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Climate of the Past
Discussions

# *Interactive comment on* "Water stable isotopes spatio-temporal variability in Antarctica in 1960–2013: observations and simulations from the ECHAM5-wiso atmospheric general circulation model" *by* Sentia Goursaud et al.

**Anonymous Referee #2**

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This paper presents updated data and new model simulations for Antarctic isotope distributions. This is a worthy contribution, and appropriate for publication in Climate of the Past.

I do have a several criticisms and technical points.

**Much of the conclusions in the paper are not new, and this should be made clear. For example, in the abstract, it is stated that "local spatial or seasonal slopes are not a correct surrogate for inter-annual temporal slopes". This is a very old result, and



doesn't belong in the abstract. Similarly, the phasing between deuterium excess and d18O has been examined thoroughly in previous work. What is new here? Throughout the paper, I would like to see better delineation between new results and reiteration of old results.

**Too little reference is given to primary results, and it is difficult to determine what data are actually being used. Reference is given to the Stenni et al compilation of ice core data, but it is not clear which of the many records in that data set are actually used. For example, there are multiple cores from Dronning Maud Land and in the vicinity of Byrd Station and the West Antarctic ice sheet divide, but only a few locations are shown on the map? What are the primary data sources? Which of the cores (by latitutde/longitude) are included here? Which are excluded, and why?

**For the temperature data, it is stated that the READER data are used, and that "AWS" data are used for Byrd and Dome C. For Byrd, the best data are the updated record for Byrd, from Bromwich et al,. 2013. This should be used! It is not clear whether it is, nor not.

**There is reference to diffusion, but it's importance is not taken into account.

"ECHAM5-wiso underestimates the seasonal amplitude (by 14 to 69%) when compared to precipitation data, but overestimates the seasonal amplitude when compared to ice core data (from 11 to 71% )." and "At Dome C, ECHAM5-wiso underestimates the standard deviation of temperature, but strongly overestimates the standard deviation of $\delta$18O."

Can diffusion explain these differences? It would be quite straightforward to evaluate whether this is likely. I suspect such difference can be explained entirely by diffusion, as has been pointed out in numerous previous papers. Also, it should be explained here whether EMCHAM-5 does a better or a worse job that ECHAM-4 or other models (GISS, for example). In other words, does ECHAM5 represent an improvement here, or not?

**It is stated that there is an abrupt warming from 1978 to 1979, *possibly* caused by a discontinuity in the European Reanalyses (ERA) linked to the assimilation of remote sensing data starting in 1979..." This is not just "possible" – it's certain. It is very well established that the ERA-interim data are essentially useless prior to 1979. I don't think including the few data-model comparison prior to that time period is useful.

**References in general are inadequate. For example, it is noted that "recent studies cast doubt on this assumption [that isotope can be interpreted as precipitation-weighted deposition signal" and a few recent papers are cited (Ritter et al., 2016;Casado et al., 2016;Touzeau et al., 2016). Those citations are good, but the original idea goes back at least to Waddington (2002: doi 10.3189/172756402781817004), and a number of papers by Steen-Larsen and others have also discussed this, some years prior to 2016. Such papers should be included. Another example is that reference is made to the impact of sea ice on isotopes, but only the very recent paper by Holloway et al 2016 is cited. This idea goes back to at least 1983 (e.g. Bromwich and Weaver, doi:10.1038/301145a0. Another key citation is Noone et al., 2004: doi: 10.1029/2003JD004228.

**There is much discussion about the phase lag between deuterium excess and d18O, but no discussion of why this is important. As the authors will be aware, Pfahl and Sodemann [2014] have suggested a completely different idea (with respect to humidity) about this than the conventional one (about the delay between SST and air temperature). I realize that the present paper is not claiming to solve this puzzle, but some reference to the scientific context would greatly improve the paper.

** The figures could use improvement. Especially, there should be a variable name and units on the color bars of the various maps. For example, Figure 10 should read "lag, in months", on the color axis.

**The calculation of statistical significance, throughout the paper, is not clear. Is autocorrelation accounted for in stating that $p<0.05$?



**Several recent papers have demonstrated that the logarithmic form of deuterium excess is a much more reliable and robust measure than the traditional linear calculation. The paper really ought to look at this as well. See Markle and others (doi: 10.1038/ngeo2848) and Uemera et al., 2012 (doi: 10.5194/cp-8-1109-2012), Dutsch et al. 2017, 10.1002/2017JD027085

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