. After the demarcation of a year, the values over a year are averaged.

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?

Table 3 intends to showcase the correlation values from both cDML and PEL regions – which are split diagonally along the value 1. Since this form of table is complicated, we provide a new table separately showing cDML and PEL values. All values in this table was at 0.01 significance level while the cDML correlation showed 0.05 significance level. These values would be added in the revised manuscript with clear marking.