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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.

Francisco Fernandoy et al.

francisco.fernandoy@unab.cl

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Major comments

OBS.1. There is a need to be more explicit about what datasets were used and exactly how. I would also like to see a better justification of their inclusion over perhaps more suitable datasets. This relates to observations (e.g., why use Bellingshausen station observations over the more proximal O'Higgins?) and the gridded datasets (why use HadISST versus a higher resolution observed SST dataset, and why not use the sea ice data from this same gridded dataset as opposed to using the NSIDC sea ice extent index). Also, how sea ice extent was measured is not explained in the manuscript.

ANS. 1. We added to the manuscript an explanation, emphasizing which datasets were selected and how they were considered in this work. In relation to choos-

ing Bellingshausen (BE) station over O'Higgins (OH) station, 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 figure attached to this comment shows the linear regression and correlation for the OH-BE and OH-ESP datasets. In relation to the gridded datasets, we have corrected the text since we unintentionally made a mistake writing HadISST, instead of HadSST. For Sea Ice extension we did not use HadISST since one of its key limitations is that higher resolution and more homogenous data are available for the modern satellite period, 1979-present. Instead of HadISST, we used NSIDC because it provides a higher resolution grid (25km x 25km). The reviewer is right by noticing that the way in which sea ice extension was not detailed in the text. To measure the sea ice extent we considered as a starting point the location of O'Higgins station and as an end point, the sea ice front in the direction towards King George Island. This procedure will be added to the manuscript.

OBS.2. To estimate dexcess meteo from the air parcel source region, SST seems to be from a fixed region whereas relative humidity was determined based on the HYSPLIT trajectories. Is this correct? And if so, why not just pull SST time series from the same geographic area as the RH reanalysis data? The regressions between observed temperature and isotopes as presented in sections 3.1.1 and 3.1.2 are not clear. Adding these regression scatter plots to Figure 7 would help.

ANS. 2. The SST datasets were obtained from a fixed region based on two main rea-

sons. The first one, because the continuity of the data in this quadrant during the time that we cover is more consistent than the neighbors, which in turn are rather limited and containing important gaps. The second reason is because almost all the air parcels that reached Laclavere Plateau, in the time interval studied, crossed through this region during a significant amount of time (one day or more). Thus, it might has likely exerted an imprint over the moisture parcel crossing this area. In contrast, out of this region, the pathways followed by the air parcels spread into different directions, characterized by data gaps and sometimes inconsistencies. Even though the SST data considered is originated from a single region, considering a rough value of the behavior of SST at this latitude is enough to estimate dexcess meteo. As we show in the text, this parameter is less dependent of SST values (mostly influenced by humidity). Therefore, dexcess meteo is highly dependent of relative humidity. The 3-day backward trajectories provided data along the whole path, those data sets were considered to give representativeness to dexcess meteo estimation. Linear regression were not added to the figure, but were discussed now in the text in section 3.1.1 and 3.2.2.

OBS.3. The regression between firn core derived dexcess and that derived from the gridded datasets (dexcess meteo) seems circular given that the gridded dataset-derived dexcess meteo was used to date the firn cores. There are multiple instances where the correlation between the core dexcess and the dexcess meteo is used to validate various parameters and interpretations of the core (including the dating), and I don't think this is supported because the firn cores were dated by peak matching with the dexcess meteo time series. If I am understanding this correctly, I believe the authors should revise the use of correlations between the two time series to support their analyses. I have documented some of these instances my comments below, but there are several other instances in the discussion that I have not mentioned.

ANS. 3. The reviewer is right by addressing this comment, we committed an unintentional error in the caption of Figure 8. As it was written at the end of in section 3.1.3 "From the relationship between monthly mean values of dexcess from precip-

itation samples and dexcess meteo constructed from meteorological parameters (rh and SST) of the high density precipitation pathways, a correlation coefficient of R= 0.86 ($p<0.01$) was obtained (Fig. 8)", the regression reflects the relation between dexcess from precipitations and dexcess meteo. The relation between both parameters is what we consider that validates the dating procedure, as precipitation dexcess is highly related with the constructed dexcess meteo. We will correct the figure′s caption accordingly.

OBS.4. The lack of melt in the cores is surprising given warm summers in this region and the literature cited in the introduction.

ANS.4. We haven′t neglected the effect of melting, however melt events are rather insignificant against the very high accumulation. During none of the field works (January – February) we have witnessed any major melt event at the highest point of the northern Antarctic Peninsula (i.e.: Laclavere Plateau). The stratigraphic profiles of the firn cores retrieved show different kind of ice layer/crust. We have attributed this to different phenomena like wind sublimation, precipitation of super-cooled humidity and in some cases possible melt layers. Nonetheless, we don′t see any possible seasonality in the distribution of the ice layer and crusts.

Specific comments:

Page 2

Line 4: Bromwich et al 2013 focuses on the central West Antarctic Ice Sheet air temperatures. I would suggest changing this reference to one that focuses specifically on Antarctic Peninsula air temperature trends.

ANS. Changed this citation to Carrasco (2013), and also indicated that this refers to the Antarctic Peninsula specifically.

Line 8: Change to "have recently lost mass"

ANS. Done as suggested

Line 9: Most modeling studies have shown that surface melt, though accelerated in places regionally, plays little direct role in the mass balance today. Only on the northernmost AP does it impact SMB, and only indirectly via ice shelf stability forcing, does it impact mass balance elsewhere – and today this is limited to the AP.

ANS. Added to the text that this refers mostly to AP

Line 12: Change "is losing" to lost

ANS. Done as suggested.

Line 13: Change "surpasses" to surpassed

ANS. Done as suggested

Line 14-15: Change to "This demonstrates how sensitive the coastal region of West Antarctica is to increased. . ."

ANS. Done as suggested

Line 16-17: I believe Trusel et al 2012 and Kuipers Munneke et al 2012 did not report significant positive trends in AP surface melt as whole. However, Abram et al 2013 and Trusel et al 2015 (Nature Geoscience) both note positive surface melt trends on the northeast Antarctic Peninsula from an ice core and from climate models and observations, respectively. I would suggest revising this sentence.

ANS. The lines were rephrased to clarify these points, as the reviewer make a valid point suggesting so.

Line 19: The first part of this sentence needs a citation indicating what studies show more wide-spread surface melt since the mid 20th century.

ANS. This was included and corrected according to the previous observation.

Line 31: Do the authors mean "tropical" ?

ANS. We refer to regular (pre-industrial period) patterns. We will include this informa-

tion to this line.

Line 32-33: The "southern oscillation" is not another term for the SAM, but rather ENSO. Please revise.

ANS. This is correct. Southern Oscillation is misplaced; we will remove this term from cited line. Page 3

Line 5: I would suggest citing Orr et al. 2008 (J. Climate) in reference to summer airflow over the AP owing to westerly wind increases.

ANS. Two cites were included to this line showing the impact of summer airflow: (Orr et al., 2008; van Lipzig et al., 2008)

Line 15: Please change to "hampers accurately determining"

ANS. Done as suggested

Line 16: Please change to "Therefore, climate models are necessary to extend the scarce climate data both spatially and temporally."

ANS. Done as suggested

Page 4

Line 8: Could you please expand upon what you mean by "improper storing"?

ANS. This was also brought up to our attention by referee 1 and clarified in the text

Line 28: Please indicate in the text how far Bellingshausen station is from the firn core sites and O'Higgins. Also, why were observations from O'Higgins not used?

ANS. Same as before. Referee 1 asked about this in his comments. We responded showing that OH data has long non-validated data periods. On the other hand, BE has an uninterrupted record since 1968. Although BE is located around 150 km NE from OH station, the correlation between both records (for valid period) is higher than 0.97 with a high statistical significance ($p<0.01$), which is higher than other station like

Esperanza (ESP) (r= 0.96) located less than 100km from OH, but on the east side of the AP. Linear regressions show that OH and BE data has a slope very close to 1, while OH-ESP is lower (s = 0.33) (with lower r). This probably reflects some influence from continental conditions. Therefore, we used BE to complement the OH data. Figure attached here.

Line 31-32: HadISST is actually on a 1 grid. Did you use a different version of the data product? Also, why was HadISST chosen over a more strictly observational SST dataset (e.g., AVHRR, AMSR-E) or the 0.25 NOAA OISST v2 product? Given the cores only go back to 2008, I would think that using observations would be the best route. The use of HadISST (and the actual resolution used) should be further justified.

ANS. This was explained before. Please see answer to Major comment 1.

Page 5

Line 7: Did you use 1 day back trajectories or 3 day? If only 1 day as specified here, why on line 1 do you state 3 day? The methods here are a bit unclear. For example, did you calculate the RH only across the areas with >50pct parcel frequency (or some other threshold)? Also, could you reference Figure 6 here?

ANS. We first used 3 day backward trajectories to figure out the provenance and distribution of air parcels that reached the study site. As we noticed that there was a high density pathway in the region, we explored the conditions that prevail in the near surroundings (limited by 1 day backward trajectories). After analyzing both datasets, we determined that the area covered by 2 day backward trajectories had a high representativeness of the maritime region that surrounds Laclavere Plateau. This area is representative because geographically includes the region affected by westerly winds and sea ice front during winter time, both factors that exerts high influence on the air parcels that approach this region. Line 14-16: It is unclear what sea ice metric was used to define sea ice extent "around the API". Was total Antarctic sea ice extent provided by the NSIDC Sea Ice Index used? If so, that dataset is certainly not suitable

for the more regional/local analysis of this manuscript. This also raises the question of why not use the sea ice concentration data that are also part of the SST dataset (whether that is HadISST, if justified, or one of the higher resolution datasets)?

Line 18-20: This information is repetitive with the previous paragraph.

ANS. Modified in the text

Line 24: Please change "obtained" to "derived" or "estimated"

ANS. Done as suggested

Page 6

Line 6: Change "has been proved" to "has been proven" or similar. Section 2.4 more broadly: Was a constant wave velocity chosen to convert two way time to depth? Certainly the firn here is quite heterogeneous given high surface melt rates. Also, was the surface actively melting during the January fieldwork?

ANS. Proved was replaced. Regarding section 2.4: Radar results were revised and re-analyzed. We now used a 2D model velocity based on the density profile obtained from the local cores for the upper snowpack and a density model for deeper snow/firn. During January, we were in the field for about 10 days. We experienced days of strong snow precipitation, wind drift and also sunny days, but we didn't see evidences of surface melting in this short period.

Page 7

Line 6: Change "seasons"' to "season's"

ANS. Done as suggested

Line 10-11: Considering the "considerable differences" between daily and monthly mean isotopic values, could you please show standard deviation error bars on your monthly mean time series in figure 7?

ANS. Explained on major OBS. 2 this discussion was addressed in the text in section 3.1.1 Line 11-16: The regression analysis presented here is quite unclear. Is the regression slope derived from 3 points each for MAM and SON? Or, are you regressing daily values? Please consider revising the text here and adding a figure to show these regressions. This would be very helpful. Also, why are only fall and spring values being regressed (or were the other seasons regressed individually, but the results were insignificant?)? Please expand on this here.

ANS. The regression slopes presented were derived from d18O daily values from precipitation samples and mean air temperatures from those days. We will add a paragraph in the manuscript which will include the slope from other seasons and the standard error and significance of each slope. Additionally, Figure 7 will be modified to include an histogram to represent how precipitation samples are distributed in time , seasonal regressions will be discussed in the text in section 3.1.1. and 3.2.2.

Page 8

Line 2: This correlation testing seems circular to me. The d excess (meteo) was used to date the ice cores by aligning the ice core d excess vs depth profiles with the d excess (meteo) vs time. So, we should clearly expect a high degree of correlation to result since these time series are already manually aligned

ANS. As explained on comment 3 this is not what we intended. Please see explanation above.

Line 8: This paragraph seems better suited for the methods section.

ANS. We consider that this section should be kept here, it is through that some part of the methodology is revised here, but still showing results of combined geophysics and stable water isotope information.

Line 9: Please change to “allowed us to derive”

ANS. Done as suggested

Line 13: Again, the methods need to be more clear about the time frame analyzed using the back trajectories. Here it is stated 2 days, but elsewhere it says 3 days and 1 day.

ANS. The answer related to this subject is written in the comment of Page 5 Line 7
Page 9

Lines 1-3 / Figure 10: The stated relationships in the text are quite difficult to see on the plot. Could you plot this instead as a scatter plot, or perhaps highlight these areas on the existing line plot? Only January is labeled on the plot, with one other tick at July (?), so it’s hard to understand. Please consider revising Figure 10 to improve clarity.

ANS. The figure was modified showing now the period of positive/inverse relationship and the time (x) axis better labeled.

Line 5-7: The methods used here for extending or contracting the relationship is unclear. Please revise.

ANS. This paragraph intends to refer to the fact that at this latitudes, calendar seasons do not play a significant role as climatic seasonality extends beyond calendar time limits. Rather than using calendar seasons we turned to define three seasons: winter (JJAS), summer (DJF) and a transitional season (MAM-ON). There paragraph will be corrected in the text in order to better address this issue.

Line 15: Methods for determining a latitude temperature correction unclear. Please clarify.

ANS. Done, new information is in the text now.

Line 16-17: Figure 12b does not show the mean annual air temperature or a negative trend over time. Please revise figure citation or consider adding this information in a figure.

ANS. Figure citation was revised as suggested.

Line 17-20: Methods for determining temperature using the meteorological observations and sea ice extent (how was this measured?) is unclear. Please revise and consider adding a figure showing these monthly correlations.

ANS. To clarify this observation, we redefined our equation to take into account the seasonal behavior in these region. We separated our equation in two branches which are dependent on the presence of sea ice. In moths with presence of sea ice in this region (May-June-July-August-September) the temperature was obtained by Tlcl= (Tbe-1.4) + 1,13(Mmonth*SIE+Nmonth), where Mmonth and Nmonth are the slope and interception of the linear relation between lapse rates and SIE presented in Figure 12. On the other hand, during months without the presence of sea ice, the temperature over the Plateau was obtained by the expression Tlcl= (Tbe-1.4) + 1,13(H(x)), where H(x) is the monthly mean lapse rate value of a given month "x" measured in Bellingshausen Station between 1978-1996. This paragraph will be corrected in the manuscript

Line 28: Please show the linear regression showing the -0.008 per mil slope on Figure 13a as opposed to the linked dots. Please also show for Figure 13b.

ANS. Figures were revised and changed as suggested.

Page 10

Line 6: Figure 14a only shows accumulation through 2014.

ANS. The most recent core analyzed goes to January 2015, therefore is not possible to have the accumulation for 2015. New cores from 2016 are yet to be analyzed will give more information about accumulation.

Page 11

Line 11-13: Again, these datasets were aligned, so the correlation reporting is circular.

ANS. As explained on comment 3, please see explanation. After taking into account our correction, the text makes sense since both parameters are independent.

Line 15-16: This could be interesting – where did the anomalous humidity air parcel originate according to the back trajectory analysis?

ANS. These trajectories are coming mostly form eastern coast of the Antarctic Peninsula and the continent, therefore reflecting depleted isotope values.

Page 12

Line 2: I would suggest revising the use of "natural" here when referring to SAM, given the anthropogenic forcing on SAM (ozone and GHG), which is appropriately acknowledged earlier in the manuscript.

ANS. To avoid confusion the word "natural" was eliminated and the phrase changed to "seasonal oscillations"

Line 17-19: I think this should be stated in reverse – that the isotopic composition is not altered by surface melt infiltration and percolation.

ANS. Corrected as suggested.

Line 20-21: I find the lack of ice layers in the firn cores due to surface melt refreezing to be unexpected. The mean monthly temperature at O'Higgins is often at or above 0°C during summer months. Do the lower elevation cores (300-600 m elevation) not have significant melt? And are smaller ice layers not present at the plateau cores? Even at -4-7°C mean monthly temperature (assumed using lapse rate for the 1100m plateau), I would expect melt each year. These layers may not coincide with summer seasons based on your age-depth scales, which is common due to melt percolation into deeper layers.

ANS. Certainly lower areas show clear sign of melting, this was widely discussed on our previous work by Fernandoy et al. (2012). We identified that melting is strong even, for the high accumulation of this region, at altitudes lower than 700 m a.s.l. As addressed before, during all of our field season (all of them between January and February), we haven't witnessed important melting events. Although we have seen the freezing of

super-cooled droplets in the irregular surface (sastrugi-like) and glazed surface due to ablation by strong wind, accompanied by snow drift.

Page 13

Line 24: Should this say “altitude”?

ANS. We actually do mean latitude, referring to west flank from the AP divisory from Laclavere.

Page 15

Line 1: Should this say “manually”?

ANS. To avoid confusion we will delete the word mainly.

Line 8: Should this say between 2008 and 2014? I don’t believe any data before 2008 are presented.

ANS. Typeset error. Thanks.

Line 9: Change to “proxies”.

ANS. Done as suggested

Page 16

Line 3: I look forward to seeing longer records from this area!

ANS. We expect to continue the work in this area, further campaigns are planned for 2017/18. Figures

Figure 1 It would be helpful to have an inset of the Antarcitc Peninsula (or perhaps even just the northern Antarctic Peninsula). In particular, having Bellingshausen station located on this map would be helpful given that some of the meteorological data analyzed are from this site. Also, a box showing the area where SST data were extracted would be helpful.

ANS. A second panel was added to this figure to show the location of both Station and LCL. Figure 7

The small dots plotted differ from the legends and caption. Notably the “small orange dots” in (b) appear the same as the small dots in (a). Please revise this figure (also see comment from page 7 about adding error bars).

ANS. Revised. The figure was improved accordingly.

References

Carrasco, J. F.: Decadal Changes in the Near-Surface Air Temperature in the Western Side of the Antarctic Peninsula, Atmospheric and Climate Sciences, 03, 7, doi: 10.4236/acs.2013.33029, 2013. Fernandoy, F., Meyer, H., and Tonelli, M.: Stable water isotopes of precipitation and firn cores from the northern Antarctic Peninsula region as a proxy for climate reconstruction, The Cryosphere, 6, 313-330, doi: 10.5194/tc-6-313-2012, 2012. Orr, A., Marshall, G. J., Hunt, J. C. R., Sommeria, J., Wang, C.-G., van Lipzig, N. P. M., Cresswell, D., and King, J. C.: Characteristics of Summer Airflow over the Antarctic Peninsula in Response to Recent Strengthening of Westerly Circumpolar Winds, Journal of the Atmospheric Sciences, 65, 1396-1413, 10.1175/2007JAS2498.1, 2008. van Lipzig, N. P. M., Marshall, G. J., Orr, A., and King, J. C.: The Relationship between the Southern Hemisphere Annular Mode and Antarctic Peninsula Summer Temperatures: Analysis of a High-Resolution Model Climatology, Journal of Climate, 21, 1649-1668, 10.1175/2007JCLI1695.1, 2008.

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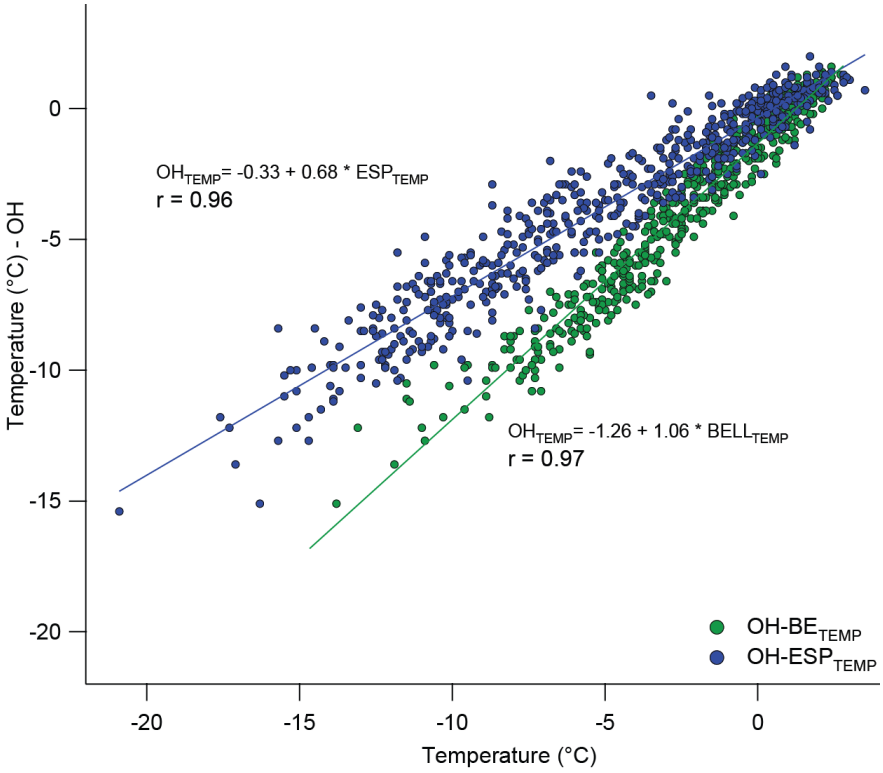

Fig. 1. OH-BE (green dots) and OH-ESP (blue dots) monthly mean air temperature correlation between 1968 and 2015