

Interactive comment on “Cloud-scale ice supersaturated regions spatially correlate with high water vapor heterogeneities” by M. Diao et al.

Anonymous Referee #2

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General Comments

Diao et al. present an analysis describing the observed dependence in the variability of RHi and regions supersaturated with respect to ice (ISSRs) on fluctuations in water vapor (H_2O) and in temperature (T) in the mid to upper troposphere, focusing on the different conditions inside and outside of ISSRs. The dataset is valuable, including a fair sampling of northern and southern hemispheres. The analysis is unique, being the first to examine this dependence using a large dataset of aircraft measurements with the ability to observe changes in relatively small spatial scales of $\approx 200m$.

On large scales, it is well established that T controls the dehydration of air entering the stratosphere, and therefore in the tropical upper troposphere T must be the dominant

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controlling factor for RHi in this region. While Diao et al. observe that ISSRs are always more humid than the surrounding sub-saturated regions (Eularian view), the transition of each ISSR air parcel from sub-saturated to super-saturated necessarily took place via a decrease in the local temperature (Lagrangian view). Therefore the conclusion that the RHi variability is dominated by local variability in H_2O is somewhat unexpected.

The data are of high quality and the analysis valuable. Therefore this paper deserves publication in ACP and will likely provide a point of reference for discussions about cirrus formation. The current manuscript is largely an account of the phenomenology of ISSRs, and I believe that the authors could improve the manuscript and our understanding of dehydration processes with a small amount of additional effort added to section 5. For example, can anything be said about typically how much mixing is required to explain the observations? What scale of vertical displacements are required to generate the variability observed from a typical vertical water vapor profile? Is there any indication from the chemical tracers (e.g. O_3 , CO) that large scale deep convection is frequently important? Is there a clear signal of convection over the continent that differs from the ocean flights, or a seasonality to the observations? Can any specific dynamical processes be ruled out or tentatively identified as likely to be the most important?

Below are a number of specific comments, suggestions and questions for the authors to consider.

22251 L2: and L22-23: “the Earth’s.” -> “Earth’s.”

22252 L10-11: “saturation vapor pressure from the Clausius-Clapeyron Equation.” -> “saturation vapor pressure.”

22252 L12: “location of the ice crystal...” -> “location of ice crystal...”

22253 L3: “This study showed” -> “That study concluded”

22253 L19: It doesn’t seem to me to be quite accurate to say that the Clausius-

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Clapeyron equation is really used here, i.e. your equation 1a is a derivative of the definition of RH_i, which does not require this equation.

22254 L22: Please define uncertainties, e.g. 1σ , 2σ , etc.

22255 L6: What about uncertainty in the equation used to calculate saturation vapor pressure from T? Please state the formulation used to calculate e_s (e.g. Murphy-Koop, Goff-Gratch, etc.).

22255 L25: The 2DC seems to be a much less confident cloud indicator than the SID, primarily since it cannot see particles smaller than 25 μm . Is it safe to assume that if SID count = 0 there is no cloud? In the SID case, why are cases where $0 < N_c < 60$ / liter considered "clear sky?" It might be best to not use that data.

22256 L7: "... using a vacuum ultraviolet resonance *fluorescence* instrument with ..."

22256 L8: I did not think that the IWC could be measured directly. Was IWC calculated using the measurements of CLH enhanced total water and VCSEL water vapor? Please clarify.

22257 L 15: This is not a Taylor expansion here, eqn 1a is a statement of the product rule.

22259-22260: I am somewhat confused about the deviation equations defined here and have a few questions. 1) Why are the denominators in eqn 3 $N-1$ instead of N ? 2) Why was eqn 3a used instead of the typical RMS deviation from the mean (standard deviation)? 3) What is the motivation for considering positive and negative contributions to the deviations (3b and 3c)? For large enough N , I would have expected these to be equal to zero, instead of carrying information about the variability in RH due to q or T . I would consider either using a more typical definition of $\sigma\text{RH}_{i,q}$ and $\sigma\text{RH}_{i,T}$ or more clearly motivating the use of eqn 3, either briefly in the text or in an appendix.

22261 L8: "latitudes" -> "latitude"

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22261 L 20 – 25: When discussing the RH_i PDF, it might be worth reminding the reader what the total uncertainty (accuracy) in the measurement of RH_i is.

22262 L4: The T range stated here is different than that stated on page 22255 L5.

22262 L11: I would say "< 20%", since 0% RH_i is never observed.

P22262 and Fig 5: Fig 5 shows a few points above the liquid water saturation line. Although it is noted in the text that these points are rare, these data are really expected to never occur. Can these be explained by uncertainties in the RH_i? Also, there are quite a few (\approx 50%) RH_i in cloud points. I agree that some of these could be due to ice crystals falling into unsaturated regions. I wonder if some of these could be due to time lags between particle and WV measurements at cloud edges? For example, looking at figure 6a it looks to me like shifting the water vapor forward in time a couple of seconds might make the WV and cloud detection features typically line up better.

In Fig 6a it would be helpful to show more of the measurements, e.g. altitude, T, H₂O.

Fig 6b: I would consider removing the fits from fig 6b unless they are discussed in the text, especially for the ISSR length plot which doesn't look like a good linear fit.

22265 L9-11: I'm not sure I follow this statement the way it is written. If you are measuring in a clear sky, how would you know that a small region with locally higher WV was not a result of ice crystals that sedimented and sublimed in that air parcel? Also, "evaporation" should be replaced with "sublimation."

22265 L12- 22266 L3: As mentioned the measurement of w is challenging, and as stated earlier in the paper the precision is much higher than the accuracy, meaning that a result might look to be meaningful within the noise, while still inaccurate. I suggest either removing this paragraph, or discussing in more detail what the typical dw values were, and how much confidence there is in the sign of the dw values.

22264 and Fig 7b: It doesn't make sense to me that the y-intercept could be significantly different than one.

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22268 L11-15: Pan et al. use this relationship between O_3 and CO to determine a local tropopause (fit to all data with $O_3 < 70$ ppb). I believe that fitting all of the data over the entire latitude range does not provide accurate information about the troposphere boundary. Also, the data above the dashed red line would be outside of the troposphere, not necessarily inside the transition layer. Some of those points have high O_3 and may be in the lower stratosphere.

22270 and Fig 14: The point of this figure is to demonstrate that the required variability in T to produce the observed RHi variability is much greater than was observed. This is a nice way to make this point, but the 2K bias that is indicated is a result only of the somewhat arbitrary choice of this segment in the time series, which includes a low RHi segment at the beginning. I would suggest not drawing attention to the lower T required on average to reproduce RHi.

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