Comments to Reviewer #3

We thank Harald Sodemann for his thorough review that helped to make necessary clarifications.
Our response to comments will be written in *italic,* the corrected text in blue.

General remark:

Our study focuses on the differences in the meteorological conditions at Dome C in 2009 and 2010 and the atmospheric flow patterns that explain those differences. This paper is neither meant to be about stable isotopes nor about moisture source analysis, the method of deriving former air temperatures from ice core stable water isotopes is only the motivation for our study. Thus we do not focus or want to be too elaborate about those topics, which will be dealt with in future papers. We rewrote the introduction to clarify this. Because of that rewrite, a number of the review comments would no longer entail particular necessary changes in the manuscript. Also our moisture source estimate is only a coarse estimate, but there are significant differences in the mean moisture sources in those two years, which can clearly be distinguished even with this coarse method.We have tried to clarify this, too.

This study focusses on the differences in the precipitation regime of two contrasting years within the short measuring period, motivated by the consequences different precipitation/flow regimes have on stable isotope interpretation. The stable istopes themselves are only discussed as additional information about the local conditions in the respective years and will be topic of a different study.

Review of "Precipitation regime and stable oxygen isotopes at Dome C, East Antarctica - a comparison of two extreme years 2009 and 2010" submitted to ACP by Schlosser et al.

This paper presents a comparison of two years of field data from a weather station and snow samples at Dome C. The causes for pronounced differences in winter temperatures between the two years are interpreted in the context of meridional vs zonal transport processes. While I think the paper is in general interesting and suitable for publication in ACP, I point out below several issues that require attention. I hope the authors may find these comments helpful for their revisions.

Comments to comments:

Major comments:

1. Literature. The manuscript currently neglects the state of knowledge on moisture transport to Antarctica and the relation between moisture transport and isotopic fractionation from the published literature. The study by Sodemann et al., 2008 in JGR, which provides a detailed analysis on how moisture source conditions, temperature difference, and temperature regime influence stable isotope fractionation during atmospheric moisture transport, should be cited in the introduction. Furthermore, the study by Sodemann and Stohl (2009) in GRL is a widely cited study of the moisture sources of Antarctica that should be taken into account in this manuscript. Further earlier work includes that of Helsen et al. (2007), and several other studies.

We added references, so the interested reader can study this.
In addition, we would like to refer again to our general comment. We want to focus on the different atmospheric conditions in the two years that are being compared and do not want to distract the reader by going into the details about moisture source diagnostics and stable isotopes.

2. Moisture source analysis. The moisture source identification applied here is very simple (and does not take into account the state of the literature, as mentioned in #1). As I understand from the manuscript, the end points of 5-day trajectories are considered as moisture sources, which is far less than the 15 days recommended by Sodemann and Stohl (2009). Longer trajectory calculation requires statistical approaches to identifying a source or origin location. As only three trajectories at different levels are considered, the result is quite subjective and uncertain.

We have rewritten the description of our moisture source estimate and hope it is clearer now for the readers.We do not define the end points of the 5-day trajectories as moisture source. We have added more information and our reasons for using this type of trajectory calculation in the manuscript.

We would like to avoid embarking on a discussion about the relative value of different methods of trajectory calculations/moisture source diagnostics in the present paper since that would strongly distract the reader from the main topic. 5-day-backward trajectories have been used by various authors (e.g. Reijmer, Scarchilli, Suzuki) to investigate moisture sources for Antarctic drilling sites. There is no "correct" number of days necessary to determine the moisture source for back-trajectory calculation.This is not possible, when only the air mass is followed. Sometimes even a shorter trajectory gives a clearer picture, e.g. at coastal stations where a 5-day backward trajectory calculation might yield a combination of katabatic flow and flow related to fast moving cyclones (Schlosser et al. , 2004).We added information about this in the paper and hope we have made it clear now how our estimate, not determination of the moisture source was done.

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. The resolution of the input data corresponds to the resolution of AMPS/WRF during the respective time period. The data are linearly interpolated in time and space. Taking into account the large uncertainties in trajectory calculations, for this case a main moisture source at approximately 40 °S was estimated. 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.

The authors cite another manuscript in preparation which contains the study of more events, but as a reviewer it is not possible to evaluate what is done in that other paper. This section needs to be heavily reworked or even dropped altogether.

The reviewers of the new paper will be able to evaluate what is done in that paper.
We do not think that it is essential to assess this in the present paper.

3. Dependence/relation to manuscripts in preparation. The isotope data set used in the study is to be described in a companion paper, which is however only in preparation at this point. This is a potentially serious issue. What if that companion paper never gets published? As a consequence, the data section must contain enough information on the isotope data set to stand on its own.

We would like to refer to our general comment again. As clarified, this paper is not about stable isotopes, they only serve as motivation for our study. Whereas for mass balance studies it is not so important where the moisture comes from (the total precipitation amount counts) for isotope studies it is of high importance. The information about the isotopes given in the present study does not mean to

be complete. And we would like to note that several isotope papers are to be published about Dome C in any case.
The data section gives information about the sampling procedure and stable isotope analysis, and this is sufficient here. Our reference to this paper is a consequence of the data restriction policy, which we are dealing with.

4. Stable isotope results. The paper is intended and starts out with the role of atmospheric conditions for stable isotope fractionation, but in the end it is only one short section of the results that presents the data from a two-year period. The analysis is restricted to the correspondence of low/high values for the warm and cold year. It is not clear what to take away from this analysis other than the very obvious finding that fractionation is stronger under colder conditions. For example, further quantitative investigation of the stable isotope data for this period could strengthen the analysis. I suggest to merge Fig.10 and Fig. 3 (remove panels b and c) and present the findings on temperature and stable isotope differences right away, before going into further analysis of the circulation differences for the two years.

We would like to refer to our general comment here. We have re-written the introduction making clear that the paper does no taim to focus on stable isotopes.

5. Presentation quality. Several of the figures have a visual appearance that could be improved.

Yes, we fully agree that some of the figures do not look great the way they were published in the landscape format by ACPD. However, in the final version in ACP the figures will have the full width of one or two columns and will indeed be greatly imporved. We are glad that the format of the discussion papers will be different from 2016 on.

Detailed comments:

P. 30474, L24: "The most important positive": are there other positive components to the mass balance?

Yes, there are: deposition (= negative sublimation), wind redistribution and freezing at the bottom of ice shelves.

p. 30475, L29: "The amount of this fractionation…" citing Sodemann et al., 2008 at the end of this sentence would fit.

Since this is not a result of Sodemann et al.'s study we prefer to cite J. Jouzel (2003; 2014) here, who was one of the early pioneers in ice core studies. We believe that the older references should be preferred in such cases to give credit to the original research.

The amount of this fractionation depends on the difference between the condensation temperature close to the initial moisture source and that at the final deposition site (Jouzel et al., 2003; 2014).

Could rephrase to "initial condensation" because condensation not necessarily starts at the moisture source. "at the final deposition site": fractionation is related to the final condensation temperature, which may be different than the site temperature due to a surface inversion.

We agree and have changed this. We gave this information for the readers of ACP since we don't believe that all of them are familiar with stable isotopes. However, for more detailed information we have to refer to the literature, since, as we mentioned above, this paper is not a stable isotope paper.

The amount of this fractionation depends on the difference between the condensation temperature close to the initial moisture source and that at the final deposition site (Jouzel et al., 2003; 2014).

Very few references in general in this paragraph.

OK. We have added some. Basically, this is all summarized in Jouzel (2014).

p. 30476, L1: "winter/glacial": I understand the general intention of such a parallel interpretation, but it would be good to substantiate this more, e.g. by an appropriate reference.

This is basic textbook knowledge meanwhile. Unfortunately we did not find this explicitely formulated in J. Jouzel's chapter, but we would be grateful for a reference here.

p. 30476, L5: "This spatially derived linear…": transition from previous paragraph not clear. Consider citing Sime et al. here.

We inserted the "spatial" at the beginning of the previous paragraph to make this clear. It is not clear to us, though, why Sime et al. should be quoted her as this was found much earlier than the Sime paper. We would like to refer to our earlier comment about original work here. Sime's findings will be very interesting for a paper that focusses on the stable isotopes.

This spatially derived linear relationship has been found not to hold temporally,

p. 30476, L7: consider adding Sodemann et al (2008) here which show the importance of these factors in relation to one another. Sodemann and Stohl (2009) provide a detailed moisture source analysis for all of Antarctica and several ice core sites which adresses these issues. Also consider citing the study of Wang et al (2013) for Dome A.

Again, we would like to refer to our earlier comment about original work here.

p. 30477, L1: Dome Fuji had a similar sampling programme for one year, published by Fujita and Abe (2006). So the Dome C series can not be the first one? I don't think it is important to make the claim here, the data are anyhow worthwhile publishing. There is also a huge body of work done on firn sampling, which gives a spatial but not a temporal picture - may be worth mentioning here.

At Dome F, the measurements cover only 1 year. We only state that Dome C has the first multi-year series of such measurements. Firn sampling is very different, we are talking about precipitation here.

p. 30478, L11: Should add citation of Gorodetskaja et al. (2014) here.

We quoted this paper in the discussion. However, we have added Gorodetskaya et al. (2013) here, which seemed more suitable at this point and that we had missed when writing the paper.

p. 30478, L18: Should add citation of Sodemann and Stohl (2009) here.

We could not find anything about orographic lifting of air masses and corresponding precipitation formation in this paper.

p. 30478, L25: Explain more what you mean by "humidity inversions".

We have added some information here.

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 (Nygard et al., 2013).

p. 30479, L1: Connection of this paragraph to the previous not clear.

This is the beginning of a new section (3.2.), and thus it is intended to break from the preceding one.

p. 30479, L15: This section is missing some important references and discussion. In particular, it is important to distinguish between backward trajectories which by themselves do not allow to infer

moisture sources or origin, but rather airmass origin, and methods to identify moisture origin from trajectories which consider for example specific humidity changes along trajectories and their vertical position. Please include a discussion of these aspects and cite the work by Sodemann et al. (2008) and Sodemann and Stohl (2009).

We agree, but would like to refer to our earlier comments here. As we have tried to convey by revising the paper, this study is not about moisture source diagnostics, and we do not want to distract from the main topic.

In particular, the study of Sodemann and Stohl showed that moisture sources are further south than anticipated from previous studies, and cluster near the SH storm track.

This is in good agreement with the results of Reijmer et al. (2002), who used 5-day back trajectories.

These authors also report an spatial gradient of moisture origin from coast to inland, placing the deep-drilling sites in a different regime than coastal sites.

Sodemann and Stohl determined mean moisture sources, whereas we are doing case studies, which changes the results considerably. Also, the deep drilling sites may not have all similar moisture sources, e.g. conditions at Kohnen and Dome A (Wang et al.) are different from Dome C and Dome Fuji.

The study by Dittmann et al. (2015) is referred to as "in preparation" and should thus not be citeable.

Dittmann et al. was submitted to ACP on Dec. 13th, but unfortuntately ACP was not able to find an editor for it. We found an editor ourselves and the paper was accepted for ACPD and went online on Feb. 1st. We have changed the reference accordingly.

Dittmann, A., Schlosser, E., Masson-Delmotte, V., Powers, J. G., Manning, K. W., Werner, M., and Fujita, K.: Precipitation regime and stable isotopes at Dome Fuji, East Antarctica, Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2015-1012, in review, 2016.

p. 30479, L25: see comment above on Dome F data.

Here we meant the first fresh snow samples at Dome C. added "at this site" in the text to make this clear.

Dome C is a deep ice core drilling site. However, the measurements presented here are the first derived from fresh snow samples at this site.

p. 30480, L6: Not clear what the focus of this section is, as the discussion changes from stable isotopes to snow type and to AWS data.

To clarify the connection between temperature and isotopes in this section, we added the influence of the fact that the precipitation temperature is higher than on average on the stable isotope ratios.

The precipitation-weighted temperature was significantly higher than the mean annual surface temperature because the precipitation events were related to warm-air advection, which leads to a warm bias in the $\delta^{18}O$ record.

See comment above on the study by Dittmann et al (2015).

Our comment above addresses this.

As this section is in the "Previous work" chapter, I would have expected more information on previous stable isotope measurements done at Dome C either in snow or firn to provide context for the data reported later on.

Once again, we would like to refer to our general comment here.(that this is not a stable isotope paper, but that the stable isotopes are the motivation for the investigation of atmospheric conditions. We state this explicitly in the introduction now.)

P. 30480: More details on the sampling and analysis procedure are required. What bags have been used, how have samples been stored, when have they been melted? Have you made checks for data quality of some kind, e.g. by transferring standard water in the same containers from the sampling site to the lab? This is important to add here since the Stenni et al (2015) reference is cited as "in prep".

See comments above, please.The Stenni et al. paper , as noted above, is now in the publication pipeline and these details on sampling and analysis are described there.

P. 30482, L. 17: This section may be shortened.

Since our analysis is based on AMPS output we think it is important to give this information to readers here.

P. 30483, L. 9: Please explain more what you mean by "coreless winter". What is the importance of cloud cover seasonality for this feature?

The "coreless winter" is a clearly defined term well-known in Antarctic meteorology and atmospheric physics. We explain it in the text and added some information following the advice of Reviewer #1. Cloud cover seasonality itself does not play a role here. (Changes in cloud cover during warm air intrusions support the warming by increasing incoming LW radiation.)

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. 19: "ever observed": for the period 1996-2014?

Yes

P. 30483, L. 21: correct to -54.9°C

Done

P. 30484, L. 1: "barely exceed": rephrase to "reach below" for clarity

We changed that to "are rarely lower than" as suggested by Reviewer #1.

Minimum temperatures are rarely lower than -70 °C

P. 30483, L. 11: "Most likely a mixture": Why most likely, are the data not available?

The crystal analysis was carried out using photos of the samples at the Avalanche Institute in Arabba. It was not always possible to clearly define one crystal type for the entire sample. We added some information about this in the text.

P. 30483, L. 16: add "(not shown)" after "moist"

It is shown in Fig. 3 and Fig. 10. Moist does not refer to humidity, but means high precipitation amounts in general meteorological terms. In German we would also say "niederschlagsreich", but this term does not exist in English.

P. 30485, L. 13: please provide a table listing these events, e.g. in the supplement

A detailed study including a "climatology" of all events for the entire observation periods will be published in one of the next papers.

P. 30485, L. 16: what distance would suffice for considering an event to be in the vicinity?

There is no absolutely defined distance here. It depends on the strength of the event. Usually, these were cases where AMPS clearly showed a case of strong southward moisture transport from lower latitudes that did not quite reach Dome C, though. So even though an exact verification of precip amounts at Dome C is not seen, the model basically did represent the synoptic situation correctly.

P. 30486, L. 24: it is not clear how the source at 40°S is obtained. How large is the uncertainty, can this be quantified? I think it is difficult to justify using 500hPa fields to infer information about moisture sources, which is a surface process. Did you take into account the vertical position of the trajectories? As Sodemann and Stohl (2009) pointed out, 5 days will in general not be long enough to obtain a reliable moisture source information from trajectories in that region. The uncertainty is typically taken into account in trajectory studies by considering many (hundreds) of trajectories at slightly offset time and space to obtain a statistical information about the possible origin locations. This is a severe limitation of the analysis done here.

We have rewritten this section of the manuscript to clarify things here. And yes, we did look at the vertical position of the trajectories, the 500hPa more often showed an end point close to the ocean surface than the 300hPa. We have explained our method and the reasons for using it in more detail now, including our request on the accuracy of the moisture source estimate.

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.

P. 30487, L. 6: I agree with these arguments as a hypothesis but not as a result from this analysis. Please clarify.

We simply give an example about a precipitation event with a relatively northern moisture source and state that this can lead to a bias in the temperature derived from stable isotopes We do not know yet whether there was an increase in the number of such situations in a different climate. This has to be studied using more proxies (e.g. sea ice extent) combined with atmospheric models.

P. 30487, L. 25: It would be very insightful to add information on the variability of the Z500 field as shading to the mean fields.

We prefer not to show this in order to keep the clarity of the signal.

P. 30490, L. 1: "Since the main motivation": if that is indeed the main motivation I strongly suggest to move these results to the beginning such that the reader has the isotope data in mind when the further analysis of the atmospheric flow situation is presented. It may also be worthwhile to show a more detailed investigation of the isotope data, for example correlations with temperature for the two years.

We would like to refer to our general comment again here.

P. 30490, L. 13: "globally averaged": what do you mean 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. 30490, L. 22: Can you provide more information on the d excess values here - what is the typical value in firn samples, for instance, and how is this parameter interpreted at Dome C ice cores? If the moisture sources really changed (I would consider that as an hypothesis at this point) then would you expect to see a change in the d-excess as well (see Pfahl and Sodemann, 2014, and references therein).

This also falls into the area of our general comment and the revised description of the foucs.The d-excess is not the topic of this paper.

P. 30493, L. 6: Consider discussing the recent work by Steen-Larsen et al. (2014) on the exchange between atmospheric water vapor, air in the snow pack and the ice crystals which may be able to change the isotope composition of the snow after deposition.

We have given this reference in the discussion.

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.

Figure 1 and 2: Consolidate into one figure. I don't think it is necessary to show the AMPS domains in this study.

We would prefer to have the figure to make it clear what AMPS has covered. We think the consolidation into one figure is a matter of taste.

Figure 3 and 10: Consolidate into one figure by removing panels 3b and 3c. Maybe add accumulated precipitation to Fig. 3a.

We think this is a matter of taste.

Figure 4: Remove legend from three panels. I recommend to not use 3D pie diagrams as the areal representation of the numbers is distorted by the oval shapes.

As in the precedeing comment, we think that this is a matter of taste and that the figure as it is suits our purpose .

Figure 5: Figure is cluttered - use same domain and panel size, arranged horizontally.

See comment above. We fully agree that some of the figures have a bad appearance in the ACPD version. 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 will 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 anymore in the future.

Arranged horizontally the figure would look poor in the final version.

Figure 6: use same domain and size and arrange horizontally. Could use white instead of blue for regions below 0.2 mm/day.

See comment above, please.

Figure 7: use square map, consider adding variability of Z500 as shading.

This is the AMPS domain. It is not square. Rest see above

Figure 9: transpose panels to horizontal alignment

Please, see comment above.

As we mentioned before, this is a problem of the ACPD format, which will disappear in ACP.

References

Gorodetskaya, I V, M Tsukernik, K Claes, M F Ralph, W D Neff, and N P M Van Lipzig. 2014. The role of Atmospheric Rivers in anomalous snow accumulation in East Antarctica. Geophys. Res. Letters, doi:10.1002/2014GL060881.

Helsen et al., 2007: The Isotopic Composition of Present-Day Antarctic Snow in a Lagrangian Atmospheric Simulation, J. Climate

Pfahl, S. and Sodemann, H., 2014: What controls deuterium excess in global precipitation?, Clim. Past 10:771–781, doi:10.5194/cp-10-771-2014.

Sime et al., 2009: Evidence for warmer interglacials in East Antarctic ice cores, Nature

Sodemann, H., Masson-Delmotte, V., Schwierz, C., Vinther, B. M. and Wernli, H., 2008: Inter-annual variability of Greenland winter precipitation sources. Part II: Effects of North Atlantic Oscillation variability on stable isotopes in precipitation, J. Geophys. Res., 113, D12111, doi:10.1029/2007JD009416.

Sodemann, H, and A Stohl. 2009. Asymmetries in the moisture origin of Antarctic precipitation. Geophys.Res. Letters 36: L22803. doi:10.1029/2009GL040242.

Steen-Larsen, H. C., Johnsen, S. J., Masson-Delmotte, V., Stenni, B., Risi, C., Sodemann, H., Balslev-Clausen, D., Blunier, T., Dahl-Jensen, D., Ellehøj, M. D., Falourd, S., Gkinis, V., Grindsted, A., Jouzel, J., Popp, T., Sheldon, S., Simonsen, S. B., Sjolte, J., Steffensen, J. P., Sperlich, P., Sveinbjörnsdóttir, A. E., Vinther, B. M., and White, J. W. C., 2013: Continuous monitoring of summer surface water vapour isotopic composition above the Greenland Ice Sheet. Atmos. Chem. Phys., 13, 4815-4828, doi:10.5194/acp-13-4815-2013.

Wang, Y., Sodemann, H., Hou, S., Masson-Delmotte, V. Jouzel, J. and Pang, H., 2013: Snow accumulation and its moisture origin over Dome Argus, Antarctica. Clim. Dyn., 40:731-742, doi: 10.1007/s00382-012-1398-9.