

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

### **Anonymous Referee #1**

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Review of TCD manuscript: New insights into the climatic signal from firn cores at the northern Antarctic Peninsula, Fernandoy et al. 2017

Notation: Page-line(s)

The paper presents valuable glacio-chemical data for the Antarctic Peninsula. The authors present water stable isotope data sets of precipitation and firn cores collected near O’Higgins station. The authors present an innovative method to obtain the time scale of the the firn cores which, the authors claim, cannot be dated by traditional methods such as annual cycles counting of d18O variations. The authors then discuss the isotope-temperature relationship at different seasons and conclude that an isotope-temperature relationship cannot be valid for all seasons, but rather depends on seasonal variability of oceanic conditions. The paper is well written, and structured.

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The data quality is good and the figures are well executed. However, I consider that the title of the manuscript is misleading because climatic signals per se cannot be obtained from such short cores, but rather the usefulness of such cores can be assessed in terms of possible tools for climate studies, given longer cores are retrieved at the site. I consider that the manuscript can be accepted for publication at The Cryosphere after the authors address the major comments described in this review.

### Major comments

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.

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.

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 though this method is quite interesting and innovative, and both profiles clearly agree

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(Fig. 3), the authors must indicate why a more traditional dating was not performed, 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.

**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.

**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”

#### 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.

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.

2-24: at several stations

2-27: with a marked warming

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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.

4-3: austral summer campaigns

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.

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

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

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

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

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.

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.

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

In Fig. 2, show source of the data.

Please add the BE station in Figure 1.

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

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.

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.

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

6-27: Remove OH

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

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

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

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figure the number of samples per month and also the volume per sample/event.

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

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

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.

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

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

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

Figure 8 can be removed and the equation of the regression line can be given in section 3.1.3.

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

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”.

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

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.

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.?

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

9-5: “monthly d18O-T relationship was considered to reflect seasonal behaviour” based on? Please add a discussion to explain the authors’ assumption.

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

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

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

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

9-20: indicate significance of the trend.

9-25: of which station? Please specify.

10-30: replace “clear” with significant or not significant.

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?

11-27: compositions

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.

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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.

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.

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

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?

I would suggest the d2H notation instead of the dD notation throughout the text to be in line with IUPAC guidelines as much as possible.

16-1: Given the accumulation rates at LCL, the low compaction at the top core meters, and the GPR data (accounting for 16 years in 41 m of snow/firn, section 3.2.4) could it be possible to have a record for the last centuries with a medium-depth ice core retrieved there? Do the authors have any preliminary age-model for the ice cap considering such high accumulations and low compaction rates?

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Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-298, 2017.

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