Comments on *New regional insights into the stable water isotope signal at the northern Antarctic Peninsula as tools for climate studies,* Fernandoy et al.

The manuscript presents valuable glacio-chemical data for the northern area of the Antarctic Peninsula. The authors present water stable isotope data sets of precipitation and firn cores collected near O'Higgins station. The authors use an innovative method to obtain the time scale of the firn cores which, the authors claim, cannot be dated by traditional methods such as annual cycles counting in the d18O profile. 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.

This is the second time revising this manuscript. Many of the comments in the first revision have been addressed and the manuscript has improved accordingly. However, I still find relevant points that were not addressed by the authors in the final corrected version of the manuscript. These should be addressed before the paper is considered for publication.

General comments

1. In section 3.1, please indicate how the precipitation samples were collected, type of sample collector, sample bottles, handling and storage, etc.
2. RC. In section 3.2: please indicate the basis to the -1.4C latitudinal correction. It is not clear for me how the authors got the -1.4 C factor, this should be clear in the manuscript. The authors added a figure in the first response letter, but it is still not clear to me how they obtained the -1.4 factor.
3. When calculating the R values, the authors indicate alpha, however, they do not indicate if the R values were corrected considering the degrees of freedom of the system. That should be included.
4. The authors write: "The latter indication is well supported by the high accumulation rate in the region that does not allow a prolonged exposition of the freshly fallen snow to the atmosphere. Furthermore, the absence of significant infiltration and percolation associated with melting and refreezing events and the lack of a relationship between ice layers and seasons as well as with the stable water isotope record implies that the isotopic composition is not altered by surface melt infiltration and percolation". The authors should show the melt percentage per depth, ice layer distribution and thickness in a plot. If they have the data, I do not see the problem on doing it. Including these data in the manuscript will improve and give support to this part of the discussion. As it is, I am still not convinced by the authors' claims, basically because they do not show the data. If the melt layers are "few" why not to simply indicate how many per mwe the cores have? And then calculate melt percentage. In the previous revision of this manuscript, I've asked: "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.?" The authors response was: 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. However, in the authors response + corrections uploaded on 2017-09-11, I still see the phrase: “Thus, this reassures that post–depositional processes in the LCL region are negligible in the time period analyzed and that ice layers likely developed by wind ablation on wind–scouring processes at the plateau.” Therefore, the authors did not include the clarification in the paper.

5. (In 10. 31.) Please define what are “elevated values”. A value higher than the mean could be simply a number 1*10^-8 above the mean, but that would not make sense to interpret as a result of a geophysical process, at least not in the manuscript context. Please define higher (or lower) values in terms of how many standard deviations the value is above or below the mean. Please revise the discussion from section 4.2.2 on, considering the above.

Minor comments

6. 30-34. Please indicate in the text how you have assessed that there is a significant difference in the Sdev of the cores.

7.20-23. Please re-write this sentence: “By studying the progression of these variations, the frequency of the second mode showed the highest frequency in the second interval, where the Sdev reaches an equilibrium. Thus, the final signal is only defined by a set of low frequencies.”, e.g. By studying the progression of these variations, the frequency of the second mode showed the highest value in the second interval, where the Sdev stabilizes?. Thus, the final signal is only defined by a set of low frequencies.

In table 2 please write the mean, max and min values according to the sdev significant digits. Please revise this along the text and tables.

8. 11. The discarding of outliers should be described here, not in the following section (4.1.2.)

8. 24. Please indicate the standard error in the slope of the linear regression when using the MAM and SON datasets.

9. 8. “and show variability”. This sound ambiguous, please elaborate on this.

10. 26-29. If the authors have the number and thickness of ice layers, and layer densities, please include the percentage of melt per m w.e. Without this calculation, this

paragraph does not support itself. This was asked in the previous revision and the authors response was that they will include this discussion, however they didn't calculate melt percentage, nor justified why they didn´t do it. In the previous revision, I asked the authors: "how the authors could explain the melt layers then if there is no signal of infiltration or connection with summer melt?" this has not been answered yet. Consequently, the phrase "(10. 26-27) The melt layers do not show evidence of infiltration" remains obscure. This must be clarified.

12. 29. Please indicate if the trend is significant and at what confidence level.
13. 10. "During MAM a clear decrease of δ18O with height (-2.4‰ km-1 from sea level at OH up to LCL) is found, whereas during JJA no decreasing δ18O trend is obtained from sea level to 1130 m a.s.l. (Fig. 13b).". Please define the significance of the trend, otherwise, the sentence is ambiguous.

14. 14. Here I am confused because the editor had already asked to correct the terms "inverse and direct relationships", however, I still find them in the text, despite the fact the authors claim, in the response to the editor, to have revised them.

2. 3. Warming of
2. 12. At an estimated rate of

2.13. Please indicate the rate of positive mass balance in East Antarctica

4.6-7. Please cite the ice-core studies that are available.

4.16. Eastern side

4.30. The authors mention "improper storing" without describing what is proper storing. See also the general comments.

5.4. Please indicate the temperature at which the cores were kept in the commercial freezer.

In Fig. 2, please indicate the resolution of the meteorological data.

8. 6. In the last →in the previous

8. 10. Was constructed → was obtained

10. 17. "an even higher"→a higher

10. 20. "more fresh and less compacted". This is ambiguous. If the authors have layer density, please indicate it, e.g. consist of snow layers with density between xx and xx, and firn with densities between xx and xx.