

Response to review by Anonymous Referee #1

We thank the reviewer for the constructive comments made to our manuscript 'Climate controls on water vapor deuterium excess in the marine boundary layer of the North Atlantic based on 500 days of in situ, continuous measurements'

The reviewer points out that we spend a significant portion of the manuscript on describing our setup and protocols. We do not completely agree that the 'Material and method' section is too long (the review claims that we spend 70% on the technical method – The material and Method section is 7 pages of text while the manuscript is 17 pages, but we do have 8 out of 12 figures dealing with characterization of the system). We strongly believe that it is very important to robustly characterize these relative novel measurement types. For example we find it very important to point out that despite the fact that these type of instruments perform measurements with ~1 second resolution it does not make sense of claiming this high resolution due to memory effects in the inlet lines. While some instruments, which we have worked with have shown significantly less drift than this instrument, it is clear that it is very important to document and correct the drift.

We have below answered the explicit comments using **green text**

1.P2377 L.7 and other part “d-excess vs wind-speed relationship” Diffusional 18-O fractionation factor dependency on wind speed was investigated using 17O-excess data (Uemura et al. 2010). The observed diffusional fractionation factors are scattered and do not show clear dependency on wind speed (Fig 2 in their paper). On the contrary recent study showed that d-excess dependency on surface wind speed (Benetti et al., 2014). This discussion paper supports the former. The inconsistency is even confusing because the first author of this discussion paper is a coauthor of Benetti et al. (2014). I guess that the wind dependency differ widely depending on sampling location/height, moisture source and weather conditions etc. The authors should clarify the differences and discuss possible reasons.

References: Ryu Uemura, Eugeni Barkan, Osamu Abe, and Boaz Luz, Triple isotope composition of oxygen in atmospheric water vapor, *Geophysical Research Letters*, VOL. 37, L04402, doi:10.1029/2009GL041960, 2010

Marion Benetti, Gilles Reverdin, Catherine Pierre, Liliane Merlivat, Camille Ris, Hans Christian Steen-Larsen, Françoise Vimeux, Deuterium excess in marine water vapor: Dependency on relative humidity and surface wind speed during evaporation, *Journal of Geophysical Research*, Volume 119, Issue 2, 584–593, 2014
DOI:10.1002/2013JD020535

We agree with the reviewer that these presented data does not give much support to the theory of MJ79. However we want to point out that we do not state that the definition by MJ79 or the results of Uemuerra et al 2010 or Benetti et al. 2014 is wrong. We simply state that the data collected in Bermuda does not give find support for the effect of wind

speed by MJ79. Specifically do we write in the text:

“

This indicates that either the wind regimes defined in MJ79 might not be appropriate for this area or the observed d-excess of the local water vapor is affected by past wind conditions.

“

We acknowledge the need to clarify that we do not state that an effect of wind speed cannot be observed other places. Hence we introduce the following sentence in the text in end of the section dealing with wind speed.

The differences between the result of this study and the results by Uemura et al. (2012) and Benetti et al. (2014) illustrates that more studies are needed to clarify the effect of wind speed on the d-excess signal in the marine boundary condition.

Technical corrections:

P2366, L21-22 “. . .which alter the slope of 8 due to temperature dependence of the equilibrium fractionation coefficient. . . .” C443

The slope is changed any process that does not follow the D/18O slope of 8 (i.e., not only the temperature). Indeed the crude approximation of logarithm $(1+\delta)$ is an important artifacts (Appendix in Uemura et al., 2012).

References: Uemura, R., V. Masson-Delmotte, J. Jouzel, A. Landais, H. Motoyama, and B. Stenni, Ranges of moisture-source temperature estimated from Antarctic ice cores stable isotope records over glacial–interglacial cycles, *Clim. Past*, 8, 1109-1125, 2012, doi:10.5194/cp-8-1109-2012

Yes – this is indeed correct. The approximation of the logarithm definition is indeed a reason why the slope decreases significantly at depleted values similar to those found in Antarctica. However the values of change in d18O we are encountering here is between -8 and -16 ‰. In a log dD vs. log d18O plot the slope of the meteoric waterline for this region will vary between approximately 9 and 9.3.

We do acknowledge that the text is not completely correct and have corrected it such that it does not give the impression that any equilibrium process will lead to a slope of 8.

P2368, L19 “water vapor isotope system” >water vapor isotope monitoring system

Corrected

P2370, L13, “. . .and provides the measurement, which are the focus of this paper”. What

does this sentence mean? All the data (fig 10 to 13) are from this top inlet?

Yes that is correct. We use only measurements from the top inlet in the paper.

P2373, L21-21, “We first injected air with two significantly different water vapor concentrations.. and secondly injected . . .with two significantly different water vapor isotopic compositions”. It is difficult to make such water vapors. How did you prepare the different vapor? Please describe in detail.

It is correct that if only the SDM Picarro module was available this would indeed be difficult. However since our self-designed calibration system was build to generate a constant stream of vapor we simply replicated this system and used it to generate the water vapor injected at the inlet.

We follow the advise of the reviewer and add the following sentence to the text

“We used a similar system as our calibration system described in section 2.2 to generate the water vapor with the given isotope and humidity value.”

P2374, L24, “. . .a second Picarro inc. analyzer ” Add product name (2120-i or 2130-i)?

This was a special test version Picarro inc. analyzer and did therefore not have a specific number. I presume it was with the similar specs as 2130-i.

P2375, L3, “. . .The mean he mean deviation for ” > Remove “he mean”.

Corrected

P2378, L20, “. . .This is supported by a wavelet analysis showing significant. . .” Where is the figure showing the wavelet analysis?

To limit the numbers of figures we choice not to show the figure. The wavelet analysis is shown below:

P2379, L16, “. . .allows for ntegration.” >integration

Corrected

