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Interactive comment on "A tropopause-based a priori for IASI-SOFRID Ozone retrievals: improvements and validation" *by* Brice Barret et al.

Anonymous Referee #1

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The paper by Barret et al. presents a comparison of two versions of the IASI O3 product based on the SOFRID algorithm, one version (v1.6) using a single a priori profile, and the other version (v3.5) using a dynamical a priori based on climatological profiles depending on latitude, season and tropopause height. The paper also presents a comparison with another IASI O3 product using the FORLI algorithm. This comparison is based on FORLI's results from literature.

The paper is interesting mainly for two reasons: - the validation approach, including raw and smoothed profile and variability analysis, is very complete and provide guidelines for validation of satellite products. - The choice of the a priori profile to use in the

retrieval is still a question for the community. The paper shows the influence this choice can have on the retrieval results. For these reasons, the paper is suitable for publication in AMT. However, several key issues need to be addressed and better discussed before publication:

1. The new SOFRID product, v3.5, shows well visible stripes, especially in the southern hemisphere. Even if the authors mentioned that these discontinuities are consistent with their retrieval errors, these unphysical discontinuities would bring some difficulties to compare and evaluate models for example. The authors show how important it is to compare raw and smoothed data to satellite observation validation but it would be the same for model comparison and these stripes will compromise the comparison. The authors should mention these difficulties in using their product for model comparison and provide some possible ways to overcome these artifacts.

2. The authors state, especially in the conclusion, that the improvement of their new approach (dynamical a priori) is mainly due to the consideration of the tropopause height in the choice of the a priori. But they do not demonstrate the impact of taking an a priori profile on this basis, as they do not consider this selection independently from the latitude and season selection. Intuitively, we would expect that a better consideration of the tropopause would help resolving the biases in the UTLS, but no specific improvement are shown between v1.6 and v3.5. I would suspect that the bias correction is more related to the latitude/season selection as the v1.6 was based on a NH a priori only. The authors should better discuss this in the paper and show how the tropopause selection impact their retrieval if they think it is a key point. Moreover, the authors state that this dynamical approach based on the tropopause selection is presented for the first time. It is not completely true. The authors should refer to different publications using the KOPRAFIT-O3 algorithm they mentioned in the paper in which the selection of the a priori (and regularization) is based on the tropopause height (Dufour et al., ACP, 2015, 2018 and Eremenko et al., JQSRT, 2019).

3. Concerning the comparison with FORLI, as the authors use information from litera-

ture, the sampled pixels are likely different between the two algorithms and it can have a possible impact on the statistics, in particular if the cloud mask considered by the two algorithms is different. No information concerning the cloud filtering is mentioned for both SOFRID and FORLI. This should be added and discuss.

4. The quality of presentation of the results is sometimes poor and not suitable for publication. Figure and Table captions miss a lot of information such as units. A lot of typos remain. The authors should have read carefully their manuscript. In section 5 the authors referred to FORLI 16 and 18 on the Figures they comments but the Figures available online only show information named FORLI in green, whereas in the submitted paper for the quick review, both were present. Please, be consistent between the figure and the text.

Specific comments:

- p1, line 18: should we read theoretical or theoretically?

- P2, line 31: troposphEric

- P3, line24: quantify “weakly contaminated”

- P4, line 1 and 20: Is there a difference for ozone between HITRAN2004 and HITRAN2008?

- P5, line 11: emmissivity should be emissivity

- P5, Equation 1 : G is not define in the text.

- P5, line 22, change “devided” to “divided”

- P6, line 13: change “chinese” to “Chinese”

- P7, line 20: could you please clarify if the monthly mean is computed with at least 3 profiles by stations or by latitude bands? If it is by latitude bands, is it sufficiently representative?

- P7, line 21: Jcost is not defined

- P8, line 8: change “interanual” to “interannual”

- p8, line 13: documentS

- Table 2: please specify the units.

- P10, line 16: mid anD high latitudes

- P10, line 19: improvEment

- P10, line 27 and p11, line26: change “dissapears” to “disappears”

- P12, line 19: missing units

- P13, lines 10-13: the discussion is not clear. The authors state first the reason of the differences is the noise level and then it is not clear. They should provide the noise levels for the different algorithms to elaborate their hypothesis.

- P13, lines 6-34: the discussion is not consistent with the figure (no FORLI 16 and 18 display in the Figures).

- P14, line 5: change “positives” to “positive”

- P15, line 12: drifTs

- Figure 1: what are the error units? The way the authors present the plots with +/- for different variables is not very conventional. They should explain more precisely in the caption how to read the figure (this is also the case for Figs. 13 and 14).

- Figure 2: units are missing

- Figure 6: explain RS and SmRS, please

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