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

Anonymous Referee #2

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Review Summary:

This manuscript examines the relationships between stable water isotopes (D and 18O) measured in northern Antarctic Peninsula firn cores and precipitation samples and regional climate variables (SST, sea ice extent, air temperature, relative humidity) to evaluate the nature of climatic information captured in firn and snow samples from this region. Sea ice extent is found to impact local temperature and atmospheric variability, whereby extensive sea ice under negative phases of SAM cause surface inversion and a skewing of the isotope-temperature relationship in some seasons. Transitional seasons (spring and autumn) lack this oceanic forcing and the resulting stable boundary layer conditions permit deriving temperature from isotopes. Though the cores presented in this manuscript only extend back to 2008, the analysis could prove useful for interpreting longer ice cores extracted from this region.

This is an important paper because it directly evaluates the isotopic record with respect to observed atmospheric and oceanic parameters. As the observational record from this region (and Antarctica more broadly) is quite limited, analyses such as this one represent a necessary foundation before interpretation of longer firn and ice core records can be attempted. Though the work presented here will certainly be of scientific interest and the discussion and conclusion sections are interesting, I find there to be some gaps between these sections and the methods and analysis performed and presented. Overall, I believe the authors could better present the methods employed in the text and figures to better support the conclusions. Below, I have included broad and specific comments to this regard.

General comments:

The authors present an interesting method to date their firn cores based on measured firn-core dexcess and "dexcess meteo" estimated from probabilistic air parcel origins (from HYSPLIT) and associated gridded SST and relative humidity datasets. This seems to work out nicely, but I do have some questions about the data and methods (see below and see specific comments).

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.

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 tem-

perature 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.

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.

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

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.

Line 8: Change to "have recently lost mass"

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.

Line 12: Change "is losing" to lost

Line 13: Change "surpasses" to surpassed

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

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.

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.

Line 31: Do the authors mean "tropical" ?

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

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.

Line 15: Please change to "hampers accurately determining"

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

Page 4

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

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?

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.

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?

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.

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

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?

Page 7

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

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?

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.

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.

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

Line 9: Please change to "allowed us to derive"

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.

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.

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

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

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.

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.

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.

Page 10

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

Page 11

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

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

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.

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

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.

Page 13

Line 24: Should this say "altitude"?

Page 15

Line 1: Should this say "manually"?

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

Line 9: Change to "proxies".

Page 16

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

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.

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).

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