The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-302-RC1, 2020

The Cryosphere Discussions
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EGU

Interactive comment on "Continuous monitoring of surface water vapour isotopic compositions at Neumayer Station III, East Antarctica" *by* Saeid Bagheri Dastgerdi et al.

Anonymous Referee #1

Received and published: 17 November 2020

This manuscript presents the first continuous monitoring of surface vapour isotopic composition at Neumayer over two years. It is the first time that such a record covering two whole years is obtained in Antarctica which makes this study of interest for understanding the isotopic composition of water in Antarctica. The author completed this analysis by an analysis of the moisture source for the Neumayer III over the year 2017 using the Flexpart back-trajectory approach.

Despite the interest of such study and of the new and original dataset, this manuscript still needs to be strongly improved to be acceptable for publication.

Major comments:

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- The data are presented but the calibration system is not well described and it is questionable to know if it is adapted to the Neumayer conditions. It would be nice to display the calibration curves obtained to correct for the instrumental drift (down to 100 ppm according to the text) as well as the influence of humidity on water isotopic ratio in the humidity range studied here. A section dedicated to calibration should be added in the supplementary material.

- It is not clear why the diurnal cycles are not shown in this study while they are expected to bring interesting information, especially when comparing to other sites as performed in sections 4.4.1 and 4.4.2. It is thus important that the diurnal cycles (at least in summer) are shown and thoroughly discussed. Otherwise, the comparison performed on section 4.4 on the correlation of the isotopic signal does not make sense since they are done on different timescales.

- The slope of the relationship between q(picarro) and q(meteorology) is very high (1.5). This is really surprising. It would be better to have a direct calibration of the humidity of the picarro (in laboratory using a dew point generator for example).

- Some d-excess peaks look strange and are not discussed (e.g. negative peak in August or September 2018). Also the authors should explain why some periods are lacking in the record.

- I do not see the interest of looking at the d18O vs temperature, then d18O vs ln(q) and then temperature vs ln(q). . .. The link between temperature and ln(q) can be mentioned in one sentence early in the manuscript stating that the Clausius-Clapeyron relationship is dominating the humidity signal at Neumayer and then remove Figure 5 and Figure 9.

- The appendix with the d-excess correlations is not useful – you could simply detail what is statistically robust or not in the correlation in section 3.2.3.

- I do not understand the sentence on l. 285 -> do you have any measurements (isotopes, temperature, humidity . . .) over some characteristic events showing that d18O

values drop intensively in a distance of 16 km between open sea and the station ? If not, such affirmation should be removed.

- I am not sure that the Figure 11 corresponds very well to the text. The association between wind from the East and higher d18O only works for 22 out of 36 days (61% - not sure to understand where does the 64% value comes from) which is not a proportion strong enough to drive such conclusion on Figure 11. The signal is increased when considering only extremely warm and cold days

- It is not clear which types of events the authors want to discuss in section 4.2 for the key controls on vapour d-excess. Indeed, synoptic events lasting several days and associated with a particular atmospheric pattern will not necessarily be detected by the proposed algorithm: if several following days have a particularly high d-excess values then the average value over 10 days may also be high and the period will not be identified. Moreover, the d-excess pattern found with this definition are not very clear and no clear explanation of the pattern is provided (observed correspondence apply to 70% or less of observed cases). This section is thus not very strong.

- The section 4.3 should be placed earlier (not in the discussion section)

- Section 4.4 cannot stand like this without showing also daily variability at Neumayer

- The section 4.5 aims at discussing the air-snow interaction but the fresh snow is sampled only after major snowfall events which is very different to the other study discussed here (Steen-Larsen et al., 2014) in which snow was sampled at high resolution in presence or absence of precipitation. It is thus impossible to discuss the link between air and snow in the case presented here when surface snow is only sampled after large snowfall events. In this case surface snow isotopic composition reflects the isotopic composition of the precipitation.

Summarizing, the whole discussion section is rather weak and additionnal work should be done on it to make a better use of the new data set.

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