

Interactive comment on “Sensitivity of the OMI ozone profile retrieval (OMO3PR) to a priori assumptions” by T. Mielonen et al.

T. Mielonen et al.

tero.mielonen@fmi.fi

Received and published: 11 June 2014

Replies to the comments by Referee 2 concerning the manuscript “Sensitivity of the OMI ozone profile retrieval (OMO3PR) to a priori assumptions”

We thank the Referee 2 for the constructive comments which we have taken into account in the revised version of the manuscript. It appears, that the aims of the paper were not stated clearly enough. To clarify this, we are not making a new version of the operational OMI retrieval, instead, we are studying if we could get more information from the troposphere with this instrument. As the referee points out, it is still an open question if OMI can provide scientific information from the lowest altitudes, and we are trying to answer that question. Therefore, we feel that these results are interesting also

C1312

for a wider audience.

Our replies to the comments are given below and the manuscript has been revised accordingly.

Though the paper is generally well written, it has a rather narrow focus. The scope of the work seems more appropriate for a technical report rather than for a scientific paper. A scientific paper needs to provide information that can be useful to the broader readers of a journal. Unfortunately, this is not the case here. The goal of the investigation described in this paper was to make limited number of changes to the input parameters used in the OMI operational ozone profile algorithm and decide which changes are improving the results. Unfortunately, this decision is based solely on their impact on the ozone column in a layer at the bottom 6 km of the atmosphere. While it is debatable whether OMI measurements contain useful scientific information about 0-6 km ozone, there is large body of work published in the past several decades that shows that satellite backscatter UV instruments such as OMI contain useful information about ozone between surface and 50 km, with vertical resolution varying from 10-15 km in the lower altitudes to 5 km near 3 hPa. Since there are plenty of high quality measurements available to validate OMI profiles in this altitude range, the logical thing would have been to assess improvements to the algorithm by comparing the full ozone profiles with other datasets. Judging the algorithm improvement by focusing on just the bottom layer is akin to looking at the tail of an animal to judge the health of the animal.

- As we stated above, the aim of the paper was to study if the OMI ozone profile retrieval could be used to obtain information on the tropospheric ozone abundances, not to present a new version of the algorithm. We have clarified the aims of this study in the revised manuscript as follows: “In this work, we studied how different assumptions in the OMO3PR retrieval affect the retrieved ozone profiles and searched for ways to improve the accuracy of the retrieval in the troposphere.” and “The main goal of this study is to see if an UV instrument such an OMI could be used to retrieve tropospheric

C1313

ozone abundance which is a very important component for air quality. ”

Though I do not recommend publication of this paper in AMT, I am providing detailed comments that can be used to revise the paper. Detailed Comments: 1. The title of the paper itself indicates that the paper is too narrowly focused and gives the impression that no definitive conclusions are derived. If the authors believe that they have come up with significant improvements to the algorithm based on their study and they propose to implement these improvements in the near future, a better title would be “Improvements to the OMI ozone profile algorithm”. Still, the paper would need to show comparisons of full profiles with other datasets to validate the improvements.

- We agree with the referee that additional comparisons would improve the paper and, therefore, the revised manuscript includes a new section (4.2) where we have compared the operational and modified retrievals with ozonesondes over North America. We also agree with the referee, that the title does not describe the content of the paper properly, and we suggest that the title will be changed to “Technical Note: Sensitivity of the tropospheric ozone profile to a priori assumptions in the OMI ozone profile retrieval (OMO3PR)”.

2. The paper uses the term “a priori” rather generously in the title and throughout the paper to describe assumptions that are typically not characterized this way. The terms “a priori” and “a posteriori” are typically used in making Bayesian inference where one updates the probability estimate for a hypothesis by new evidence.

- Use of the term “a priori” in the paper follows the style that has been used in previous OMO3PR publications.

I find it hard to conclude based on the evidence presented that we know more about OMI stray light correction accuracy, spectral dependence of surface reflectance in the UV, or about ozone profiles and their covariances, based on this study. These are the four issues that the paper focuses on. The following discusses each of these 4 items in more detail. 3. The study on stray light described in Section 3.1 is puzzling. It is

C1314

not clear what was the objective of this study and what conclusions have been derived. One assumes that the stray light correction applied for producing calibrated OMI radiances are based on hard instrument data, and the procedure has been validated by more direct methods than looking at derived ozone. If not then this would appear to be a serious shortcoming of the OMI project. However, I can see one using the derived ozone data to assess if the straylight correction is working well. If that was the objective then the upper stratospheric ozone should have been the primary focus since the shorter wavelengths are much more sensitive to stray light. One could have compared upper level ozone with sensors such as MLS and MIPAS that do well in the upper stratosphere and could have examined if the differences are correlated with cloud reflectivity or lower level O₃, which are the primary causes of variability in straylight at the shorter wavelengths.

- The object of the study was to see how the stray light correction affects the ozone profile retrievals, especially at the lowest altitudes. The conclusion we draw from this study is that stray light correction plays a crucial role at the highest altitudes but it does not have a significant effect close to the surface. Therefore, in our attempt to improve the retrieval of the lowest layers, we can trust that the stray light correction is done well enough. We have clarified the text accordingly.

4. The study of surface albedo parametrization discussed in section 3.2 is equally flawed. Firstly, if a quantity is made a part of the state vector in an OE retrieval then it is important to look at the retrieved product to see if it makes any geophysical sense. No evidence has been presented to indicate that it is so. One wonders if what the authors call “surface albedo” is actually a proxy for something else, such as error in instrument calibration, aerosols, Ring effect etc. Secondly, as discussed earlier, validation of this correction method requires looking at other layers of the retrieved profiles and comparing with other datasets. How about the total or tropospheric column ozone obtained by integrating the retrieved profile? Is it affected by changing the albedo parametrization. If it is then such columns should be validated by comparing with other

C1315

instruments, such as ozonesondes, Dobsons and Brewers.

- The referee is right that the surface albedo is not a geophysical parameter in the algorithm. As we mention in the text "The wavelength dependence of the albedo in both UV1 and UV2 channels is described with a second order polynomial. Surface albedo is fitted for all wavelengths (although the shortest ones do not "see" the surface) to partly account for the presence of aerosols which are not known or modeled specifically in the retrieval." In Table 4 of the manuscript we compare different versions of the OMO3PR algorithm to IASI retrievals. We show there that with the operational surface albedo parameterization and with the TpO3 climatology the OMO3PR retrieval overestimates tropospheric ozone by 5.19 DU (23 %) on average. With the new surface albedo parameterization the overestimation is reduced slightly to 4.32 DU (19 %) on average.

5. *In section 3.3 the authors look at the effect of changing the a priori ozone profiles from a static climatology that varies with latitude and month to a dynamical climatology that also varies with tropopause height. This is the only part of the study that would be of interest to the broader science community. However, it is disappointing that the authors have failed to show that this change significantly improves the retrievals in the upper trop/lower strat (UTLS) region when compared to other sensors. This is the region where the impact of a dynamical climatology is likely to be largest.*

- Sofieva et al. (2014) have already shown that the effect of using the TpO3 climatology maximizes between 60S and 70S, where a nearly stationary springtime ozone zonal anomaly is observed, and the abundance of stratospheric ozone and the tropopause height are interrelated. In the revised manuscript, we discuss this topic in Section 4.2, where we compare our retrievals with ozonesondes: "the modification of the OMO3PR algorithm improves the agreement with ozonesondes at the lowest two layers and at altitudes over 20 km. In the upper troposphere/lower stratosphere (UTLS) the operational retrieval performs better. The reason for this is the significantly larger climatological ozone values and a priori errors used in the modified algorithm at these altitudes.

C1316

Although the modified algorithm is not able to improve the performance of the retrieval for the whole atmosphere, it improves the correspondence with ozonesondes in the troposphere which was the main goal of this research."

6. *Finally, the authors discuss the impact of changing the fixed 20% standard deviation of ozone assumed in the operational algorithm to a more realistic covariance matrix derived from prior data. Since 20% assumption is not consistent with the known variability of ozone in the earth's atmosphere, changing it to a more realistic value is a good idea. But, as before, the impact of this change is discussed in a qualitative way and more important aspects of this change has been missed. In OE the a priori covariance matrix in combination with the measurement error covariance matrix determines the vertical resolution of the retrieved profile as well as the sensitivity of the retrieval profile to measurement errors. If one increases the assumed variance of ozone in a layer one improves the vertical resolution but one makes the algorithm more sensitive to measurement (and forward model) errors. Based on Fig. 5 one expects that the new covariance matrix will degrade the vertical resolution of the retrieved profiles in the upper stratosphere but will reduce their sensitivity to instrument errors such as stray light, which is desirable. In the lower levels one expects the opposite, which is also desirable. It seems that the authors have missed this important aspect of changing the a priori covariance matrix completely.*

- We agree that the a-priori covariance matrix in combination with the measurement error covariance matrix determines the a posteriori error covariance matrix and the averaging kernel. The reviewer points out that the change in averaging kernel also leads to a change in the vertical resolution of the retrieved profile. In our view there are two ways to discuss the vertical resolution, namely considering the width of the averaging kernel or by considering the degrees of freedom (DFS). For instance, if the DFS increases for the troposphere then, automatically, the vertical resolution in the troposphere increases. In section 3.5 we discuss the changes in terms of the DFS and not the vertical resolution. Hence, we do not agree that we missed this aspect of changes

C1317

in the vertical resolution due to changes in the a priori error covariance matrix, but simply described it in terms of changes in the DFS. In order to avoid possible misunderstandings, we added the following paragraph to Section 3.5: "Above we discussed the effect of changes in the a priori error covariance matrix on the degrees of freedom of the troposphere and stratosphere. Alternatively, one could discuss these changes in terms of changes in the vertical resolution of the retrieved profile, which provides essentially the same results. If the degrees of freedom of the troposphere increase, the vertical resolution in the troposphere will also increase. Similarly, if the degrees of freedom of the troposphere decrease, the vertical resolution decreases as well."

References

Sofieva, V. F., Tamminen, J., Kyrölä, E., Mielonen, T., Veeffkind, P., Hassler, B., and Bodeker, G.E.: A novel tropopause-related climatology of ozone profiles, *Atmos. Chem. Phys.*, 14, 283-299, doi:10.5194/acp-14-283-2014, 2014.

Interactive comment on *Atmos. Meas. Tech. Discuss.*, 7, 1835, 2014.