We appreciate reviewer’s comments.

Below we have compiled our answers to the reviewer’s questions and comments.

Please note that the Swv samples were excluded in the modified manuscript as lack meteorological parameters for them. Therefore only samples Nwv at the sampling height of 15m were taken for the paper.

All the modified and new figures and tables have been included in the Author Comments.

The reviewer’s comments and questions are presented as italic while our answers are written as bold.

The paper presents isotopic water vapor and meteorological data collected during two ‘summer’ cruises on RV S.A. Agulhas in 2017-2018 is the Indian Ocean sector of the southern Ocean. The sample data are not numerous, but are very valuable, as meteorological conditions are well described and cover a large spread of conditions. The paper attempts two things: first to check the local equilibrium assumption that atmospheric water vapor results from local sea water evaporation using two models (TCG and UCG), and then attempts to find where and why this breaks down (in the southern part with continental air outflow). The approach is valuable, but should be much more clearly outlined. For example, lines 40-45 of the introduction should be expanded and it should be made clearier what is the approach adopted. This should be reminded at the beginning of the discussion 4 (line 143), so that the reader does not have to wait on lines 219-220 to understand what is the approach followed (first ‘local’ evaporation, thus the equilibrium model; then, remote/mixing contributions).

The introduction is now modified for clarity with more description on the approach adopted in the study.

When assuming local evaporation, the abstract and conclusion state that the UCG model with a 0.5 CD molecular diffusivity ratio performs best. I find that conclusion not substantiated, and believe that there are cases with TCG which perform as well. This could be clarified.

In the modified version of this study, the Craig-Gordon models (both UCG and TCG) were run for different fractions of the turbulent indices, x (i.e. the fraction of molecular and turbulent diffusion). The value of x was varied from 0 to 1 with an interval of 0.1. We found that the UCG and TCG models both perform well for MJ and CD molecular diffusivity ratios, while for the PW diffusivity ratios the difference between the observed and the modelled values is the largest. This is explained in the paper. We found that UCG MJ for x=0.8, UCG CD for x=0.6 and TCG MJ for x=0.6 and TCG CD for x=0.7 perform equally well within the uncertainty limits.

The main issues I have are the followings:
1: how well were meteorological parameters measured (in particular, for relative humidity, and was this measurement done very close to where the air was collected (upper level). I find the very large relative humidities in the southern part of the first cruise transect surprising in view of the much weaker relative humidities of the second cruise. Of course, this is possible, but would suggest, almost fog-like conditions for the first cruise. If this is the case, this could involve very different processes and thus expected isotopic properties of the near-surface air mass (what type of clouds, then; subsidence or not?)

The meteorological parameters were measured at the height of ~15 m from sea level, which was the sampling inlet for water vapour isotopic measurements. The relative humidity was estimated at the dry and the wet bulb temperature using the psychrometric charts. The two cruises differs in the latitudinal coverage area. Water vapour sampling in the first cruise was carried out until 69.3°S, whereas for the second cruise, the sampling was conducted until 66.8 °S. We recorded continuous precipitation over a duration between 20/01/2017-25/01/2017 (SOE IX) when relative humidity level approached higher values.

2. *The meteorological situations are described in ways which are too vague. For example, names of regions a little strange on lines 82-86 (for example melting/freezingstarting at 47 ◦ S?) On what is it based? What does it represent? I think that the different situations: cyclonic/ anticyclonic/ precipitating systems, presence of rain or snow should all be also reported and analyzed to provide a more relevant context that just the HYPSLIT trajectories, which as they are triplets are very hard to read as presented(and are not really commented upon. . .). Also, in which conditions were the high-winds encountered.*

This section is now modified.
The triplet HYSPLIT trajectories are shown together to give comprehensive overview about the overall moisture transport pathways.
The Southern Ocean is generally associated with high wind speed conditions.

3: *I was surprised to find why the (local equilibrium) model fit seem to work less well with the near surface samples. Any explanation for that? Alltogether, how samples are collected at the two levels should be more clearly described (maybe in supplementary materials, see comments below). Otherwise, it is very hard to understand what to make of the differences in results for the two sets of data.*

The meteorological parameters which goes as input parameters are measured at the height of ~15 m. This would be one of one of the major reason responsible for the discrepancy between near surface samples (Swv) and the samples collected at ~15m above the ocean surface.

- **Therefore, the Swv samples have been omitted from discussion.**
- **Additional information on the sampling procedures has been added in the supplementary document.**
- **The Swv samples were collected when the ocean surface conditions were calm i.e. when there was no visible wave in the ocean (No wave breaking near the sampling inlet and the ship was kept stationary for conducting the CDT depth profiling and sampling. The ocean beyond 65S latitude was relatively calm as compared to other latitudes.**

I will now provide a few comments on the figures:

Figure 1 : What are the white circles (compared to all the other red circles) ?

- **The white circles depict the locations of the water vapour (Swv) samples i.e. the vapour samples collected close to the ocean surface by placing the inlet tube just 1 m above the Sea water level. Since these samples were omitted from the discussion now, the figure has been modified and the caption has been rewritten to accommodate the new changes. (Figure 1 in the Author comment file)**

Caption figure 2, last sentence: 'Read and blue colors. . . temperature, respectively.' It is hard to differentiate the two colors. Maybe air temperature should be open dots with no color inside?

- **The figure has been modified according to the suggestion. (Figure 2 in the author comment file)**

Figure 2 suggests that average conditions were with much less relative humidity at high latitudes during second cruise than during the first one. What is the big difference between the two cruises. I find relative humidity so high and for so long on SOE-IX (first cruise) a bit surprising.

- **There was continuous cloud cover for the days with high RH levels. Also, precipitation event lasted for the entire length with low pressure system as mentioned earlier.**

In particular, this does not seem to be so substantiated by figure 3, but I don't read very well figure 3, which I don't find very clear.

- **The triplet HYSPLIT trajectories are shown together to give comprehensive overview about the overall moisture transport pathways. There exists a few cases for both the expeditions when there was a complete mismatch between the observed and the HYSPLIT predicted RH values (governed by the Reanalysis dataset). The RH along the back-trajectories in close to 0 for all elevations. This may be due to missing data values in the input dataset used for generating the HYSPLIT model results.**

It does not seem either the values that are retained afterwards for humidity (such as in figure 6). Have they been adjusted afterwards?

- **In the old Figure 6, samples collected exclusively north of 65S latitude were plotted. The reason being the mixing of Antarctic vapor with depleted heavy isotope values south of 65S latitude. Large relative humidity were mostly observed for the region south of 65S and during the passage of an extratropical cylone. This figure has been modified to include all the data points and the regression equations for various sample classifications have been provided in a separate table.**

Figure 4, top panel a) suggest south of 40 ◦ S SOE-X lower than SOE-IX, itself lower than SW global database. It is a bit surprising, but could be associated with different surface salinities and freshwater sources. What are the salinities and how do they compare with surface water isotopic composition; differences between the two cruises?

- **The cruises were not during the same month, for SOE IX the sampling was performed in January 2017 while for SOE-X the samples were collected during December-2017 to mid January-2018 . The salinity values measured along the transect are now included in the revision.**

For figure 4, b), d18O SOE-IX and SOE-X seem rather similar, except maybe some d18O Swv de SOE-X, which could be a little higher near 40-50 ◦ S. For dxs less obvious. I don't see a clear (panel c) difference in dxs near 45-65 ◦ S between the two cruises, despite very different relative humidities? (big apparent jump near 45 ◦ S).

- **The Swv samples is now removed from the modified figure.**
- **In addition to the local factors, the dxs is also controlled by the mixing of the advected vapor this is probably the reason.**

Last sentence of the caption of Figure 4: This is not just zonal variation, but maybe statistical distributions grouped by latitudinal bands.

- **Latitudinal ranges (Zones) are grouped according to the approximate positioning of the general wind patterns (easterly or westerly) decided based on the HYSPLIT trajectories. The caption has been modified to make the idea clear.**

Figure 5: dependency in humidity seems dubious, but could it be poor measurements and would'nt it be then important to separate the cruises (issue also of cross wind-dependency) Water samples south of 65 ◦ S clearer, as well as Swv wamples (but very scattered and how does one measure those).

- **We agree the plot is confusing, the plot has been modified and some information has been provided in the tables (Table 2 for d18O and Table 3 for d2H). The relationships for separate cruises has been added.**
- **The sampling for Swv water vapor samples was totally dependent on the ocean conditions encountered. The dependency might be dubious to some extent as the measurements for relative humidity were done at the height of the Nwv samples (~15m above the sea level).**
- **The Swv samples have not been included in the modified version of the manuscript due to the reasons stated before.**

Plot on b) dependency of SST is clearly wrong panel (not right SST! And same clearly as for a; this raises issues of whether one trusts fully the other figures).

- **I don't understand this question. Nevertheless, this figure has been modified and additional information has been added in Table 2 and Table 3.**

Figure 6 on d-excess dependency and meteorological conditions seem rather in agreement (north of 65 ◦ S) with Uemura et al. (2008) (also a bit shifted down compared to Uemura's values which could suggest an average bias in one of the data sets).

- **This is due to the fact that low temperatures (both sst and air temperatures) are low beyond 65S and hence limit the evaporation. Moreover, beyond 65S there is advection and mixing of very light Antarctic vapor.**
- **This really answers the doubt you raised and clears it regarding the dubiousness of RH measurements. It has been mentioned in the text that if considered separately the relationship is stronger and -0.64h+ 57.4‰ (r2 =0.77) and -0.64h+48.7 (r 2 =0.61) for SOE IX and SOE X respectively comparable to the that from previous measurements in the Southern Ocean (Uemura et al. 2008) -0.61h+55.71(r2=0.63).**
- **The figure has been modified and the additional information has been provided in a table (Table 4)**

Figure 7. UCG presents some strange peaks. Why? That could be an argument for preferring TCG, but I don't know how realistic the different ocean surface conditions selected are. On this figure, there is no particular need for the colors of the diamonds

- **The larger variability for in the isotopic values predicted by the both UCG and TCG models for MJ and CD molecular diffusivity ratios and thus maybe a reason for a better representation of the conditions under which evaporation happens in the open ocean by CG models for these molecular diffusivity ratios. The model runs for different molecular diffusivity ratios have been separated in the modified figure and are much clearer now.**
- **The ocean conditions x (turbulence coefficient) are selected are assuming ratios of molecular (x=1) to turbulent transport (x=0) and x=0.5 for equal contribution by molecular and turbulent diffusion. In the modified version the models are run for x varying from 0-1 with an increment of 0.1.**
- **This figure has been modified and the data are presented separately for d18O and d-excess. (Figure 7 shows the comparision between modelled and observed d18O and Figure 8 between modelled and observed d-excess)**

Figure 8: interesting. Actually, both UCG and TCG models don't explain observed d-excess at humidity larger than 90%. Otherwise, dependency in SST and wind speed seem OK for both (TCG gives a reduced range compared to UCG, so should the results be considered better?). Some cases better both in term of correlation and small misfits in slope and intercept.

- **This is a known shortcoming of the model as it doesn't perform well for high RH conditions, where greated influence on the isotopic composition is exerted by the advected vapor.**
- **During the period of sampling, high RH conditions were observed when precipitation occurred and when extratropical cyclones were encountered (as evident from the low pressures).**
- **In the modified version as explained before, it was found that UCG MJ for x=0.8, UCG CD for x=0.6 and TCG MJ for x=0.6 and TCG CD for x=0.7 perform equally well within the uncertainty limits. The comparision between the observed and these models have been added as a separate figure. (Figure 10)**
- **The models that best describe the conditions are selected for which there are least differences between the regression parameters (slope and intercept) of the meteorological parameters (rh, sst and wind speed) vs d-excess of water vapor. (Figure 9, Figure 11 and Figure 12)**

(figure 9; the caption should mention whether the comparisons are done with UCG or TCG). The different curves and what they mean (their caption) are hard to read on figures 7 to 10.

- **The comparisons have been presented for both UCG and TCG models. Since in the modified version, the models have been run for x varying between 0-1 with and increment of 0.1 as opposed to 0,0.5 and 1 in the previous version. This additonal information has been clearly presented in Figure 9, Figure 11 and Figure 12 in the modified version.**

Figure 10, probably interesting, but very hard to understand what is presented. Caption should be clearer, and as is does not explain what is presented. The yellow and green bars are too difficult to separate, and should be presented separately (for example one above the other)

- **Noted. The figure has been modified, all the model runs which perform well have been included and the caption rephrased. (Figure 13 and Figure 14 in the revised manuscript)**

In Supplementary material, presenting d18Osw is instructive, but it would be good to add a salinity column to increase the possible use of these data (the data were collected from a rosette with a Seabird CTD, so salinity was measured). It can also help validity-controlling the isotopic sea water data. For example, there is an isolated very negative value: if salinity low and/or collected near an iceberg, it is possible. Otherwise, questionable.

- **There were two ways the surface water samples were collected. The CTD, when the ship was kept stable for the stations where the depth sampling was being done and from a bucket thermometer when the ship was moving.**
- **The salinity values were measured from the CTD during the stations and 6 hourly using an AutoSalinometer during the whole period of the expeditions taking samples from the bucket thermometer.**
- **For the depth sampling stations, the salinity and the isotopic composition is from the same sample. Whereas, in most of the cases, when the ship was moving the surface water samples for mesuring the isotopic composition of the are not same as the ones for which the salinity was measured. Nevertheless, the surface salinity values from the Autosalinometer which were measured along the sampling transect have been plotted in Fig. 4a.**
- **The salinity values have been added and the sample with a negative was in fact collected near the iceberg. The other probable reason for the negative value is the freshwater mixing of precipitation as this sample was collected during the passage of a low pressure system.**

Detailed (mostly minor or editorial) comments:
Line 53: '. . . shows the sampling locations. ' How is Swv collected: at which height above the sea surface? How is spray avoided whether it is for Swv or Nwv. . .) .The detail of the collection method could be key for the results obtained. Later, it is mentioned that Swv samples could only be collected by fair weather. What kind of wind/swells/sea states are conductive to this measurement. All these relevant informations should be provided in the supplementary document.

- **Swv samples have not been discussed in the modified version due to the reasons stated before.**
- **This information has been included in the supplementary document.**

l. 72 '. . ., the change between trajectories corresponding to trade winds from the ones corresponding to westerlies happened at 31 ◦ S, whereas at 630S, a change. . .'

- **Done**

l. 106: the sentence 'The role of sst in governing d-excess. . .' This is only direct role, there is the indirect one of the air water mass and dependency on temperature.

- **Noted. The sentence has been rephrased**

l. 113: 'The strength of the correlation is slightly higher. . .'

- **Done**

l. 120: more sensitivity of ï Ad'H to wind speed is not what comes out clearly from figure 5b. It might be a scaling issue of the ï Ad'18O versus ï Ad'D (note that R-square are small for both variables)

- **Noted, the sentence has been omitted.**

Line 129, the slope is the opposite in this latitude range between the two expeditions. This illustrates, I believe, either very different weather conditions (as seen in r of figure 2) or some issue somewhere, which would need to be further clarified

- **This was a typing error the values of slopes are in fact same for both the expeditions.**

Line 132? Sentence?

- **The sentence has been rephrased**

l. 134: '. . . complement. . .'

- **Done**

Line 135: correlation with SST of d18OSwv. Interesting, but these samples only taken when sea state not strong. Relating the dxs of seawater with relative humidity is a bit strange, as this is close to sea surface but with little wind/sea. I am not sure of what that brought?

- **As explained earlier, the conditions deemed feasible for sampling close to the surface were constrained by the ocean surface conditions. In order to completely minimise the influence of sea spray on the water vapour samples. As mentioned before, since the meteorological measurements were done at the height of 15m the discussion on Swv samples hasn't been included in the modified version of the manuscript.**
- **We haven't compared the dxs of seawater with relative humidity.**

l. 140: '. . . to the near-surface water vapour. . .' I am wondering whether the regression is significant, and in that case, whether this sentence is not anecdotical, and should be omitted.

- **The sentence has been omitted.**

In description of UCG (l. 157-191), the ratio term is ï A ˛ğ-h, which makes sense in this context. However, note that at saturation (fog, for example), thre is a definition issue. For that, The near saturation values reported on Figure 2 almost everywhere south of 50 ◦ S during SOE-IX is a major issue. How was this dealt with (and again, what confidence does one have in these near-saturated conditions, not witnessed the second year).

- **As mentioned before, these were the conditions observed during the passage of low pressure systems as seen from the atmospheric pressure measurements.**

Line 175: remove a 'same'. After that, the global closure is assumed as in Merlivat (1978)

- **Done**

l. 201. I would mention the caveat of the issue of sea spray for high winds. Evaporation from sea spray is a large contribution to total evaporation in these conditions, and follows different laws (in the extreme case of all sea spray evaporating, this would for example yield Rev=Rl) There is also the caveat of below freezing temperature, and low SST close to sea ice formation temperature (just below fresh water freezing points), but I gather from figure 2 that this almost did not happen (correct?)

- **We have no way of knowing the fraction of sea spray contributing to the local moisture or whether all the sea spray that is formed is evaporating. So we have not discussed the influence of sea spray.**

l. 219-220. This sentence is key. I think that this should be presented earlier.

- **Done**

l. 229: '. . . where westerlies dominate. . .'

- **Done**

L. 233: end of sentence missing

- **Sentence rephrased**

l. 236 '. . . is less, and is largely local. . .'

- **Done**