Reply to Reviewer #2 (2nd round)
(Referee comment on "Correction of temperature and relative humidity biases in ERA5 by bivariate quantile mapping: Implications for contrail classification" by K. Wolf et al. (egusphere-2023-2356), 2023)

We thank the Reviewer for the time she/he spent on the manuscript and for the useful comments. Addressing the comments has further improved the manuscript.

For better legibility, the Reviewer's comments are highlighted in **bold** and changes in the manuscript are in *italics*.

Before addressing the specific comments of Reviewer 2 we would like to highlight that we have decided to make a small but important change to the title of the manuscript. The previous title "*Correction of temperature and relative humidity biases in ERA5 by bivariate quantile mapping: Implications for contrail classification*" has been changed to "*Correction of temperature and relative humidity biases in ERA5 by bivariate quantile mapping for contrail prediction and classification*". With this title change we want to clarify that we do not want to make a universally applicable correction of humidity in ERA5 but only provide a corrected humidity to enable better estimates of contrail occurrence consistently with IAGOS measurements.

I very much appreciate the considerable effort by the authors to improve the paper. Nevertheless, there are a few points that should be considered in order to improve the manuscript even further.

- **I appreciate the modifications of the text connected with the question if ERA5 has a dry bias. We agree that 'there is no consensus whether ECMWF re-analysis products are subject to a moist or dry bias in the upper troposphere'. I am happy with the changes made in the introduction. But I am surprised that the rest of the paper completely ignores those important changes. It is not acceptable to first say that there is no consensus on a moisture bias and then talk in the remainder of the paper about the moisture bias and how it can be corrected.**

  As noted in our responses to Reviewer 2 (1st Round), there is no consensus in the literature as to whether ERA5 is generally too dry or too moist in the UTLS. However, by analyzing the IAGOS data we found a dry bias in ERA5 with respect to the IAGOS measurements. Since IAGOS has been found to be in good agreement against more accurate in situ measurements, there is a strong basis to say that ERA5 has a dry bias in the regions (locations and pressure levels) sampled by IAGOS aircraft, and by extension in the corresponding air traffic regions. Here, and as already stated in the first round of replies, we are particularly interested in correcting ERA5 temperature and relative humidity for predicting potential contrail formation and relying on IAGOS data allows to do so exactly at the locations and pressure levels that are relevant for aviation studies. We have added an additional sentence to justify our approach and the rationale for correction ERA5 against IAGOS. The additional sentence reads as:

  "It is important to stress that we do not seek to make a universally applicable correction of humidity in ERA5 but rather provide a corrected humidity to enable better estimates of contrail occurrence. Relying on IAGOS data allows us to do so

exactly at the locations and pressure levels that are relevant for aviation studies."

To provide further clarification, qualifiers have been added to manuscript to explicitly indicate that the identified biases in temperature and RH are in relation to IAGOS.

- **Furthermore, from your answers to my comments I gather that we agree that pilots appear to avoid clouds. This would lead to IAGOS sampling clouds with close to 100% RH less often and, accordingly, higher RH values more often. This would mean that a certain bias between ERA5 and IAGOS needs to be expected. I have not seen text connected with this within the paper – I am sorry if I overlook connected text. There are several places within the paper where the impact of avoiding to fly through clouds should be discussed (but I did not find it mentioned). This argument should be included in the introduction right after your sentence 'Contrarily, studies that compared water vapor concentrations and ….. in ERA … with aircraft in-situ observations ….. This should also be mentioned in the results section e.g. when comparing the PDF of ERA5 and IAGOS (lines 336-338) and in section 3.2.**

  We agree that pilots tend to avoid deep convective clouds (such as cumulonimbus) and other clouds that are potentially indicative of dangerous weather and/or turbulence. However, cirrus clouds are typically not avoided because they do not pose a threat to flight safety. More importantly, cirrus clouds are often optically thin and barely visible or even invisible to the pilots, when viewed from flight altitude. Therefore, we are not convinced that a potential sampling issue with respect to cirrus clouds plays a role in our diagnosis of a dry bias of ERA5 against IAGOS data. Even if cloud avoidance was introducing a dry bias, we do not need to correct for it because our objective is to go from ERA5 humidity to humidity as sampled by commercial aircraft. It is not obvious to us that cloud avoidance would lead to higher RH values, as suggested by the Reviewer. Even if cirrus cloud were subsaturated more often than they are supersaturated, which is not a given (e.g., see Li et al (2023)), sampling supersaturated cloud free areas is also infrequent.

  Even though we raised awareness for the sampling issue, we added the following sentence to the introduction at the position suggested by the Reviewer.

  "[...]Contrarily, studies that compared water vapor concentrations and ice supersaturation in ERA-interim and ERA5 with aircraft in-situ observations found that conditions of ice supersaturation are not frequent enough in those reanalysis products, suggesting a dry bias (Kunz et al., 2014; Dyroff et al., 2015; Gierens et al., 2020; Reutter et al., 2020; Schumann et al., 2021). Consequently, there is no consensus whether ECMWF re-analysis products are subject to a moist or dry bias in the upper troposphere. It is noted that in-situ observations are potentially biased by avoiding deep-convective clouds and the outflow of such clouds. However, cirrus clouds are typically not avoided (Petzold et al., 2020) and, therefore, a potential sampling issue with respect to cirrus clouds plays more than a minor role"

  The following sentence was added to the second paragraph in the summary:

  "It is again noted that in-situ observations from IAGOS are potentially biased by

avoiding deep-convective clouds and their outflow, while cirrus clouds are generally not avoided."

- **The PDFs of RH (figure 3) are compared and the 'lack of ISSR in ERA5' is suggested to be connected with the use of saturation adjustement within IFS (line 341) which is explained in section 2.2.1. The whole section 2.2.1 is dedicated to 'in cloud ice supersaturation'. The possibility of a lack of ISS coming from cloud-free areas appears to not be discussed. As far as I can see figure 3 does not contain any information on if the difference in ERA5 and IAGOS RH comes from cloudy or cloud-free areas. Nevertheless, it is discussed as a problem stemming from the cloudy areas. In section 2.2.1 the authors write that ' they (the models) currently lack in the appropriate representation of ISS under cloudy conditions'. You do not mention that the models furthermore lack in the appropriate representation of subsaturation under cloudy conditions due to the saturation adjustment. In the last little while in-situ measurements pointed at contrails often persisting in ice subsaturated air. The analysis of in-situ measurements from Krämer et al 2009 show a large probability of subsaturation in cloud free air. Dekoutsidis et al. 2023 show that the most probable relative humidity within midlatitude cirrus clouds is 96% and only 34.1% of in-cloud RH values are supersaturated. Given those analysis I am surprised that in section 2.2.1 it isn't even mentioned that in-cloud subsaturation is underestimated as well. So, I doubt that the peak at 100% humidity needs shifting over to higher ISS. Based on Dekoutsidis et al. you may want to claim that it needs to be shifted to RH=96%. But I think 100% is not too bad an approximation.**

  We agree with the Reviewer that cirrus clouds may be subsaturated or supersaturated given the slow kinetics of condensation on or evaporation from ice crystals. Neither process is well represented in large-scale models. Moreover, ERA5 does not aim for an accurate representation of relative humidity within clouds. Instead, it provides information regarding whether a grid-boy is cloud-free or cloudy, as well as the cloud coverage. Additionally, (relative) humidity is generally complicated to determine as it is influenced by many processes, such as atmospheric dynamics and temperature fields. The objective of our study is not to identify and quantify the individual sources of humidity errors in ERA5.

  Section 2.2.1 is dedicated to the discussion of in-cloud representation of supersaturation in ERA5 as the in-cloud saturation is technically clipped to 100% relative humidity. Clipping to 100% is a necessary workaround, as the temporal evolution of in-cloud humidity is not tracked within ERA5. With our proposed QM technique we want to remove the effects of the RH clipping concerning contrail estimation.

  A discussion of the distributions and PDFs of humidity inside and outside of clouds as simulated by ERA5 and measured by IAGOS is given 3.2, where we actually do see a good agreement for in RH between IAGOS and ERA5 for intermediate cloud conditions.
  Furthermore, we would like to direct the Reviewer to Fig. 6 in the paper by Sanogo et al (2024). Figure 6 in their paper shows the distributions of RHi as measured by IAGOS. RHi are separated by threshold of ice particle number concentration, which is used as proxies to differentiate between measurements in clouds and outside of

clouds. Their paper includes a detailed discussion of RHi determined from in-cloud and cloud free measurements.

- **In line 354-355 you say that smoothing leads to a reduction in extreme values in RH. Comparing grid box mean values and in-situ observations therefore will have very different extrema and differences should not be interpreted as a dry bias. Ignoring this difference means that you disregard the subgrid variability of humidity within IFS. We know that this is a bad approximation.**

  We agree with the Reviewer that smoothing leads to a reduction in extreme RH values. However it does not affect the mean value and therefore has no impact on a dry or wet bias. Furthermore we do compare RH at comparable resolution between the model and the observations. We are not sure about the relevance of this comment and, as a consequence, no change was made to the text.

- **The analysis in figure 6 is interesting. But the analysis cannot fully support the conclusions:**

  a) In the introduction it was mentioned that IAGOS pilots often choose to fly outside of clouds which leads to a sampling issue when comparing ERA5 and IAGOS RH data. I am missing a discussion of the impact of this sampling issue in the discussion connected with figure 6. E.g. in the RH PDFs for lower cloud cover including the RH from cloudy patches vs not including them makes a difference of the RH PDF and ISSR estimates.

  As stated above, pilots typically avoid deep convective clouds, mesoscale convective systems, and the outflow thereof. However, cirrus clouds are typically not avoided as they do not threat flight safety. More important, cirrus clouds are often optically thin so that they are barely or not visible when viewed from flight altitude. Therefore, we have no evidence that the sampling issue with respect to cirrus clouds plays more than a minor role.

  b) I am also missing a discussion of the surprising RH PDF from IAGOS for full cloud coverage. Do we really expect values of above 150% in the main air traffic areas and do we really expect them within cirrus? Aren't values like that a tropical UT issue? I expect to see RH > 140% extremely seldomly when sampling fresh nucleation events. The frequency with which we see 140% is only one order of magnitude lower than seeing 100%? I think it would be good to compare the results to Dekoutsidis et al. 2023.

  Our manuscript does not address the investigation of supersaturated or sub-saturated cirrus. IAGOS measurements have been successfully validated against other high-precision measurements, for example, the Fast In-situ Stratospheric Hygrometer (FISH). However, due to the response time of IAGOS observations they are not very well suited to investigate humidity fluctuations and humidity gradients between cloudy and cloud-free air masses. The response time was already explained and quantified in the first version of the manuscript. As our objective is to validate ERA5, a large-scale model with an average spatial resolution of approximately 19 km, we do argue that IAGOS data are still suited for this kind of analysis.

In response to the Reviewer, we prepared a plot to compare with Dekoutsidis et al. (2023). The appended plot (see below) shows the density distribution with temperature on the x-axis and RHi on the y-axis. The measurements are separated for measurements inside and outside of clouds. The transition between both is called "intermediate". Cloud-free conditions are defined by ice crystal number concentrations N lesser than 0.001 cm^{-3}, while cloudy conditions are defined by N > 0.015 cm^{-3}. Intermediate conditions are defined by 0.001 cm^{-3} < N < 0.015 cm^{-3}. The x and y-axis are scaled such that they use the same value ranges as in Dekoutsidis et al. (2023). A quantitative analysis of our plot indicates that the distributions observed are similar to the distributions reported by Dekoutsidis et al. (2023). Other than stated by the Reviewer we do see a differences by two orders of magnitude between RHi at 100 % and RHi at 140 %.

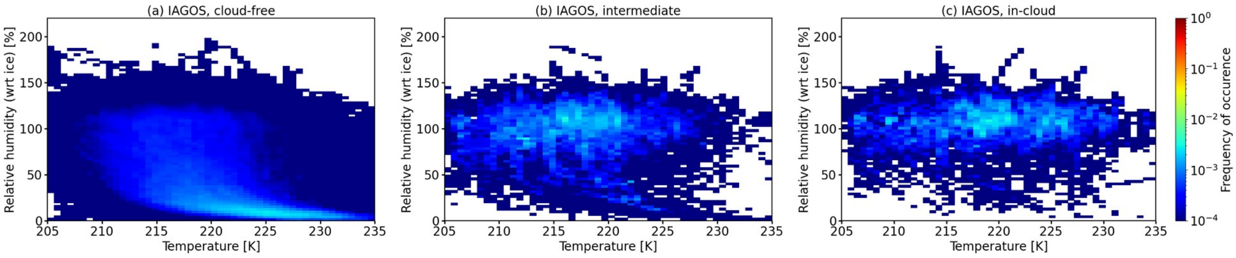

Figure 1: Density distributions with temperature on the x-axis and relative humidity (w.r.t. ice) on the y-axis.

Again, we would also like to direct the Reviewer to Fig. 6 in the paper from Sanogo et al (2024). Figure 6 in their paper shows distributions of RHi measured by IAGOS. RHi are separates for different thresholds of ice particle number concentration that are used as proxies to separate between measurements in clouds and outside of clouds. Their paper includes a extensive discussion of Rhi of in-cloud and cloud free measurements.

c) A short sentence about the stability of the in-cloud RH PDF based on less than 1% of observational data would be helpful.

We have added a sentence to say that the in-cloud RH PDF is less robust because of the smaller amount of data.

"The relatively limited number of samples (<1 %) from within clouds causes a less robust PDF compared to the PDF based on measurements conducted outside of clouds."

- **As said above, the summary should also include the open issues and their possible impact on the conclusions. Is there actually a dry bias or do we just correct RH to have a more convenient estimate for contrail formation? What does the sampling problem, stemming from pilots avoiding clouds, mean for**

the analysis? How good is our knowledge of in-cloud RH? Can we really call the corrections improvements or are they only improvements when trying to find the suitable input parameters for estimating contrail formation.

Please see our reply above.

Following the remarks of the Reviewer, several modifications have been made to the text. The individual lines are copied below. We also direct the Reviewer to the provided track-changes file:

“The QM method allows the removal of biases based on the statistical distributions of an observed and modeled quantity, for example temperature and relative humidity, with the aim to better estimate the contrail formation potential in air traffic regions.”

“In this study we proposed a temperature and relative humidity correction method for ERA5 based on a bivariate quantile mapping (QM) technique to better estimate the contrail formation potential.”

- **Line 19: It should say ‘for non-persistent and persistent’ and not the other way round.**

  The Reviewer is correct. The text has been corrected to read as follows:

  “The original ERA5 analyses show corresponding numbers of 50.3% and 7.9% for non-persistent and persistent contrails, respectively”

- **Line 26: The sentence is not clear: Are you talking about the remaining bias after the bias correction? Or are you saying that regionally there are still some deviations?**

  The sentence has been rephrased to the following:

  “Despite this improvement, differences in contrail occurrence persist after the correction, which are traced back to the underlying biases in temperature and relative humidity, as well as to the non-linearities in the Schmidt-Appleman criterion.”

- **Line 45 + 47: It is not clear to me why Lee et al is not cited anymore. The citation would fit perfectly.**

  In the first round of reviews, Reviewer 2 argued (see Minor issues number 1) that Lee et al. 2021 present ERF values instead of RF values.
  Original comment: *“1.) Lines 34-38: I suggest that you state that RF is a global (or at least regional) quantity, averaged over a long time period. On first reading, it was not so clear to me whether you refer to single contrails or contrails in general. Furthermore, aren't the quoted values ERF values (in Lee et al.) rather than RF values?”*
  Our response to the comment: *“To be consistent within the text, we removed the citation from Lee et al. (2021) and only kept the references from Boucher et al. (2021) and Burkhardt and Kärcher (2011), who give estimates for the radiative*

forcing."
As mentioned in the first round of replies and above, we want to be consistent in the compared values.

- **Line 72: You cite Bickel et al. when talking about the implementation of a contrail scheme within a climate model. Bickel et al does not include a description of the modelling approach. The description can be instead found in Bock and Burkhardt (2016).**

  We thank the Reviewer for this comment. Following the Reviewer's suggestion, we changed the citation to Bock and Burkhardt (2016).

Li, Y., Mahnke, C., Rohs, S., Bundke, U., Spelten, N., Dekoutsidis, G., Groß, S., /Voigt, C., Schumann, U., Petzold, A., Krämer, M. Upper-tropospheric slightly ice-subsaturated regions: frequency of occurrence and statistical evidence for the appearance of contrail cirrus, 2023, Atmos. Chem. Phys. , Vol. 23, No. 3, p. 2251-2271

Sanogo, S., Boucher, O., Bellouin, N., Borella, A., Wolf, K., Rohs, S., Variability in the properties of the distribution of the relative humidity with respect to ice: implications for contrail formation, 2024, Atmos. Chem. Phys. , Vol. 24, No. 9, p. 5495-5511