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Atmospheric Chemistry and Physics Discussions
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Interactive comment on "Cloud-scale ice supersaturated regions spatially correlate with high water vapor heterogeneities" *by* M. Diao et al.

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Response to Reviewers Format: reviewers' comments are quoted between asterisks.

Line number in the response refers to the revised manuscript with highlighted changes.

Quotation stands for revised/added text in the revised manuscript.

Overall comment: We thank the two reviewers for their helpful comments, suggestions and points of clarification. Below are their individual comments, our detailed responses to them, and the corresponding changes to the manuscript where appropriate.

Response to Reviewer #2

*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 (H2O) 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 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 H2O 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. O3, 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?*

We thank the reviewer for these interesting questions. To address the questions that Reviewer #2 has raised, we added more comments in the discussion on the importance of future study to quantify the contribution of individual dynamical processes. Also we added new Table 3 for analyses at different latitudes, over ocean versus land, as well as at different times of a year. The responses to these questions are similar to our response to Reviewer #1's general comments. We attached these responses to Reviewer #1 as below:

We appreciate the reviewer for pointing out the importance of large scale environments for the ISSR formation. We note that our observations of the dominance of H2O spatial variability over relative humidity (RH) horizontal spatial variability is a ubiquitous feature from the surface to the tropical upper troposphere (UT) or extratropical lower stratosphere (LS), over both ocean and land, at five different times of the year, and in both hemispheres (shown in new Table 3). This means that the feature happens even at regions with different large scale dynamical background, such as tropics and polar region. Two case studies of Spichtinger et al. (2005 a and b) have previously reported that certain large scale dynamics could contribute to the formation of ISSRs in the extratropical regions, such as warm conveyor belt and gravity waves. Yet it requires a lot more efforts to assess the role of individual types of dynamics in generating this global feature of the strong contribution of H2O variability to RH variability. The analyses deserve a new set of work to case study each type of dynamical background during the ISSR observations. In addition, for the suggestion on back trajectory, it would requires the knowledge of 3-D wind field on ∼200 m high resolution on the global scale in order to resolve the origins of these microscale ISSRs, and the current models still have large uncertainties to generate a global wind field at this high resolution. For the suggestion on the contribution of gravity waves, because the HIPPO and START08 campaigns do not target on gravity waves, there were very limited observations on gravity waves with a clear structure. We mentioned this in the first version of manuscript that the gravity wave with "a clearly observable wave structure. . .were not typically seen during the START08 and HIPPO flight campaigns" (22256 Line 16-19). For the suggestion on the contribution of turbulence, we add comments on the necessity of future study to quantify its role in generating H2O spatial variabilities. We feel that if we only focus on one or two dynamical process in generating H2O variability in this work, it would be biased and misleading as if those processes are the dominant cause of the global feature of H2O variability. Thus a more comprehensive work in the future is needed to fully compare each dynamical process in generating H2O variability.

We add comments in the discussion to point out the importance of future work on analyzing the roles of dynamical processes in ISSR formation (Line 629-641): “The formation of the microscale ISSRs are likely attributed to many dynamical processes on various scales. For example, on the microscale, processes such as small scale turbulence, small gravity waves, entrainment mixing and ice crystal sedimentation could generate the microscale spatial variability of H2O, which defines the location and magnitude of microscale ISSRs in the horizontal Eulerian view. On the other hand, mesoscale processes such as uplifting, large scale gravity waves and deep convection could contribute to the overall environment of cooling, which lower T and increase RH in the Lagrangian view. Future work is needed on both scales in order to quantify the contribution of individual processes to the formation of ISSRs. For example, back trajectory analyses are needed to assess the roles of large scale dynamics in setting the overall cooling environment, while tracer analyses are needed to assess the roles of turbulence and mixing between air masses in contributing to the heterogeneities of H2O field. In particular, specific case studies will be helpful in order to quantify the scales of vertical displacements that can generate the observed H2O variability based on a local H2O profile.”

In order to show that our conclusion holds at all latitudes, over ocean and land, as well as at different times of a year, we added new Table 3 and explained in Line 531-537: “In order to examine whether the dominant contribution of H2O variability varies with latitudes, seasons and between over land and ocean, we analyzed the contribution of H2O and T variabilities to the 1 Hz dRHi at different P bins. Table 3 shows that the H2O variability contribution at each pressure bin does not vary significantly when binned by latitude (0–30°N, 30°N–60°N, 60°N–87°N, 0–30°S, 30°S–60°S, 60°S–67°S), nor does it vary between observations over land and ocean, or at different times of a year.”

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."

Revised according to the reviewer’s suggestion.

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

Revised.

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

Revised.

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

Revised.

22253 L19: It doesn’t seem to me to be quite accurate to say that the Clausius Clapeyron equation is really used here, i.e. your equation 1a is a derivative of the definition of RHi, which does not require this equation.

Revised as: “To compare the contributions of H2O and T to RHi variabilities, the Clausius Clapeyron equation is usually used to decompose the changes of RHi are separated into the contributions from changes of H2O and T.”

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

We added the uncertainties based on Zondlo et al. (2010) in Line 124-125: “Noise levels (1σ) are typically 1–3% at 25 Hz for flight measurements.”

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

We added the comments in Line 134-136: “The saturation vapor pressure is calculated based on Murphy and Koop (2005), which stated that all the commonly used expressions for the vapor pressure over ice are within 1% of each other at 170–273 K.”

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 um. Is it safe to assume that if SID count = 0 there is no cloud? In the SID case, why are cases where 0 < Nc < 60 / liter considered “clear sky?” It might be best to not use that data.

We added comments on the caveat in our definition of “clear-sky” and “in-cloud” data and point out that the vagueness in the definition will not influence our conclusion (Line 157-165): “We note that there might be a small amount of data that should be “in-cloud” but are categorized as “clear-sky” condition, but these data only contribute to a very small percentage of the total clear-sky data. For example, the SID-2H measurements at 0 < Nc < 0.06 cm-3 could be either liquid aerosols or ice crystals, but these data only contribute to 1.9% of the total “clear-sky” data at T $\leq$ 40 °C. In addition, for 2DC probe, ice crystals smaller than 25 μm are not detected. But according to the measurements of SID-2H, the observations with ice crystal mean diameter < 25 μm is only 1.6% with respect to the total “clear-sky” data at T $\leq$ 40 °C. Therefore, even if these “clear sky” data were indeed clouds, they are not going to impact our results significantly.”

22256 L7: “. . . using a vacuum ultraviolet resonance fluorescence instrument with . . .”

We rewrote the sentence in Line 176-178: “CO was measured using the NCAR vacuum ultraviolet resonance fluorescence instrument with an accuracy of 9% and precision of 0.8 ppbv (Tilmes et al., 2010).”

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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.

We clarified in Line 180-182 that: "Ice water content (IWC) value is derived by deducting the water vapor measurements of VCSEL hygrometer from the total water content measurements of the CU hygrometer."

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

We thank the reviewer for pointing out the typo in equation labels. We revised the sentence in Line 214-216: "We assume that the magnitudes of d(1/es) and de terms are relatively small, so that only the first terms of the Taylor expansion are considered in Eqs. (1) and (2) (1b) and (1c)".

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 σRHiq and σRHiT or more clearly motivating the use of eqn 3, either briefly in the text or in an appendix.

To address reviewer's question 1) of using N-1: Theoretically, when a population mean is known, 1/N is used to calculate the population variance. But for a population with unknown mean value (such as an experiment or observation with limited sampling), the Bessel's correction is applied to the calculation of population variance. That is, 1/(N-1) is used to instead of 1/N because the latter underestimates the population variance. This is equivalent of multiplying N/(N-1) when calculating the population variance, and this factor of N/(N-1) is also the Bessel's correction factor.

To address reviewer's question of "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", we clarified the meaning of Eqs. (3a, b and c) and the sign function. We note that the original explanation in the original manuscript was not fully right, and we revised in Line 269-280: "According to these definitions, when the changes in H2O concentration have the same sign as the changes in RHi, the H2O contribution to RHi fluctuation is positive, and σRHiq term is positive. Similarly, when the changes in T have the opposite sign as the changes in RHi, σRHiT term is positive. The σRHi term should always be greater or equal to zero, because it is the sum of absolute values. Therefore at least one of the terms of σRHiq and σRHiT should be non-negative, which means that at least one of the H2O or T fluctuations positively contribute to the direction of RHi fluctuation. For example, for a segment of aircraft observations of fluctuating RHi, when the RHi value at one point is smaller than the mean RHi value, there should be lower H2O concentration or higher T, or both, at this point with respect to the mean H2O and T values of the segment. Using Eq. (3) can help us separately quantify the contributions of H2O and T to the fluctuation of RHi, no matter the RHi change is positive or negative at each point." Thus the sign of σRHiq and σRHiT term is actually not related with the sign of RHi changes, but rather defined by the direction of H2O and T changes with respect to the direction of RHi changes. Therefore the sum of σRHiq and σRHiT over a large sample is not going to average out to zero. In fact, most σRHiq and σRHiT terms are positive, as shown in Fig. 11B, since most of them both positively contribute to RHi changes at the same time. Yet σRHiq term is almost always greater than σRHiT term.

To address reviewer's question 2) and 3) of why using this method, we added the clarification of why we use this new method of mean absolute deviation of RHi (σRHi) instead of the typical root mean square (RMS) deviation (Line 281-296): "We note that the current method differs from the previous variability analyses of Gierens et al. (2007). Although Gierens et al. (2007) first decomposed dRHi into the fluctuations of H2O and T (similar to Eq. (1a) in this work), they however used the standard deviations

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of H2O and T to compare the contributions of H2O and T variabilities to RHi variability. The standard deviations of H2O and T do not account for the direction of their changes, and therefore cannot address whether the fluctuations of H2O and T are in the same (or opposite) direction with RHi fluctuation. For example, for a times series of continuously increasing RHi with increasing H2O and T, the increasing T actually provides a negative contribution to the increasing RHi along the time series. But this negative contribution of increasing T is treated as the same as the positive contribution of increasing H2O when comparing the standard deviations of T and H2O. In other words, using the expression of standard deviation for this type of analyses would overestimate the contribution to RHi variability when the changes of H2O (T) are in the opposite (same) direction of RHi changes."

22261 L8: "latitudes" -> "latitude"

Revised.

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

We added the comments in Line 340-341: "In addition, the uncertainties from different measurements may also contribute to the differences in RHi peak, for example, the uncertainty of RHi is 8–14% in this study."

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

We thank the reviewer for pointing out this mistake. We corrected the range for calculating RHi uncertainty in Line 132-134: "The uncertainties in RHi were 8–14% at 233.15–196 233–205 K after combining the uncertainties from the VCSEL hygrometer (6%) and the T probe (± 0.5 K)."

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

Revised.

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 RHi? Also, there are quite a few (≈< 50%) RHi 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.

For the few data over the liquid water saturation line, we add comments in the revised manuscript in Line 348-350 that: "We note that there are a few points in Fig. 5 that have higher RHi than the liquid water saturation line, but the differences between these points and the liquid water saturation line are still within the combined uncertainties of 8–14% in RHi."

We note that the grey area in the original Fig. 6A was only meant to show ISSRs more clearly and does not stand for clouds. This was mentioned in the original legend of Fig. 6A in page 22287: "Fig. 6. Aircraft observation of ISSR. (A) ISSR 1-D segment lengths (L_ISSR) (grey area) and spacings (L_spacing) (blank area)." In addition, the time mismatch between the grey area and the ISS was a mistake in the graphing display (they both stand for ISSRs and should be exactly synchronized). It has nothing to do with the correlation between ice crystal and water vapor measurements. We removed the grey area from Fig. 6A to clear the confusion.

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

We added the measurements of altitude, T and H2O into Fig. 6A. The descriptions of these time series are also added in Line 360-362: "A typical time series of the aircraft observations of RHi (black line), T (red dotted line), H2O (blue) and altitude (green) are shown in Fig. 6A. The ISSR is defined as the region where RHi is consecutively above 100% (thick red line)."

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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.

Please see our response to Reviewer #1's comment on Page 22263 Line 10-20 of the original manuscript. Three paragraphs are revised from Line 354-406.

In particular, for Reviewer #2's comment on using the power law distribution to fit the number of events of ISSRs versus the ISSR length. We clarifies the reason of using the power law fit since the previous work by Wood and Field (2011) also used the power-law distribution to fit the distribution of cloud length. This is explained in Line 375-382: "Because the previous work of Wood and Field (2011) used a power law distribution to fit the distribution of 1-D cirrus cloud chord length, here we also apply a power law fit to the distribution of ISSR lengths (red dotted line in Fig. 6B), that is, log10(Number of events of ISSR) = a + b × log10(ISSR length). The intercept and slope of the fit are 4.7 ± 0.4 and -0.77 ± 0.11, respectively. The ± one sigma represents one standard deviation for all linear fit in this work. The slope -0.77 ± 0.11 of ISSR length distribution observed in this study is comparable with the slope -1.66 ± 0.06 fitted for cirrus cloud length distribution in Wood and Field (2011)."

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."

We agree with the reviewer's comment and removed the comments on the causal relationship between ice crystals and water vapor heterogeneities in Line 459-463: "These results demonstrate that the strong contributions of H2O heterogeneities to the spatial characteristics of ISSRs exist before, during and after ice nucleation., which suggests that the H2O spatial heterogeneities are not just a result of ice crystal sedimentation and sublimation, but are already in place well before ice nucleation."

*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. *

We agree with both reviewers' suggestion on deleting the discussion on vertical velocity variations due to the large uncertainties in these measurements. The original Fig. 10 and the discussion are therefore deleted, which includes the discussion of vertical velocity measurements in data and instrumentation, the result section and the discussion section.

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

We clarify that the intercept of the fits in Fig. 7B indeed should be almost zero (not one, as suggested by the reviewer). It should be zero because when dRHimax approaches zero, the dRHiq and dRHiT term should also approach zero. Thus the intercept of the linear fit for dRHiq and dRHiT should be almost zero. We clarify the value of the intercept in Line 435-439: "We note that for the linear fit of dRHiq and dRHiT, the (%) sign in the intercept is omitted in order to be consistent with the RHi value on the ordinate. For example, the intercept for the fit of dRHiq in Fig. 7B stands for 0.19 (%) ± 0.07 (%), which is very close to zero. This is because that when dRHimax approaches zero, the dRHiq and dRHiT term should also approach zero."

22268 L11-15: Pan et al. use this relationship between O3 and CO to determine a local tropopause (fit to all data with O3< 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 O3 and may be in the lower stratosphere.

We note that the stratospheric section of the chemical tracer plot is actually outside the scope of the current Fig. 12A (original Fig. 13A) because Fig. 12A only zooms in the ISS part of the whole tracer plot. In fact, all the points in Fig. 12A are on the right hand side of the fit for chemical stratosphere. We have to zoom in from the full picture in order to clearly show the mixing lines of ISS observations. To clarify this point, we added comments in Line 556-566: "The stratospheric branch is fitted by a quadratic polynomial fit: x = a0 + a1y + a2y2, where y is O3 (ppbv), and x is CO (ppbv). σx is the standard deviation of the residuals. Here a0 = 15.0 $\pm$ 0.2, a1 = 0.0299 $\pm$ 0.0005, a2 = -2.37E-05 $\pm$ 3E-7, and σx = 0.667. Above the fit of chemical troposphere and on the right hand side of the fit of chemical stratosphere is the extratropical transition layer (Pan et al., 2004). In Fig. 12A, the linear fit of the chemical troposphere and $\pm$ 3 σy of the fit are shown as the black and red dotted lines, respectively. We note that Fig. 12A is only a part of the whole O3-CO tracer plot and therefore the fit of the chemical stratosphere is not shown in Fig. 12A. All the ISS observations in START08 happened either in the chemical troposphere or in the transition layer."

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.

We clarified that the 2 K bias is only with respect to this specific example in Line 616-619: "In addition, if one uses T fluctuations to compensate for the neglected H2O fluctuations, even though the RHi field is the same, the generated T in ISSRs would be unnecessarily much lower (by up to ~2 K in this example, Fig. 13C) than the observed T in ISSRs (Fig. 13A)." We disagree with the reviewer that the low RHi segment at the beginning would influence the calculation of T bias. This example is a Eulerian view of the observed 1-D spatial distribution of RHi, T and H2O, which means that even if the part of the 1-D segment in Fig. 13 is cut off, the remaining segment of RHi would

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still have the same value of RHi, T and H2O. Thus the T bias in Scheme 3 (neglecting H2O variability and using fake T spatial fluctuation) would be the same even if only a section of the data are analyzed, and the largest T bias would still happen in ISSRs (up to ∼2K), which is not affected at all by what other parts of the observation are in space.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 22249, 2013.

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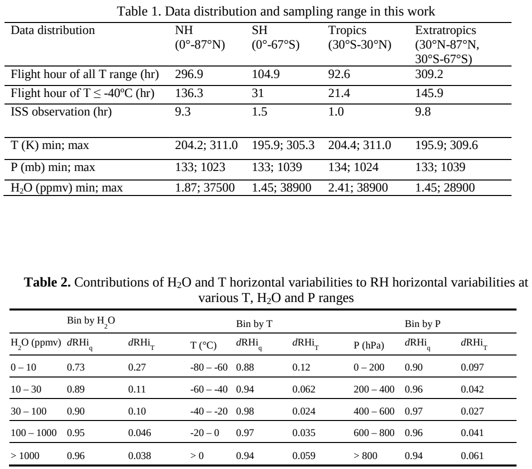

Table 1. Data distribution and sampling range in this work

Data distribution	NH (0°-87°N)	SH (0°-67°S)	Tropics (30°S-30°N)	Extratropics (30°N-87°N, 30°S-67°S)
Flight hour of all T range (hr)	296.9	104.9	92.6	309.2
Flight hour of T ≤ -40ºC (hr)	136.3	31	21.4	145.9
ISS observation (hr)	9.3	1.5	1.0	9.8
T (K) min; max	204.2; 311.0	195.9; 305.3	204.4; 311.0	195.9; 309.6
P (mb) min; max	133; 1023	133; 1039	134; 1024	133; 1039
H_2O (ppmv) min; max	1.87; 37500	1.45; 38900	2.41; 38900	1.45; 28900

Table 2. Contributions of H_2O and T horizontal variabilities to RH horizontal variabilities at various T, H_2O and P ranges

	Bin by H_2O			Bin by T			Bin by P	
H_2O (ppmv)	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$	T (°C)	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$	P (hPa)	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$
0 – 10	0.73	0.27	-80 – -60	0.88	0.12	0 – 200	0.90	0.097
10 – 30	0.89	0.11	-60 – -40	0.94	0.062	200 – 400	0.96	0.042
30 – 100	0.90	0.10	-40 – -20	0.98	0.024	400 – 600	0.97	0.027
100 – 1000	0.95	0.046	-20 – 0	0.97	0.035	600 – 800	0.96	0.041
> 1000	0.96	0.038	> 0	0.94	0.059	> 800	0.94	0.061

Fig. 1.

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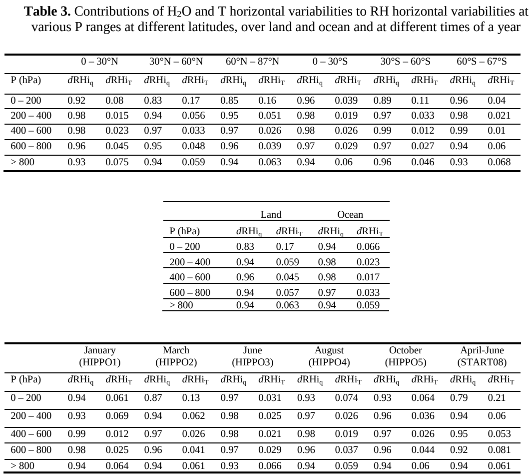

Table 3. Contributions of H_2O and T horizontal variabilities to RH horizontal variabilities at various P ranges at different latitudes, over land and ocean and at different times of a year

	0 – 30°N		30°N – 60°N		60°N – 87°N		0 – 30°S		30°S – 60°S		60°S – 67°S	
P (hPa)	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$
0 – 200	0.92	0.08	0.83	0.17	0.85	0.16	0.96	0.039	0.89	0.11	0.96	0.04
200 – 400	0.98	0.015	0.94	0.056	0.95	0.051	0.98	0.019	0.97	0.033	0.98	0.021
400 – 600	0.98	0.023	0.97	0.033	0.97	0.026	0.98	0.026	0.99	0.012	0.99	0.01
600 – 800	0.96	0.045	0.95	0.048	0.96	0.039	0.97	0.029	0.97	0.027	0.94	0.06
> 800	0.93	0.075	0.94	0.059	0.94	0.063	0.94	0.06	0.96	0.046	0.93	0.068

	Land		Ocean	
P (hPa)	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$
0 – 200	0.83	0.17	0.94	0.066
200 – 400	0.94	0.059	0.98	0.023
400 – 600	0.96	0.045	0.98	0.017
600 – 800	0.94	0.057	0.97	0.033
> 800	0.94	0.063	0.94	0.059

	January (HIPPO1)		March (HIPPO2)		June (HIPPO3)		August (HIPPO4)		October (HIPPO5)		April-June (START08)	
P (hPa)	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$	$d\mathrm{RHi}_q$	$d\mathrm{RHi}_T$
0 – 200	0.94	0.061	0.87	0.13	0.97	0.031	0.93	0.074	0.93	0.064	0.79	0.21
200 – 400	0.93	0.069	0.94	0.062	0.98	0.025	0.97	0.026	0.96	0.036	0.94	0.06
400 – 600	0.99	0.012	0.97	0.026	0.98	0.021	0.98	0.019	0.97	0.026	0.95	0.053
600 – 800	0.98	0.025	0.96	0.041	0.97	0.029	0.96	0.037	0.96	0.044	0.92	0.081
> 800	0.94	0.064	0.94	0.061	0.93	0.066	0.94	0.059	0.94	0.06	0.94	0.061

Fig. 2.

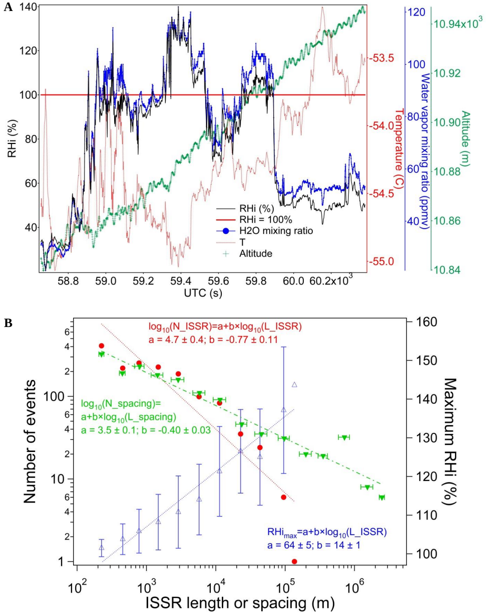
A
RHi (%)
140
120
100
80
60
40
UTC (s)
58.8
59.0
59.2
59.4
59.6
59.8
60.0
60.2x10^3
Temperature (C)
-53.5
-54.0
-54.5
-55.0
Water vapor mixing ratio (ppmv)
120
100
80
60
40
Altitude (m)
10.94x10^3
10.92
10.90
10.88
10.86
10.84
RHi (%)
RHi = 100%
H2O mixing ratio
T
Altitude
B
$\log_{10}$(N_ISSR)=a+b×$\log_{10}$(L_ISSR)
a = 4.7 ± 0.4; b = -0.77 ± 0.11
$\log_{10}$(N_spacing)=
a+b×$\log_{10}$(L_spacing)
a = 3.5 ± 0.1; b = -0.40 ± 0.03
RHi$_{max}$=a+b×$\log_{10}$(L_ISSR)
a = 64 ± 5; b = 14 ± 1
Number of events
1
10
100
Maximum RHi (%)
160
150
140
130
120
110
100
ISSR length or spacing (m)
10^2
10^3
10^4
10^5
10^6

Fig. 3.

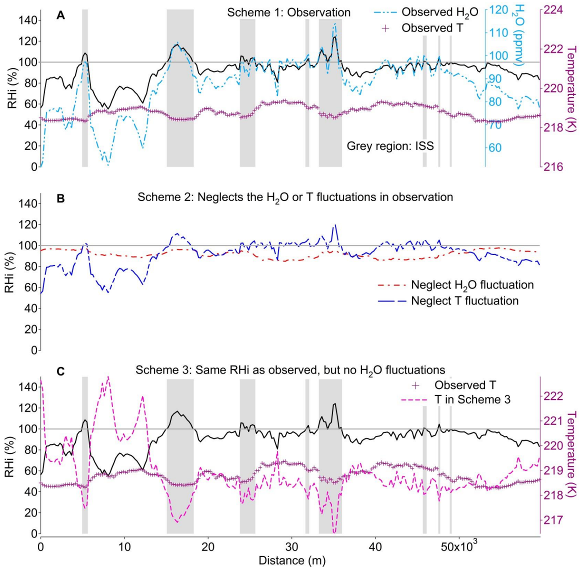
A
Scheme 1: Observation
Observed H_2O
Observed T
Grey region: ISS
RHi (%)
0
20
40
60
80
100
120
140
H_2O (ppmv)
60
70
80
90
100
110
120
Temperature (K)
216
218
220
222
224
B
Scheme 2: Neglects the H_2O or T fluctuations in observation
Neglect H_2O fluctuation
Neglect T fluctuation
C
Scheme 3: Same RHi as observed, but no H_2O fluctuations
Observed T
T in Scheme 3
217
218
219
220
221
222
0
10
20
30
40
$50x10^3$
Distance (m)

Fig. 4.