

## ***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 #2**

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### **1 General comments**

This paper presents an analysis of data of relative humidity for the period 1995 to 2010, obtained via instrumented passenger aircraft in the framework of IAGOS and MOZAIC over the northern midlatitudes (40-60°N) in 3 longitude ranges: Northeast America, North Atlantic, and Europe. The huge amount of data makes it possible to cover several vertical altitude ranges of 30 hPa thickness with sufficient data density to allow robust statistics. The altitude bands are defined with respect to the thermal

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and the dynamical tropopause, respectively, and the "troposphericity" (i.e. the fraction of tropospheric origin in an airparcel) is determined using simultaneous data of ozone VMR. The focus of the study is ice supersaturation.

The data show, that ISSRs (ice supersaturated regions) occur most often directly below the thermal tropopause, rarely directly above it, and almost never further up in the stratosphere. There is a distinct seasonal cycle in all 3 considered regions, but no significant trend over the 15 years of the study. The north-atlantic oscillation seems to have an influence on the occurrence of ISSR over the North Atlantic and Europe, but not over North America, which is physically plausible. ISSRs are colder and moister than their subsaturated surroundings (in agreement with earlier results), and they are poorer in ozone and have accordingly a larger troposphericity than the subsaturated environments, which is plausible as well considering the fact that most ice supersaturation is formed by uplifting of airmasses. The data show also that ice supersaturation is very closely related to cloudiness, that is, most ice supersaturation is found within clouds.

Thus, this paper provides a number of new and interesting results. It is well written for the most part. There are only a few points where I think the presentation can be made clearer and where perhaps the discussion can consider one or two more points. The paper should surely be published after the issues below are addressed.

### **2 Major issues**

1) The paragraph lines 388 to 395 should be reworked; it is unclear what you did. For instance, what is an "average occurrence probability"? Do you mean the average frequency of occurrence or something else? What is the pdf of ISSR occurrence? Is this simply the probability of ISSR occurrence? I also do not understand what the distinction between seasons has to do with statistical quantities like median and percentiles

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and how these two non-related things are linked here in one sentence. And finally, what is the statistical entity?

2) The comparison between statistics relative to the thermal and the dynamic tropopauses is not easy to understand, perhaps because it is unclear what exactly has been done. The first issue that must be clarified is whether the tropopause pressure and the pressure of the 2 PVU surface are available for each single measurement or are there only average values available, which would be bad for the analysis. What is the average  $\Delta p$  with respect to (wrt) the thermal tropopause of the 2 PVU surface? It seems that 2 PVU occurs quite often or in the majority of cases in the UT1 layer. This should be stated. However, it does not seem as if the mean profiles wrt to the dynamical TP are just shifted versions of the profiles wrt the thermal TP definition. Is this a consequence of averaging or why is this so? Next, Table 3 lists under Thermal TP numbers which I expected to be annual mean values of numbers in Table 1 under AVG, but a quick calculation shows something different. Is this because of different weights for the seasons or what is the reason? (For instance take the  $20.0 \pm 6.5$  in column 3 of Table 3. Should this not be the mean of 21.3, 18.6, 17.1 and 18.4 in the right hand box AVG in table 1?). And finally, Fig. 10 shows different behaviour in the left and right panels. Although you give a good physical explanation, I am not fully convinced. In the thermal TP coordinates there is a strong difference between ISSR and non-ISSR already at 30 hPa above the TP, but in the dynamical TP version there is only a much smaller difference at 60 hPa above the 2 PVU level. Is it possible that, on average, the 2 PVU surface is more than 60 hPa below the thermal TP? Eventually we should expect to see qualitatively the same profiles, irrespective of the actual choice of a vertical coordinate, isn't it?

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### 3 Minor issues

1) Occasionally the term UTH is used. This should be avoided. UTH is a radiance based measure of a kind of mean relative humidity in a thick layer in the upper troposphere; it is a non-local measure. In contrast, IAGOS and MOZAIC yield local measures of relative humidity, and even after averaging over certain layers they should not be called UTH to avoid confusion. Better call it "the relative humidity field of the UT" or similar, but avoid UTH.

2) The last sentence of the introduction should be changed. The middle atmosphere is hardly relevant for IAGOS.

3) Figure caption 1: I do not understand what you mean with the pdf of data points. Do you mean simply the number of measurements or the fraction of measurements in a certain grid box?

4) Line 251: "Figure 4 illustrates ... of RH...": is this with or without IFC applied?

5) Figure 4: Please describe how these data are normalised. Is the sum over the whole figure 1?

6) I am a bit puzzled by the kind of averages applied. In line 176 it says " data are consolidated to 3-months season files", but in line 292 we have monthly mean profiles. Furthermore, is the distance of the current pressure level of a single 4-sec data point to the tropopause pressure recorded for every data point, or is the tropopause pressure averaged over a month and this average taken as reference (which would be a bad strategy to my view)?

7) Figures 6 and 7: why do you use geometrical height instead of the  $\Delta p$  for these figures?

8) Line 373/4: The statement may be wrong or perhaps right for the wrong reason. If the mean value of a positive quantity gets small, the variability usually gets smaller

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as well. Thus I suggest you to consider instead of  $\sigma$  the normalised  $\sigma: \sigma/\mu$  (i.e. std. deviation divided by mean value).

9) Comparison with RS Lindenberg (Figure 12a): has the same pressure band be selected for the RS data as for the MOZAIC/IAGOS data or are these the plain overall figures from the old publication?

10) It is not clear why CALIPSO can have higher cloud frequency than ISSR frequency. The argument that CALIPSO sees subvisible cirrus (SVC) explains only that it sees more than other satellite instruments do, unless SVC can survive in subsaturated air for a quite long time, where it is unclear to me what quite long actually means. I think that the reason for this result is rather in the difference of local vs. non-local measurements, just as the cloud fraction in a single level is always smaller than the cloud coverage over several levels.

11) Final paragraph of 3.5: Misuse of "cross-correlation". A cross-correlation is simply a correlation between two different quantities (as opposed to auto-correlation). Furthermore I suggest to replace "probability" in this paragraph with "fraction" in order to avoid wrong connotations. I am also a bit unhappy with "correlation" since I do not see that the two time-series have been correlated (in this case indeed cross-correlated) which would easily be possible. In this case there are also standard techniques to evaluate the statistical significance of the result (i.e. whether the correlation coefficient is significantly different from zero). The sentence "we consider ... statistically significant" should be deleted. This is not a question of "consideration" but of calculation. However, the physical explanation for your result is plausible. In the same sense, the statement in line 645 "significant correlations..." should be reformulated, for instance "physically plausible influence of the NAO on ISSR occurrence is detected in the time series...).

12) The data show that "by far the largest part of ISSR occurs inside cirrus clouds". You should ask yourself: Why? Doesn't this imply that most ISSR reach the humidity

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threshold for heterogeneous or even homogeneous freezing shortly after the air mass began to be supersaturated? Are there further implications? Since the NH has more heterogeneous IN than the SH, do you expect that on the SH a smaller fraction of ISSR is inside clouds?

#### 4 Language, typos, etc.

Line 71: remove comma after supersaturation.

Lines 227/8: Details of ... in detail .. Please reformulate.

Line 275: Please replace "validation" with "comparison". And then "The MOZAIC ... IS plotted..."

Lines 327 and 329: change to "north" and "south" (i.e. use lower case).

Line 340: I suggest to write "Similar AVERAGE values of ..."

Line 353: delete "set".

Line 491: add comma after dynamics.

Line 579: thus HAS positive ...

Figure 15: in my printout there is no grey shading.

Line 596: warMer and moister (or do you indeed mean more, i.e. a larger quantity of, moist air?)

Line 613: MOZAIC (with I).

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