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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Dehydration in the tropical tropopause layer estimated from the water vapor match" *by* Y. Inai et al.

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General:

Inay et al. present an analysis of in-situ measurements of ozone and water vapour in the TTL over the Western Pacific obtained during the SOWER campaign. They compare observations from different locations for cases where they have indications based on trajectory calculations that two stations observe the same air mass, one observation being downstream of the other. Comparison of the two measurements then is used to constrain what has happened to these airmasses between the two observations, with a focus on dehydration. The data is very interesting, and the analysis is very carefully done. I am a little surprised that no remote sensing data, e.g. MLS/Aura, is used to better establish the larger-scale structure of the water vapour field during the period

of in-situ observations. One may be a bit disappointed that - ultimately - these observations seem insufficient to make a substantial step forward. However, I think this is also an important result, and I recommend publication of this paper. Below, I provide a list of "minor" comments/questions the authors may want to consider for the revised version. Also, I strongly recommend to re-think the organisation of the paper. I do not want to make specific suggestions, but in its present form method description, case studies and more general statements are presented in a way that tends to leave the reader confused.

Signed review, S. Fueglistaler, March 2013.

Minor comments:

Abstract:

L1-4: Sentence is confusing, be more specific what is poorly understood.

L4: "Match method" - this term is not broadly known.

L7: Add information on dataset used for the trajectories.

L15: Unclear here how the accuracy is determined (i.e. to what does 1-sigma refer - termperature uncertainty, or spread of results for different "matches"?)

Text:

P636/L15: I don't think that this absolute statement ("Variations in SWV are poorly understood.") reflects the state of understanding adequately. I think that to leading order variations may be actually even better understood than absolute values (see Fueglistaler et al., 2013; I do not ask to cite this paper, but it lays out the arguments better than what I can do here in this review).

P636/L26: "Cold trap" - there is no definition of what this term is supposed to mean. It refers loosely to the notion that the quasi-stationary temperature field shows a clear zonal structure, but when considering the full space-time varying temperature field,

nucelation may occur anywhere, it's just that the probability may be higher in this region. I recommend to not use this term - it has created enough confusion in the community. It is entirely sufficient to say that you have a set of observations in the regions of on average lowest temperatures at tropopause levels.

P645/L13ff: The fact that you find dehydration below 360K is interesting because in general at that level the horizontal temperature gradient in the region of the observations is not very large, I would think. What can be deduced from the fact that apparently in the layer where gradients should be largest, least dehydration is observed? Are the temperature variations along the isentropic trajectories primarily wave events, or is latitudinal motion important? (I.e. the gradient may be larger in latitudinal than zonal direction?)

For example, when looking at Figure 5b, is the oscillation because of a wave traversing the area, or because of a latitudinal gradient of the isentropes? (Discussion on page 646, line 1-18 does not say much about this.)

P649/L18ff: Yes, that's an interesting observation - even more so given that Figure 8 shows actually a local maximum in ozone at 80hPa, which seems not consistent with the explanation provided either (i.e. if injection were higher up, and the local H2O maximum below is due to evaporation of sedimenting condensate, I would expect a local minimum in ozone aloft).

p650/L15: I don't think this is true - the broad general statement would be that convection reaches up to about the level of neutral buoyancy, not the level of zero net radiative heating.

P650/L18: Strictly speaking, the ascent is not "caused" by radiative heating, rather, radiative heating balances the dynamically forced ascent.

P653/L11: Replace "there are little" with "there is little".

P655/L15ff: This sentence does not make sense to me - less efficient than what?

P656/L1: Boehm et al. is an interesting paper, but reference for sedimentation velocities should be probably the book by Pruppacher and Klett, or a reference therein.

P656/L20: There is a lot of "if we could" in this section here - this section can be shortened.

Figures:

The paper shows a number of scatter plots showing first versus second observation. In addition, the temperature histories along the trajectories are shown for some specific cases. Would it be possible to make a figure that shows the temperature evolution between two points for all matches, with temperature shown relative to that of the first observation? (From a statistical point of view, one might expect temperatures between the two observations to be both higher and lower than at the first observation - but, if I understood correctly - above 360K this seems not to be the case.) It would then also be instructive to show this relative temperature a few days upstream for all matches.

Figure 8: Caption - replace "those" with "the".

References:

Fueglistaler et al., The relation between atmospheric humidity and temperature trends for stratospheric water, JGR, 118, 1052-1074, doi:10.1002/jgrd.50157, 2013.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 633, 2013.