

Interactive comment on “Spatial distribution and post-depositional diffusion of stable water isotopes in East Antarctica” by Mahalinganathan Kanthanathan et al.

Anonymous Referee #1

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This manuscript presents results about superficial snow cores across the Dronning Maud Land and Princess Elizabeth Land, as well as a superficial ice cores from near the DML transect. The produced data are of high value as they can help identify in two coastal areas of East Antarctica if the deposition leading to the formation of the firn is homogenous over areas characterised with largely changing conditions (temperature, accumulation...). This type of study is very valuable because they will help in the long run to improve the interpretation of the water isotopic composition from ice core records.

At this stage though, I cannot recommend that the manuscript is accepted for publica-

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tion. While I'm really keen on seeing these results published, I believe that the current version of the manuscript present significant flaws which do not honour the high quality of the dataset which was I'm sure produced at high costs (at least 3 Antarctic campaigns).

I have included some more specific points as major and minor comments on the manuscript. In general, I do not believe that the manuscript is providing any critical interpretation of the results. While the authors compared their results to various other results from past publications (for instance the diffusion models of (Münch, Kipfstuhl, Freitag, Meyer, Laepple, 2017) or the isotope-isotopes or isotope-temperature slopes from (Masson-Delmotte et al., 2008)), they fail to include the added values obtained by the comparison, or to bring up new interpretation as a scientific paper should do. In the current state, the manuscript provides and makes public this very valuable dataset, but, in my opinion, is not using it to learn something that could help the scientific community. It saddens me to give this harsh review to this manuscript which I would have liked to see through. I sincerely hope that my comments below will help the authors in the complex interpretation of this very valuable dataset.

Major Comments:

The introduction could be improved. It is difficult to follow what is the goal of the introduction in respect with the abstract, and the title of the article. At this stage, I could expect anything from a review on water isotopes in Antarctica to the interpretation of a new ice core record. Overall the manuscript seems to cover a wide range of different aspect of what could be done with these data, but do not actually follow through on any of these. I would suggest to refocus the introduction, and the manuscript on the main point of the manuscript, which in my opinion is the comparison between the two transects and the comparison between the ice core and the cDML transect.

One major aspect comes from the evaluation of the accumulation rates for the difference sites. If I understood well the paper, they were estimated from the successive

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maxima/minima from the isotopic profiles, yet, there is evidence that diffusion of white noise would also create successive cycles that look similar to the seasonal cycle (Laepfle et al., 2018). I think it would be very valuable for the manuscript to have rigorous tests that the accumulation rates determined here are not artefacts created by the diffusion length. Typically, a good test would be to diffuse with noise for each site and compare the mean distance between maxima/minima of the diffuse core with the one of the observed core. If the accumulation rate predicted for the diffused white noise is close to the observed accumulation, this would cast doubts on the determination of the accumulation rate in my opinion. Typically, we have been able to observe this phenomenon for accumulation rates up to $80 \text{ kg}\cdot\text{m}^{-2}\cdot\text{a}^{-1}$ (w.e.), and have never tried this for accumulation as high as observed here. Yet, this could also be affecting the results here for sites such as cDML 15, 24, 25. Observing the results from Fig. 5 where the diffusion length is roughly 6cm for the first meters (lines 110 to 111), one would expect white noise diffused cycles of 5 times the diffusion length, so roughly 30cm, which seems rather similar to what is observed around 4m deep in Fig. 5. Also, are the accumulation rates in water equivalent? As this will have a strong impact on the diffusion.

The interpretation of the multiple regression model seems to be arbitrary to me in the present form. In Table 3 one can see the correlation between the different variables (even though the part of the table for PEL seems to be missing which prevents from interpreting Table 4). In cDML, roughly all the variables are very well correlated with one another, as a result, when you do the multiple regression model in Table 4, you're not proving that the distance and the elevation explain negligible variance, but that the variance explained by the temperature and the accumulation is most likely linked already to the distance and the elevation. The argument for the entire paragraph from line 135 to 145 seems completely specious to me.

The comparison of the ice core and the snow cores is probably the most important aspect that could be included in this manuscript. At this stage, it is not possible to

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evaluate how much the signal got dampened considering how different appear to be the sites of the cDML9 and the ice core drilling site. The accumulation of the ice core drilling site appears to be twice as large as of cDML9, while this is not mentioned anywhere in the manuscript. Overall, “I have the feeling” that the ice core drilling site would match what the authors refer to as a “coastal” type of site, while it’s compared to cDML9 which is classified as “mountainous”. First, I should not have to guess or “have a feeling”, and this should be studied in the manuscript. In general, considering that you have 25 snow cores, I feel it would make sense to align all of them to the core and to have statistical constrains on the original amplitude, as the sites were actually different (25km apart I would guess from Fig. 1). Second, there is no clear constrain of post-depositional processes here, not any use of the comparison between the snow core and the ice core. For instance, can you use this method to actually deconvolute the diffusion from the climatic signal and reconstruct the temperature from the firn core ? If so, does it compare with the model temperature time series for this site ? Only if you are actually able to do this have you properly constrained the post depositional processes. Also, for a coastal site like the one of the ice core, I have trouble to imagine that diffusion is the most important post-depositional process affecting your ice core, compared to sublimation/condensation combined with katabatic winds, wind redistribution or scouring. . .

Minor Comments:

Lines 58 to 60: “After recovery, the snow cores were transferred directly into pre-cleaned high-density polyethylene bags and sealed immediately to avoid contamination during storage and shipped under frozen conditions to the National Centre for Polar and Ocean Research, India. The cores were stored at -20°C till analysis.” Is there any evidence that diffusion cannot happen in highly porous snow at -20°C ? These conditions are very close to summer firn conditions at places like Dome C, where diffusion still takes place.

Lines 75 to 77: “The seasonality in snow cores was determined by establishing the

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summer and winter peaks in isotope records where a minimum amplitude of 4‰ between summer and winter was used to differentiate these peaks as detailed in figure 2 of our previous publications (Mahalinganathan et al., 2012; Mahalinganathan and Thamban, 2016).” There are evidences that the cycles observed in isotopic profiles of water isotopes can be linked to diffused white noise, especially for low accumulation areas (Laepplé et al., 2018), as a result, they can be deceived when used to identify annual layers and date ice cores. It is not necessarily going to be the case for snow cores that close to the coast, but it’s definitely something good to discuss. Lines 88 to 93: Are the accumulation rates obtained from the distance between maxima/minima in the isotopic profiles ? In which case, as mentioned in my previous comment, I’d strongly suggest to provide evidence that similar distance would not be created artificially by diffusion of white noise.

Lines 112 to 113: “Five-day back-trajectory frequency maps of coastal, mountainous and inland locations showed vast differences in the sources between summer and winter (Fig. 6)” Are they vastly different? I’m a bit confused for several reasons. First, what months did you choose for summer and winter ? Antarctic summer is rather brief for a lot of sites, with a large asymmetry between summer months (DJF), and winter months (AMJJASO) (Van Den Broeke, 1998). As a result, statistically, you could already explain having less events in summer, and as a result, a small range of possible storms visible in the sample. Second, it seems that if the dark red and dark green envelopes do cover a large area, most of the trajectory originates from very similar cones both in summer and in winter, which is quite unexpected, and very intriguing. Considering that the dark red corresponds most likely to a very small number of trajectory, it would be interesting to have quantitative numbers of trajectories for each of the sectors you mention here, to actually be able to evaluate information beyond the plot. Finally, this does not necessarily reflect the actual contribution to the different ice cores, indeed, a small number of events can contribute a large amount of the accumulation, and I have serious doubts that the trajectories originating from the Plateau contribute for a lot of the accumulation for instance. I would recommend to realise an analysis similar to the

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one found in Figure 5 of (Genthon, Six, Scarchilli, Ciardini, Frezzotti, 2015) and include the relative contributions to the snow fall amount for clusters of trajectories.

Lines 119 to 120: “Spatial variations of snow accumulation in Antarctica are primarily due to the presence of physical barriers during snowfall and snow redistribution post deposition (Melvold et al., 1998; Vaughan et al., 1999).” I would say I disagree with this sentence, and that there are a lot of literature that has been produced since 1999 going in a completely different direction. Depending on your definition of “spatial variations of snow accumulation”, you can obviously interpret this sentence in a lot of different ways, so if you look at small scales, it’s been shown that at Dome C, the accumulation can vary by large amount over short distance without any physical barriers like montains (Genthon et al., 2015). Looking at typical coast-to-interior patterns, I would recommend for instance to consider (Agosta et al., 2019) which shows that even for other sites without mountain ranges, the dominant pattern is the coast-to-plateau gradient.

Lines 125 to 130: “These mountain chains in cDML act as a physical barrier to the air masses arriving from the Southern Ocean impacting the snow accumulation and redistribution. As a result, the study area could be separated into three distinct accumulation regimes. The physiography and topography of the cDML region evidently influenced the snow accumulation rates showing a strong correlation with distance and elevation (Table 3). On the contrary, the PEL transect showed moderately high accumulation with little variation between the coastal (276 kgm⁻² yr⁻¹) and the inland (260 kgm⁻² yr⁻¹) sections.” While it’s true that it seems that the accumulation gradient is larger for the DML site, and that one could expect the mountain range to affect the accumulation along the transect, I don’t think you’ve proven it yet. Indeed, the PEL transect does not reach altitude as high as the cDML transect, which could also explain the difference in accumulation. As previously mentioned, I would also be careful with the evaluation of the accumulation rates from the isotopes, considering you’re using the temperature from RACMO, I would suggest to also include the accumulation rates from RACMO

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which might help in the interpretation here (similar to (Agosta et al., 2019)), or to even remove this paragraph.

Lines 137 to 138: “However, the multiple regression models using the geographical parameters and $\delta^{18}O$ showed negligible variance with distance and elevation (Table 4).” Considering how well correlated are the temperature, the elevation and the distance to the coast, I don’t think you can make this assumption here.

Lines 150 to 151: “The slope of the LMWL in cDML (7.9) is lower than that of the global meteoric water line (GMWL) while the slope of LMWL in PEL (8.12) had a slope close to GMWL.” It would help here to have error bars on the MWL slope, as well as correlation coefficient and significance tests to assess the robustness of the variations of the slopes of 0.1 around the MWL.

Lines 163: “Therefore, the proposed spatial slope (0.80‰ /C) by Masson-Delmotte et al. (2008) seems to be reasonable.” I would say that the slopes you obtain seem reasonable compared to the ones found in (Masson-Delmotte et al., 2008), considering that in the aforementioned article were included over 1000 snow pits and ice cores, across all over Antarctica. The two sentence are equivalent as the statement is a bijection, but it seems more that your results are validated by what was already found in this study, than you are validating this previous study considering the content provided in both cases.

Lines 180 to 181: “The detailed stable isotope records and chronology of this ice core is discussed in an upcoming paper (Tariq et al., 2020, unpublished).” Considering that the synchronisation of both cores is key, while the Tariq et al is not available to evaluate how the two records was synchronised, it is difficult to evaluate the work in the section. Indeed, the accumulation for cDML9 is $157 \text{ kg.m}^{-2}\text{.a}^{-1}$, assuming that the value is in water equivalent (which it ought to be), this means that in 5 years, you expect 2.75m of accumulation, very far from what is shown in Fig. 5. As the core is still quite far from the site, it is possible that the accumulation was slightly different, and the value

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of cDML9 seems to be much lower than the neighbouring sites, so even taking into account the values for cDML8 and 10, we would expect 3.94m, quite short to what is described in Fig 5. If the accumulation rates at the ice core site are that different from the accumulation rates along the transect, can you provide evidence that the amplitude of seasonal cycle of isotopic composition in the precipitation was the same? Typically, considering that the cDML transect sites neighbouring the ice cores are all mountainous sites, while the ice core seems to be in a more “coastal” sites, can you illustrate if you obtain similar amplitudes in the different firn cores of the transect that would justify that the original amplitude of the ice core could reasonably be close to what was in cDML9. Considering the large difference of amplitude between what is found for cDML9 (>5permil) and the surface of the ice core in summer/winter 2012-2013 (<2permil), one could also wonder if the ice core site just has singularly less pronounced seasonal cycle of precipitation isotopic composition.

Figure 5: Which snow core is included in the figure ? I couldn't find the information easily.

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