

Interactive comment on “Improved GOMOS/Envisat ozone retrievals in the upper troposphere and the lower stratosphere” by Viktoria F. Sofieva et al.

Anonymous Referee #1

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This paper presents a new retrieval algorithm for ozone profiles from the GOMOS instrument. The currently available data product, version 6, has a clear positive bias in the UTLS region that appears to be a result of interference from aerosol. This new ALGOMs v1.0 data product uses a well demonstrated DOAS-like retrieval technique to retrieve ozone in such a way as to be less sensitive to aerosol interference. The resulting data product appears to significantly reduce the bias in the UTLS in much better agreement with sondes and other satellite data sets. As always, it is important to publish updates to and validation of retrieval algorithms of widely used data sets and thus this paper is suitable for publication in Atmospheric Measurement Techniques. However, I do have a few questions/concerns regarding this paper and the work therein.

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Major Corrections:

The use of the triplet method to produce ALGOMs v1.0 data is a good idea to mitigate the influence of aerosol in the UTLS. However, the methodology ought to be valid as a means of retrieval for the entire profile as well and, as such, would present an entirely new data product. The authors should present the fidelity of this data product across the full range of altitudes and compare to the V6 method. This is particularly applicable as the authors appear to choose an altitude cutoff ($Z_{TROP}+6\text{km}$) above which the triplet method is not included (or in the case where V6 terminates above the tropopause, the triplet method is not performed at all). Generally, arbitrary changes or transitions in the retrieval algorithm will cause anomalous effects that are revealed when analyzing data in bulk. While one would assume that the two methods would be nearly equivalent in the absence of aerosols higher in the atmosphere, I would guess that, due to algorithmic effects, they may not be. If the two methods do differ significantly at higher altitudes, my recommendation would be to release both as separate data products as well as a third, merged-data product (what the authors are currently presenting) as their recommended product to use. This would give maximum utility to the data user while presenting the opportunity for additional validation of the new retrieval. Of course, this recommendation of an extra data product is only a side-note to the authors and is not required for this paper.

Since the new ALGOMs methodology attempts to retrieve ozone in a way that is less sensitive to interference from aerosol, it would stand to reason that a better ozone retrieval should also result in a better aerosol retrieval. Since the retrieval of different species are often inter-dependent, it would be interesting to see how this new ozone retrieval impacts the resulting aerosol extinctions and whether they make sense or not when compared with other data sets. At the very least, the ALGOMs aerosol and V6 aerosol should be compared to ensure there are no sudden jumps in aerosol extinction at the transition, unless of course this new ozone retrieval has not been incorporated into a new aerosol extinction product as well. Then again, and at the discretion of the

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editor, perhaps this is beyond the scope of this paper.

I do not agree with the use of the term “aerosol-insensitive” retrieval. While I would expect the triplet method to be much less sensitive to interference from aerosol loading, no sensitivity analysis is performed as was done to quantify the effect of different aerosol models on V6. This ties somewhat into the previous comment about needing to investigate the result on the aerosol product.

Minor Corrections:

Table 1 shows the number of colocations in the UTLS. How is this defined? Is it simply $Z_{TROP}+6\text{km}$ as used later in the paper?

Page 7, Line 1 notes that “the results are in perfect agreement” with Hubert et al. 2015. “Perfect agreement” is too strong of a statement, particularly since no direct comparison is made between this study and Hubert et al. and cannot be made given the differences in scale of figures in that paper and this one. Additionally, Hubert et al. 2015 is now fully published and so the reference should be updated accordingly.

Earlier in the paper, the term ozone “horizontal column density” is used but then it appears this terminology is changed later in the paper to ozone “line density”. To avoid any confusion related to spectroscopy, I would suggest maintaining the usage of the term “horizontal column density” throughout the paper. For the sake of brevity, simply introducing “HCD” may make things easier.

Page 15, Line 13: “Also reduction of the spread in the UTLS is clearly observed for new ALGOM2s retrievals, as illustrated on the right panels of Figures 9 and 10.” With the exception of comparisons at La Reunion, I do not agree that a reduction is “clearly observed.”

With regard to Figure 11 (right), normally I would advocate for showing percent comparisons over absolute comparisons. However, for this particular case, I would argue that 11 (right) does not add any informational value over 11 (center). Instead, given the

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reduction in bias in the UTLS, it requires additional explanation to explain why the absolute uncertainty decreases and the relative uncertainty increases. As such, I would advocate eliminating the rightmost figure in Figure 11.

I would generally clean up the figures. At full size, they are legible but once they are shrunk down to a standard size for a published paper, many of the axes and labels will be too small to read.

Grammatical Corrections:

Page 9, Line 7: “ozone and aerosol number density” should be “ozone number density and aerosol extinction”

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