

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

Anonymous Referee #3

Received and published: 4 January 2016

This paper attempts to integrate isotopic variations in Antarctica from a few sets of intensive observations, covering spatial and temporal (seasonal and longer term) trends in surface samples, precipitation, and snow pits. The most novel part of the paper is that ^{17}O -excess is measured along with $\delta^{18}\text{O}$ and δD for the same samples. Compared to most previous studies, ^{17}O -excess potentially provides additional degrees of freedom by which the data can be used to infer and constrain processes related to the moisture evolution. Unfortunately, the integration and interpretation are not as insightful as I expected. There is also at least one fundamental error (General Comment 3) that must be corrected before publication of this manuscript.

C2630

General Comments:

1) The framework of interpretation is largely based on simple one source Rayleigh distillation. This implies that the source moisture is constant, although this is not stated explicitly. In recent years, large quantities of data and analysis have demonstrated significant isotopic variations in the marine boundary layer vapor that serves as the beginning of the Rayleigh trajectory. For example, the source can contribute up to 10‰ variations in d-excess (Steen-Larsen et al., 2014). Related to this issue, the source region also influences moisture transport and distillation. Compared to coastal precipitation sites, inland sites tend to get condensed moisture from greater heights, where the moisture has transported (and thus lifted) for a longer distance. While I do not disagree that snow formation may change d-excess and ^{17}O -excess, these additional processes (particularly the variability of the source) should not be ignored. The observed result is a combination of these processes - as well as others. Acknowledgements and discussions about the moisture isotopic property variations at the source as a potential mechanism of their observed isotopic variations should be made in the revised manuscript, and the size of the error attributable to neglect of this mechanism should be evaluated.

2) One reviewer was disappointed that the data interpretation was not more quantitative. I agree. Ideally, a simple Rayleigh model (since they really emphasize Rayleigh processes) including both d-excess and ^{17}O -excess, should be used to interpret the data, which would have made the paper more interesting, more informative and more original. I do recognize that this task may lead to its own independent contribution. Therefore, I support publication of this paper if all the data are included in the supplementary material (it seems to me). Other investigators may use the data for additional interpretation and modeling.

3) One mistake has to be corrected before publication. Equation 2 is the basis for discussions of d-excess through a Rayleigh process, in which condensation occurs upon cooling. However, this equation is not appropriate for the discussion. Equa-

C2631

tion 2 is derived from the integrated Rayleigh equation for delta values of liquid based on the unstated assumption that the isotopic fractionation factor between liquid and vapor is constant. Therefore, this relationship is not applicable to an explanation of changing slope with temperature along a single Rayleigh trajectory, which is exactly what they did. Obviously there is a logical contradiction, i.e., assuming no temperature change to obtain the equation and then using the equation to discuss the effect of temperature change. What Equation 2 does allow is a comparison of slopes for CONSTANT-fractionation Rayleigh distillation processes at two different temperatures. This comparison would be pointless, however. A valid argument has to be based on a Rayleigh process with cooling. In this case, the cooling history would have to be provided. The slope change during simple cooling scenarios, such as adiabatic or isobaric cooling, was discussed by Dansgaard (1964), a half-century ago. Therefore, Equation 2 and related discussions have to be removed or redone based on correct Rayleigh curves. (It is also unacceptable that neither they gave the source of the equation, nor did they state the assumption for its derivation.)

Specific Comments:

P6281, last paragraph through P6282 first paragraph: The idea of tuning is to assume that ALL the changes in d-excess from coast to inland is caused by kinetic fractionation during snow formation. This assumption has to be stated explicitly. With that I do not understand how, after tuning, the information about the source can be extracted without circular reasoning. Also see my General Comment 1 about contribution of isotopic variations at the moisture source region. P6284, line 14: Please distinguish the "meteoric water line" (MWL) from "Rayleigh line" (you should definite latter term). These two are fundamentally different. At best, the Rayleigh line is an interpretation of the MWL. P6284, Equation 2. See my General Comment 3. Please fix all the discussions and conclusions based on this equation. P6288, line 18: "interpolated". Do you mean "extrapolated"?

C2632

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

C2633