Response to review by Anonymous Referee #2

We thank the reviewer for the detailed and helpful 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"

We notice that Reviewer # 2 has raised concerns with our conclusion regarding the finding that the wind speed does not affect the observed d-excess. We agree in general with the reviewer that this is an important finding, but we do not agree that we make a strong conclusion based on this finding. Instead we merely report our findings and show that they cannot be used to support the predicted relationship between d-excess and wind speed given by MJ79:

Below we list the sentences from the manuscript, which deals with this finding. We do not find that these sentences constitute strong conclusions.

However, in contrast with theory, no effect of wind speed could be detected on the relationship between *d*-excess and relative humidity.

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.

However, we could not identify any shifts in relationship with wind speed.

We have below commented on the individual concerns by Reviewer #2 using green text.

General remarks: The scientific investigation is focused on the relationship between deuterium excess and relative humidity, based on the model assumptions of Merlivat and Jouzel (1979). It is certainly good to test the MJ model assumptions and results us- ing this new data set. However, the paper would profit from a discussion of the model assumptions. In particular, I think here of the threshold in wind speed for "smooth" and "rough" conditions (corresponding to up to 4Bft below and larger than 5Bft above the threshold, thus not that different) and also of the assumption that the vapour is produced locally. The influence of waves on surface roughness and on potential evap- oration area is not discussed either. From Fig. 1 it is obvious that 5-10 m/s is the most frequent wind speed, which means that most of your measurements are in the upper range of "smooth conditions" and the lower range of "rough conditions". I would not expect to find a large difference for those two regimes. It is a too strong conclusion that the wind speed has no influence on the deuterium excess or the d-humidity relation- ship. It would be better to really compare the low and high end of the wind speed range to get a clearer picture.

The reason that we choose to separate the wind speed for a smooth regime define for wind speed < 6 m/s and a rough regime defined for wind speed > 7 m/s is due to discontinuity in the 'k'-factor (MJ79) giving rise to differences in kinetic fractionation (Figure 2 – MJ79 – inserted below)

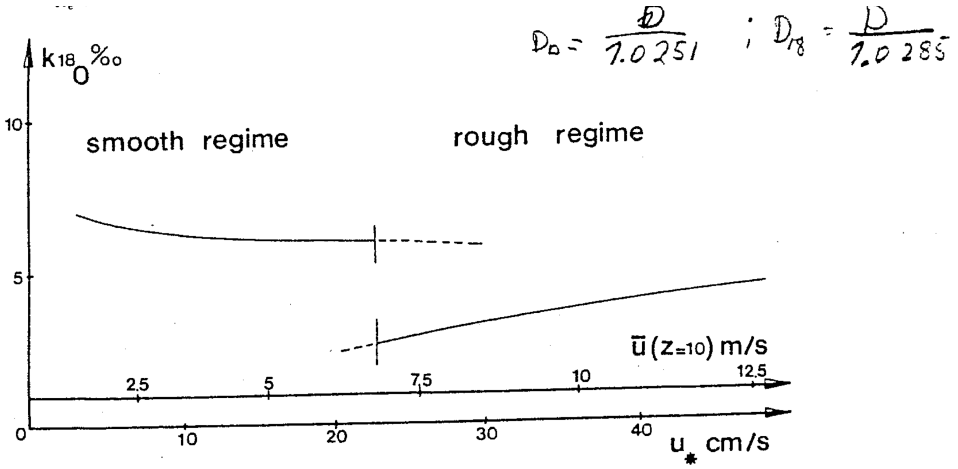
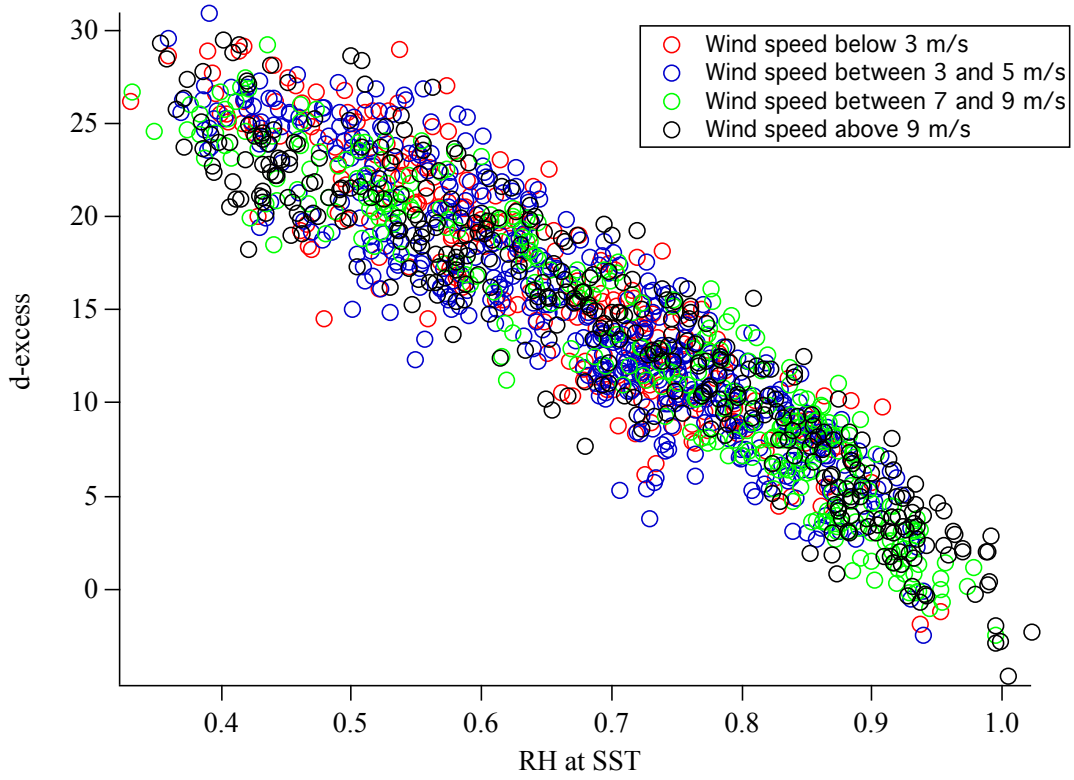


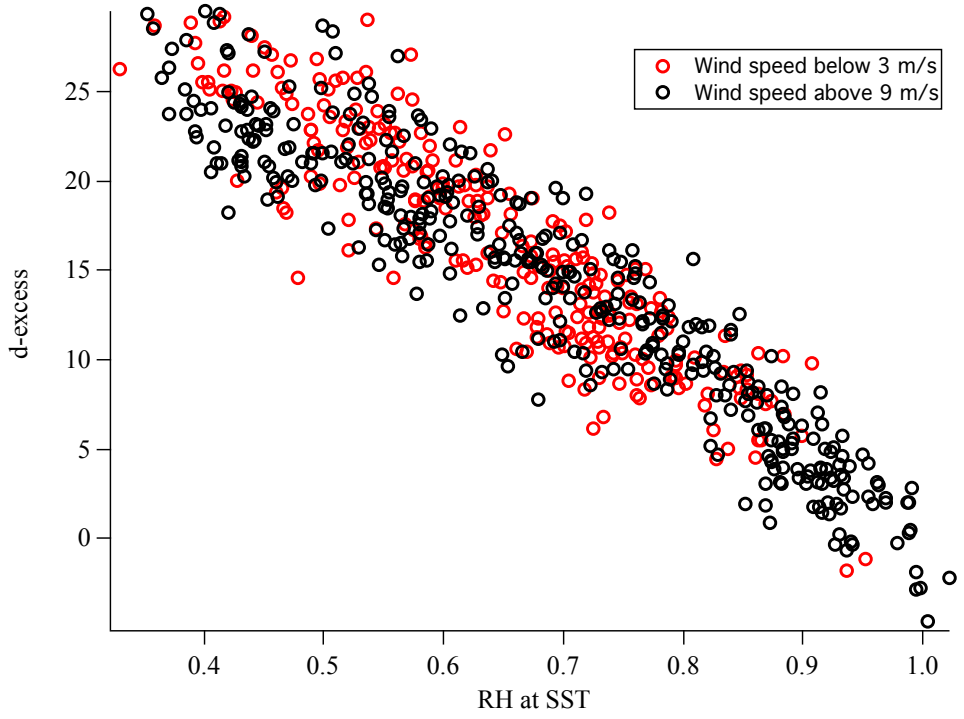
Fig. 2. Variation of the kinetic fractionation factor for oxygen 18, k_{18_O}, with the friction velocity or the mean wind speed at z = 10 m.

In this figure it can be clearly seen that the discontinuity arises between 6 and 7 m/s – hence why we make this separation.

We have, in order to illustrate the finding that our data does not support the theory related to effect of wind speed on the d-excess, separated the observations into 4 different wind speed regions: below 3 m/s, between 3 and 5 m/s, between 7 and 9 m/s and above 9 m/s. The result of this is shown in the figures below. We believe that these figures also support our finding that observations from Bermuda is not in agreement with the theory related to wind speed of MJ79. We want to highlight that we do not state that MJ79 is wrong but just that data collected from Bermuda is not consistent with the theory of MJ79. There can be several reasons for this.

We notice that for wind speeds above 9 m/s we see for RH<0.5 a slightly lower d-excess values compared to wind speeds below 3 m/s. This is of course consistent with theory of MJ79 but the magnitude is not as large. We would expect a difference in d-excess according to MJ79 of about 10 o/oo (we might observe a slight decrease of about 2-3 o/oo but only for RH<0.5). We notice that evaporation of sea spray droplets would indeed have a similar effect as we would include water vapor of same isotopic composition as the ocean.


Wind speed below 3 m/s
Wind speed between 3 and 5 m/s
Wind speed between 7 and 9 m/s
Wind speed above 9 m/s
d-excess
RH at SST
30
25
20
15
10
5
0
0.4
0.5
0.6
0.7
0.8
0.9
1.0


Wind speed below 3 m/s
Wind speed above 9 m/s
d-excess
RH at SST
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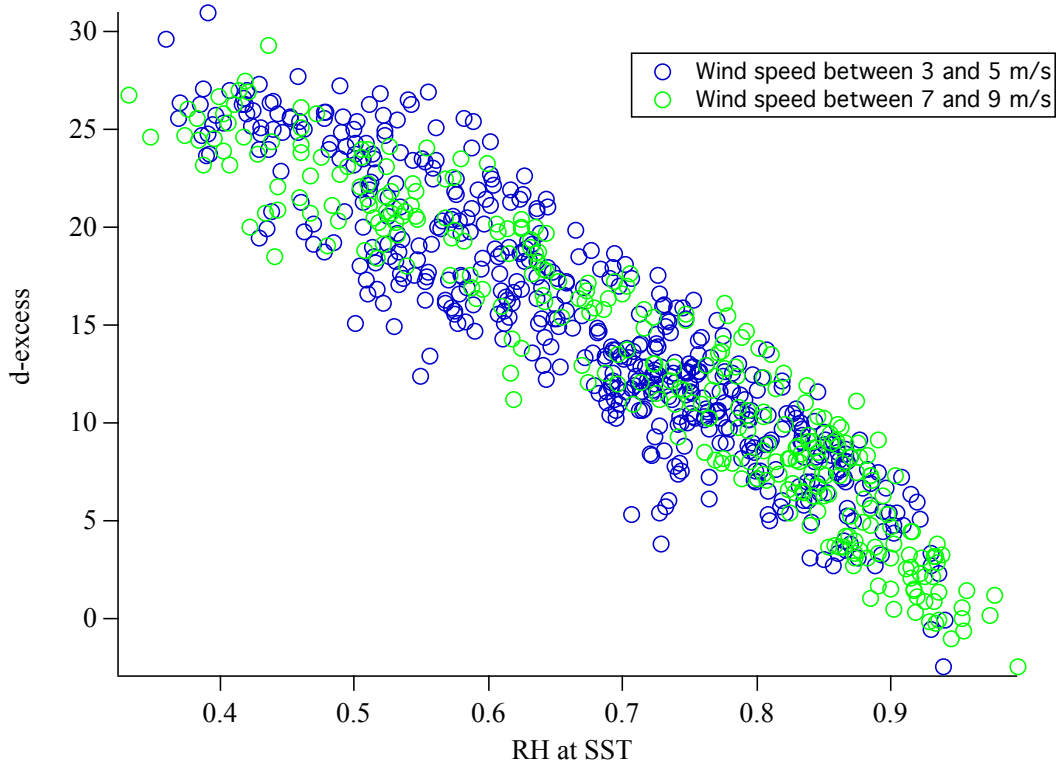
1.0



Also, the question of local or non-local origin of the moisture is only briefly discussed. Low wind-speed could also be associated with high-pressure and thus more local origin of the moisture, whereas higher wind speeds might mean advection of moisture. The differences in the results for different wind directions also hint at some influence here. These points should be addressed more clearly.

Yes we completely agree on this point. We have therefore updated the sentence to read:

“

… the observed d-excess of the local water vapor is affected by past wind conditions at remote areas of evaporation

“

Specific remarks: In atmospheric science, humidity is commonly defined as relative, absolute or specific humidity or as mixing ratio. Another measure is humidity ppmv, but this can refer to dry air or to humid air (mostly it refers to dry air, but it is not clearly defined). You can choose the humidity measure you like, of course, but, for the above mentioned reasons, it would be good if you could define the humidity measure you use in

your study.

We acknowledge this issue and have inserted the following equation defining our humidity measure:

“

Humidity (ppmv) = $p_w/(p_{tot}-p_w) 10^6$, where p_w and p_{tot} is the vapor pressure and the total pressure.

“

“Relative humidity normalized to SST” is an expression that is not only confusing but incorrect. Relative humidity is always given AT a certain temperature. What you use is the relative humidity at SST, but it is not a normalized value.

Yes – we acknowledge that our choice of words were not optimal. We have corrected this through out the paper and in the figures.

Technical remarks:

Check the use of “respectively”, in most cases it is used incorrectly

Corrected

P2367: l22: but the strong correlation. . .

Corrected

L23: do you mean “not allow identifying the impact of rhSST alone?

No we meant

“not allow identifying the independent impact of SST.”

We have corrected this.

P2375, l11: better: at the beginning of autumn

Corrected

L16: parallel to

Corrected

L23: do you mean “without any significant local effect”?

Yes - Corrected

P2376/fig. 10: the x-axis should be months not tenth of a year, that is hard to read, and in the text you refer to months, too.

L22: here you refer to <6m/s and~10m/s, later you compare <6 and >7m/s. ??

Corrected to the following text:

and a~rough surface (wind speed above 7\,\unit{m\,s^{-1}} – kinetic fractionation value take for 10\,\unit{m\,s^{-1}})

P2377: l25: approximately rather than around

Corrected

P2378, L1: lower winter SST (not colder)

Corrected

L7: moisture back trajectory calculations is not a good expression. You have quite a few VERY long words that should at least contain a dash (-) at the right position.

Corrected

L25: prevents frontal systems from passing through

Corrected

P2379: l6: signal-to-noise ratio

Corrected

L15: increasing the average time from 2 weeks to 1 month does not significantly further accept this slope.

Corrected

L16/17: This sentence is not understandable, please reformulate

Yes we absolutely agree – sorry. This has been corrected now.

“

We note that time averaging allows for integration across synoptic systems. This is comparable to a spatial averaging across air masses in the region around the Bermuda Islands.

“

L19: become closer

Corrected

L23: reflectSpeculation is not a very scientific method, better use hypothesis

Corrected

P2380: l11: coming from the American continent (or do you mean off?)

Yes – we meant ‘off’. This is corrected now.

P2381: l2: suggesting a limited. . .

Corrected