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Atmospheric Measurement Techniques Discussions

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

Anonymous Referee #2

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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.

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 pair 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. 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. 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 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. 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 instruments, such as ozonesondes, Dobsons and Brewers. 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. 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.

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