

Interactive comment on “The stable isotope composition of water vapour above Corsica during the HyMeX SOP1: insight into vertical mixing processes from lower-tropospheric survey flights” by Harald Sodemann et al.

Anonymous Referee #3

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General Comments

High resolution in situ aircraft based isotopic ($^{18}\text{O}/^{16}\text{O}$ and $2\text{H}/1\text{H}$) measurements of atmospheric water vapor between 150 and 4500 m a.s.l. using a laser based optical analyzer are reported. The low abundance of water vapor in upper tropospheric air, the large range of concentrations and the low abundances of Oxygen-18 and Deuterium render such measurements difficult and referee 1 raises some points of concern (Teflon is mentioned wrt to memory effects, perhaps the filter used (Figure A2) should be described. The interpretation of the experimental data in the light of adequate me-

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teorological information is complete and attention is paid to proper experimental procedures for calibration. Although the reader needs some patience to follow all details of the many series measured the paper does justice to the data and will be valuable for those embarking on acquiring and using similar data. An important step is made by explaining profiles as the result of mixing of air masses and not only local Raleigh type stable isotope fractionation. One question a reader may have is why not any other data from these flights were used. In other words, the isotope data and physical data are used together, without any other tracer data. It may well be the unique nature of water vapor and its isotopic composition that make it hard to find any other tracer that supports interpretation.

Below are my (mostly technical) comments.

The title should be “The stable isotopic composition of water vapour above Corsica during the HyMeX SOP1 campaign...”

“Stable water isotopes” I think this sloppy descriptor ought to be removed from the paper because it is wrong. We get throughout the paper statements like: Stable water isotopes, stable water isotope composition, stable water isotope profiles, the SWI composition of atmospheric water vapor (page2, line 18) and so on. Does SWI stand for vapor or liquid? Better is to write what it is, use isotopic composition (IC), for instance the IC of snow, or the IC of water vapour. In case one is concerned to wrongly generate the impression that Tritium measurements were involved, then use SIC. Or one can use HIC for Hydrogen isotopic composition and OIC . . . such logical abbreviations will not be forgotten for years to come, and do not sound as bad as some recent Twitter messages.

The delta values are defined (since at least 6 decades) as atomic ratios. Reading the introduction, a reader may think that delta values are based on molecular ratios. The laser analyses absorption features based on molecular properties. Which standards are used, and how. Why does WS9 have a large deuterium excess?

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For non isotope colleagues (stable), perhaps explain why delta d is use and not delta 18O. The 18O/16O ratio is larger than the D/H ratio (which is twice the DHO/H2O ratio).. Is the systematic error in the d- excess due to a noisier Deuterium or 18O signal? On page 8, line 23 delta 18O is used and not delta D.

In the introduction it is emphasized that these are the first airborne spectroscopic stable water isotope measurements over the Mediterranean. The reader may well think, “what about the other seas and oceans, have they been left out? The coveted quantifier “first” can be used by narrowing down in space. But it helps no-one.

Page 2, line 8. To what does “these” pertain?

Page 2, line 11. I am sure that by far most delta 18O and delta D measurements have been made for hydrological purposes, e.g. precipitation network, ground water and aquifer studies, not palaeoclimate.

Page 3, line 31. “advection” Do the authors mean diffusion? What exactly is advection and why would that fractionate? Convection does not fractionate, neither does advection, I suspect.

Page 5, line 26. This sentence means that humicap (should it be Humicap?) provides slow accurate measurements. Is this true? These are small sensors that need calibration. How can they be accurate? I assume only if they have a stable response from before to after a measurement series.

Page 6, line1. “installation of a replacement pump” It is a bit unclear what has happened. Referred to is A2, where is written “not shown”.

Page 6. What is q?

Page 6, line 31. What is the source of such very precise numbers, e.g. -78.68 per mil for deuterium.

Page 7, line 27. ..lower and higher.

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Page 9, line 31. Replace “air” by water vapour” and replace “value” by “values”.

Page 10, line 2. The deuterium excess is not measured directly, but I think derived from the D and 18O signals. Can you pinpoint which has most influence on the deteriorating precision and accuracy?

Page 10, line 6. The sentence starting with “Only few..” can be deleted.

Page 10, line 11. “remarkable” is perhaps not the correct description. Ehhalt was a very good experimental scientist. The reader has problems to get convinced. The red dots (Fig. 5 (a)) roughly fall in the measured range, that is all. Is a curve through the mean or median values not better? Also, in the same figure box, we do not see a zero gradient between 0 and 1500. A small decrease is visible.

Page 10, line 28. Perhaps replace “common” by “earlier” and insert “is based on”.

Page 11, line 1. “is constant up to almost 1200 m a.s.l.” OR “is almost constant up to about 1200 m a.s.l.).

Page 11, line 3. Here is suddenly written “major isotope species” This sentence needs to be corrected. “with their very depleted conditions” (sounds like people being robbed).

Page 12, lines 27-29. The argument is not convincing and referee #1 mentions the problem of the d-excess data. I do not know what to advice here.

Page 14, line 1. Please insert (Fig. 1) after “pattern #3”.

Page 14, line 18. Which flight?

Page 16, line 20. Why flight 10?

Page 19. Summary and Conclusions. I am sorry, but the conclusions need to be partly rewritten. There are valuable findings which allow some conclusions and these conclusions should not be cluttered up with much less relevant information.

Line 3. add “campaign”.

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Line 3. It may be the first such data for Corsica, but it will not make Napoleon come back. Safe and appropriate to write is that it is if not the first, one of the first extensive airborne datasets in the framework of a well documented measurement campaign. Later is written, that your finding is confirmed by Dyroff, who actually published BEFORE you did. Please change.

Line 19. It is a bit hard on Claude Taylor, who after all pioneered similar isotope measurements over 4 decades ago. The statement does not make the paper or experimental work more valuable.

Line 26. “non-linearities in the delta scale” This needs an explanation, are there more than one type on non-linearities. Perhaps deal with this issue earlier in the paper, explaining the delta values and ratios.

References. perhaps useful: Paired stable isotopologues in precipitation and vapor: A case study of the amount effect within western tropical Pacific storms Conroy, JL et al. JOURNAL OF GEOPHYSICAL RESEARCH-ATMOSPHERES. DOI: 10.1002/2015JD023844

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