Response to reviews of "How adequately are elevated moist layers represented in reanalysis and satellite observations?"

Response to referees' comments

Title: How adequately are elevated moist layers represented in reanalysis and satellite observations?
Author(s): Marc Prange et al.
MS No.: egusphere-2022-755
MS type: Research article
Iteration: Revised Submission
doi: https://doi.org/10.5194/egusphere-2022-755

General Remarks

Dear Editor,
we thank the reviewers for their constructive comments and suggestions on improving the manuscript. In general, we are happy to read that the reviewers view the manuscript to be in a mature state and recommend publication after consideration of their comments. In the following we respond to each of the reviewers' comments point-by-point, starting with the comments of Nadia Smith. We also describe the associated change to the manuscript for each comment.

Comments by Nadia Smith:

Referee comment:
Line 3: should be "significantly affect" not effect

Response:
Implemented.

Referee comment:
Line 14: "compared to the three other datasets…" This creates confusion because the first sentence of the abstract lists three datasets in total. It was only when I read the rest of the paper that I learned the authors meant GRUAN, IASI L2 and ERA5. I suggest either listing GRUAN in the first sentence or removing the word "three" from this sentence.

Response:
We can see that removing the word "three" makes the sentence easier to follow, so we removed it.

Referee comment:
Line 15: "moist layer height of about 1.3 km…" I wonder if 1.3 km can realistically be called a "significant" bias (Line 14) given that the satellite sounding retrievals have a vertical resolution between 1 and 4 km, depending on pressure. Can the authors elaborate on this?

Response:
We agree with the referee that limited vertical resolution can cause height shifts of moisture features in the retrieval, for example when vertical resolution is height dependent. While in individual profiles this effect may sometimes be significant, it is unclear to us how this effect would be systematic and manifest as bias as strong as 1.3 km on the mean profile. If at all, we would expect an upward shift in the mid-tropospheric RH maximum associated with this effect because vertical resolution is typically better in higher altitudes which may cause a shift in a moisture anomaly's center of mass upwards where there is less smoothing. In addition, the other investigate datasets agree well, including the IASI L2 retrieval which has a similar intrinsic vertical resolution as CLIMCAPS. Hence, we view the moist layer height bias of 1.3 km in CLIMCAPS to be outside of typical vertical resolution induced errors and declare it as significant.

Line 225-238: We added a more detailed discussion about our reasoning for regarding the bias as significant in consideration of the retrieval's vertical resolution while discussing the mean profiles, which marks the first point in the manuscript where the bias is described.

Referee comment:
Line 55: "We address this gap in this study." While I loosely agree with the authors that the study of EMLs are underrepresented in hyperspectral IR product evaluations, I think it prudent to add a qualifier to this statement to suggest that the study of EMLs go beyond what the authors present in this paper. I suggest one of following edits: "We take steps to start addressing this gap" or "We partially address this gap". EMLs are three dimensional features spanning hundreds of kilometers, can last a day (many hours!) and are associated with deep convection globally and especially in the extra-tropics. In short, these are large features on a global scale. In this paper, the authors do not do a 3-D analysis, nor do a global evaluation. Instead, they use a point source dataset (GRUAN radiosondes) at one single location as the reference set, against which all other datasets are evaluated. At best the authors can conclude that at a specific site and for a specific location within an EML feature (3-D blob), their results hold true. Would this not be more accurate? Or do the authors feel confident that their results can be extrapolated globally? If so, kindly motivate.

Response:
We agree with the reviewer that choosing a more nuanced phrasing to describe our contribution to the scientific gaps around EMLs and their representation in satellite retrievals is beneficial here. The points raised by the reviewer make up a nice framework to describe our specific contribution to the field, which is a 1D analysis of EMLs, while questions remain about their 3D structure and representation in the investigated data products.

Line 56: We adjusted our phrasing according to the suggestion of the referee and qualify more what part of the scientific gap about the representation of EMLs in satellite products we aim to address in our study.

Referee comment:
Line 109: "The also available purely operational IASI L2 retrieval data…" Confusing sentence. Rephrase.

Response:
Line 115: We agree with the referee and adjusted the sentence.

Referee comment:
Section 2.4, Lines 129-143: The authors opted to use the relative humidity field that is reported in the CLIMCAPS L2 file on 66 pressure levels. This field is derived from the water vapor column density field [molec/cm2] retrieved directly from the IR radiances and reported on 100 pressure layers. It is possible that the vertical bias reported here is due to a shift from pressure layers (air_pres_lay) to levels (air_pres_h2o) when converting to relative humidity. Another issue, and one that is entirely the fault of the product team, is that the relative humidity field already has the boundary layer adjustment applied but this is not communicated in the technical documents (I discover to my dismay). The authors, therefore, didn't need to do this adjustment. I commend them, however, for following the science guides to a fault. In future I will be curious to learn if the authors report a similar bias when starting their analysis with the column density field instead (mol_lay/h2o_vap_mol_lay).

Response:
All results in our study are based on either the relative humidity profiles (Sect. 3, Climatological mean) or the specific humidity profiles, both of which are provided on pressure levels (air_pres_h2o). We transform the specific humidity values to volume mixing ratios for the analysis of moisture anomalies, which is simply the convention we stuck to from previous work (Prange et al., 2021). We understand that the used water vapor variables are derived from the water vapor column density field that is retrieved on 100 pressure layers (air_pres_lay). In this context, we understand the referee's comment as a hint that possibly the transformation of variables defined on pressure layers to variables defined on levels may be erroneous in CLIMCAPS, causing errors in the derived humidity fields that we use for our study. Unfortunately, we did not store the column density field as part of our data base associated with this study, but only the data fields that we used. We recently suffered a severe data loss on our server, making it quite work intensive to analyze additional data other than the data included in the paper. We still wanted to do some evaluation of whether there could be some error in the transformation from pressure layers to levels in CLIMCAPS that may contribute to our identified biases. I recently downloaded some new CLIMCAPS data for the subtropical region around Barbados. I randomly selected one overpass of the region, including 1350 retrieval pixels. Based on this testdata, I compared the derived CLIMCAPS mean profiles of specific humidity (spec_hum) and relative humidity (rel_hum) on the 66 pressure level grid (air_pres_h2o) to the mean profiles I derive myself from the H2O column density field (h2o_vap_mol_lay) provided on 100 pressure layers (air_pres_lay). To derive specific humidity from H2O column density I first had to transform H2O column densities [mol/m**2] to concentrations [mol/m**3] by dividing by

the height intervals between the pressure layers. I obtain the vertical height intervals of the pressure layers by assuming a hydrostatic atmosphere. For this purpose, I transformed the temperature profiles from levels (air_pres) to layers (air_pres_h2o) using Eq. 8 in the science application guide and applying the boundary layer adjustment as given by Eq. 12. Then I plugged the derived temperature profiles on layers and the pressure layers into hydrostatic relation and obtain heights associated with pressure layers. Before I transform the H2O column density to concentrations [mol/m**3], I apply the boundary layer adjustment described by Eq. 13 in the science application guide to get the bottom layer right. Then I divide the column densities by the height intervals between pressure layers to obtain concentrations [mol/m**3]. I transform these concentrations into volume mixing ratios (VMRs) given the following relation, where C is the concentration, p is pressure, k the Boltzmann constant and T the temperature:

$$\mathrm{VMR} = \frac{C}{p} kT$$

Finally, I transform the obtained VMR to specific humidity and relative humidity, with a saturation vapor pressure that depends on temperature as described by the IFS documentation (doi: 10.21957/4whwo8jw0). I find that the derived profiles of specific humidity and relative humidity in the CLIMCAPS product on pressure levels agree well with the profiles I derived myself from the H2O column densities on pressure layers (Fig. 1). Hence, it appears unlikely that the biases we find originate from an erroneous transformation of the derived humidity variables in CLIMCAPS to pressure levels.

Line 244: When first describing the identified biases in CLIMCAPS in the climatological profiles, we now added a discussion about having conducted this analysis that suggests the transformation from retrieved to derived variables and from pressure layers to levels is not likely to cause the found biases.

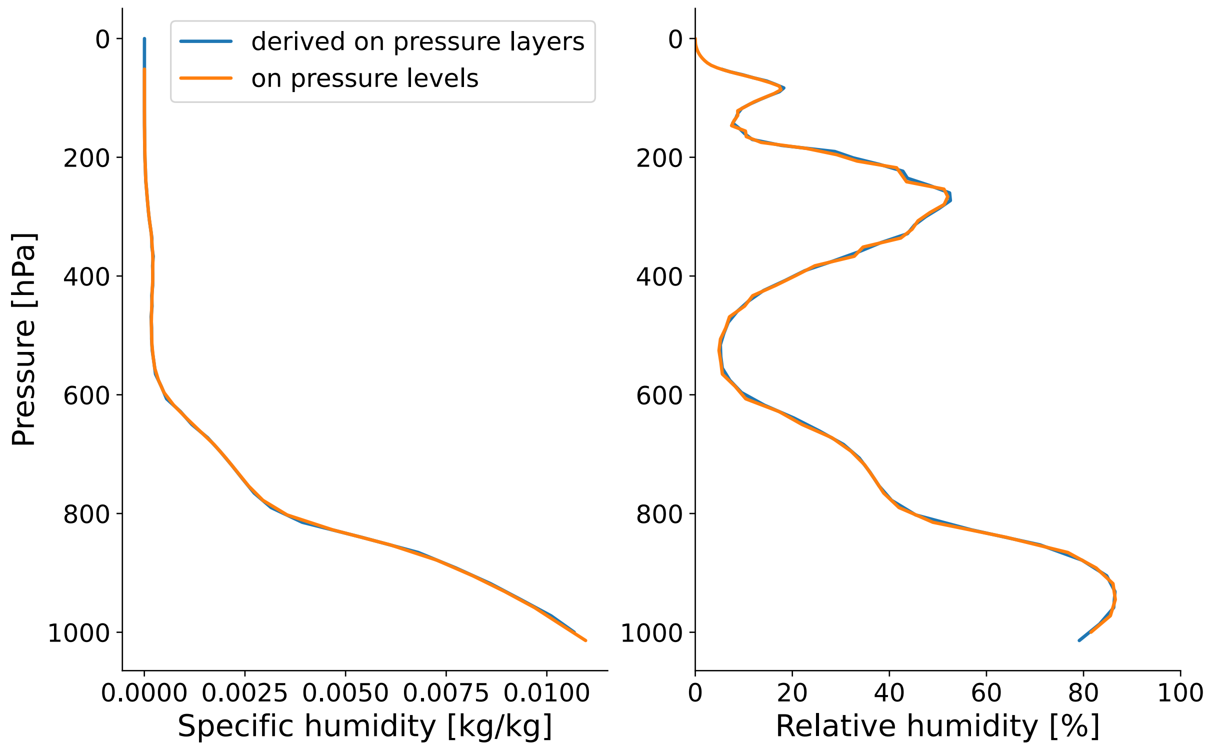

Figure 1: Profiles of specific humidity and relative humidity based on one AIRS overpass near Barbados on 2020/01/17. The profiles denoted as "derived on pressure layers" are derived from the H2O column density field (h2o_vapor_mol_lay). The

profiles denoted as "on pressure layers" are the profiles provided by the CLIMCAPS product on pressure levels (air_pres_h2o).

Referee comment:
Lines 154-156: "…the IASI product attempts retrieval through the clouds, CLIMCAPS…represent the atmospheric state around the clouds…". Does the IASI L2 product really retrieve through clouds? Can the authors explain this algorithm component in a sentence or two? Thinking out loud, I wonder if IASI L2 uses the collocated AVHRR cloud fractions to determine which regression coefficients to apply. But even then, the cloudy regression retrieval would not represent the atmosphere through the cloud. Infrared radiance is highly sensitive to clouds and does not transmit through opaque clouds. The IR radiances, therefore, do not contain information within and under such clouds. Can the authors elaborate on this distinction they're drawing here? This will help the reader better understand the results. As it is written and laid out currently, it appears that the authors say that there is no difference in EML detection between an algorithm scheme performing cloud clearing (aggregate footprints) and one retrieving through clouds (usually single footprint). But the IASI fields are also on aggregate footprints… I'm confused.

Response:
We can see that it is worth writing some more words about the differences in cloud handling of the IASI L2 CDR and CLIMCAPS. We agree with the reviewer's statements about inherently limited IR information content in the presence of clouds. However, a fundamental difference between the IASI L2 CDR and CLIMCAPS lies in the fact that for IASI there is humidity information available from a microwave instrument (MHS), which in fact contains humidity information in the presence of clouds. Hence, the IASI L2 CDR can actually be thought of as representing the atmospheric states through the clouds, as we write in the manuscript. In addition, we stand corrected on the spatial resolution of IASI L2 CDR retrieval pixels based on comments from Anonymous Referee #2. The retrieval resolution is actually the native resolution of IASI at 12 km. Given that the retrieval in cloudy scenes is conducted on the native pixel resolution, it has to be based on the limited information content within the scene rather than the state around the clouds in an aggregated retrieval footprint as is the case in CLIMCAPS.

Line 161: We added some more explanation to more clearly draw the distinction between the two datasets in how they are handling cloudy scenes.

Referee comment:
Lines 170-173: "As spatial and temporal collocation criteria we use 50 km and 30 min. These criteria are…conservative since the EMLs…have lifetimes of about a day." Given this, the authors could easily justify collocating the CLIMCAPS profiles to GRUAN sondes. Can the authors explain their adoption of this conservative approach? Do their results change when they adjust these criteria?

Response:
We chose our collocation criteria as a compromise of considering the match of satellite overpass times to radiosonde launch times, the temporal and spatial resolutions of the

datasets and atmospheric variability. As outlined in Sect. 2.5 of the manuscript, a 30 minute collocation criterion is the most conservative criterion to still obtain a high number of matchups between IASI and GRUAN and also between ERA5 and the other datasets. As discussed for example by Buehler et al. (2012), constraining collocation criteria to rather conservative values is important to assure direct comparability of the datasets because atmospheric variability can quickly become the significant source for differences. We see the referee's point that our phrasing gives the impression that due to their lifetime, EMLs may be comparable over several hours. However, comparability is not solely limited by the lifetime of the phenomenon of interest, but also by variability in other atmospheric variables, for example variability in cloudiness, which can change significantly over several hours. Hence, the question is whether an increase in temporal matching from 30 minutes to about 4 hours to obtain matchups between GRUAN and AIRS outweighs sacrificing direct comparability of the datasets. We think the answer depends on the added value from the GRUAN/AIRS collocations compared to what we already present. Since we find ERA5 to agree quite well in EML characteristics with GRUAN soundings based on a high number of matchups, we would argue that the most important biases in EML characteristics of CLIMCAPS can be identified with reference to ERA5 data. Hence, we would prefer to keep our analysis to the strict collocation criteria and not introduce different criteria for CLIMCAPS specifically.

Line 180: We rewrote the respective paragraph in Sect. 2.5, including a more nuanced discussion for our choice of sticking to strict collocation criteria.

Referee comment:
Lines 175-176: "In these cases [where multiple ERA5 pixels match up within an IASI FOV], we randomly select one of the matching pixels to assure that datapoints are only used once." I have two questions:

- *Can the authors clarify what they mean by using a datapoint only once? I struggle to understand under which conditions an ERA5 pixel will be used twice. The IASI/AIRS FOVs do not overlap and therefore would not contain sets of collocated ERA pixels that share members.*
- *Can the authors justify their choice against averaging the ERA5 pixels within each satellite sounding FOV? The authors demonstrate how the comparison between ERA5 and GRUAN sondes can be improved by vertically smoothing the sondes, which have higher resolution. Do the authors think that their comparisons between IASI/CLIMCAPS and ERA5 can be improved by spatial "smoothing" (averaging) of the higher resolution ERA5 data?*

Response:

- The referee raises valid points given our sparse description of this issue in the manuscript. In the updated manuscript we elaborate much more on what we mean by using a datapoint only once and in which cases this is relevant. The key issue here is the combination of the spatial resolution of the dataset and the collocation radius. In a case where the dataset resolution is higher than our 50 km collocation radius, several datapoints of a dataset can match with one pixel of the reference dataset. In these cases we do a random selection of the collocation. In addition, when the reference dataset has a higher resolution than the collocation radius, it can occur that the same pixel of the secondary dataset collocates with several pixels of the reference dataset. We reject such cases to end up with completely independent collocations where no data points are used more than once.

Line 194: We added a paragraph elaborating on this specific issue.

- The referee makes an interesting suggestion of averaging ERA5 pixels within the satellite sounding's FOV. It is true that no perfect agreement between two datasets on different horizontal resolutions can be expected given that the sampled atmospheric field is not completely homogeneous. The inherent assumption in our analysis, which we admittedly did not communicate clearly in the manuscript, is that differences in the datasets' effective vertical resolution are significantly bigger than the averaging effect of different horizontal resolutions on the vertical moisture structure. Given that we conduct a direct comparison of GRUAN point source measurements to 30 km resolution ERA5 data where we find a good agreement after just vertically smoothing the GRUAN profiles, we would argue that sub-grid variability in AIRS/IASI pixels is unlikely the main cause for the differences to ERA5.

  Line 172: We added a discussion about comparability of datasets on different horizontal resolutions to our description of the collocation procedure to give a more nuanced impression about "direct comparability" of the datasets.

Referee comment:
Lines 177-178: "Applying these collocations criteria…we obtain…2500 AIRS/ERA5 collocations." I find the discrepancy in total number of data pairs confusing. It will help the reader if the authors can explain these numbers here. Also, the total number of 2500 AIRS/ERA5 collocations looks like a rounded-off number.

Response:
We agree with the referee that the significantly deviating number of collocations between the different data pairs beg for an explanation to the reader. The first reason is that matches with ERA5 within our collocation criteria are available for all data points of the other datasets, yielding a high number of collocations for every dataset with reference to ERA5. The reason for the number of IASI/ERA5 and AIRS/ERA5 collocations deviating significantly lies in the different reduction of data points in IASI and AIRS retrievals when applying the respective quality criteria described in Sect. 2.3 and 2.4.

Although seeming rounded-off, the number of 2500 collocations for AIRS/ERA5 is correct.

Line 203: We added some discussion of what causes the severe differences in number of collocations between the collocation pairs.

Referee comment:
Lines 317-319: "Nonetheless, the number of moisture anomalies in the AIRS CLIMCAPS retrieval speaks [of] a good capability…to capture vertical moisture capability." This is a positive result as far as CLIMCAPS goes and surprised me. From the abstract and

introduction, I expected only negative results for CLIMCAPS. I wonder if the authors can update their abstract to reflect the value in different retrieval approaches, as far as EMLs go.

Response:
We understand the referee's perception here and thank them for pointing out this inconsistency in summarizing the notion of our results.

Line 16: We added two sentences in the abstract to also emphasize the capability of CLIMCAPS with regard to EMLs, which is by no means bad.

Referee comment:
Do the authors think that their results apply to reanalysis models in general, or to ERA5 specifically? CLIMCAPS uses MERRA-2 as a-priori for its water vapor column density retrievals and it will be interesting to know how much CLIMCAPS follows or deviates from the MERRA-2 fields, especially since it uses an optimal estimation scheme that gives it the ability to adjust a-priori fields based on scene-specific information content from the measurements. In future the authors could include an evaluation of the averaging kernels to help make sense of this.

Response:
We agree that an evaluation of other reanalysis models would be interesting and beneficial for our study, especially MERRA-2, which fills a similar role as ERA5 does for the IASI L2 CDR. We agree that it would be particularly interesting to see how an optimal estimation scheme deviates from the prior information compared to a regression-based scheme and whether one of the retrieval's priors (ERA5 and MERRA-2) performs significantly better than the other, possibly explaining some differences we find in the retrieval performances. However, we do not see that this can still be achieved within the frame of this study due to time constraints.

Line 516: We added some motivation of analyzing MERRA-2 in the context of our results in the future in our concluding remarks.

<u>Comments by Anonymous Referee #2:</u>

Referee comment:
The authors explain that the profiles come along with uncertainty estimates and that they reject the cases where errors on temperature are larger than 4K. This has the merit of rejecting the obvious poor retrievals from the all-sky retrievals (<1% of occurence), but still leaves retrievals of moderate to poor quality in the pool which is assessed. E.g. retrievals with temperature errors higher than 2 to 3K are arguably of lesser interest, esp. compared to models. Also, as evaluated in other studies, the cloudiness represented by the parameter OmC within the CDR has proven a valuable complementary information to the temperature uncertainty estimates for quality control (see work by Kirsti Salonen, ECMWF, https://www.eumetsat.int/IASI-assimilations, https://www.eumetsat.int/media/45896).

Including the OmC in the present evaluation might introduce too much complexity which *may not be necessary at this stage. At the minimum, the authors are encouraged to revisit if the statistics with IASI products significantly differ having selected the best and good retrievals (e.g. temperature uncertainty typically <1K and within 1-1.5K or 1-2K, respectively).*

Section 5/5.1: It would be interesting to be a bit more conservative with QC on IASI L2. temperature uncertainties >4K are the extremely poor retrievals. Completing the study by retaining the retrievals better than e.g. 1.5K (or 2K) for instance would be advisable.

Response:
We appreciate the referee's elaboration on why our applied quality filter criteria for IASI may be too loose and followed his suggestion of repeating our analysis based on a more strict filtering criterion. In Fig. 2 and 3 we show the resulting moist layer statistics based on the old temperature / dew point temperature uncertainty threshold of 4 K and for a more strict threshold of 1 K, respectively. The number of resulting collocations between IASI/GRUAN and IASI/ERA5 is a bit more than halved in doing so. While we see slight changes in the distributions of IASI moist layer statistics and a slight reduction in biases of EML thickness, the changes do not appear significant. Therefore, we would suggest to leave the results as they are and add a discussion about the effect of a more strict quality criterion.

Line 132: We adde a sentence about having tested the effect of changing the filtering criterion to 1 K.

It is interesting to learn about the use of OmC values for quality control. We agree with the referee that this could be interesting to evaluate. Since applying the strict quality filter of 1 K and also limiting the study to cases with AVHRR cloud fractions < 0.2 in Sect. 5.2 did not change our results significantly, we would be surprised to see a significant effect when using OmC. Due to a recent severe data loss on our server and the fact that we did not store OmC in the uploaded dataset on Zenodo, doing this analysis would mean a bigger amount of work, which we currently struggle to find the time for. Hence, we would leave the manuscript untouched in this regard.

While repeating our analysis we found an error in the calculation of smoothed GRUAN profiles, where we erroneously applied the 1 km smoothing twice. Hence, the revised manuscript contains an updated Fig. 4 where this is corrected. The qualitative conclusions deduced from our results do not change due to this mistake.

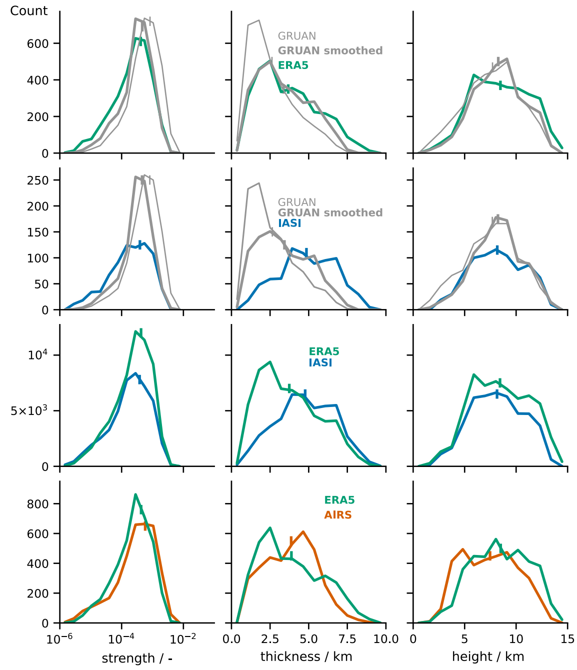

__Figure 3:__ Same as Fig. 4 in the manuscript. Moist layer characteristics for all-sky scenes of collocated datasets. For IASI a temperature and dewpoint temperature uncertainty threshold < 4 K is applied.

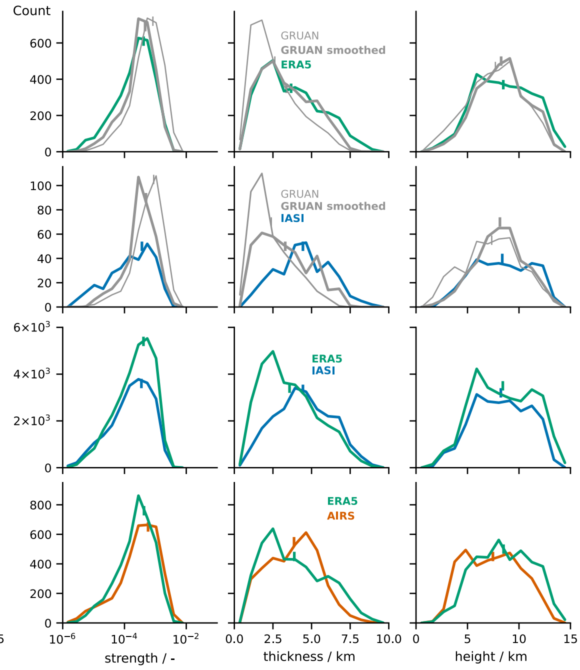

__Figure 2__: Same as Fig. 2, but for IASI a temperature and dewpoint temperature uncertainty threshold < 1 K is applied.

Referee comment:

The NOAA algorithms are also applied to IASI. It would be interesting to evaluate this dataset as well and inform further the reasons of CLIMCAPS-AIRS and EUMETSAT-IASI respective characteristics. At least mention this point in the conclusion/outlook as the merits of the respective methods are discussed.

Response:

We agree with the referee that expanding our analysis to other retrieval datasets such as NUCAPS (NOAA Unique Combined Atmospheric Processing System), which is applied to CrIS and IASI. However, we understand that the scopes of NUCAPS and CLIMCAPS are different, in that CLIMCAPS has the purpose to provide a consistent long-term climate data record of satellite derived geophysical variables while NUCAPS is aimed at real-time processing of satellite data (https://weather.msfc.nasa.gov/nucaps/). We are interested in EMLs to better understand their role for meso-scale dynamics and in the end for convective aggregation and cimate feedbacks. Hence, we chose the retrieval algorithm more dedicated to climate applications.

Line 72: We agree that we did not make this clear in the manuscript and added according information in the data section.

Referee comment:

- water-vapor —> water-vapour

- MetOp ==> Metop - Official spelling

Response:
Implemented.

Referee comment:
P5.L116: IASI L2 are actually retrieved at the native IASI pixel resolution: 12km at Nadir. Only the AMSU information is available at 2x2 IASI pixels resolution, IASI and MHS are exploited at their native sampling. This is different to CLIMCAPS AIRS/IASI/CrIS, whose retrievals are not at full IR sensor resolution, but at 2x2 or 3x3 pixel resolution due to the cloud-clearing.

P14.L345: IASI L2 comes at native IASI pixel resolution. 12km at Nadir, not 50km.

Response:
Thanks for pointing out this erroneous understanding of ours about the IASI L2 product. We corrected this information throughout the manuscript.

Referee comment:
P6.L162: why isn't there any direct CLIMCAPS vs GRUAN comparison? This does not sound logical and should be explained.

Response:
See response to comment of Nadia Smith.

Line 180: We rewrote the respective paragraph in Sect. 2.5, including a more nuanced discussion for our choice of sticking to strict collocation criteria.

Referee comment:
P7.L184: the notion of static stability would deserve a short explanation for the broader reader audience. It is important for the rest of the paper.

Response:
We can see that our introduction of static stability up to now was too crude.

Line 214: We added some explanation, also about how static stability is important within the context of this work.

Referee comment:
Section 5/5.1: why not smoothing GRUAN with 2km Gaussian window? This is the commonly accepted resolution of IASI and the basis for User Requirements. AIRS and CrIS also provide humidity profiles with a similar intrinsic vertical resolution. The 5km smoothing which is not helping is noted, however 2km would be more appropriate still wrt to User Requirements.

Response:
We applied varying smoothing windows with widths ranging from 1 to 5 km, including 2 km. None changed the resulting biases in moist layer characteristics significantly compared to

the 1 km window. Hence, we prefer to show the result for a 1 km smoothing window since it retains comparability to the results of ERA5/GRUAN.

Line 349: We slightly adjusted our phrasing to make clear that we did not only attempt smoothing with 1 km and 5 km windows, but also values inbetween.

Referee comment:
P12.L324-326: I would be careful with the statement that ERA-5 represents an upper limit. Mathematical modelling proves to yield higher precision than the original set, provided there is sufficient information in the predictors. In other words, IASI L2 is trained on ERA-5 and one would expect that it would perform at least as good as ERA-5 in terms of precision, at the scales that are accessible to the passive IR remote-sensing. The fact that it does not here (at least less than AIRS to some extents) may speak towards a suboptimal machine learning concept in view of EMLs. But it could also be the result of a loose QC on IASI L2. This is why it is important to confirmed the findings having applied criteria aiming the best retrievals.

Response:
We see the referee's point that the training dataset should rather be viewed as a baseline to which the remote sensing information is added, rather than an upper limit of what can be resolved. Hence, we removed the sentence where we made this point and leave the discussion with the suggestion that the machine learning algorithm may be insufficiently accounting for EMLs (Line 367).

Referee comment:
Conclusion - P21.L468: Actually EUMETSAT IASI L2 includes an optimal estimation (OE) as a second step - as explained in https://doi.org/10.1016/j.jqsrt.2012.02.028. The OE is part of the near-real production and further improves the PWLR (https://www.eumetsat.int/iasi-level-2-geophysical-products-monitoring-reports), yet it is not known whether this would be sufficient for the present application. The OE has not yet been applied in generating the CDR. Given the scientific feed-back and outlook proposed by the authors here, it would be useful to clarify and reference this.

Response:
We thank the referee for pointing this out to us. Indeed, our results based on CLIMCAPS suggest that an optimal estimation step may be worth implementing in addition to PWLR to the IASI L2 CDR.

Line 512: We added an according sentence to our concluding remarks.

References:

Prange, M., Brath, M., and Buehler, S. A.: Are elevated moist layers a blind spot for hyperspectral infrared sounders? A model study, Atmospheric Measurement Techniques, 14, 7025–7044, https://doi.org/https://doi.org/10.5194/amt-14-7025-2021, 2021.

Buehler, S. A., Östman, S., Melsheimer, C., Holl, G., Eliasson, S., John, V. O., Blumenstock, T., Hase, F., Elgered, G., Raffalski, U., Nasuno, T., Satoh, M., Milz, M., and Mendrok, J.: A multi-instrument comparison of integrated water vapour measurements at a high latitude site, https://doi.org/10.5194/acpd-12-21013-2012, 2012.