Comments to Reviewer #2

We thank the reviewer for the constructive comments that have helped to improve the manuscript. In the following, our comments are written in *italic.*

Manuscript # 2015_734 entitled 'Precipitation regime and stable oxygen isotopes at Dome C, East Antarctica
– a comparison of two extreme years 2009 and 2010'.

This aim of this paper is to compare the precipitation at Dome C in East Antarctica for two years, 2009 and 2010. The authors present a clear and concise analysis of the circulation differences and their relationship with observed precipitation.

Comments

1. Abstract: Please define SAM and ZW3.

Done

2. Page 30477, line 14. Please define w.e.

Done

3. Page 30478, line 2. What is the word 'synoptics' referring to in this section?

We changed this to "large-scale circulation patterns".

4. Page 30478, line 22-24. Could the authors explain in more detail the sentence that precipitation is 'formed close to the upper boundary of the temperature inversion layer assuming that the largest moisture amounts are found where the air temperature is highest'? Which inversion layer is this referring to and why is the air temperature high there?

We added information in the text that this is a very simplified assumption widely used in ice core studies.

assuming that the largest moisture amounts are found where the air temperature is highest (Jouzel and Merlivat, 1984). This is a very simplified view that is, however, widely used in ice core studies. It assumes that there are basically no multiple temperature inversions and that humidity is only dependent on temperature through the Clausius-Clapeyron equation, which describes the temperature dependence of vapour pressure. This would mean that humidity and temperature inversions would always have a similar profile. However, more recent studies have shown that humidity inversions are parallel to the temperature inversion only in 50% of the cases, and often multiple humidity (and temperature) inversions occur

5. Page 30479, line 12. Does 'accumulation' refer to precipitation accumulation?

We changed this to "precipitation".

6. Page 30479, line 21. Are the authors claiming that it is not plausible to get high moisture levels above 500hPa? Frontal systems can transport moisture up to the tropopause in the warm conveyor belt airflow. This seems inconsistent with the author's statement.

Wwe are not claiming this. However, precipitation at Dome C is usually not connected to frontal systems whereas WCBs mostly are. Our statement referred to the low humidity values calculated along the 300hPa trajectories. That does not mean that no substantial amount of moisture is found above 500hPa, but we assume that the flow at 500hPa is representative for a thicker layer. We added this in the text.

7. Page 30483, line 10. What do the authors mean by 'mean annual course'?

Mean annual cycle. We have changed this.

8. Page 30484, line 10. Could the authors expand their reasons for thinking that there is an error in the observation of diamond dust for this event? Is this a systematic error in the observations or a single occurrence? If it is a systematic bias in the observations, would this affect the results?

We have added some information in the text to explain better. We do not think this is a systematic bias in the observation since only a few comparable events occurred during the observation period.

A very high value precipitation value of 1.36 mm on 9 February 2010, followed by 0.67 mm on 10 February, both classified as diamond dust from the photographic crystal analysis, stems from only one event around 9 February. These values should be considered with care given the high error possibilities of the measurements. Considering the extremely low density of diamond dust, a diamond dust amount of more than 1mm/day seems to be unlikely. However, the model data do show a precipitation event connected to warm air advection from the north (see below) for this day, which would indicate the occurrence of snowfall rather than diamond dust. Most likely a mixture of crystal types was found during this event with the diamond dust on top of the snow crystals, which possibly led to the classification of the event as diamond dust. (Note that the crystal classification was carried out purely from photographs by an expert at the Avalanche Institute in Italy and that snow crystals are also comparatively small at the temperatures prevailing at Dome C).

9. Page 30485, line 17. A forecast bust of 25% seems to be very large. Are these missing precipitation events typically with low precipitation rates, just above the threshold, or are more substantial precipitation events missing?

It should be kept in mind that "substantial" precipitation still means daily sums of usually clearly less than 1mm. It cannot be expected that the model represents cases of increased diamond dust precipitation since diamond dust is not parameterized in the model. But, comparison of AMPS data with AWS data in Dronning Maud Land, including the deep drilling site Kohnen (EPICA DML) yielded a very good agreement between AMPS and AWS data for precipitation events, which were usually characterized by an increase in temperature and wind speed, sometimes also a decrease in pressure.

10. Page 30486, line 25. How was the 'main moisture source' defined and how were the source regions estimated? Were the trajectories moisture weighted or are they simply the location of the back trajectories 5-days previously?

The trajectories were not moisture weighted. As we have tried to clarify now in the text, we did not take the location of the back-trajectories 5-days previously as the moisture source. This would definitely be too inaccurate and simple, even in the context of our simplified estimate. We also never considered the trajectories alone (as is often found in the literature, particularly for HYSPLIT users), but we have tried to understand the dynamics of the actual synoptic situation and find a plausible likely moisture source area by combination of back-trajectory and synoptics analysis info.

We added information about this in the manuscript.

Note, that the moisture source is not defined as the location of the trajectory five days previous to the precipitation. Instead, for this estimate, the combined information of the trajectories and the 500hPa geopotential height fields is used. Different from the approach of Sodemann and Stohl (2009) and Sodemann et al. (2008), who calculated 20-day back-trajectories, for a 5-day trajectory it is possible to comprehend the dynamics of the synoptic situation that causes the precipitation. That way the trajectory results can be cross-checked with the geopotential height fields. Even though the trajectory not explicitely deals with moistuer, it gives information about the origin of the moist air mass. The northernmost "point" of the trough that causes the northerly flow to Dome C is supposed to be the northern limit of the potential moisture source since no substantial meridional flow is observed north of this limit. (The 500hPa trajectory seems to have some inconsistencies (e.g. kinks) on the 5th day, which should not be over-interpreted). Whereas it is not possible to exactly determine the moisture source (under the simplifying assumption of a single moisture source) with this simple method, the information is sufficient to distinguish between a source in the Southern Ocean and one at middle latitudes, which is most important for ice core interpretation and for simple isotope modeling.

11. Page 30486, line 28. Why do the authors assume that the northernmost point of the trough corresponds to the northern limit of the potential moisture source? Does this assumption rely on steady state conditions, i.e. that streamlines and trajectories are equal?

No, it does not. We have changed the text, also referring to the comments of Rev. #1, to make it clear that we cannot pinpoint the moisture source with the described method. It is only a coarse estimate. However, we think that no matter if conditions are steady state or not, if the main westerlies have been situated south of a certain latitude for several days before the precipitation, neither the streamlines nor the trajectories hint at a moisture source distinctively north of that westerly band.

12. Page 30487, line 1. What do the authors mean by 'inconsistencies'?

Mainly kinks. We have added this in the text (see above).

13. Page 30487, line 11. Please could the authors clarify what they are referring to by the meandering branch and zonal branch of the main flow?

Yes; we have added an explanation in the texts.

It was also found to be typical for precipitation events at Dome C that the main westerly flow is split into a northern branch that remains zonal, whereas the southern branch starts meandering with a strong meriodional component.

14. Page 30487, lines 15-20. The authors present an account of a forecast bust but it is not clear how representative this is of the other missing precipitation events. Please could the authors expand on this section, to put the case-study analysis into context?

We would have to elaborate on this in a separate study that would inlude the entire data set.

15. Page 30488, line 15. Should 'am SAM index' be 'a SAM index'?

Well, this depends on the pronounciation of SAM. We usually pronounce it "es-a-em", which would make it an SAM index. However, we notice that many people pronounce it like the male name "Sam", so we use the indefinite arcticle "a".

16. Section 5.22 and 5.23. The authors link the precipitation differences to modes of variability for the two years in the study. It is not clear why this is done. Are the authors suggesting that predictability of large-scale modes of variability could inform seasonal prediction of Arctic precipitation, or are they going to use these relationships to look at multi-year analysis or changes in a warming climate, or some other reason? How do the differences in SAM and ZW3 compare to other years?

We are simply saying that the precipitation and temperature measured for these extreme periods may be linked to the flow associated with the atmospheric circulation defined by these two modes of variability. Given that the precipitation dataset is only eight or nine years long at the time of writing we could only speculate about prediction/predictability but the potential may be there. Note that in 2006, 2007 and 2008, the index of ZW3 during the period of interest was more variable in sign.

17. Page 30491, line 19. 'locatation' should be 'location'.

Corrected

18. Figure 5. The contour labels on figure 5a are too small to read.

We fully agree. This is due to the old landscape format of ACPD. In the final paper the figure will have the width of a full column and thus be well readable. Unfortunately, this is the case for several of the figures. ACPD layout has been changed for papers submitted after Dec 2015, and the authors now have more influence on the size of the figures by creating the pdf themselves. Thus, this problem should not appear in the future.

19. Page 30493 lines 16-18. The future work section here is very general and brief. It would be nice to discuss in more detail where this analysis could lead.

Fair point. We have added some more information.

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.