

The Cryosphere Discuss., referee comment RC1  
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## **Comment on tc-2022-161**

Anonymous Referee #1

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Referee comment on "Stable water isotopes and snow accumulation from the Weddell Sea sector imprint the large-scale atmospheric circulation variability" by Andressa Marcher et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-161-RC1>, 2022

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Review on „The stable water isotopes and snow accumulation from Weddell Sea sector imprint the large-scale atmospheric circulation variability“

General comments:

The authors present new stable water isotope and accumulation data of two firn cores extracted from the Möller Ice Stream in the Weddell Sea Sector (WSS) of Antarctica. They investigate the temporal variability of the isotopic and accumulation records and their relationship to meteorological station and reanalysis data and large-scale atmospheric modes. They identified temporal changes in the phase of the Southern Annular Mode (SAM) and associated pressure changes of the Amundsen Sea Low (ASL) and the WSS as well as temporal changes in sea ice concentrations in the WSS as the most important factors influencing interannual isotope variability. Furthermore, the authors attempt to reconstruct the depositional history of the study site. They suggest that strong wind events (SWE) and extreme precipitation events (EPE) are the main drivers of local snow accumulation, acting like a sea saw, i.e., accumulation increases when SWE frequency increases but EPE frequency decreases and vice versa. They emphasise the importance of their records for reconstructing atmospheric circulation patterns in the WSS, for understanding the mass balance of the Möller ice stream and how this might change under a future climate warming scenario.

There is no doubt about the uniqueness of the presented datasets and their potential to contribute to a better understanding of cryosphere-atmosphere interactions in the Weddell Sea Sector and Antarctica in general. The authors put a lot of effort in analysing the derived isotopic and accumulation data and understanding the reasons for observed temporal changes in the records. However, I have a little doubt that the manuscript is suitable for publication in its current state, unless major revisions are made.

There are two main reasons for this:

- In general, it was quite exhausting for me to read through the manuscript, since it is very long, mainly because the authors lose themselves in the details. The methods are described very much in detail, but many of the provided information is not necessary to understand the records and the story of the paper. In contrast, the discussion section has no subsections, which makes it very difficult to follow the authors' thoughts. Better structuring of the discussion section is of major importance for the improvement of the manuscript. In addition, results and discussion are mixed up in some parts of the manuscript. However, all these issues regarding the structure of the paper can be more or less easily fixed and are not an obstacle to the publication of the manuscript.
- What troubles me more are the correlation analyses of isotopes and snow accumulation, respectively, with meteorological records and climate indices. There is nothing wrong about finding no correlations or trends in your data, but non-existing trends and statistically insignificant correlations should not be stated explicitly, i.e., by listing all  $r$ - and  $p$ -values, in the text. To read through all the insignificant numbers makes it very hard to follow the story. Honestly, I got a bit confused by all the correlations, partly because there are many not statistically significant ones that are described too much in detail. It also makes no sense to discuss such insignificant relationships. If the  $p$ -value is much larger than your  $\alpha$ -value, there is no correlation or trend that can be reasonably discussed, even if the correlation coefficients are something like  $\pm 0.3$  or  $\pm 0.4$ . The "high" correlation coefficients may only have occurred by chance.

Specific comments:

Title: Rephrase to "Stable water isotopes and snow accumulation from the Weddell Sea sector imprint the large-scale atmospheric circulation variability"

Abstract: The abstract is too long and detailed. Even if there is no limit to the number of words, I think you should try to shorten it to a maximum of 250-300 words. Do not state numbers like coordinates, correlations with  $p$ -values in the abstract. Also, abbreviations like AWS and  $\delta s$  are not explained. If just reading the abstract, I would wonder why the isotopic and accumulation records are of different length, although they are derived from the same cores.

Introduction:

P4 L120 ff.: This paragraph already provides a detailed description of your methods. It is

ok to give an overview of your methods in the introduction but keep it short. Do not explain each analysis step here as this belongs into the methods section.

## Materials and Methods

Your methods section comprises almost 10 pages and you provide a lot of details that are not necessary to understand how you derived your records. In general, it is good to describe the methods carefully, but all the tiny details you give make this chapter very tiring to read. If you really want to provide all these details, consider putting some of them into the Supplementary.

P 5 L 146 It is not important where the name Criosfera comes from. Skip this information.

P 5 L 156: I am a bit in doubt that you can determine the ice thickness with such a high precision even if the BEDMAP 2 dataset might provide this number. Round it to 1900 m or at least to 1870 m.

P 6 L 161: Which changes do you mean with "such changes"? Be more specific.

Figure 1: Same as in the text. I suggest rounding the ice thickness.

P7 L 185: Where does this depth of 106.11 m come from? Table 1 says that CR1 is 9.13 m deep.

P 7 L 195ff. These are again a lot of unnecessary details. Shorten here (e.g., it does not really matter for the story of the paper whether samples were transported by air or road).

P 8: I suggest combining 2.3.1 and 2.3.2 with chapter 2.2 into one chapter called "Firn/ice core collection and processing". The method descriptions you provide in chapters 2.3.1 and 2.3.2 are very detailed. Simply describe what was measured with which instrument. All the details on how the instruments work or which water was used for rinsing are unnecessary for the story of the paper. You do not write a paper on methodology. If you want to keep all the information in the paper, consider putting them into the Supplementary.

P 9: I suggest combining chapters 2.4.1 and 2.4.2 into one chapter 2.4 called "Snow isotope and ionic chemistry analysis".

P 10 L 267: Rephrase: "Precision of the measurements was... "

P10 L 274 ff.: You already list the measured ions in Table 2. Just refer to the Table here and do not list everything again in the text.

I also think that the details provided on the instrument are not necessary to understand what you did.

P 10 L 281 ff. Same as before, too many details. Just state that you used the methodology of Thoen et al. with slight modifications and refer to the Supplementary, where you can provide all the details.

P 10 L 293: You have already provided this information in Table 2.

P 11: Replace "estimates" by "estimation" in the title of chapter 2.5

Please provide a reference for equation 2.

P11 L 309: What do you mean with "others"? Either be precise or delete this.

P11 L 315: Better use 1<sup>st</sup> January, than "New Year's Day".

P11 L 320f.: Which cores did you use? Be precise and provide references if applicable.

P11 L 325f. Delete that the approach was challenging and provide the dating uncertainty for both cores instead.

P 12 L 342 What does "satisfactorily" mean? Can you quantify that?

P12 L 343f. How do you conclude from the correlations provided in Table S1 that ERA5 overestimates/underestimates the temperature/wind velocities? I do not understand how you can read this information from correlations without looking at the absolute values.

P 14 L 396: What do you mean with "mean pressure at the level"? Which level?

## Results

P 14 L 406: Did you consider calculating diffusion lengths for your cores. Then you can better quantify the possible effects of diffusion on the isotopic values.

P14 L 419: What is nssSx? Please rephrase the whole sentence. I guess you want to say that you detected peaks in both cores which are correlated.

P15 L 425/L431: Please provide a unit for the dating uncertainty (I guess year?).

P 17 L 446: Use plural here: "... the minima and maxima of the cores..."

Figure 3 shows the records on the depth scale. How can you conclude from this that the minima and maxima mismatch over time? You would first need to place the cores on the same depth scale by aligning them to a tie point.

P 17 L 447f. This belongs into the discussion.

P 17 L 462 ff. Did you assess the possibility that isotopic signals near to stratigraphic features might be altered, i.e., that hoar frost and ice lenses (probably resulting from some melting?) may have modified the isotopes?

P 18 L 473f.: Reduce the sentence to "This may indicate that the isotopic signal ..."

Do you mean here "in the snowpack"? Something is missing at the end of the sentence.

P 18 L 476: Delete "co-isotopic".

Figure 4d and e: I do not really understand what you are showing in these two figures and what the outcome/benefit of/from this analysis is. Why are the correlation coefficients larger than -1/+1? You first state that there is no linear relationship between  $\delta$  excess and  $\delta$ -values and then you analyse the temporal variability of the linear relationship (i.e., of the slope). Maybe I misunderstand your statistical analysis. Please explain.

I also do not understand what you mean with your last sentence, i.e., that the changes in the relations are not justified by the stratigraphy.

P 18 L 484 ff. You are listing here statistically not significant trends. P-values of 0.75 and 0.87, which strongly exceed your  $\alpha$ -level, mean that there is no real trend in your data. The same is valid for the  $\delta$  excess with p-value 0.2. Hence, stating trends here in  $\text{‰}/\text{year}$  is misleading, as they are most likely to occur by chance, at least this is what the high p-values imply. Just say that no statistically significant trends were found, either in the  $\delta$ -values or in the  $\delta$  excess. The only trend that can be considered statistically significant is the  $\delta$  excess trend in TT01 for 2000-2011 with p-value 0.06. Leave out the numbers for all other "trends".

P 21 L 506ff. The same as before: Do not list slopes as trends which are statistically not significant. Only the  $\delta$  excess composite record shows a statistically significant trend and therefore has to be included with its slope and p-value.

Figure 5: The trend lines in (a) and (b) are misleading since the "trends" are not statistically significant. Remove them. In general, I would recommend to only show the  $\delta$  excess with its statistically significant trend. Please provide either  $r$  or  $R^2$ , otherwise you provide the same information twice.

Figure 6 and Figure 8: Please include the p-values in the figure. Then you can refer to Figures 6 and 8, whenever you have statistically not significant correlations without mentioning all the values in the text.

P 22 L 524: Please rephrase the last part of the sentence. I guess you want to express

that the composite record shows the same relationships as TT01.

P 22 L 530 ff. Again, you provide values for statistically not significant correlations. If correlations with autumn and summer SAM are not statistically significant it also makes no sense to calculate running correlation for these two periods.

P 22 L 537f. This sentence belongs to the discussion.

P 22 L 551 ff. The same as above. The correlations of the d excess with  $WSS_{SIC}$  and  $ABSS_{SIC}$  are not statistically significant. Hence, as there does not exist any relationship between these parameters, it makes no sense to analyse them for their temporal stability.

P 23 L 560: The correlations were verified by what? Something is missing here.

P 23 L 560 ff. The same as above, all presented correlations are not statistically significant. So just state that you did not find any statistically significant correlations instead of listing all the values which are of no importance. Also, if a relationship is statistically not significant, you cannot call it a "weak relation", because there is just no real relationship.

Figure 6: Please explain in the captions what are  $a_{SW}$  and  $a_{HSDW}$ .

P 24 Chapter 3.5 Please round all accumulation rates to a maximum of two decimal places.

Again, you provide numbers for trends that are statistically not significant. Just say that accumulation rates show no statistically significant trends.

P 24 L 580: 2012-2000 period?

P 24 L 584 and Table 5: What is the reason behind choosing these three time periods for calculating accumulation rates?

Time periods in the Table have to be reversed, e.g., 2009-2012 instead of 2012-2009.

P 24 L 590 f.: This belongs to the methods section.

P 24 L 598 f.: You have to keep in mind that the ERA5 data is calculated by a model for certain grid points that are usually at another altitude than your actual study site. What is the altitude of the used ERA5 grid point(s)? Please add the ERA5 grid point(s) used in Figure 1. ERA5 does not capture orographic effects on precipitation very well, hence it is no surprise that trends in ERA5 do not match trends in firn core data. Actually, it is quite surprising that both records match quite well in some periods.

Figure 7: Delete "the" before ERA5.

Was the snow accumulation composite record derived by averaging or linking? This is not clear.

P 25 L 623 to P 26 L 650: These paragraphs belong to the discussion.

P25 L 603f. If the p-value largely exceeds 0.05 then the correlation between ASL and WSS pressures is not statistically significant and hence cannot be further interpreted.

P25 L 607 It is not necessary to provide the correlation values for the SOI index as they are not statistically significant.

P 25 L608 ff. If the running correlations are statistically significant, the p-value range should also be provided.

P25 L 614 Delete the statistical values for the not significant correlation with the SAM.

P 25 L618f. This sentence is misleading. The relationship is not statistically significant, so it is wrong to state that the HSD frequency is related to ASL latitude.

P 25 L 625 ff. This sentence is confusing. Did the accumulation also start in spring and late winter in 2013 and 2015? Please rephrase.

Delete "years" after 2015.

P 26 L 640 ff. Does the value 0.06 m snow height result from ERA5 or in-situ measurements? Did you check how well ERA5 reproduces snowfall and hence accumulation at your site, since ERA5 generally tends to underestimate precipitation amounts, at least in West Antarctica (see Tetzner et al., 2019, doi:10.3390/geosciences9070289). Otherwise, it is a bit difficult to draw reliable conclusions from the differences in ERA5 snow height and snow height ultrasonic sensor measurements.

P 26 L 645f. Rephrase to: "...dominantly from the SE sector and rarely exceed 15.5 m/s at 850 hPa" (round up to a maximum of one decimal place).

P 26 L 648: Rephrase to: "air-mass incursions from the Peninsula region to the east coast of the WSS have intensified ...". Or what do you mean with "air-ass incursions by WSS"?

P 26 L 647ff. Do you have any further evidence, i.e., references for the stated trends in air mass incursions and westerly winds or do you simply conclude this from your own calculations? I think some references are needed here.

## Discussion

In general, the discussion section definitely has to be divided into several sub-chapters in order to better guide the reader through your thoughts. I also would be more careful with absolute statements. Your data suggests certain relationships between isotopes and influencing climate factors, but you cannot be sure by a hundred percent.

P30 L 674-678: In my view, this paragraph belongs to the conclusions.

P 30 L 680-687: In my view, this belongs to the introduction. There you should explain the role of the SAM for Antarctica.

P 30 L695ff.: The same as above. I think the explanation of the ASL should go into the introduction.

P 30 L 698f. But your study site is rather located in East Antarctica or at least at the intersection between Antarctic Peninsula, WAIS and EAIS. How do you know that warm air masses reaching the WAIS and the Antarctic Peninsula during a positive SAM phase and deep ASL also reach your study site? Or maybe I get you wrong here?

You showed earlier that during days with (high) snowfall events, which probably produce your isotopic signal (HSDs – 42% of annual precipitation), a S/SE-wind regime prevails. Also, SWEs which may contribute to your isotopic signal through removal and redistribution of snow are connected to southern winds. Warm air masses advected towards the WAIS and Antarctic Peninsula during a positive SAM phase predominantly come from the west/northwest. So, it seems contradicting to me to connect the negative/positive correlation between ASL pressure/SAM and your isotopes with the warm air advection to the WAIS and Antarctic Peninsula (unless the warm air masses cross the mountain range of the Antarctic Peninsula to reach the Weddell Sea sector). Of course, you have the negative correlation between the isotopes and the ASL pressure, which I guess partly results from the fact that your isotopes are positively correlated with the SAM and that the SAM in turn is negatively correlated with the ASL pressure.

I think backward trajectory modelling would be essential to better understand the relationship between your isotopes and air mass advection from different directions.

P 30 L 700: You should introduce the IPO already in the introduction section.

P 32 L 755ff. The correlations you are discussing here are not statistically significant.

Technical corrections:

P1 L17: Rephrase to: "Therefore we investigated the relationship between isotopic (..) and snow accumulation data [...] and glaciological information, [...]"

P 3 L72: Rephrase to: "[...] that occurs between evaporation from the moisture source and

deposition at a specific site [...]"

P3 L 76: "on local and regional scales"

P3 L82: Use plural here: "... changes in moisture sources..."

P4 L101: "... circulation on large and synoptic scales"

P4 L108: "Antarctic ice-core studies ..."

P4 L114: "... in each Antarctic sector"

P4 L116: Delete "the" before "two high-resolution shallow firn cores"

P4 L117: "...between the West Antarctic Ice Sheet and the East Antarctic Ice Sheet ..."

P4 L 119: Use plural here: "post-depositional processes"

P4 L 125 f.: Delete "high snowfall days" and use only the previously introduced abbreviation.

Rephrase to: "... in order to assess from which sector the storms that reach the study site come." (I guess this is what you wanted to say, didn't you?)

P4 L 128: I think the abbreviations ABSS and WSS have not yet been explained. Explanation in the abstract does not count here.

P5 L 143: "The Criosfera 1 site is located in the MIS basin catchment area ...."

P5 L 145: Delete the hyphen.

Caption Table 1: "... temperature at 10 m depth"

Caption Figure 1: "... glacier outlines at 250 m depth"

P 6 L 171: "The Criosfera 1 site ..."

P 7 L 176: Use plural here: "... cold conditions ... are broken off..."

P 7 L 180f.: "low-scale dune fields of 20-40 m height"

Delete "in wide extension".

P 7 Title chapter 2.2: Firn/ice core collection

P 7 L 191: Delete "at".

P 8 L 224: Add "a" before "class 5 clean room".

P 9 L 245: "...the analytical method used for each core..."

P12 L 343: Insert "it" before "overestimates the temperature and ..."

P13 L 376: Delete the colon after "coastal areas".

Title of 2.8: Replace the first "and" by a comma.

P 15 L 425: "covers 16 years"

P 17 L 449: "... has a noisier signal..."

P 17 L 456: Delete "cores" after CR1.

P 17 L 459: Add a comma before "then".

P 17 L 464: Replace the colon by a comma.

P 18 L 479: Replace "unjustified by" "not justified".

P 21 L 506: Replace "to" by "for".

P 24 L 590: "firn core data" (delete the "s")

P 24 L 590: Add "the" before "composite record".

P 25 L 620: Delete "the" before "high snowfall rate".

P 25 L 631: Rephrase to: "Furthermore, in 2016 there was no additional accumulation towards the end of the year, as most HSDs took place in the middle of the year."

P 26 L 634: Replace "in" by "at the".

P 26 L 644 and L 645: Add "the" before "Criosfera 1 site".

P 26 L 647: "Data on the mean .."

P 30 L 688: Change "trigger" to "triggers".

P 30 L 695: Rephrase to: "...the SAM strongly influences the depth of the ASL..."

P 30 L 700: Rephrase to: "... are linked to the shift of the Interdecadal Pacific Oscillation to the negative phase".

P 31 L 716: Delete "For".

P 33 L 797f.: Add "the" before "EPE signal", delete "the" before 1999. Use "approaching" instead of "has approached".

P 34 L 812: Delete one "also".

P34 L 820: "The stratigraphic analysis..."

P 34 L 834: "... ice lenses were observed..."

P 35 L 841: "Since spatial variations of accumulation ..."

P 34 L 843: Use "are located" instead of "lied".