

## ***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.***

**Anonymous Referee #2**

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### **Summary**

This manuscript presents an original and unprecedented data set about precipitation at dome C, East Antarctica. The daily amount, type and stable water isotope ratios of precipitation have been monitored for about 3 years (2008-2010) at dome C. Using atmospheric models, the synoptic conditions generating precipitation at dome C are clustered and analyzed. Back trajectories are also computed to identify the dominant sources of moisture leading to precipitation at the considered location. A simple fractionation model is run but the simulated isotope ratio values are biased and poorly compare to local measurements. These results question the usual assumption related

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to the interpretation of isotope ratio values in ice cores, and motivate further research to better understand the complex mechanisms governing precipitation and isotope fractionation over Antarctica.

### **Recommendation**

This manuscript presents original and useful data, and raises important questions about the interpretation of isotope ratios for climate perspectives. The data and methods are well described and seem solid. I have no major issue, and I hence recommend to send the manuscript back to the authors for minor revisions. I have some comments and suggestions, listed below.

### **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.
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 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...
3. The authors must make an effort to explain the new contribution of the present

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work with respect to previous studies by some of the authors (ex: Dittmann et al., ACP, 2016; Schlosser et al, ACP, 2016).

### **Specific comments**

1. P.2, l.8: models do not provide data, but simulations (potentially constrained by observations).
2. P.10, l.13-24: in mixed-phase clouds where ice and liquid water particle co-exist, the Bergeron-Findeisen 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?
3. P.10, l.30: I suggest to use "positively skewed distribution" rather than "L-distribution".
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.
5. P.12, l.6: "considerable amount": please provide numerical values. For readers not very familiar with Antarctica, the numbers may seem quite low...
6. P.15, l.1: Figure 10 is referred to in the text before Figure 9.
7. P.15, l.6-7: how (and why) were the events selected for the computation of the back trajectories?
8. Figure 1: it seems that the y-axis correspond to the number of occurrence rather than the frequency.
9. Figure 10: the legend should be moved in the plot to avoid masking points.