

Comments on “Variability of the properties of the distribution of the relative humidity with respect to ice: Implications for contrail formation” by Sanogo et al.

General Comments:

This study by Sanogo et al. updates and extends a number of studies that have analysed the large MOZAIC and IAGOS data sets. Specifically, the PDFs of RHi have been derived for the period 1995 – 2022 for the upper troposphere (UT) and the lower stratosphere (LS) separately as well as combined for different geographical regions, three pressure layers and cloudy and clear-sky conditions. The authors find that the PDFs differ substantially between the UT and the LS as well as between cloudy and clear-sky conditions. Moderate differences appear between different geographical regions and pressure layers. Furthermore, the authors analysed the occurrence frequencies of ISSRs and of persistent and non-persistent contrails for different combustion fuels. Here, they also find differences between the considered geographical regions and pressure layers. The frequency of contrail occurrence increases from kerosene over bio ethanol to liquid hydrogen.

Overall, the study contributes to the detailed documentation and analysis of the relative humidity field at common cruise altitudes and provides a valuable basis for the evaluation of the quality of atmospheric models. This fits into the scope of ACP and is of scientific interest, in particular since NWP models need to improve in order to make operational contrail mitigation feasible. Thus, the manuscript is suitable for publication in ACP.

However, the quality of the manuscript is substantially lowered by a lack of mathematical and physical precision in its formulation as well as deficiencies in the use of the English language. Therefore, the manuscript should be revised considerably, before it can be published. Please note that I focused my review on scientific issues and, beyond this, only commented on typos. However, I also noticed numerous language errors including additional or missing words, unsuitable words or the inappropriate use of singular/plural. I recommend that the authors go through their manuscript sentence by sentence to improve on this and may consider to consult a native speaker if necessary.

Specific Comments:

- The abstract appears a bit too detailed to me, especially between lines 6 and 14. Please consider summarizing the results more concisely.
- Abstract, line 7, lines 192f, 195, 202, 207, 222, 232, 348, 350, 353: “the probability (P) of observing a certain RHi” is mathematically unprecise as the following sentences are clearly describing the behaviour of the probability density. Specifically, “the probability of observing a certain RHi” is always zero as the boundaries for integrating the PDF are identical in this case. I suggest sticking to the terms “PDF” and “probability density” and not denoting the PDF with “P”, as this is the common symbol for the probability.
- Abstract, line 19, lines 226, 308, 314, 325, 365, “decreasing pressure level”: Unclear what you mean. Apparently, you sometimes mean increasing and sometimes decreasing pressure by that throughout the text. I suggest leaving out “level” and just using “increasing/decreasing pressure”.
- Line 28: “saturated” → “saturated with respect to ice”

- Lines 29, 130ff, 152, 179, 238, 271: I am sure what you mean is that the “water vapour” is ice supersaturated. “RHi” may rather be above 100% or above saturation.
- Lines 34f: In the common definition of supersaturation, 140% ice supersaturation would refer to $RHi = 240\%$. Suggestion: “ice supersaturation” → “RHi”
- Page 2, third paragraph: I missed a sentence on recent research suggesting a low efficacy for contrail cirrus potentially resulting in a low temperature response despite the high ERF (e.g. Bickel 2023).
- Line 51 f: An accurate representation of ISSRs is important for the prediction of contrail persistence, not “contrail-prone conditions” in general.
- Line 58: The way the sentence is written, it seems to state that ISSR are more frequent between 400 and 300 hPa in the midlatitudes and between 300 and 200 hPa in the high latitudes, which is not the case when looking at figure 9 in Lamquin et al. (2012). Please switch the order of either the pressure levels or the latitude regions.
- Line 101: As the three pressure ranges appear numerous times in the paper, you may consider labelling them with something like “lower”, “intermediate” and “upper” layer to improve the readability.
- Line 104: Is the number of measurements in the HL missing a digit or is it really an order of magnitude lower than in the other latitude regions on this pressure level?
- Line 108: Why is this a good replacement? Are the quality flags of RHi and RHI correlated during periods, where the “grounding problem” did not exist?
- Lines 130f: Not RHi of the mixture but RHi in the ambient atmosphere has to be below 100% for non-persistent contrails. I suggest that you leave out “If RHi of the mixture is subsaturated,” and simply state that contrails in general form under these conditions, as you mention the criterion for contrail persistence in the next sentence anyway. Also, please specify that you mean the properties of the ambient atmosphere by “T” and “RHI”.
- Lines 156-160: Unclear description of fig. 3. I cannot reproduce the 30% for the 0.05 threshold in the tropics from the plots. Also, you mention two thresholds for the ML, but only give the percentages for one of them.
- Line 191: Apparently, this is only true for the ML.
- Lines 236f, “It is the case here for the stratospheric PDF of RHi in the 325-275 hPa layer ($b \approx 0$, Fig. 4a,e,...”): Unclear. What is the case? Under sampling? Also, b is clearly not zero for the stratosphere and figure 4e does not correspond to the mentioned layer.
- Line 237f: Hard to verify this from the plots. The opposite seems to be the case for example for EU.
- Figure 6: The PDF of in-cloud RHi increases for subsaturated conditions and decreases for supersaturated conditions when decreasing the detection threshold, indicating that the measurements that are added in this process were mainly sampled under subsaturated conditions, some of these under strongly subsaturated conditions, as even a “dry mode” appears in the PDFs. You briefly mention “detection uncertainty” in section 2.3, but never come back to this issue. Could these ice crystals in very dry air be crystals that have been turbulently mixed out of cirrus nearby or even false measurements? In both cases, these measurements would not really correspond to cloudy conditions. Please discuss your choice of threshold.
- Line 306: I don’t see, how the results for the other pressure layers are similar, as the frequencies are (almost) zero there, regardless of the fuel.
- Line 311f: Repetition of line 307f
- Line 318: Please be more precise what you mean. This is certainly only true when referring to the same pressure level. The tropical tropopause is usually colder than the tropopause in the

ML and HL, but it is also located at a much lower pressure such that the tropical UTLS is not properly represented in your data set.

- Page 19, first paragraph: Please rewrite the paragraph. Your argumentation is hard to follow. Try structuring it:
ISSR + low ambient temperature → mixture of exhaust and ambient air exceeds water saturation due to curvature of vapour pressure curve, regardless of fuel. → persistent contrail
ISSR + higher ambient temperature → mixture of exhaust and ambient air may stay subsaturated with respect to liquid water if ratio of EI_{H_2O} to Q is low. → no contrail with kerosene, persistent contrail with ethanol or hydrogen
- Line 330: “the properties of RHi” → “properties of the atmospheric RHi PDF”
- Line 334: “RHi” → “RHi PDF”
- Line 340f, “This frequency can be interpreted as the IAGOS aircraft flying time in cirrus clouds.” This information belongs into the paragraph above, as this is not a result. Also “estimated by” might be better than “interpreted as”.
- Line 343: A dot is missing in “14 %”. Anyway, I do not think this amount of details is necessary in a summary. “of the order of” implies that you give a rough estimate, but you repeat the exact numbers from the results section. I think, the interesting results are that cirrus is more frequent in the tropics than in the ML, that it occurs higher up in the tropics and that the frequency decreases if a higher detection threshold is chosen.
- Line 362: “In this region, aircraft form persistent contrails...” Suggestion: “The reason is that, in this region, aircraft running on kerosene already form persistent contrails...”
- Line 364: “contrail” → “persistent contrail”, also, the lower level in the tropics seems to be an exception from this rule.

Technical Corrections:

- The formulation of the title seems a bit unwieldy to me. Suggestion: “Variability in the distribution properties of the relative humidity with respect to ice: Implications for contrail formation”
- Abstract, line 2, lines 89f: Several alternatives can be found on the internet for the meaning of the acronym MOZAIC. The IAGOS website itself gives the two possibilities “Measurement of OZone and water vapour on Airbus in-service airCraft” and “Measurements of OZone and water vapour by in-service Airbus airCraft”. I suggest using one of these.
- Abstract, line 15: I suggest starting a new paragraph after “large-scale simulations of RHi.”.
- Page 2, third paragraph: The last sentence seems a bit unconnected. I suggest inserting “also” after “Natural cirrus clouds”.
- Line 87: Typo in “relative humidly”
- Line 112: Typo in “comparaison”
- Figure 2: The font size of “High-latitudes” appears to be larger than above the other two panels.
- Figure 3: It would be easier to compare the two latitude regions if equal pressure ranges were plotted with the same line style.
- Line 201: b is close to -4
- Line 208: Typo in “Nord”

- Table 3, caption: "RH_i" is missing. Also, ML and HL have to be switched. Actually, this bracket is unnecessary anyway, as these abbreviations are not used in the table. The same holds for the caption of table 6.
- Figure 4: "ML" is redundant in line 3 of the caption. Also, the font size of "Probability Density" appears to be larger in the last row of images. Furthermore, I would not call the plots of the last row "global", as they are clearly not.
- Figure 5: Typo in the figure key: "LT"
- Table 5: A dot is missing in the bottom right cell.
- Line 252: The reference probably should be figure 4, not 5.
- Line 253: The second "RH_i" is redundant.
- Line 257: Typo in "aformentioned"
- Line 260: Typo in "completly"
- Figure 6, caption: The "C" is missing behind the temperature value.
- Figure 7, caption: Typo in "blye"
- Figures 7 and 8: A grid or at least horizontal lines would improve the readability of the figures. Also, one might consider combining the two figures into one by plotting the non-persistent contrails on top of the persistent ones. This way, the frequency of non-contrail conditions could be read from the figure as well.
- Line 305: Typo in "hyrogen"
- Line 342: "respective" is redundant.
- Line 355: "saturated" → "supersaturated"

Reference:

Bickel, M. (2023). *Climate impact of contrail cirrus* (Doctoral dissertation, Imu).