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Interactive comment on "Ice-supersaturated air masses in the northern mid-latitudes from regular in-situ observations by passenger aircraft: vertical distribution, seasonality and tropospheric fingerprint" *by* Andreas Petzold et al.

Anonymous Referee #3

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This manuscript describes in situ measurements of relative humidity (RH) in the upper troposphere and lower stratosphere (UTLS) from commercial aircraft and presents a detailed statistical examination of ice-supersaturated air masses (RHice >100%). The analysis is confined to a region of high measurement density between latitudes 40°N-60°N and longitudes 105°W and 30°E, for the years 1995-2010. Several conclusions are drawn regarding the probabilities of encountering regions of ice supersaturation (ISSR) in three different longitude regimes, based on distance from the tropopause and season. There is also a minor attempt to attribute interannual variations in these

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probabilities to the North Atlantic Oscillation (NAO).

Major Comments ==========

Uncertainties are calculated and presented (typically 1 standard deviation) for most mean values derived in this paper. However, the uncertainties are often ignored when interpreting the mean values and making quantitative statements about them. One example, in Lines 396-397: "... the average ISSR occurrence probability is 29% in the troposphere and increases to 34% when approaching the tropopause layer." Given that the standard deviations of these mean values are each at least $\pm$9%, the two averages are not statistically different, and the claimed "increase" is not significantly different from zero. A second example is Figure 11, where a horizontal line (indicating no seasonality) can easily be drawn within the uncertainties in each panel that show "annual cycles". Therefore, the statement (Line 506), "For all regions, ISSR occurrence probabilities are highest in the winter/spring and lowest in summer ..." is not supported by these seasonal averages with their statistical uncertainties.

In view of this, why are most uncertainties in this paper calculated and presented as 1 standard deviation when the vast majority of scientific uncertainties are reported as 95% confidence intervals (i.e., approximately 2 standard deviations for large sample sizes)?

The "occurrence probability" statistics are simple to understand, based on the numbers of RH measurements reflecting subsaturation, saturation, or supersaturation during a flight segment, an entire flight, or a number of flights. But it is not clear how "occurrence probability standard deviation" statistics were calculated. Are these based on calculating an average of the occurrence probabilities for a number of flights, reflecting the variability of the occurrence probabilities for individual flights around the average? This should be briefly explained, early in the paper, so the reader can immediately grasp the concept of the "occurrence probability standard deviation".

Why is the requirement for supersaturation RHice >100% when the measurement un-

certainties are approximately 5% RH in the middle and upper troposphere? If some part of these uncertainties is a systematic error (a high bias of 3%, for example), wouldn't this lead to artificially high occurrence probabilities if measurements of a 98% RH air mass are 101% RH? How much do the occurrence probabilities decrease if you instead require RHice >103%, or even RHice >105% for supersaturation?

I'm not convinced that the comparison of supersaturation occurrence probabilities for atmospheric layers relative to the lapse rate ("thermal") tropopause vs the 2 PVU ("dynamical") tropopause shows much of a difference. If 95% confidence intervals of the mean values in Table 3 are considered, none of the "thermal" and "dynamical" averages for any atmospheric layer are statistically different. A lot of text, Figures and Tables are devoted to this comparison, and what does it show? Very little, in my opinion. Instead (or in addition), I'd prefer to see some assessment of the accuracy of the ERA-Interim tropopause heights that are absolutely critical to this paper. Since ozone mixing ratios were also measured as part of MOZAIC, and ozone can be used to define a "chemical" tropopause, can you compare ozone-defined tropopauses to the ERA-Interim tropopauses to evaluate at least the consistency of the latter? For example, if ERA-Interim puts the tropopause 1 km above the aircraft and the ozone mixing ratio is 1 ppm that indicates a large (>1 km) error in the tropopause height. I'm not suggesting a full-scale comparison, but rather some comparisons that illustrate the possible errors in tropopause heights.

Water vapor mixing ratios are discussed in some sections of the paper and are shown in some Figures, but nowhere in the paper is there a description of how these were determined. Were they measured directly with different instruments (as implied in Line 17 of the abstract) or were they calculated from the RH measurements, requiring concomitant measurements of pressure and temperature with their associated uncertainties?

There are some awkward and confusing sentences in the paper that could benefit from re-writing. I will point out a few of these below, but I suggest the paper be proofread by a native English speaker to clean up and clarify some sentences.

Minor Comments ==========

Lines 23-25: This statement implies there is an increasing trend in summertime water vapor mixing ratios in the lowermost stratosphere, but no similar trend in RHice. I don't think this is what you mean to say, rather that mixing ratios in this region are highest during summer months without corresponding maxima in RHice. If this is the case, doesn't it imply that temperatures in this region are also highest during summer months?

L51: The term "tropopause layer" is used throughout this paper, but where is it geophysically defined? On page 5 you limit the TPL to "tropopause pressure $\pm$ 15 hPa", but that's a definition that is neither common or geophysically-based. It would enlighten the reader to know why you chose these limits for the TPL.

L64: Why would an "increase in pressure" change the RH of an air mass? RH is the partial pressure of water vapor divided by the saturation pressure over ice at a given temperature. Neither of these is affected in any way by an "increase in pressure". Please either explain this statement more clearly or remove it.

L109-111: Why are radiosonde network measurements of RH "considered insufficient for detecting trends and variability in UTLS water vapor"? I believe the GRUAN radiosonde data product for RH will be sufficient in this regard, and that GRUAN represents another existing global-scale network of in situ observations of atmospheric composition in the Ex-UTLS. A good reference for GRUAN is:

Bodeker, G.E., Bojinski, S., Cimini, D., Dirksen, R., Haeffelin, M., Hannigan, J., Hurst, D., Leblanc, T., Madonna, F., Maturilli, M., Mikalsen, A., Philipona, R., Reale, T., Seidel, D., Tan, D., Thorne, P., Vömel, H., and Wang, J.: Reference Upper Air Observations for Climate: From Concept to Reality, B. Am. Meteorol. Soc., 97, 123–135, https://doi.org/10.1175/bams-d-14-00072.1, 2016.

L180: Please insert "attached" between "inlet" and "to"

L184-189: How about the uncertainty of RH measurements in the lower stratosphere? LOD is one measure, but since you determine supersaturation occurrence probabilities for several layers above the tropopause this must be somewhat known.

L211: Please change "sequences" to "segments". "Sequences" is also awkward in L105.

Figure 3: I think you intend "w/t" to mean "without" in both panels. Please change to "w/o".

L251-253: Presumably Figure 4 shows the RHice with the IFC applied, so please make this clear.

L286-287: "highest possible quality achievable by this kind of routine observations" sounds great, but what does it actually mean? This sounds like an advertisement instead of a scientific claim and I suggest toning it down or removing it.

L290-297, Figure 6: Up to this point, everything has focused on RH measurements and the tropopause-relative pressure bins you have defined. Here, the discussion suddenly turns to water vapor mixing ratios and tropopause-relative altitude bins. As above, where do the VMR data come from? And why does Figure 6 use altitude instead of pressure (or log pressure) as the vertical coordinate?

L327: "are bounded to the Great Lakes area and further North". Given the size of Figure 1, it is difficult (without magnification) to find the Great Lakes. A better description would be "are within the northern half of the continental USA and southern half of Canada".

Figure 8, Tables 1 and 2: It is not clear what the tropopause-relative pressure boundaries are for the different layers. Are the average values plotted (Figure) and presented (Tables) at deltaP = -30 hPa for the layer bounded by TPpress-15hPa and TPpress-45hPa? This should be clearly stated.

Given the standard deviations (Table 2), are the average values for different seasons

or longitudinal regions (Table 1) statistically different at the 95% level of confidence?

L373: Why is the annual cycle of UTH increasingly damped as you get closer to the tropopause?

L377: As noted above, please explain how an increase in pressure can cause supersaturation.

L429-433: This long sentence is confusing and requires re-wording for clarification.

Figure 9: What does the black horizontal line represent at +70 hPa in the H2O panels?

L454-455: The 33.5 ppb O3 value from ERA-Interim is representative of what altitude and region?

L460: Why the sudden switch from P99 to P95, without explanation?

Figure 10: Error bars for each marker would clearly show if the tropophericity values are statistically different (or not).

L500: Here in the text you claim that Figure 11 shows results for the "top UT layer", but the caption for Figure 11 says "calculations were conducted for the two UT layers positions closest to the tropopause."

L506-507: This statement is not supported by the average values when their uncertainties are considered.

L513: I assume the Lindenberg radiosonde RH data has been corrected using the GRUAN-recommended corrections? It is important to say this because the reader may assume that uncorrected RH data from radiosondes are good enough (they are not!). You might also reference the paper describing corrections to the Vaisala RS92 data:

Dirksen, R. J., Sommer, M., Immler, F. J., Hurst, D. F., Kivi, R., and Vömel, H.: Reference quality upper-air measurements: GRUAN data processing for the Vaisala RS92 radiosonde, Atmos. Meas. Tech., 7, 4463–4490, https://doi.org/10.5194/amt-7-4463-

2014, 2014.

Figure 12: I don't see any information about the layer or layers for which the results are shown. Please state this in the caption.

L549: typo "15y ears"

L530: Why only 15 months of Lindenberg radiosonde data? There are more than 9 years of GRUAN-corrected Vaisala RS92 RH data from Lindenberg.

L551: "fits well" is an overstatement since the DJF and MAM averages for Lindenberg lie outside the MOZAIC mean $\pm$ 1 standard deviation envelope.

L549: I don't know what a "first exemplary analysis" is. Please explain.

L553: Trend analyses are performed on supersaturation occurrence probabilities based on which tropopause definition?

L579: "thus" must be a typo because it makes no sense in this sentence. Also, please change "long-term average values" to "long-term seasonal average values."

L581-582: Three significant figures for trends and their uncertainties is not justified when the uncertainty values are nearly as large as the trends themselves. Why present the 1 standard deviation uncertainties when, presumably based on 2 standard deviation uncertainties, you claim in the next sentence that none of the trends are significant?

L597: If the westerlies bring "warmer and more moist air to Europe", why would you expect a higher probability of supersaturation in the UT? More moisture increases the RH, but warmer air lowers the RH.

L608: "we consider the correlation of signs statistically significant". This is a very qualitative conclusion that needs support from a quantitative explanation.

L651: "which then generates more frequently ISSR" is awkward phrasing. Please re-write.

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