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.

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

Detailed comments:

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

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

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

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.

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.

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

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

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

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

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

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

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

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".

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

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

P. 30483, L. 19: "ever observed": for the period 1996-2014?

P. 30483, L. 21: correct to -54.9°C

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

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

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

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

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

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.

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

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.

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.

P. 30490, L. 13: "globally averaged": what do you mean here?

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).

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.

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

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

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.

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

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

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

Figure 9: transpose panels to horizontal alignment

References

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