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Atmospheric Measurement Techniques Discussions

Interactive comment on "Validation of the IASI FORLI/Eumetsat ozone products using satellite (GOME-2), ground-based (Brewer-Dobson, SAOZ) and ozonesonde measurements" *by* Anne Boynard et al.

Anonymous Referee #2

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Review of "Validation of the IASI FORLI/Eumetsat ozone products using satellite (GOME-2), ground-based (Brewer-Dobson, SAOZ) and ozonesonde measurements"

General comments

The manuscript titled "Validation of the IASI FORLI/Eumetsat ozone products using satellite (GOME-2), ground-based (Brewer-Dobson, SAOZ) and ozonesonde measurements" give a thorough validation study of 9 years of IASI ozone measurement. The manuscript is well written, clear and easy to read.

However, it is not easy to understand whether this paper (Boynard et al., 2017) presents novel concepts, ideas, tools or data, especially when we compare Boynard et al. (2017) to Boynard et al. (2016). Many conclusions in Boynard et al. (2016) and in Boynard et al. (2017) are similar.
Were the authors expecting different results between IASI v20140922 and v20151001? According to Boynard et al. (2016), the improvement of IASI retrieval was already found to be mainly located in the middle stratosphere.
How much could the bias assessment change with 2 more years of data?
9 years of data allow the authors to address the long-term stability of IASI. This is the most interesting and the newest part of the study. Unfortunately the significant drift in the troposphere is barely explained and addressed.
Furthermore, Boynard et al. (2017) didn't address open questions already mentioned in the conclusions of Boynard et al. (2016), such as the large bias found in the UTLS, still not fully understood.
For these reasons, I would suggest major corrections before the current manuscript can be published in AMT.

Specific comment:

- Section 3: Intercomparison between IASI-A and IASI-B ozone products
Line 18 p. 5: Change “the figure” to “Figure 2”

Line 2 p. 6: Change “then” to “October 2015”

Line 14 p. 6: April-October 2015 shouldn't be included in the combined IASI-A/IASI-B product (as explained in Line 10), because of instrumental issues on IASI-A. Should this time-period be excluded from any time-series studies with IASI-A?

- Section 4: Validation of IASI-A and IASI-B total ozone columns
I would suggest to move all the validation method in one method section. The method section could then include: (1) the formulae of differences calculation, (2) The method of co-location between IASI and reference observations, (3) the characteristics of the

data used for the comparison. This change would help the authors to shorten several sub-sections.

Line 25-27 p. 7: This statement is almost word to word the same as in Boynard et al. (2016). Don't the author think that "Further investigation would be needed to understand the reasons of these larger differences at high latitude" should be addressed in the current study?

Line 33 p. 7: Would it be possible to quantify the "better agreement"?

Lines 3-11 p. 8: All this paragraph is already stated in Boynard et al. (2016). It could be either removed or shortened.

Line 10 p. 8: Would it be possible to quantify the "magnitude"?

Line 28 p. 8: Could you explain why the number of stations would influence the dependency on the latitude of the differences between IASI and GB measurements?

Line 29 p. 8: The differences between IASI-A and Dobson seem to reach 3.5% in NH, while the authors report [0-2.5%]

Line 30 p. 8: "Lower than 40°S" would mean somewhere between 0 and 40°S. Do you mean between 40°S and 60°S? Please clarify.

Line 1 p. 9: It is worth to notice there is no Brewer measurements in SH.

Line 2 p. 9: Change "belt" to "region'.

Lines 5-6 p. 9: Could you explicitly mention the 1-3% requirement from the Ozone_cci project instead of " within $\pm 3\%''$.

Line 11 p. 9: Change "small" to " < 3% ".

Lines 12-13 p. 9: In the (new) method section I would suggest to explain the ozone-cci project and their requirement in term of satellite products stability. Could you explain how the 1-3% requirement has been decided? According to the 1-3% requirement, “IASI-A TOC products are reliable for trend studies”. Does it mean no drift adjustment at all is required? And does it mean that the drift, even small, is not taken into account in the ozone trends uncertainties? Could you please explain?

Line 13 p. 9: Which criteria is used to qualify differences “within 1.1%” as “very good agreement”?

Lines 16-20 p. 9: This paragraph is not clear. It is hard to understand what would explain differences in the seasonal variability between Dobson and Brewer. What does 0.6% represent?

- Section 5: Validation of IASI-A and IASI-B partial ozone column products
As mention for Section 4, I would suggest to move the comparison method in one method section.

Line 16 p.10: Could you report the numbers of the “small or non-significant negative decadal trends”?

Lines 16-17 p.10: Could you refer again to the 1-3% requirement with the reference of the Ozone_cci project?

Line 27 p. 10: “[. . .] their uncertainties are lower than other types of ozonesondes [. . .]” Could you quantify?

Lines 6-12 p. 11: The common method to compare satellite data with ozonesondes is to degrade the high vertically resolved ozonesondes by applying the AKs and a priori ozone profiles used to retrieve satellite ozone products. In Huang et al. (2017), they use the high vertically resolved ozonesondes (without degrading the vertical resolution) in the regions and altitudes when the satellite has low retrieval sensitivity.
Could you comment on this? Is such analysis could be done in your study?
Huang, G., Liu, X., Chance, K., Yang, K., Bhartia, P. K., Cai, Z., Allaart, M., Ancellet, G., Calpini, B., Coetzee, G. J. R., Cuevas-Agulló, E., Cupeiro, M., De Backer, H., Dubey, M. K., Fuelberg, H. E., Fujiwara, M., Godin-Beekmann, S., Hall, T. J., Johnson,

B., Joseph, E., Kivi, R., Kois, B., Komala, N., König-Langlo, G., Laneve, G., Leblanc, T., Marchand, M., Minschwaner, K. R., Morris, G., Newchurch, M. J., Ogino, S. Y., Ohkawara, N., Piters, A. J. M., Posny, F., Querel, R., Scheele, R., Schmidlin, F. J., Schnell, R. C., Schrems, O., Selkirk, H., Shiotani, M., Skrivánková, P., Stübi, R., Taha, G., Tarasick, D. W., Thompson, A. M., Thouret, V., Tully, M. B., Van Malderen, R., Vömel, H., von der Gathen, P., Witte, J. C., and Yela, M.: Validation of 10-year SAO OMI Ozone Profile (PROFOZ) product using ozonesonde observations, Atmos. Meas. Tech., 10, 2455-2475, 10.5194/amt-10-2455-2017, 2017.

Lines 18 and 31 p. 12: The selection of the ozonesondes stations are confusing. Why don't you use all the ozonesondes stations that meet the criteria needed for the comparison such as long-term time series, statistics of the data, etc. . .?

Lines 14-24 p. 13: This part of the discussion is one of the most interesting but it is too short. Would it be possible to address at least one of the speculative explanation for such a drift?

- Summary

Line 4 p. 14: Would you suggest to remove the data between April and September 2015 (October 2015 in the main text) for trends studies? If so, could you mention it? Would it be possible to apply any corrections factor on the data for this time-period?

Line 12-14 p. 14: What do you mean by "due to larger differences at the southern high latitudes"? The sentence is not clear.

Line 20 p. 14: Could you report the numbers for "insignificant negative trends"? Do you refer to the P-value for "insignificant"?

Line 25 p. 14: The statement about the large biases found in the UTLS was already mentioned in Boynard et al. (2016), but still it is not fully understood. Could you address this question in your study?

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