

Review for „Data treatment and corrections for estimating H₂O and CO₂ isotope fluxes from high-frequency observations“ by Robbert P.J. Moonen et al.

General comments:

The authors have successfully included the feedback from the first review round and the manuscript has improved in clarity. I also thank the authors for the extensive replies to my comments, which resolved most of the questions I had. Some general questions remain which I will outline below.

In general, I think the manuscript is a valuable contribution to the field of measuring stable isotope fluxes and isofluxes and will help in designing future field campaigns and post-processing procedures.

Specific comments:

Mole-fraction calibration for H₂O:

Thank you for outlining the calibration protocol in Table A1 which resolved many questions I had. I admit though that I still don't fully understand what was done and why a leak would explain the cross dependency between mole fraction dependence and standard water isotopic composition considering that the leak/ambient isotopic composition before and after the campaign when the calibrations were done was probably not identical in itself. I also still think that a visual representation of the *calibration coefficients* in Figure 3 is not very helpful and should at least be accompanied by the final values that you used in a table format (possibly together with the CO₂ coefficients in Table 2) and a graph showing the fitted quadratic curves to the datapoints from the calibration (in the Appendix). Could you also outline what mole fraction level you used as reference for the correction function? A priori I would assume that a fit to "3 humidity levels with 6 and 4 datapoints each" (did I understand correctly?) in the range 7000-24000ppmv is not very robust and might be the reason for differences in *calibration coefficients* before and after the campaign rather than a leak. Similarly, you are attributing the differences in span calibration (0.4‰ for 18O is rather high) to drift alone but this could also just be due to noise in your calibration pulses and should be phrased more cautiously in my opinion. As you say though, the accuracy of your measurements is arguably less important than the precision. That being said, I wonder if the mole-fraction calibration is indeed as crucial as you say since the fluctuations within the 30min averaging window will be small, even if the mole fraction sensitivity of your instrument seems very high if I read off the coefficient values correctly from Figure 3. And lastly, I don't understand the uncertainties of the standard waters you give in Table 1 and A1 in terms of significant figures/decimal places and origin of these values. Does the standard error come from a separate analysis of these standard waters?

CF factor analysis

I still think it would be interesting to see or discuss the CF factor **values**. The statement of Line 415 that the authors were willing to include (comparison between CF for dD and d18O) is very limited and does not discuss the "difficulty" of correction values that are VERY high for either isotope species. I think this aspect of correcting a measured signal by a very high factor (at least for H₂O isotopes) should be discussed in the section 6 as a general problem of measuring isotope or delta fluxes. It could be discussed also in the context of the

intriguing thought of precision vs. sampling frequency in new instrument development (L. 489-492).

Additionally, I think it is worth to include a discussion of the CF factors for CO₂ that are ≈ 1 and thus in line with previous studies which I think is supporting your new correction technique. I therefore recommend to include a short CO₂ CF factor discussion in section 6.

Data availability statement:

I do support the general community effort of making data publicly available with a doi upon publication in line with the FAIR principles:

https://www.atmospheric-measurement-techniques.net/policies/data_policy.html

https://www.atmospheric-measurement-techniques.net/policies/data_policy.html#data_availability

Technical corrections:

L. 16-18: It sounds as if you have used your results to validate models in this manuscript. Maybe rephrase.

L. 21: missing space after CO₂

L. 56 & 60: “ δ -flux” term used before its introduced/defined/explained in section 2.2

L. 76: change “isotope fractionation” to “isotopic fractionation”

L. 129: correct “we included figure is included”

L. 154: correct “and an scroll additional scroll pump”

L. 164: repetition of “which”

L. 190: something missing: “the calibration of the isotopic against a reference standard”

Section 4.2, L. 229: I think you should include a short explanation here why you chose to time shift 10minutes instead of 30 minutes.

L. 287: repetition of “that”

L. 301, 452, 456, 457: I think the term “isotopic *exchange* in the inlet line” is misleading. I suggest to use “isotopic retention” or, as you use in Line. 562, “inlet line attenuation” instead.

L. 321, L. 329: How much of the data (30min windows) did you eventually filter/exclude from the analysis? This is interesting for the reader to know.

Figure 7 caption, L. 342, L. 346, Figure A3: replace “R2” with “r”

L. 343: I don’t see why a linear interpolation is the “likely pattern” during the night given that the variability in periods of similar length during day time is much more variable than linear. Since you exclude night time periods from your analysis (L. 353) I recommend to not show linear interpolations in Figure 7 and remove the “likely pattern” statement.

L. 383: delete “=”

L. 493: Replace “still” with “however”

L. 495: include here a statement on the choice of LPF frequency

L. 547: delete “in”

Section 7: I would also include best practice recommendations not only for the technical set-up but also for the post-processing procedure since this is what you are newly proposing in this manuscript (LPF, averaging time, clock synchronization...)

L. 567: Consider moving the sentence “The attenuation of the isotopic signal...” to Line 564 after “reported previously.”