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Climate of the Past Discussions

Interactive comment on "Deglaciation records of ¹⁷O-excess in East Antarctica: reliable reconstruction of oceanic relative humidity from coastal sites" *by* R. Winkler et al.

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Overall Comments:

This paper reports the variability of 17O-excess during the last deglaciation at two East Antarctic ice core sites; EPICA Dome C (EDC) and TALDICE (TD), and compares these new measurements to a previous record from Vostok.

At EPICA Dome C, an increase in 17O-excess during the deglaciation is of smaller magnitude, but consistent with the trend observed at Vostok. There is, however, no such increase at TD between the last glacial maximum (LGM; 21 ka) and the early Holocene (EH; 8 ka). Using a Lagrangian Rayleigh-type model and back-trajectory

analyses, the authors suggest that a different moisture source for TD (and hence a different sea surface relative humidity) may be a controlling factor in 17O-excess. While the reasons for the spatial variability in 17O-excess (e.g. different source regions, trajectories, water vapour recycling, the superstauration dependency on temperature, clear sky precipitation and stratospheric effects) are preliminary at this stage, this nonetheless opens up some interesting future research questions and scope for additional ice core intercomparisons using 17O-excess.

Overall, this paper is an important contribution to the developing literature concerning the interpretation (and reproducibility) of 17O-excess measurements in polar environments, and is one of the few analyses of the spatial variability in 17O-excess. It will also be of considerable interest to researchers working with more conventional (d18O and dD) data due to the clear description of considerations associated with applying isotopic models to polar regions. For these reasons, I believe that this paper should be published, but requires clarification in places, particularly with respect to tuning the MCIM for 17O-excess.

Specific Comments:

1. Fractionation Factors

p. 1849: the choice of fractionation factors needs some additional explanation. For d2H and d18O, equilibrium fractionation factors from Majoube (1971) and Merlivat and Nief (1967), are reported for temperatures below 0 degC, but presumably the model is initiated at source regions with higher temperatures. Did the authors consider using published fractionation factors for >0 degC? Also, Barkan and Luz's study for 17O is for the temperature range 11.4 - 41.5 degC. Some comment on why it is valid to extend this to polar temperatures would be valuable.

2. Methods

p. 1852: The experimental methods section needs some brief introductory material

e.g. “In order to measure both d17O and d18O in ice core samples from EDC and TD. . .”

3. Inter comparison

Figure 1: It would also be good to see the mean values from Landais et al. also plotted in the figure for comparison. Also, is there any reason for the very large difference in the sample at ∼325 m? Was this re-measured, or could other samples be measured at around this depth?

4. MCIM Tuning (and Figure 3)

p.1855, lines 22-25: It is not clear in this section why the authors adjusted the tuning parameters to obtain the best fit along the transect, along with tuning to reproduce values at the three ice core sites. Should the model not simply be tuned for the ice core sites?

p. 1856, line 23: What is the rationale for using 1-year (2005) calculations for moisture source identification as opposed to the methodology originally applied by Sodemann and Stohl (2009)?

Also, Figure 3 is introduced in the text (out of sequence) after Figure 4. It would be helpful to the reader to be referred to Figure 3 alongside the discussion of tuning parameters on p. 1855.

Furthermore, it is difficult to understand from Figure 3 how the model was tuned to fit mean values at the ice core sites. For TD, in particular, mean 17O-excess is a poor fit to any of the supersaturation functions. Also, for d-excess it appears that S=1-0.004*T_c results in the best fit?

Figure 3 would be better broken into parts a and b for clarity (particularly for the figure caption, which contains quite a lot of explanation of symbols etc).

4. MCIM Results

p. 1859, lines 1-3: This last sentence needs clarification.

p. 1859, line 16 onwards:

Tuning the proportion of re-evaporation appears to produce a better fit for Vostok and Dome C 17O-excess data, but a poorer fit for transect data? Also, this is somewhat in conflict with the explanation that water vapour recycling is a contributing factor to the differences observed at Vostok and EDC (p. 1862, line 20).

Line 21: It is difficult to understand here how this test confirms that 17O-excess reflects the signature from low latitude evaporative source regions.

Line 25 onwards: The comments regarding TD are not clear in this paragraph. It may help to state the temperature change represented by the range of d18O values reported, so that the covariation of 17O-excess with Tsite can be compared to Vostok and EDC.

Technical Corrections:

p. 1846, l. 22: suggested change: “. . .have permitted the reconstruction of. . .”

p. 1847, l. 1: “polar regions of final precipitation” needs rewording e.g. “final precipitation site” or “final polar precipitation site.”

p. 1847, l. 8: change “d-excess with” to “d-excess on”

p. 1848, l. 10: suggested change: “. . .provide continuous and high quality information. . .”

p. 1856, l. 5-7: “. . .and applied to Antarctic ice cores. . .”

p. 1856, l. 25: “The annual mean moisture source longitude. . . ”

p. 1857, l. 9: “. . .Institute. . . ”

p. 1866, l. 6: “Southern Ocean ”