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Atmospheric Measurement Techniques Discussions

Interactive comment on "Deriving the slit functions from OMI solar observations and its implications for ozone-profile retrieval" *by* Kang Sun et al.

Anonymous Referee #1

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The article, "Deriving the slit functions from OMI solar observations and its implications for ozone-profile retrieval", by Kang Sun et al., addresses two important subjects.

1. Until recently, a proper characterization of the on-orbit evolution of the OMI's instrument transfer function (ITF) was lacking. This subject was discussed in Beirle et al. (2017) and Schenkeveld et al. (2017), however assessing the OMI ITF via different means.

2. The article also breaches the important and rarely discussed subject of the potential impact of the variable (∼11yr cycle) solar spectrum on the ITF characterization.

This is a comprehensive and timely study that deserves prompt publication.

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Discussion paper

The final revision may benefit from addressing the following points:

1. Over the OMI mission time there are small, but systematic wavelength shifts (e.g., Schenkeveld et al. 2017) that may affect the ITF evaluations, the UV1 range in particular. The ITF fitting algorithm includes the wavelength shift/squeeze terms (p.4,l.7). How do these terms compare to the estimates from Schenkeveld et al. (2017)? Decision about the optimal choice of ITF form should rely on a multitude of criteria (see below). The temporal, spatial (FOV, i.e., row-wise) and wavelength dependence of the shifts may be one of such criteria.

2. The solar irradiances are practically row-anomaly free in the UV2 and VIS channels. The current study uses the 3-year averaged reference spectrum, yy2005-2007. Would the UV2 and VIS fitting trends change if the averaged solar-minimum spectrum ($\sim$mid2007- $\sim$mid-2009) is used instead? This deserves a comment, potentially strengthening the author's conclusion that most of the detected temporal ITF variability is related to the Solar Cycle.

3. p.5., l.6: 'The OMI ozone-profile retrievals have substantially higher relative accuracy than other OMI products...'. The proposed O3-based validation clearly shows the need for adjustment of the pre- flight ITF values. The study, however, provides no clues to what parametric ITF form could be the most beneficial to the various trace-gas retrieval algorithms that may have far higher sensitivity to the ITF changes. As formulated, this extensive study falls beyond the scope of the paper. However, the revised text may include some additional stats that quantify performance of the explored ITF approximations. Besides the wavelength shifts (as mentioned above), the temporal and x-track behavior of the fitting residuals may help to decide about the optimal ITF representation. In order to provide a compact, but conscious record, such fitting-residual stats could be agglomerated to within the OMI channel, i.e., to be averaged for the UV1, UV2 and VIS ranges. They may be shown in an appendix, per author's choosing.

4. The super-Gaussian approach is rapidly 'gaining traction', considering the number of applications adopting this particular ITF form. Fig. 5 shows, however, that in the OMI case this approach leads to more noisy (far more, e.g., the 1st window in the UV2 channel, the last window of VIS) x-track behavior. Is this because the other methods are less sensitive or because the super-Gaussian parametrization is less stable? I suspect the latter. This should be verified and commented on by: (a) constructing an alternative solar reference spectrum (see point #2 above) and showing (probably, in the appendix) the similar to Fig. 5 plot; (b) comparing the temporal (e.g., 1-year blocks) behavior of the fitting parameters for all applied ITF forms and all RA-free rows/spectral regions.

5. Based on the additional metrics suggested in points #1-4, please provide, if feasible, a summary (preferably in a tabular form) of performance of different ITF approximations for all 3 OMI channels.

6. Also, please add a statement about the x-track behavior in the VIS channel to the 1st par. of Conclusions, in line with the UV1 and UV2 conclusions.

——————————————————————— Minor suggestions/corrections:

Abstract, p.1,l.7: '... are up to 20 %...' should be changed to '... are up to 30 %...' (cf. Fig. 5, upper-left).

Abstract, p.1,l.8: I suggest adding '... and practically flat in VIS. Nonetheless...'

p.3,l.3: '...description of the RA can be found in...'

p.4,l.3: '... (Lambda_1 and A_1 are zero).' The original reads '... (Lambda_0 and A_1 are zero).'

p.6,l.10: '... are kept the same as in the operational algorithm...'

p.8,l.6: '... by up to 30 %...'

p.10,l.7: '...changes mostly due to variations in faculae.'

p.14,l.19: '...due to the loss of most...'

p.17,l.1: '... by up to 30 %...'

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