

## ***Interactive comment on “Deglaciation records of <sup>17</sup>O-excess in East Antarctica: reliable reconstruction of oceanic relative humidity from coastal sites” by R. Winkler et al.***

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This paper presents the second-ever-published data set on <sup>17</sup>O<sub>excess</sub> changes from glacial to interglacial. For that alone it is worth publishing. The finding that there is very little change in <sup>17</sup>O<sub>excess</sub> at Talos Dome, a larger change at Dome C, and an even larger change at Vostok is novel and somewhat unexpected. The explanation of this result is in many ways preliminary, and must ultimately be addressed with GCM studies, but this will take some time. For now, the approach used by Winkler et al. – a combination of simpler isotope transport models and back-trajectory analysis – is very reasonable, and leads to some interesting insights. In particular, the finding that

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it is possible to match the results with rather small differences in the relative humidity is good evidence that the  $17\text{O}$  parameter will indeed live up to its promise of better constraining the entire isotope system. The suggestion that the Vostok results – which orientally were assumed to be regionally representative – may be comprised by local effects is very important (though I do not find the argument that the stratosphere can have a significant influence convincing).

I do have a few technical and presentation concerns.

It is clearly shown both here and in previous work that it is the normalized relative humidity that matters, yet throughout most of the paper the simple relative humidity is referred to. I think that this is confusing. For example, it is concluded that "RH of the OSR for TD remained almost constant." Does this refer to RH, or to RH<sub>n</sub>? I think that in general, it must refer to RH<sub>n</sub>, but this is not clear. My suggestion would be that throughout the manuscript, RH<sub>n</sub> be used.

There is not yet an accepted international standard for  $17\text{O}$  excess, other than SMOW. As the authors point out, this needs to be addressed, and a manuscript is evidently in preparation on this. However, a bit more needs to be said in the current manuscript. In particular, reference is made to normalizing the results to those of Barkan and Luz, but is not clear how this normalization is done. Barkan and Luz report, for example, a value of -55.11 for the  $d18\text{O}$  of SLAP, but the accepted value is -55.5. Which values was used? And what was assumed for  $d17\text{O}$  of SLAP? It is also not clear whether the data have been normalized to the SLAP/VSMOW scale, or were simply relative to VSMOW. If the latter was done, this should be stated. If the former, what was assumed about the  $d17\text{O}$  of SLAP (and what is the resulting  $17\text{O}$  excess of SLAP)?

In previous work, 'per meg' is used for  $17\text{O}$  excess, but here, ppm is used. Unless there is a good reason for it, per meg should be used.

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Interactive comment on Clim. Past Discuss., 7, 1845, 2011.

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