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Discussions

Interactive comment on "The influence of the synoptic regime on stable water isotopes in precipitation at Dome C, East Antarctica" *by* Elisabeth Schlosser et al.

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Final response to Referee 1

We appreciate Referee #1's effort to provide a review within short time. However, we got the impression that several things have been overlooked or understood incompletely.

This manuscript presents published measurements of d18O and d-excess of precipitation at the Dome C site as well as an application of a modeling approach combining the mesoscale atmospheric model and a simple isotopic model. The authors conclude that the model underestimates the depletion of d18O in precipitation in Antarctica. This

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study does not provide any new data, despite the fact that the “Precipitation and stable isotopes data” are not presented in the part “Previous work”. Everything has already been published in the paper by Stenni et al. (The Cryosphere, 2016).

It is true that a large part (not all!) of the data used in this study are already published, however, they have never been combined in the way we did it in the presented manuscript and were also supplemented by additional data. In order to make a publication self-contained, it is almost always necessary to explain some things that have been published before. We would like to stress that our study does yield new results that have not been published elsewhere. Stenni et al. (2016) stress the relationship of stable isotope ratios with meteorological station data. They discuss in detail the ïĄď-T slope for various time periods and isotope variables, compare this to other locations and also look at the relationships amongst the isotope variables. The general atmospheric flow conditions are discussed only briefly, whereas in the new study we present a detailed analysis of the various synoptic situations that lead to precipitation at Dome C. Stenni et al. (2016) also state that hoar frost has a distinct fingerprint among the various precipitation types, implying that moisture sources and or the hydrological cycle might be different for hoar frost. Our more detailed study showed that this “fingerprint” is due to the fact that hoar frost occurs predominantly during the cold period. Relatively large amounts of hoar frost are measured after synoptic snowfall events, when humidity is still increased after moisture transport from lower latitudes, which means that hoar frost basically has the same moisture sources as the other precipitation types. The results on Dome C meteorological conditions and synoptic patterns during precipitation are already largely shown and discussed in the paper by Schlosser et al. (ACP, 2016).

In Schlosser et al. (2016), the stable isotopes served mainly as motivation for the study. They only discussed the meteorological conditions in two extreme years, without any isotope modelling or specific discussion of the stable isotope data and without any general analysis of the synoptic conditions during precipitation events. For instance, the conditions shown in Fig. 4d and 4e did not occur in the analysis of 2009 and

2010. Especially the situation in Fig 4e is highly interesting due to its relation to the Amundsen-Bellingshausen Sea Low. Nothing comparable occurs at Dome Fuji, so it was not discussed in the study by Dittmann et al. (2016).

The results of the isotopes modelling (part 5.4) have already been discussed largely by Dittmann et al. (ACP, 2016) for the Dome F site with the same conclusion. I thus do not see the added value of this study which is basically only a second application of the Dittmann et al. (2016) study on another site with similar characteristics. The main conclusion of this paper and of Dittmann et al. (2016), i.e. that the MCIM does simulate too high d18O in Antarctic precipitation is not new.

Dittmann et al. (2016) used a very short time series (less than 1 yr) from a different Antarctic location to study synoptic conditions and model stable isotopes. Even if we had done only the same for Dome C, it would be a valuable result to confirm Dittmann's findings with a longer time series from another location. Additionally, we used radiosonde data (not available for Dome Fuji for Dittmann's study) to determine the temperature at the lifting condensation level (LCL). This temperature, additionally to the temperature at the upper limit of the inversion layer, was used as input for the isotope model. It is a surprising result that this did not improve the model simulations. It is a very critical point for the relationship between temperature and stable isotope ratio, WHICH temperature is considered here. For many years, the temperature at the top of the inversion layer has been used, which is a strong simplification and more research is needed here.

This has already been noted for example in Uemura et al. (CP, 2016).

There is no publication by Uemura et al., CP 2016 (as suggested by the referee) to be found on the CP homepage. We are not aware of any study by Uemera et al. that investigates/models data from single precipitation events.) We did include a study by Uemura et al. from JGR2008

I also feel that the introduction part is misleading with very few references to previous studies while much has been done in the recent years on the study of precipitation patterns and water isotopic composition in sites of the Antarctic plateau. The 2 recent papers mentioned in the introduction refer to Greenland studies. Similarly, the conclusion is very poor and only rephrase conclusions from previous studies (Schlosser et al., 2016; Dittmann et al., 2016; Stenni et al., 2016) without anything more.

We included new references concerning Antarctic synoptics/precipitation and about stable isotope work in Antarctica and in the lab. We also re-structured the manuscript by combining the sections “Introduction” and “Previous work” and we also re-wrote the conclusions. (see also comments to Ref.#2)

Response to Referee 2

We would like to thank the reviewer for the thorough and constructive review. In the following we address all single points: General comments

1. The fact that the precipitation measurements are not reliable in case of (relatively) strong wind is mentioned (e.g. p.14, l.5-6) but its influence on the presented analyses and results is not discussed. It would be instructive to provide the frequency of such "windy precipitation events" so the reader can figure if it is only marginal or on the contrary quite usual.

We added information about the frequency of such events in the text and also about the possible consequences for our results. Comparison of the total precipitation amount derived from the sampling to data from an accumulation stake array yields that the amount of sampled precipitation is lower than the measured accumulation. However, accumulation is also influenced by the wind (during and after precipitation), so an exact comparison is not possible.

2. the organization of the paper is a bit strange (at least to me): the introduction is quite short, and previous work is discussed in Section 3 (I would expect this in the introduction). The limitation of the present organization is that the motivation for the

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present work, given in the introduction, is a bit weak because not put in the more general context presented later on. Up to the authors...

We agree that the introduction is very general and we need the information given in Section 3 (Previous work) to explain our motivation in more detail. Thus we re-structured the manuscript by combining the sections “Introduction” and “Previous work”

We also included new references concerning Antarctic synoptics/precipitation and about stable isotope work in Antarctica and in the lab following the advice of Referee #1.

3. The authors must make an effort to explain the new contribution of the present work with respect to previous studies by some of the authors (ex: Dittmann et al., ACP, 2016; Schlosser et al, ACP, 2016).

We re-wrote the conclusions stressing the new achievements and also added an additional result in the abstract.

Specific comments

1. P.2, l.8: models do not provide data, but simulations (potentially constrained by observations).

This is a semantic question. “Data” is a very general term, not even restricted to science, and does not, by definition, mean observational data (otherwise the widely used term “observational data” would be a tautology. Most modellers (including ECMWF and other big modellers) call their products model data. We do not see any problem here.

2. P.10, l.13-24: in mixed-phase clouds where ice and liquid water particle co-exist, the Bergeron-Findheisen process is one possible mechanisms, but riming could also take place with very different cinematic (and involving collision). Could riming have different influence on fractionation?

Riming would involve freezing of supercooled droplets. There is a kinetic effect during freezing of liquid water. However, since the freezing occurs rapidly, the fractionation is so weak that the model assumes fractionation for this phase transition is negligible. We changed the text accordingly.

3. P.10, l.30: I suggest to use “positively skewed distribution" rather than “Ldistribution".

We changed this in the text.

4. P.11, l.26: how this classification was conducted? this is an important methodological aspect that must be clarified for the repeatability of the work.

The classification was done manually. This has the advantage that the investigator is in full control of the process and the classification system can be tailored precisely to the researcher’s needs. We added this information in the text.

5. P.12, l.6: “considerable amount": please provide numerical values. For readers not very familiar with Antarctica, the numbers may seem quite low...

We fully agree. We added information about precipitation amounts in the text.

6. P.15, l.1: Figure 10 is referred to in the text before Figure 9.

We exchanged the Figure numbers.

7. P.15, l.6-7: how (and why) were the events selected for the computation of the back trajectories?

Back trajectories were calculated for all cases where the synoptic situation seemed to be suitable for it, meaning a rather clear atmospheric flow. When this was not the case, trajectories tended to have kinks and loops and were not plausible or convincing. Again, this choice was made manually. We added this information in the text.

8. Figure 1: it seems that the y-axis correspond to the number of occurrence rather than the frequency.

We changed this in the figure to "number of observations".

9. Figure 10: the legend should be moved in the plot to avoid masking points. Done We also noticed that we had forgotten the reference Ciais and Jouzel, 1994 (and, incorrectly had used (Ciais et al., 1994) in the text. We corrected this and added the reference in t

We add the revised version with the corrected figures as supplement.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/tc-2017-21/tc-2017-21-AC2-supplement.pdf

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