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Atmospheric Measurement Techniques Discussions

Interactive comment on "Improved Methodologies for Continuous Flow Analysis of Stable Water Isotopes in Ice Cores" *by* Tyler R. Jones et al.

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The manuscript under discussion represents an amended version compared to the initial submission in April 2016 when I provided some comments.

In general, this works demonstrates a development of very interesting set-up, and carefully designed and sophisticated tests. To start the discussion (full review will be provided later) I stress a few aspects related to uncertainties. In order to validate the improvements in the continuous-melting analysis of ice cores, one has to consider all sources of the uncertainty as well as potential biases. There I see some open questions and potential problems.

1. The uncertainty propagation due to reference materials (RMs) (authors call it as standards) in use, calibration procedures and measurements is not clear. Table 2

demonstrate obvious problems, namely the uncertainty in dD for several lab-standard waters (calling as secondary standard is confusing) is 0.2 to 0.3 per mille, equal or even smaller than the uncertainty assigned for the primary international RMs VSMOW2 and VSLAP2 (0.3 and 0.3 per mille respectivvely). This cannot be true; the uncertainty for any lower level material (secondary RM or lab standards) must include the uncertainty of the higher RMs which was used for calibration. Thus, for lab standards one can expect about 0.5 per mille in the best case and larger if GISP was taken for calibrations (GISP has $\pm$1.2 per mille). The 0.2 per mille uncertainty stated for lab-standards clear demonstrates a problem; the same is valid for uncertainties for lab-standards given in d18O data in table 3.

2. Correspondingly, the uncertainty for sample results appear to be largely underestimated, at this must include the uncertainty of lab-standards (see above) in use as a single component.

3. Given continuously changing isotope signals of continuous-melted ice (due to natural variability in ice core) one may ask how the integration time of CRDS contributes to the smoothing of the natural variability measured on the melted ice by CRDS.

4. Referring to the traditional mass-spectrometry, the authors have missed to mention TCEA method which is widely used for discrete sampling of ice (e.g. http://digitalscholarship.unlv.edu/cgi/viewcontent.cgi?article=1917&context=thesesdissertatic resulting in 1 StDev of under the best conditions at 0.5 and <0.1 per mille for dD and d18O correspondingly (e.g. see https://tools.thermofisher.com/content/sfs/brochures/D21627∼.pdf)

5. The uncertainty cited for traditional mass-spectrometry (p 12 line 24) - does it refer to water equilibration methods or to TCEA?

6. The authors are not very careful in references, for instance reference given to (Lin et al. 2010) when referring to dD and d18O values of VSMOW2 & VSLAP2 is not correct. (This publication was aimed to determine d17O, without effecting d18O and

dD values accepted for VSMOW2 & VSLAP2.) The initial manuscript submitted in April did contain numerous incorrect and irrelevant references, for instance those related to 17O which is not addressed in the work; I have not fully checked this version being under discussion.

7. When one discusses effects of corrections, not to forget that the magnitude of corrections may be rather significant, reaching 1 per mil in d18O for mixing corrections (Fig 4) and about 20 per mille for diffusion-mixing corrections (Fig 6). What about the uncertainties of the corrections? In particular interesting is whether the corrections (introduced for dD and d18O separately) may result in an artefact for d-excess values (means – whether uncertainties in dD and d18O are well correlated or not?) Please provide a clear explanation on this.

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