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Interactive comment on “A 6-year global climatology of occurrence of upper-tropospheric ice supersaturation inferred from the Atmospheric Infrared Sounder after synergetic calibration with MOZAIC” by N. Lamquin et al.

N. Lamquin et al.

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We copied referee 1 comments and then replied using ->

The title is very wordy and not entirely clear. “Synergetic” isn’t commonly used, “synergetic” seems more widely used. How about ‘A 6-year global climatology of upper tropospheric ice supersaturation frequency inferred from the Atmospheric Infrared Sounder and calibrated by MOZAIC’, or something more concise than at present?

→ we have changed the title

Abstract: p. 12890, l3: 'One one hand, infrared sounders. . .' l20: is this ECHAM5?

Introduction L26: 'To date, few climate. . .' p. 12891, l2: 'radiosounding' isn't commonly used. How about 'radiosondes'? l3: ' . . . provide relative humidity. . .' l17: ' . . . occurs in thin layers of the. . .' l18: ' , thus it is even. . .'

p. 12892, l5: ' . . . are briefly introduced. . .'

→ ok

p. 12893, l6-10: The justification provided by the reference of Montoux et al. (2009) for including low values of AIRS water vapor mixing ratio is not described. This needs to be spelled out in careful detail. The authors make it clear that they are aware of the issues in the AIRS retrievals at nominally low values of mixing ratio, for instance, lower than 15-30 ppmv, depending on the reference (e.g., Gettelman et al., 2004, GRL; Read et al., 2007, JGR; Fetzer et al., 2008, JGR). AIRS tends to saturate at higher mixing ratio values than coincident in situ observations (Fig. 2b, Gettelman, 2004, GRL). Wouldn't this cause a much higher rate of perceived supersaturation if these values were retained, thus the figures presented later in the paper on AIRS supersaturation frequency would potentially be biased high? Proper justification outside of citing this reference is not provided. Furthermore, it is not clear to the reviewer how the reader should be cautious of these values when no further details are provided on the limitations of the data, nor is this point raised when the results are presented later in the paper.

→ as answered to referee 2 200 hPa is a realistic limitation to be confident in the final ISS statistics. Using threshold to further select the data would bias the result even more. The text has been enhanced.

L17 and l19: The percentages should be listed to the nearest tenth as with the Table. It is not true that there are 0

→ changed in the text

Also, the authors may want to consider referencing the following paper that presents results of combining Microwave Limb Sounder (MLS) and AIRS data together to study

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UT/LS RHI, among other quantities: Citation: C. K. Liang, A. Eldering, A. Gettelman, B. Tian, S. Wong, E. J. Fetzer, and K. N. Liou (2011), Record of tropical interannual variability of temperature and water vapor from a combined AIRS–EREGMLS dataset, *J. Geophys. Res.*, 116, D06103, doi:10.1029/2010JD014841.

→ reference added

p. 12894, l5: '... obtain global. . .'

→ ok

p. 12895, l12-14: Can the authors provide justification on why the highest RHI value is chosen? Why not some other measurable such as the mean or mode of the distribution within the AIRS observation, or something else? Please clarify this approach.

→ as answered to referee 2 the point is to detect ice supersaturation at least once within the AIRS FOV both vertically and horizontally, therefore we use the maximum to increase the chances of detecting it. The mean within the FOV would slightly lower the probability (about 5

L15: In regards to the time match-ups, over a period of 30 min, a parcel of air can move 50 km horizontally given a wind speed of 100 km/h. This could place coincident MOZAIC measurements in neighboring AIRS FOVs (at least at nadir) with strong upper-level flow. Also, how does one deal with strong vertical motions such as those found adjacent to convective systems? It would be helpful to reference some studies that have considered these types of details on satellite/in situ match-ups and the possible implications on the interpretation of the results.

→ we have tried using 10 minutes but no strong difference except what is expected from something less statistically representative, also most collocations are found out of convective systems

L17-18: This is true at near nadir. At higher scan angles the number of coincidences (presumably) is much higher.

→ we have selected the MOZAIC coincidences residing inside the FOV which leads to a maximum of about 50 coincidences (all scan angles considered)

p. 12896, l1-2: The authors appear to vertically match the MOZAIC observations within

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+/- 50 hPa of the AIRS cloud top pressure. However, it is well known (Stubenrauch et al., 2008, JGR, and references cited therein) that IR-derived cloud tops tend to be biased anywhere from 1-3 km too low in the atmosphere, depending on the situation. Will the results of this work be impacted if the 50 hPa window to retain MOZAIC observations is treated as a bias rather than as a random error? The reviewer would be curious to see if this changes any of the results reported later in the paper, or if this turns out to be a minor detail.

→ we have looked further into this and no significant difference in the RHi distribution for cirrus was found whether MOZAIC is close or far from the clouds (see comment to referee 1). Therefore we removed this detail and changed the text.

p. 12898, l2: 'However, lowering this threshold. . . '

→ ok

p. 12900, l2-3: Can the authors be more specific in how they use the AIRS quality control to 'reject' bad data?

→ more precision has been added in the data part.

L22-23: The 'non-linear effects of the AIRS vertical resolution' is not clear. Please explain.

→ detail has been added

p. 12902, l16: 'satisfying such condition with either' is a bit awkward.

→ changed into "such condition. We use either. . . "

p. 12903, l16: also supercooled liquid droplets are an issue as well l18: why -30C for a cut-off rather than something more stringent like -40C?

→ we base on the reference cited in the introduction, the same criterion is applied to other datasets. Using a more stringent dataset would have the same effect on both dataset.

p. 12904, l4: If the authors are going to make the argument that the supersaturation frequency is underestimated, it should be made clear this is because of an increasingly smaller percentage sample/yield inside increasingly more opaque clouds. However, the reviewer would also like to raise the issue about retaining very low values of AIRS

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water vapor that might have the effect of overestimating supersaturation (as described in a comment above).

→ we have enhanced the text and figure 9 has been extended and transferred to the introduction

L26-27: The description of the connection between the histograms and the strength of convection is not clear. Please clarify and explain in detail. As it reads, it seems entirely speculative without any explanation why this is the case.

→ we have removed this comment

p. 12907, l27-28: Did the authors obtain r^2 values to quantify the correlations? Also, since these are averaged values in wide bin widths with presumably large sample sizes, one would expect the scatter to wash out compared to similar scatter plots that could be obtained from L2 FOV data. Have the authors looked at these kinds of FOV-scale scatter diagrams? Is this what the vertical bars are attempting to capture?

→ the cirrus occurrences from CALIOP are quite dispersed when computed over 1x1 lat x lonpixels, thus computing r^2 values is misleading. The term correlation is misleading and we don't use it anymore, the text has been adapted

L28-29: As with the previous comment on connections between supersaturation and convective intensity, this is not at all obvious from the previous discussion. Please clarify in detail or toss it because it reads as only speculation

→ some text has been removed as well, including this comment

p. 12908, l4: Here comes one example of a discussion of results in the 100-150 hPa layer where one expects very few AIRS samples that have values large enough to be considered valid (see Kahn et al., 2009, JGR). It would be very helpful to see the impacts of removing values below established sensitivity thresholds. Or please justify why the map at this particular pressure bin is defensible as is.

→ the sentence has been removed, we are not allowed to say anything strong here

p. 12910, l12-13: Although the authors caution against false interpretations of AIRS results at low pressure bins, they provide no means in which to discriminate what is defensible and what is not. The reviewer thinks that by sub-sampling the values above

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some nominal threshold may yield some interesting results showing how sensitive the supersaturation frequencies are to assuming different AIRS cut-off thresholds.

→ see again previous answers.

p. 12911, l3: 'consistently'?

→ ok

p. 12913, l15-16: are only indicative of what? Not clear.

→ changed text

L17: pre-existing

→ ok

Fig. 1 color bar on bottom is only one color.

→ it does not appear using our pdf reader, this should be checked at final edition

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 11, 12889, 2011.

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