Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-950-RC1, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Craig-Gordon model validation using observed meteorological parameters and measured stable isotope ratios in water vapor over the Southern Ocean" *by* Shaakir Shabir Dar et al.

Anonymous Referee #1

Received and published: 21 February 2020

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).

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.

Also, note that this issue of non-local contributions is also addressed experimentally for different weather condition in Benetti, M., J.-L. Lacour, A.E. Sveinbjörnsdottir, G. Aloisi, G. Reverdin, C. Risi, A.J. Peters, H.-C. Steen-Larsen. A framework to study mixing processes in the marine boundary layer using water vapor isotope measurements. Geophys. Res. Lett., published 10 March 2018 https://doi.org/10.1002/2018GL077167

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?)

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/freezing starting 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

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.

I will now provide a few comments on the figures: Figure 1 : What are the white circles (compared to all the other red circles) ?

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?

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

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

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). 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).

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).

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

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 (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.

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)

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.

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.

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. . .'

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.

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

l. 120: more sensitivity of ïĄďH to wind speed is not what comes out clearly from figure 5b. It might be a scaling issue of the ïĄď18O versus ïĄďD (note that R-square are small for both variables)

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 Line

132? Sentence?

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

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?

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.

In description of UCG (l. 157-191), the ratio term is ïĄğ-h, which makes sense in this context. However, note that at saturation (fog, for example), there 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).

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

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?)

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

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

L. 233: end of sentence missing

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

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-950, 2019.