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Interactive comment on "Acquisition of isotopic composition for surface snow in East Antarctica and the links to climatic parameters" *by* A. Touzeau et al.

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General comments:

This is a timely and excellent paper compiling a remarkable data set for stable isotopic measurements in high latitude. It is an important contribution to the growing literature on 17O-excess signatures of the hydrologic cycle, past and present. The authors speculate qualitatively on some of the climatic information extracted from the observed isotopic variations in East Antarctica. It should definitely be published following minor revisions.

My main suggestion for the authors is to make their arguments about the data more quantitative. In particular, because some complexity processes such as post-deposition of snow, by their nature, I understand the authors desire not to over-interpret the data. Note that one of the active debates in the cryosphere science is what information does the ice core record. A large dataset like this in snow precipitation, 'upstream' of the where this debate is centered, should be a more comprehensive angle for future research.

Specific comments:

P6279 L3: Here the authors define the two important parameters d-excess and 17O-excess. I suggest putting the definition into context of global meteoric water line, as a better preparation when the authors mention the slopes 8 and 0.528 in P6284 L21 and P6285 L10, respectively.

P6280 L5: Reference is needed for the information "30ppmv at Vostok, ranging from $\sim$ 1ppmv in winter to $\sim$100 ppmv in summer".

P6281 L16-19: I suggest putting "(MCIM)" and "(AGCM, LMDZ-iso)" behind the two types of models as preparation for your follow-up discussion.

P6282 L7: Regarding the calculation of supersaturation function: S = 1-aT, people realize that the a value is still not well constrained by observations. But you could mention a common range of "a" values observed in experiments or modeling.

P6283 L28: The authors should be more specific when reporting the standard deviation (1σ). This may be difficult as this paper compiles a large dataset that includes data from other publications, with different ways of reporting the analytical precision (e.g., pooled standard deviation, Student"s t-test with certain confidence limits, etc). Even just for the standard deviation, readers will want to know whether it is calculated based on the lab working references or for replicates of each samples (n$\geq$?). Alternatively, you could re-calculate the precision in a chosen way and describe clearly how you did it, or list all

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the manners of precision calculation and summarize an upper limit of error.

P6284 L15 and P6285 L9: The authors should be careful here to "head off careless readers at the pass" by making a distinction between what exponent is intrinsic to a process vs. what exponent is measured. For example, in the case of purely unidirectional kinetic flow, e.g., a Rayleigh fractionation process, the measured slope would express itself in the residue as an array in δ'17O vs. δ'18O space with the slope of the line equal to $(17\alpha - 1) / (18\alpha - 1)$ whereas pure diffusion process, the intrinsic slope is calculated as ln(D/D17) / ln(D/D18).

P6285 L10: The authors should cite Luz and Barkan (2010), as they pioneered the definition of the slope of meteoric water line as 0.528.

P6285 L28: Why is S=1-0.002T "too low"? It should represent a stronger supersaturation than S=1-0.0033T does.

P6286 L18: At the end of this line, what does "1" represent? Is there a slope unit missing there?

P6288 L1: I suggest changing "than" into "as".

P6288 L5: In "analyzed in δD and δ18O", I suggest change "in" into "for".

P6288 L6: Cut off "s" in "samples collection".

P6288, the last paragraph starting from L25: I would provide the time scheme of the collection of surface snow at Dome C, just as you did in descriptions of precipitation collections. From Frg. 4, is it between Dec 2010 and Dec 2011?

P6290 L7-21: For the two time slots, what are the R and p values for δ18O-T correlation for each scenario, respectively?

P6290-6291, for Section 3.3: Figure 3b hints a negative correlation between 17O-excess and d-excess for precipitation at Dome C, which is not observed for either precipitation at Vostok or for surface snow at Dome C. As the authors discussed in Section

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2.3, variation of the slope in δD-δ18O space, and thus d-excess, reflects both equilibrium fractionation during distillation and kinetic effect during supersaturation. Variation in 17O-excess is dominantly controlled by kinetic effects. Then, a combination of 17O-excess and d-excess should help tease apart distillation process from supersaturation condensation. Correlation between 17O-excess and d-excess (Fig.3b) should reflect that kinetic effect (supersaturation) in colder conditions is the major control of the isotopic fractionation. Otherwise, the lack of such correlation (Fig.3a and Fig. 4) indicates a less stable/warmer condition, which is consistent with the discussion in P6291 L15-20., just from another aspect.

P6297 L9: I suggest the information in Table 2 to be mentioned earlier in each method sections. This table should be cited along with the figures when any correlation among parameters are discussed throughout the text, so that the readers could have a clearer sense of these relationships.

Interactive comment on The Cryosphere Discuss., 9, 6275, 2015.