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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.

Elisabeth Schlosser et al.

elisabeth.schlosser@uibk.ac.at

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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.

Although it is true that a large part (not all!) of the data used in this study are already published, 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) stresses the relationship of stable isotope ratios with meteorological station data. They discuss in detail the delta-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 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.

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).

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.

Our main conclusion is not that the model underestimates the isotopic depletion. Modelling is only a part of our study. For the isotope part, it is considerably more important that the deuterium excess showed no relationship with relative humidity or wind speed at the estimated moisture source. This relationship has been a general assumption in isotope studies for decades.

Likewise, the assumption that a more northern moisture source automatically means stronger depletion was shown to be not true for single precipitation events and the involved physics suggest that this applies generally to Antarctic precipitation. This confirmed the results of the Dome Fuji study.

We agree that it would be worthwhile to include more studies from the Antarctic plateau in the section “previous work”. (Unfortunately, reviewers rarely agree about the length of the “previous work” and “introduction” sections.) We mentioned the two Greenland studies because they used continuous measurements of water vapour stable isotopes. This kind of work has only recently started in Antarctica, but we will try to discuss some more references in a newer version of our manuscript.

(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.)

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