Atmos. Chem. Phys. Discuss., 15, C8867–C8869, 2015
www.atmos-chem-phys-discuss.net/15/C8867/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Detecting moisture transport pathways to the subtropical North Atlantic free troposphere using paired H₂O-delta; D in situ measurements" *by* Y. González et al.

Anonymous Referee #1

Received and published: 3 November 2015

In this manuscript, the authors present a new, long-term dataset of water vapor isotopic composition from the Canary Islands and use several diagnostic techniques to interpret the data in terms of water vapor transport. Overall, this is a very good manuscript and dataset from a very interesting and important part of the world. I do have some serious concerns about the standardization protocol used by the authors, and I hope that by simply expanding more on what they did that they can convince me the data are reliable, but they may need to do more than that.

Major Comments: (1) I have several concerns about the isotopic calibrations. First, the authors use standards that only span a narrow range (-142 per mil and -245 per mil). This is problematic because the data appear to span from ∼-100 per mil (hard to say because the authors don't report the total range of observations) to apparently as low as -500 per mil. This means that their calibrations are based on extrapolation and it makes be concerned especially about the reliability of the delta values below -245 per mil, which constitute a large fraction of the dataset. The authors need to explain in more detail how they do the corrections - are they generating a 'stretching factor' (which they should be doing)? How do they justify extrapolating over such a large part of their dataset? The reader needs to see in substantial detail how the authors carry out the calibrations.

(2) I am also surprised and skeptical at their statement that their bubbler concentration-dependence study showed "no humidity dependences on the Picarro's isotopologues readings". They need to show a figure illustrating that, because I am not sure I believe it. There is almost always some concentration dependence, and it can vary substantially over time and can vary with delta - in other words a heavy standard can have a different concentration dependence than a light standard. This has to be quantified and presented to the reader. Some of their most interesting results are from the drier conditions, so they need to be more rigorous in their evaluation and presentation of concentration dependence.

(3) The paper would benefit from an expanded discussion in which the authors relate their results to other studies from subtropical sites. This dataset fits nicely into an existing suite of studies from Mauna Loa, Hawaii, and from the Chajnantor Plateau, Chile, and I would like to see the authors attempt to place their results into the growing context of subtropical humidity and isotopes. In particular, they need to expand upon their discussion of Noone et al 2011, Samuels-Crow et al 2014, and add other relevant studies by Hurley et al 2012, also on Mauna Loa, and the Chilean studies of Galewsky, 2015 and Galewsky and Samuels-Crow 2014. What are we learning from the use of

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water vapor isotopic measurements in subtropical sites? What are they telling us about the humidity of the global subtropics?

(4) I find many of the figures hard to follow because there are so many (too many) data points. Instead of plotting a giant blob of data, can they authors use contours of the 2D histogram (or equivalently the PDF)?

(5) Along the same lines, I find the units of mmol/mol to be hard to follow and prefer either ppmv or g/kg. Can the authors at least translate mmol/mol into these other units in a few places in the paper? This is especially relevant because the standardization section uses ppmv and it's hard to relate that to the measurements presented in the paper.

(6) In general, the authors refer to moistening from the marine boundary layer via evaporation from the ocean surface. Can the isotopic measurements be used to distinguish between this direct moistening mechanism and moistening from the outflow of shallow convection? I suspect the latter is a more frequent moistening mechanism, but it would be interesting to see if the isotopes can help constraint that.

Minor Comments: (1) The citation to 'Kimberly et al, 2014' is incorrect - the author is Kimberly Samuels-Crow, so the citation should be 'Samuels-Crow et al, 2014'. (2) Rather than referring to 'depleted dD values', it's much better to say 'lower dD values'. A value can't be depleted or enriched. Please edit the use of 'depleted' or 'enriched' as those words are used incorrectly throughout the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 27219, 2015.