Comments to Reviewer #1

We thank the reviewer for the thorough review and the constructive criticism. In the following responses we address each point. The corrected parts of the manuscript are included in blue.

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.

Done

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.

We agree and have changed this.

water are used primarily. A linear spatial relationship has been found between mean annual stable isotope ratios in Antarctic precipitation and annual mean air temperature

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

We added this reference.

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.

We added this reference.

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

Corrected

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.

We rephrased this and also added Schwerdtfeger and King and Turner as references.

The mean annual cycle exhibits the typical coreless winter (van Loon, 1967) with a distinct temperature maximum in summer (December/January), which has no counterpart in winter, where the months May to August show relatively similar values. This is due to a combination of the local surface radiation balance and warm air intrusions. During the first part of the polar night, with the lack of short-wave radiation, anequilibrium of downwelling and upwelling longwave radiation is reached; advection of relatively warm air from lower latitudes further reduces the possibility for cooling. Thus the temperature does not decrease significantly after May (King and Turner, 1997; Schwerdtfeger 1984).

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

Done

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

Done

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

Done

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.

Since WCB are usually a phenomenon related to frontal systems and the precipitation at Dome C mostly stems from (non-frontal) orographic lifting of moist air masses, we don't think we can imply that WCB processes are present here. We do appreciate the advice and will keep this in mind for further studies. It is very interesting, actually, if there is a

combination of the two effects at play sometimes, but a real analysis of this is beyond the scope of the present study, which mainly deals with the differences of the atmospheric conditions in 2009 and 2010. Adiscussion of WCBs in this extreme high-latitude setting would have a rather speculative character. We would like to investigate this properly before publishing anything about it. Note that a study about all precipitation events at Dome C during the measurement period is in preparation. (We suspect that WCBs could be more important for the other deep drilling site Kohnen, which is closer to the coast and sometimes influenced by frontal systems.)

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

We have added this information in the text. Actually, in the first submitted version, we had used the formulation "simple trajectory model", which made the editor doubt that it was fully three-dimensional, so we changed that. It still is simple, in that it does not define a threshold for convergence. However, given all the uncertainties in trajectory calculations, it seemed to be exact enough for practical use for our purposes. We added some information about the estimate of the moisture source, according to Reviewer #3's comments. This is really just an estimate, not an exact determination, and we also never use trajectories alone, without cross-checking with the general atmospheric flow.

The time step we used was 600s. For simplicity's sake, RIP does not define a threshold for convergence, but simply does two iterations for each time step, which turned out to be exact enough in the praxis for our purposes.

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

Done

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.

We thank the reviewer for this input, and we reformulated this paragraph. The Simmonds Annals paper is very interesting, but in this case we would still like to refer to the original work by G. Marshall.

The differences between 2009 and 2010 are not extraordinarily high compared to other years (e.g. 2001/2002 as seen at http://www.nerc-bas.ac.uk/public/icd/gjma/newsam.spr.pdf), however, qualitatively they are in agreement with the observed flow pattern. Furthermore, it should be kept in mind that SAM explains only about one third of the atmospheric variability in the Southern Hemisphere (Marshall, 2007) and that the SAM index alone gives no information about the location of respective ridges and troughs in a highly meridional flow pattern..

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.

Irving et al. (2015) was not published at the time our paper was written. We have added this reference now.

We agree with the reviewer that the ZW3 index used here does not fully capture the shift in phase of the wave. Raphael (2004) recognised this and did note that the net effect is a small reduction in the amplitude of the wave but the sign of the index is not influenced. We added this in the text, too.

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

We removed the "globally averaged". That was just a thought while writing and came in by mistake, it actually referred to the mean value of d=10, which we did not mention)

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.

We have rewritten this.

Looking towards future work, the results here indicate that a combination of process studies using recent data and modelling of the atmospheric flow conditions on larger time scales will lead to a better quantitative interpretation of ice core data. Apart from the factors influencing precipitation itself, it has become clear recently that post-depositional processes between snowfall events are more important than previously thought because additionally to processes within the snowpack the interaction between the uppermost parts of the snowpack and the atmosphere is very intense (Steen-Larsen et al., 2013). Parallel measurements of stable isotope ratios of water vapour and surface now, combined with meteorological data will give more insight into these processes in Antarctica.

Altogether, this means that the relationship between air temperature and stable isotopes of Antarctic precipitation/ice is anything else but straightforward, since the isotope ratio measured in an ice core (or in the snow) is the result of a complex precipitation history that is strongly influenced by the synoptics and general atmospheric flow conditions, followed by post-depositional processes. Without thorough knowledge of all the processes involved a quantitatively correct derivation of paleo temperatures from ice core stable water isotopes is thus not possible.

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.

We have added the doi.

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.

We have updated this, thanks.