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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Impact of convection on the upper-tropospheric composition (water vapor/ozone) over a subtropical site (Réunion Island, 21.1° S–55.5° E) in the Indian Ocean" *by* Damien Héron et al.

Anonymous Referee #1

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1 Content

Ozone measurement within the SHADOZ network and humidity measurements from the years 2014-2016 are used to show the convective hydration of the upper troposphere in the vicinity of Reunion Island. Trajectory calculations in combination with METEOSAT brightness temperature are used to confirm the convective influence. Lower ozone mixing ratios are found in humid air masses, which are shown to be originating from the boundary layer. Part of the hydration is associated with tropical cyclones

passing the area around the island.

2 Overall impression and rating

The overall impression of the manuscript is good in general. The manuscript is well structured and the text is mostly easy to understand. I agree with most of the interpretations and I think you are right that most parts of the humidity can be linked to convection. The measurements and the message of the manuscript is a good contribution to the community. However, I have one concern of handling the relative humidity profiles with respect to clouds (see below). For these reasons, I recommend publication in ACP after major revisions.

3 Major comment:

My main concern in this study is the handling of RH observations itself and with respect to clouds. Cirrus clouds is the prevailing cloud type in the altitude range used in this study. These clouds are often found in the outflow of convective systems (e.g. Fierli et al, 2008) but they occur also due to other dynamical situations like warm or cold fronts, gravity waves etc. which produces also a slow to moderate uplift of air (see Kraemer et al, 2016). Clouds are always an indication of uplifted moisture, but they also redistribute water vapor to lower altitudes due to sedimentation of ice particles. Therefore, they can also partly weaken the hydration signatures in your observations.

In the whole study, all humidity measurements are shown as relative humidity with respect to water (I guess) which is the standard output of meteorological radiosondes. In the used altitude region (10-13 km) relative humidity with respect to ice would be the better quantity to identify cirrus clouds. For example, on page 5 (lines 161-166) the

60% RH (with respect to water) corresponds to 100% RH with respect to ice (RHi) at assumed temperature of 220K (higher RHi with even lower temperatures). RHi values around 100% are an indication for clouds. And I think there are a lot of cloudy profiles in the measurements, as you can see for example in Figure 3 or 4.

Figure 5 shows the distribution with all RH measurements between 10 and 13 Kilometers. The measurements between RH of 50 to 60% which corresponds approximately to 90-100% RHi are most likely in cloud observations of cirrus. This could be discernible by a small increase in the number of observations between 50 to 60%. Since, the most frequent Rhi is expected to be around 100% RHi due to thermodynamical reasons (see Kraemer et al, 2016). Another example is visible in Figure 4 where a RH of 90% (April 2015) is shown, which would correspond to RHi of 136-167% for temperatures of 230-200K.

If there is a significant amount of cloudy profiles within you measurements, I'm sure that there is also a part of it not generated by convection and formed by other uplifting prozesses (examples see above). These uplifting prozesses just transports air masses more from the middle troposphere into the upper troposphere and not from the boundary layer. This can be also seen by Figure 10. There are a lot of signatures in the mean relative humidity where no indicator either sRTLT or the product of RTLT and DCCO create a coincident signature. This could also partly explain the higher ozone concentration found in moist profiles in the upper troposphere compared to other studies, because the air is just originating from higher altitudes above the boundary layer with a higher ozone concentration.

Another point, is the usage of relative humidity. The RH as well as RHi depends beside water vapor also strongly on temperature. If you see an increase in relative humidity it can also be due to a temperature decrease. Therefore, I suggest to show in Figure 4, a seconds panel with the water vapor mixing ratios between 10-13 km instead of RH to confirm that the summertime increase in RH is due to an increase in the water vapor concentration and not only due to a temperature artifact/anomaly.

In general, I think it is correct to use RH as an indication of hydration, but at least there must be more discussion about the usage of RH like the dependence on temperature. I still agree with the main message of the study. However, I think that the humidity measurements must be discussed in a more balanced way including also a short discussion about the effect of clouds also in combination with Figure 10.

4 Specific comments/questions:

- Page 1, line 31: Maybe add here Riese et al. 2012. They showed nicely how changes in ozone and water vapor due to mixing processes change the radiative budget of the UTLS region.
- Page 2, line 48: "Marine boundary layer to the upper troposphere (Jorge ozone chemical lifetime is on the order of 50 days,". Can you please specify where you have the information from and for which altitude the stated lifetime of ozone is valid. Usually you have a strong increase of ozone lifetime with altitude in the troposphere.
- Page 3, lines 81-82: It would be consistent and also helpful to also report about precision and uncertainties of radiosonde measurements of the Meteomodem M10 sonde. In particular, the uncertainties of the humidity sensor would be helpful for the interpretation of the measurements.
- Page 4, lines 124-126: Can you state something about how frequent you detect the anvil outflow instead of the anvils. Maybe you have a rough estimate. Also I'm wondering about in situ formed cirrus clouds which are typically colder than 230K. Are they not detected by the Meteosat 7 SEVIRI instrument? If the they are detected , they would also strongly distort your statistics of DCCO. Can you please comment on this also.

- Page 4, line 130: What is actually meant by "backtrajectories were calculated every hour at 0.25° resolution...." ? The resolution of the wind field is already mentioned in the text. And can you please also add the vertical resolution in altitude range of interest. Please add also information about the temporal length of the backward trajectory calculation.

- Page 4, lines 130-132: Are you sure that the ECMWF forecast data are used for the backward trajectory calculation and not the analysis data? The deviation between forecast and analysis data could be large. The analysis data would better represent the real meteorological fields.

- Page 5, 172-174 in combination with Figure 4: It would be helpful, if you could include the monthly mean as a time series in the plot (e.g. black line). Because of the strong scatter, it is difficult to see where the mean value of each period would be. With a monthly mean RH climatology in addition to the scatter, it would be easier to see.

- Page 6, 192-195 in combination with Figure 5: It would be good to discuss the measurements in cloud at this point. Especially the increase in the distribution between 50-60% RH. Additionally, a the line which marks approximately the 100% RHi in Figure 5 would help to see which observations could be potentially effected by clouds. All measurements strongly above 60% RH could be potentially clear sky observations again, because in clouds the humidity is diminished by the diffusional growth of ice particles to the thermodynamical/dynamical equilibrium around 100% or slightly above.

- Page 6, lines 200-220: The process of mixing typically depends on the dynamics (e.g. wind shears, wave breaking etc.) but also on time as you wrote. I suggest to include a plot or at least some numbers how long the air masses stayed in the upper troposphere after the convective uplift and the time of the measurement. This would give some indication if the duration is on the typical timescale of mixing.

- Page 8, lines 252-253: I agree, that the signature in the RH profiles around 11km could be potentially from the cyclone. But I'm also sure that there is also a cloud layer at 11 km altitude. Because the RHi is around 100% (RH eq. 60%) and the decrease in the humidity below the layer is slower than above the layer which indicates the hydration effect due to sedimented ice crystals. Here, I would suggest a bit more discussion about that.

- Page 9, lines 299-305 in combination with Figure 10: There are many peaks in RH which do not correspond to any signature in both trajectory products (e.g. around 01/Dec 2015, End of Jan 2015, beginning of summer 2015/2016 and many more). It would be good, if there would be more discussion in the text to explain those peaks and possible reasons (see also my main comment above).

- Page 9: In Section 3.4.3 the geographical origin of convection is discussed with the help of the product of DCCO and RTLT. In text the discussion is linked to the respective cyclones. It would really be helpful to follow the discussion, if the names or at least the letters of the occurring cyclones (A-F) are written in each subplot of Figure 11. For example for March 2015 G and H or the full names of the cyclones.

5 Technical comments/suggestions:

- Page 4, line 123: period of "the" study

- Page 4, line 126: to our treatment "of" convective

- Page 5, lines 141-142: ". Residence times are computed using the model gridded output domain (1°x1° grid cells) values combining the results of the 8 3-hourly runs to provide a daily estimate of the source regions for air particles." This sentence is difficult to understand. Can you please rephrase.

- Page 5, line 146 and 147: "low" should be "lower" ?
- Page 8, line 269: Skip the word "was"
- Page 9, lines 288-290: Sentence needs to rephrased " estimated by correspondes ".
- Please check the capitalization of the word Figure in text (e.g. page 8, line 273). It should be consistent throughout the manuscript.
- Figure 8: It would be good, if you could include the location of Reunion island in the maps.

6 References:

- Fierli, F., Di Donfrancesco, G., Cairo, F., Marécal, V., Zampieri, M., Orlandi, E., and Durry, G.: Variability of cirrus clouds in a convective outflow during the Hibiscus campaign, Atmos. Chem. Phys., 8, 4547–4558, https://doi.org/10.5194/acp-8-4547-2008, 2008.
- Krämer, M., Rolf, C., Luebke, A., Afchine, A., Spelten, N., Costa, A., Meyer, J., Zöger, M., Smith, J., Herman, R. L., Buchholz, B., Ebert, V., Baumgardner, D., Borrmann, S., Klingebiel, M., and Avallone, L.: A microphysics guide to cirrus clouds – Part 1: Cirrus types, Atmos. Chem. Phys., 16, 3463–3483, https://doi.org/10.5194/acp-16-3463-2016, 2016.
- Riese, M., F. Ploeger, A. Rap, B. Vogel, P. Konopka, M. Dameris, and P. Forster, Impact of uncertainties in atmospheric mixing on simulated UTLS composition and related radiative effects, J. Geophys. Res., 117, D16305, doi: 10.1029/2012JD017751, 2012.

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