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The Cryosphere Discussions

Interactive comment on "New insights into the climatic signal from firn cores at the northern Antarctic Peninsula" *by* Francisco Fernandoy et al.

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Major comments

OBS1. Title I found the title misleading since the firn cores temporal extent is too short as to infer climatic signals from them. A suggested title would be: Water stable isotope and deuterium excess records from precipitation and firn cores from northern Antarctic Peninsula as tools for climate studies in the region.

ANS.1. The reviewer has a valid point here, as the word "climatic" could mislead the readers. Instead we propose the following title: New regional insights into the stable water isotope signal at the northern Antarctic Peninsula as tools for climate studies

OBS2. Database I am concerned about the author's use of the Tair and altitudinal T

profiles from BE station. Why the authors would expect this temperature to be representative for the core sites? Later in section 2.3 the authors mention that daily mean temperatures are available at OH, then why not to use that data set instead of the BE-data? Please elaborate and justify the use of the BE data set.

ANS.2. The BE dataset was used instead of OH, since the latest has numerous gaps of validated data (see: https://legacy.bas.ac.uk/met/READER/surface/O_Higgins.All.temperature.html). This is especially important for the most recent years. Some years (e.g.: 2015) even with 50% of the monthly data validated under 80% of daily temperature records. On the other hand, BE has an uninterrupted validated record from 1968 to the present. Both data sets (OH-BE) correlate (for validated months) with a r-value higher than 97% ($p<0.01$). This correlation is even higher than the correlation between the OH Station data and Esperanza (ESP) Station (96%), which is located less than 100 km away. However, ESP is located at the east coast of the Antarctic Peninsula and therefore, is partially influenced by continental conditions. The Fig.1 attached to this comment shows the linear regression and correlation for the OH-BE and OH-ESP datasets.

OBS3. Stable isotope time series analysis In section 2.3, page 5-25, the authors state that the d-excess signal obtained from the firn cores was depicted against depth and filtered using IFFT to be compared with d-excessmeteo, but the authors never showed the original raw core isotope data, nor discussed the quality of it, e.g. amplitude of the signal, seasonality, possible melt, signal differences /similitude at the different core sites, etc. The authors must describe the raw isotope data before applying any further statistical method to compare it with either instrumental or modelled data. Before assessing the quality and representativeness of the raw data it is not possible to carry on with other comparisons. Further, the authors obtained the time scale of core d-excess based on the "the strong similarities between both signals (d-excess and d-excess meteo)"; even thought this method is quite interesting and innovative, and both profiles clearly agree (Fig. 3), the authors must indicate why a more traditional dating was not

preformed, e.g., annual layer counting, which would be the obvious first attempt on dating the cores. The authors must address this issue and justify the dating method used in order to show its value over more traditional approaches. The authors must also indicate the error in the time scale obtained. This is shortly introduced in the text in section 4.2 but it should be mentioned already in section 2.3.

ANS.3. The reviewer is right by addressing this comment, we certainly did not discuss properly the quality either the representativeness of the RAW data. We will add a new paragraph at the beginning of section 2.3, which will include a general description of the RAW data, a discussion about the quality and representativeness and we will also refer to the issues that we had attempting to carry out a traditional dating method. Furthermore, as it is suggested, we will highlight the value of the new method proposed over other more traditional approaches. In relation to the displaying the RAW data, this is available from the assets to this publication: https://doi.pangaea.de/10.1594/PANGAEA.871083 https://doi.pangaea.de/10.1594/PANGAEA.871080 Figure 3 already includes this data, nevertheless, the color that we have chosen was probably not the best one. We will change the color in this figure in order to represent the RAW data in a better way. Since our dating was carried out using a signal-tuning method, the error associated with this method is usually dependent on the error that the reference signal has and also to the differences between the two signals that are compared. In our context, as both signals should be dependent on almost the same variables which have seasonal behaviors, the error associated with the signal matching can be estimated as to be of +/- 1 month.

OBS.4. Seasonality The authors define seasons and sample sets to each season. However, the authors have a limited number of years (2008-2014) as to construct a representative seasonal signal of d18O in precipitation at the study site to use as a baseline to compare with particular monthly means of a given year. The authors use 1 month of a particular year to describe a seasonal signal without discussing its representativeness. Therefore, the results must be explained as results for a particular year

rather than as to a “seasonality”, mentioning the results limitations.

ANS.4. This comment is right in relation to the seasonal long-term approach that we are not able to achieve with our limited dataset. A paragraph will be added in section 4.1 specifying the limitations and representativeness of our dataset. We will highlight the fact that the information presented in this study is intending to give a rough idea about changes identified in the isotopic signal throughout the year in a short time span, rather than creating a regional baseline.

OBS.5. Glaciological setting The authors must provide a more detailed description of the study site, e.g. glaciological setting, meteorology of the study site, earlier studies of in the OH area, if available. This could be included in an additional section as “study site”.

ANS.5.The authors believe that a new section is not absolutely needed. The introduction already describes most available (rather scarce) glaciological information for the study region. Others like meteorological and geographical references are widely discussed along this manuscript and in a previous work of this group (Fernandoy et al., 2012), also published in the journal. Nonetheless, we will add more details to the last part of the introduction section.

Minor comments

1-23. Be more specific when given the results. The results presented here are a snapshot of a region situated at Antarctic Peninsula but they don’t necessarily reflect the whole Antarctic Peninsula situation. Unless a geographical significance study is done, please clearly state to which region of the Antarctic Peninsula are your results representative.

ANS.1-23. This was corrected along the whole manuscript to emphasize that our discussion is valid for the most northern portion of the Antarctic Peninsula, i.e.: Study region close to the Laclavere Plateau and nearby west flank.

2-2. I have seen Antarctic Peninsula being referred as AP or APIS (when talking about Antarctic Peninsula Ice sheet), I wonder why the authors chose API.

ANS.2-2. The abbreviation API was taken for simplification reason from the sample codes analyzed here and used also on our previous publication. In order to keep a consistency with other authors from the region, we changed the abbreviation API to AP for this manuscript.

2-24. at several stations

ANS.2-24. Corrected as suggested

2-27. with a marked warming

ANS.2-27. Corrected as suggested

3-22. the authors only mention the “high temporal resolution” of their records but did not talk about the temporal extent of their records. Since the title of the manuscript involves climate, the temporal extent of the firn cores is as important as the resolution and should be introduced together.

ANS.3-22. Corrected as suggested

4-3. austral summer campaigns

ANS.4-3. Corrected as suggested

4-3: please remove “several shallow–depth firn cores (totalling more than 60 m) were retrieved from the northern part of the API”. There is no need to add this vague information if the authors are going to give more details of the cores in the next sentences.

ANS.4-3. Corrected as suggested

4-4: Add link to Figure 1. Label O’Higgins station as “OH”, also add info on image source and contours details.

ANS.4-4. Corrected as suggested

4-8: Please be more specific about how many samples were discarded and why.

ANS.4-8. Additional information was added to this section. The samples were discarded using a statistical outlier test (modified Thompson tau technique) (Thompson, 1985). Using this criterion all samples with d-excess values lower than -9‰ were discarded, as they lie outside the normal distribution of regular samples.

4-10/13. ...profile of density and physical properties of the ice

ANS.4-10/13. Corrected as suggested

10-13: add info of the drill, handling, storage, and sampling for all cores. This can be summarized in a table.

ANS.4-10/13. This information is included in section 2.1, however additional antecedents on the handling of water samples will be included in the manuscript.

4-16. To which institution/facility in Viña del Mar? Please add information about sample melting and storage during melting (type of vial). As mentioned by the authors in page 20-21, secondary processes during storage and transport (and also melting of the sample) can perturb the isotopic signal.

ANS.4-16. As for the previous observation additional information of the protocol followed both in Chile and Germany will be included in section 2.1. This basically consist in melting the snow and firn samples at controlled conditions (4°C) in sealed bags overnight. At the next morning previous to running the isotope analysis, each sample was agitated for homogenization.

4-18. Indicate where the water stable isotopes were analysed (instrument, method, etc) and the accuracy for all the cores. This is depicted in Table 1 but it is not clearly stated in the text. Cite references to values shown in Table 1.

ANS.4-18. As before, additional information was added and table 1 referenced as suggested.

4-18. Please indicate where the firn and precipitation samples were collected at OH.

ANS.4-18. This information was added to 4-7

OBS. In Fig. 2, show source of the data. Please add the BE station in Figure 1.

ANS. The source of the data (LIMA-Landsat Image Mosaic of Antarctica) was added to the figure. Station BE (Bellingshausen) is located on the South Shetland Island, therefore out of the scope of this image. An insert to this figure will be added.

5-1: Please indicate the arrival point of the HYSPLIT trajectories. Was 1-backtrajectory estimated or a cluster of backtrajectories?

ANS.5-1. This information was added, corresponding to OH coodinates at an isobaric arrival point of 850 hPa (around 1500 m a.s.l.). The backward trajectory analysis corresponds to a cluster analysis and not single trajectories.

5-22. "for the whole region". The author's refer to the whole Antarctic Peninsula? If, yes, the authors might reconsider limit the interpretation to the nearby study area. It is unclear to me if the HYSPLIT backtrajectories were set to end at OH or to other sites in the Peninsula. Please clarify this.

ANS.5-22. This was corrected to express that our discussion correspond to the nearby region of the OH Station and Laclavere Plateau, and doesn′t mean to extend this conclusions to the whole Antarctic Peninsula region, which clearly exceed the scope of this work.

Figure 3: it is not clear to me if cores OH-10, OH-9, and OH-6 were drilled at the exact point, how close were there drilled? This info cannot be inferred from Table 1 which shows the exact coordinates for all three cores. The core sites could be shown as a zoomed-in section in Fig. 1.

ANS. The location was intentionally selected at the same position for the cores OH-6, OH-9 and OH-10, but retrieved in different years. This will be explained on the revised

version of this manuscript.

6-25,26: this should be stated earlier in the text, e.g. in section 2.3 when figure 3 was shown.

ANS.6-25,26. This paragraph was moved as suggested.

6-27: Remove OH

ANS.6-27. Removed as suggested.

6-28: Please mention how many samples were rejected and why. This is mentioned earlier in the text but is not discussed.

ANS.6-28. This information was added to section 2.1. A total of 13 precipitation samples were discarded.

Figure 6: please add labels for the study site, and Bellingshausen Sea in the map.

ANS. Labels were added to the figure

7-6: please mention how the authors defined the seasons, e.g. DJF-! summer. Also, please explain better how you selected the set of samples representative of each season; as it is written in the text, it appears that the set for each season was selected upon the number of samples of arbitrary months which might cause bias, especially in section 3.1.1 where the seasonal regression slopes are discussed. Please consider using all samples available for each season or limit your discussion to the represented months but not to seasonal scales. Please discuss the annual precipitation distribution at the study site if available and put your results in that context. Also discuss how precipitation samples were taken, is a precipitation event identified as one precipitation sample or are the samples taken and identified on a daily (hourly) basis? Indicate in a figure the number of samples per month and also the volume per sample/event.

ANS.7-6. Done as suggested. Regarding the sample selection, we extended the explanation of our selection criteria. Related to precipitation sample acquisition, there was

already an explanation in the text referring to the daily scheme in which precipitations were measured. Figure 7 shows the precipitation sample distribution.

7-9: Please discuss how outliers were removed in section 3.1.1 (when first referred).

ANS.7-9. Done as suggested

7-11: please include the error and significance of the regression slopes.

ANS.7-11. Done as suggested

7-13: the authors must justify why they believe a particular month is representative of a season. If the authors have data for all months, why to assume only one month as representative of a season? This issue has been addressed in a previous comment (7-6). This is very important to clarify as the authors are attempting to link their results to climatic features.

ANS. 7-13. Answered in 7-6

7-16: An inverse behaviour between July and June or between July-June compared to MAM and SON? Please explain.

ANS.7-16. An specification was added to the text

7-22: do not use "weak", use instead: correlation coefficients for these comparisons are not significant (indicate level of confidence).

ANS.7-22. Done as suggested. Also the value of the rejected samples was wrongly stated in this section ("lower -2‰"), as showed before, the statistical outlier test points out to eliminate all samples deviating more than 2σ from the mean value (i.e.: <-9‰ and >12‰.

7:3.1.3 Please indicate geographical sites in a figure (in Fig. 6 for example).

ANS. Done as suggested

Figure 8 can be removed and the equation of the regression line can be given in section

3.1.3.

ANS. Figure 8. The authors consider that this figure is needed to give a better idea of the correlation for different values of d-excess. However, we do recognize that this figure is not key for the paper. We would still propose to keep this figure.

8-5: define GMWL and LMWL in the text (now is only defined in Fig. 9)

ANS. 8-5. This was done as suggested.

8-6: previously you defined the slope as "s", now it is mean slope as "m" but not defined, either use s again or define mean slope as "m".

ANS. 8-5. This was corrected throughout the paper to keep consistency using the "s" for slope.

8-9: Please remove the first sentence as it is unnecessary.

ANS. 8-9. We considered that this sentence works as a short introduction for the following section. We think it should be kept in this sense.

8-10: as mentioned in a previous comment, a discussion about the quality of the isotope raw data must be addressed earlier in the text.

ANS 8-10. We recognized this point and added a discussion on section 2.3

8-26: how the authors could explain the melt layers then if there is no signal of infiltration or connection with summer melt? Could the authors include the percentage of melt per m w.e.?

ANS. 8-26. This issue will be discussed at the end of section 4.1

8-31: higher than the annual, monthly mean?, please specify.

ANS.8-31. We mean monthly means, this was corrected in this section.

9-5: "monthly d18O-T relationship was considered to reflect seasonal behaviour" based

on? Please add a discussion to explain the authors' assumption.

ANS. 9-5. This phrase was eliminated from the text, as it was unnecessary and could lead to confusion of the following text.

5-9: what is the general trend? The data sets are too short as to describe or assume they represent a general trend.

ANS. 9-5. This was revised and corrected in the text

9-15: please indicate the basis to the -1.4C latitudinal correction. Also indicate lapse rates used.

ANS. 9-15. The correction procedure was now added to this section, the basis of doing so is previously exposed on the major comment 2.

9-16: Indicate the significance of the trend, this is important due to the short period covered (only 5 years).

ANS. 9.16. This information was added to the revised version of the text

Figure 10: indicate the resolution of the temp. data.

ANS. Added to the figure "monthly resolution"

9-20: indicate significance of the trend.

ANS. 9-20. Done as suggested

9-25: of which station? Please specify.

ANS. 9-20. OH (BE) station was added to this sentence.

10-30: replace "clear" with significant or not significant.

ANS.10-30. This sentence was modified, as it doesn′t mean to point out an statistical trend, but rather a tendency of increase in density of the firn pack.

10-3.2.3 Is there any evidence of wind redistribution of snow that could be operating at lower elevations? How is the amplitude of the seasonal d18O cycles at different elevations? Is there any sign of melt at lower elevations?

ANS. 10-3.2.3. This matter was not discussed here in extent, since was already exposed by Fernandoy et al. (2012). A noticeable effect of melting is present bellow 700 m a.s.l., that it′s location of OH-4 and bellow. Wind redistribution will play a role in some geographical singularities like depression or valley-like features. All cores were retrieved from geographical height in order to minimize this effect (Fernandoy et al., 2012). Nonetheless, these redistribution effects are much less important against the high accumulation rates for this region.

11-27: compositions

ANS. 11-27. Corrected in the text.

11-14: I would be cautious to extrapolate the results to the whole Peninsula region and rather specify that the result is valid for the study site. Reference to data from additional sites at the Peninsula is needed as to assure what the authors claim. The authors also need to address that the time extent of the cores prevent to robustly interpret their results into a climatic scenario. The results presented in the study are a snapshot representing and must be carefully put into a climatic context in order to avoid speculative interpretations.

ANS. 11.14. This observation was taken into account along with other previous similar observation referring the regional vs. local extension of our results. We will revise the text to make it clear, that our investigation shows the situation for a specific portion of the Antarctic Peninsula (i.e.: northern AP) and for a restricted time frame (2008-2015).

11-18-20: The authors need to show evidence of similar findings elsewhere in the Peninsula as to support their claim, otherwise the claim is highly speculative when extrapolating the study results to the whole Peninsula area.

ANS. 11-18-20. We will restrict our discussion concretely to our study region, since similar study are non-existent or scarce to rest of the Peninsula.

Figure 12: please correct x-axis label to “sea ice extent”. Indicate the SIE data source. Indicate also the definition of the SIE index used.

ANS. 12. The axis of figure was modified accordingly, and the source and definition of SIE was added. SIE data was obtained from the NSIDC Sea ice index (https://nsidc.org/data/seaice_index/) and the definition of the sea ice extent is according to . Defining the edge of the sea ice, as the portion of sea surface covered by at least 15% of ice.

12-8: it is important to address if the trend is significant or not.

ANS. 12-8. As in previous observation, we don′t mean to express a statistical trend of the isotope values, but rather a tendency of depleted values with higher altitudes. This will be corrected on the text.

12-20: “ice layers likely developed by wind ablation on wind–scouring processes at the plateau.” Could the authors explain how wind could create ice layers?

ANS. 12-20. In this line we expressed our self not right. We actually don′t mean to state that a proper ice layer could form purely from wind in a high accumulation region like the AP. We refer here to actual glazed (ice) “crust”, only a few mm wide as shown by the stratigraphy of the firn cores. This wind crust could form a thin glazed surface due to sublimation and snow drift abrasion, and in some opportunities by solidification of super-cooled droplets flowing against ground surface irregularities (sastrugi-like). During our field work, we witness these processes during different years. âĂČ

References

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R.: A Note on Restricted Maximum Likelihood Estimation with an Alternative Outlier Model, Journal of the Royal Statistical Society. Series B (Methodological), 47, 53-55, 1985.

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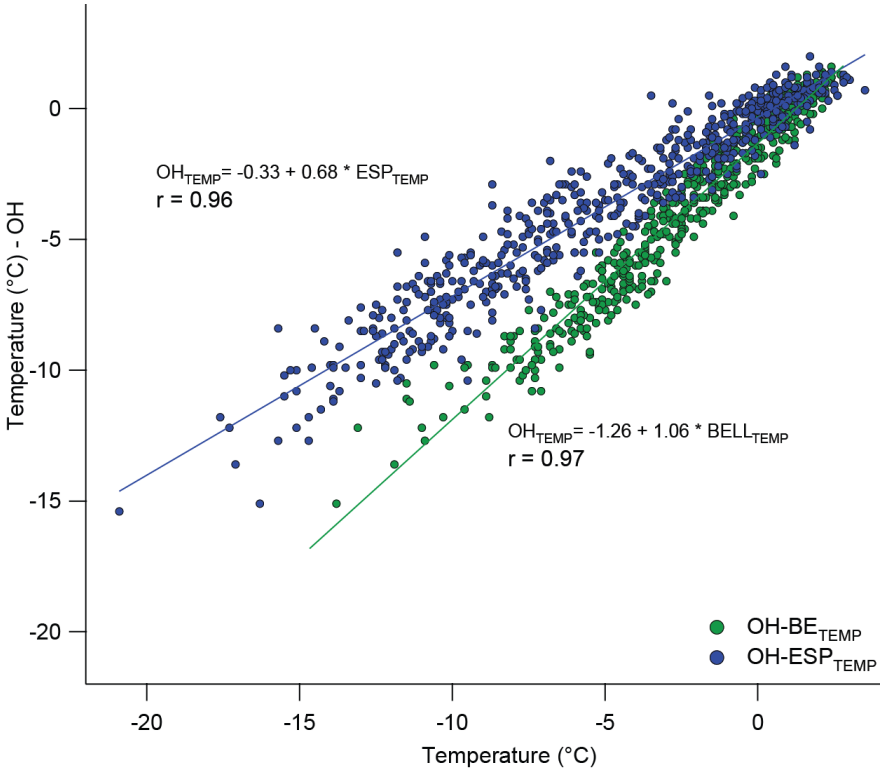

Fig. 1. OH-BE (green dots) and OH-ESP (blue dots) monthly mean air temperature correlation between 1968 and 2015