Discussion manuscript:

https://egusphere.copernicus.org/preprints/2024/egusphere-2024-2360/

The paper presents a mapping and adjustment procedure for IAGOS-MOZAIC and IAGOS-CORE humidity data from the ICH instruments using the more precise IAGOS-CARIBIC humidity measurements as reference. The idea to adjust ICH data looks certainly promising since it would allow to exploit the unique spatial and temporal coverage of the IAGOS-MOZAIC and CORE airborne data. As such, the resulting climatological data set can be of interest for a wider community of atmospheric scientists and is thus worth to be published. The paper is generally well written with a couple of minor issues stated in the specific comments and technical corrections. However, I have a couple of major concerns regarding the scope of the presented paper and the methodology which are detailed below and need to be addressed before publication.

Major comments:

- Scope of paper and journal: In the current state, the paper focusses on the description of mapping and adjustment methodology providing an adjusted H2O/RH data set as output. The scientific interpretation of the data set is limited to a short description of a few features which are directly evident from H2O maps at two different potential temperature levels (Fig. 10). In that sense, I see a lack of scientific novelty for an ACP research article and would rather shift it in the category "Technical note" where it would fit the scope of the journal. If the article shall stay in the research category, I strongly recommend to include one of the studies mentioned in the outlook section to strengthen the scientific impact (while dismissing parts which do not contribute to the main message of the paper, see below).

- Description of adjustment method (Sec 4.1): It is very hard for the reader to follow the description of the adjustment process since the paper only describes the performed steps without providing a context why they are performed. The authors should provide (1) a motivation why slicing the data as shown is done and (2) a structured guide through the process since the reader should theoretically be able to reproduce the data from the given description. Especially the reasoning behind the different FO thresholds and their subsequent treatment is not clear to me.

- In order to focus on the main message of the paper, I suggest moving the comparison between CARIBIC and JULIA to the appendix. It was certainly an important step in the data evaluation process, but a short description of the outcome (CARIBIC is suited as reference) is probably sufficient in the main text in order to focus on the main message of the work.

- A critical evaluation if adjusted data set has a real physical meaning in the LMS is missing in the paper. Or maybe more concise: Are you sure that ICH data in the LMS really represent the physical state of the atmosphere or could it just be "instrumental background"? At least from Fig 2 d and g this is very hard to tell and one might be trapped into fitting pretty constant background signals to also pretty constant stratospheric mean humidity values. Following that line of thought, is a bias correction for those altitudes justified given the very high variance in the data set? A discussion of detections limits, instrumental variance is necessary here.

- I would appreciate a quantitative discussion on the tradeoff "higher spatial-temporal coverage" vs "higher uncertainty" in Section 4.3 in order to see the added value of the adjusted MOZAIC/CORE dataset relative to the existing CARIBIC (optional JULIA) data sets.

- A statement on data availability is missing. Since the adjusted data set is the main result of this work, it should be available to the scientific community (at least after a certain amount of time). As a follow up on the previous comment, I would also encourage the authors to extend on the "guidelines" given in the outlook section regarding possible scientific use cases for the adjusted data set and state its limitations.

Specific comments:

- Fig. 3:
    - Dotted line is not explained in caption. Is it the actual flight path with color coded temperature?
    - Consequently, the bottom right RHliq Box in the figure should read $T^{(k+1)}$ instead of $T^{(k)}$.
- l. 161: The statement in that sentence is difficult to be confirmed by Fig 2: Where is the "wet stratospheric blop" from Fig 2f located in the potential temperature space?
- l. 272: from the Figure, there is no "scattering along the 1:1 line" below 6ppm but higher values for CARIBIC as also indicated by the reported mean value and stated in l. 279. Is the mean calculated for the whole 0..10ppm bin? Shouldn't it rather be a regression line with error margins?
- l. 284: Fig 4b does not show discrepancies, should it be Fig 4a?
- l. 286: that statement is strong and need discussion considering the difference lying at the edge of combined uncertainties and the error bars indicated in Fig 4a touching the identity line only for a rather small subset of points.
- l. 297ff: what is the hypothesis here? Is the fraction of RH values below 10% or the mean the driver for the bias as suggested by the reference? The pdfs in Fig 6b also show significantly higher values for MOZAI above 10% in both shown RH bins, so it looks more like a shift of the complete MOZAIC pdf to higher values plus a substantial increase in pdf width for the lower RH bin. I think that needs to be discussed since it is a clear indicator for the lower measurement limit of MOZAIC.
- l. 307: Fig 6a does not show distance to TP, please reformulate text.
- l. 314: obviously there are systematic biases, the filtering just eliminates one specific calibration issue -> please reformulate.
- l. 315: What is meant by "stronger variation of the bins"? is it higher variation of humidity values within the bins or a higher variation of bin mean values?
- Fig. 7a: black dot and black line is not explained, what are the leftover red lines? -> please consider revising the plot.
- l. 342: there are no regression lines in Fig 7cd -> do you mean the black dots?
- l. 359: Where is the black line in Fig 7b?
- l. 386: what is the "relative bias" and how is it calculated? Is that value considered as the uncertainty of the MOZAIC bin mean? In that case, this value is crucial for further handling of the data set and needs to be explained comprehensively. If I misinterpret that, such an uncertainty analysis has to be incorporated elsewhere in the manuscript.

- l. 391: due to which reason?
- Fig. 9a: the error budget seems very counter-intuitive: how can the error margin for the measurement uncertainty shrink to zero when approaching very low RH values? That looks like a flaw in the calculation method (or needs to be explained in detail)
- l.450: rephrase "detect" -> maybe "quantify"
- l. 452: There is likely also a sampling bias in IAGOS, e.g. due to avoidance of convective systems, turbulence and due to flying well within the jetstream on westbound flights and vice versa. That might lead to e.g. regions at the edge of the jetstream being underrepresented. Could be considered in the discussion.

Technical corrections:

l. 144: Figure 2 h & i instead of h-i

l. 154: Figure 2 e & f instead of e-f

l. 160: Figure 2f instead of Figure 2b

l. 163: Figure 2c instead of Figure 2a

l. 330: comma to be deleted

l. 316: right parenthesis missing

l. 359: calculated -> d is missing

l. 379: dot to be deleted

l. 457 & 458: H_2O and RH_{liq} with subscript

Fig. 8: reference in caption should be to Fig 6 instead of Fig 8