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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Precipitation regime and stable oxygen isotopes at Dome C, East Antarctica – a comparison of two extreme years 2009 and 2010" *by* E. Schlosser et al.

Anonymous Referee #1

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Review of 'Precipitation regime and stable oxygen isotopes at Dome C, East Antarctica: A comparison of two extreme years 2009 and 2010' by Schlosser et al.

This is a nice paper which contributes to the understanding of the observed stable water isotope ratios in precipitation at Dome C. The analysis is conducted for only two years but those years sizeable differences in metrological parameters at the Dome. This has allowed a detailed investigation of the del differences between the two years, and have revealed subtleties which might have otherwise gone unnoticed.

A key aspect of the paper is that synoptic behavior is analysed to inform the particular processes which, on short time scales, govern the del signals. The authors make clear

the importance and relevance of understanding the isotopic chemistry for single events, and allow them to unravel the complexity of fractionation history on way to Dome C.

I would like to see the authors revise, in some modest but important ways. These are itemised below.

p 30475, l 9 In this relevant broad overview of the continental mass balance recent analysis of Harig, C., and F. J. Simons, 2015: Accelerated West Antarctic ice mass loss continues to outpace East Antarctic gains. Earth and Planetary Science Letters, 415, 134-141, doi: 10.1016/j.epsl.2015.01.029 should be cited.

p 30475, l 16 To be completely unambiguous as to what Dansgaard showed, should change 'linear relationship' to 'linear spatial relationship'. Important to differentiate this from the temporal relationship discussed below.

p 30476, l. 25 Include here also reference to Noone et al., 1998: Implications for the interpretation of ice-core isotope data from analysis of modelled Antarctic precipitation. Ann. Glaciol., 27, 398-402.

p 30476, l. 25 On modeling approach add Noone and co-authors, 2002: 'Associations between d18O of water and climate parameters in a simulation of atmospheric circulation for 1979-95'. J. Clim., 15, 3150-3169.

p 30482, l. 21-22 Please present citations in order of year of publication.

p 30483, l. 14-15 Please reword this. At its simplest the 'coreless winter' is associated with the balance of the net OLR and the atmospheric energy transports into the Antarctic region. This balance is reached quite quickly once the Sun has disappeared.

p 30483, l. 21 Negative sign missing here. That is, '54.9C' should be '- 54.9C'

p 30484, l. 1 Would be clearer to replace 'barely exceed -70C' with 'are rarely lower than -70C'.

p 30485, l. 1 'available' better word than 'given'

p 30485, l. 5 and on to next page This section on the synoptics would warrant mention of Warm Conveyor Belts and what their potential role might be. Catto, J. L., E. Madonna, et al, 2015: Global relationship between fronts and warm conveyor belts and the impact on extreme precipitation. J. of Clim., 8411-8429 (his Fig. 6) shows a case of a WCB originating just to the south of Australia and terminating in the Dome C region. Whether or not a WCB is involved in a specific precipitation event will greatly influence the del O18 at the deposition site (via depletion during ascent). A few words should be devoted to this important aspect here.

p 30486, l. 19 Make clear how many iterations were performed with Mark Stoelinga's scheme at each time step. Comment on the convergence.

p 30488, l. 14 'Marshall' (and in caption of Fig. 8)

p 30488, l. 24 and on top of next page When seen in the broader perspective the difference between the winter and spring SAMs in these two consecutive years is not particularly great. For example, the change in spring SAM from 2001 to 2002 (see, e.g., recent analysis of Simmonds, 2015 - Comparing and contrasting the behaviour of Arctic and Antarctic sea ice over the 35-year period 1979-2013. Ann. Glaciol., 56(69), 18-28) was much greater. I suggest this be mentioned here, and that the direct links between the SAM and the ridges in a given sector need not be a straightforward as the authors appear to be suggesting.

p 30489, l. 21-25 I like this part of the paper dealing with k=3. However, the index defined by Raphael is based on points fixed in space. Hence it is not able to fully capture (or can misrepresent) phase shifts in the zonal direction. I don't see this as a great problem here, but it is important to mention there are other approaches which are not phase-locked, such as that of Irving ett al, 2015: A novel approach to diagnosing Southern Hemisphere planetary wave activity and its influence on regional climate variability. Jour. Clim., 28, 9041-9057.

p 30490, l. 21-25 I am not sure what 'globally averaged' means here.

p 30491, l. 1 (Discussion and conclusion) This section presents a nice closing discussion and conclusions. However, I would like to see the authors emphasise a little more the importance of the synoptics, and how they directly influence temperature and moisture flow. That is, that there is, at best, a tenuous DIRECT physical link between temperature and depletion. The point is essentially made in the Abstract but should be reinforced here.

p 30495, l. 5-6 EPICA community members, 2004: Eight glacial cycles from an Antarctic ice core. Nature, 429, 623-628, doi: 10.1038/nature02599.

p 30499, l. 7-9 The web address give here points to the EARLIER version of Mark Stoelinga's software package (namely version 4). The appropriate citation for Version 4.5 is Stoelinga, M. T., 2009: A Users' Guide to RIP Version 4.5: A Program for Visualizing Mesoscale Model Output. University of Washington. http://www2.mmm.ucar.edu/wrf/users/docs/ripug.htm.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 30473, 2015.