Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-94-RC2, 2020

Atmospheric Measurement Techniques Discussions
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Interactive comment on "Impact of using a new ultraviolet ozone absorption cross-section dataset on OMI ozone profile retrievals" *by* Juseon Bak et al.

Anonymous Referee #2

Received and published: 18 May 2020

1 General

The manuscript AMT-2020-94 provides a comparison of UV ozone retrievals from the OMI instrument using a new cross section data set (BW, provided in the frame of the ESA SEOM-IAS project) with the standard data set from Reims (BDM). Overall, the manucript is very well written, nicely structured and argued. Selected figures do well illustrate the discussion in the manuscript. The presentation is scientifically sound and clear. The topic fits nicely within the journal scope and, therefore, I can fully recommend publishing the manuscript.
There are a few issues to the current paper that need to be addressed before publica-

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Discussion paper

tion, however.

1. The analysis is based on a new cross section data set (BW data) that at this point of time is openly available, but has not yet been published in the scientific literature. It therefore lacks yet the scrutiny of the peer-review process. While this is a regrettable fact, it does not invalidate the present work. But the authors must carefully discuss what might possibly be an inherent contradiction. In a previous study (Liu et al., 2013), the authors have concluded that another recent UV cross-section data set (the SER data from Bremen, Serdyuchenko et al. (2014); Gorshelev et al. (2014)) was less suited for ozone retrievals using the OMI-spectrometer than the BDM data, despite a similar spectral resolution (0.01 nm – 0.018 nm for the 210 – 350 nm range) and a much better temperature coverage (data between 193 K and 293 K on a grid of 10 K; see Weber et al. (2016) for example). Surprisingly, the same data set (SER) is now used to 'calibrate' the new BW data (see lines 95-99 of the manuscript):

   > *Offset corrections were made for each of the 6 temperatures by fitting to the SER dataset since it was measured at higher ozone column density and thus considered more reliable regarding offset . . . The offset corrections have minor effect on the cross-sections except for wavelengths above ∼330 nm.*

   The procedure of dismissing the SER data set for ozone retreival, but using it for calibration is confusing and needs further explanation. The calibration procedure is even more surprising as the correction actually does not seem to impact the results of the present paper, because corrections are claimed to have minor effects within the OMI windows ($\leq$ 330 nm). The necessity of making an offset correction arises from the measurement technique/setup at DLR. It thus needs to be explained why there is the need to make an offset correction in the first place and why the SER data do not suffer from the same problem.

2. In the introduction, the authors give the impression that new cross sections should be measured at a resolution of 0.01 nm or better. This contradicts the use of new cross section data that have been obtained at about 3 ($\lambda > 285.7$ nm) to 5 ($\lambda < 285.7$ nm) times lower resolution (see description of BW data set in section 2).

3. The authors use the terms Hartley and Huggins bands as well as OMI instrument windows to discuss different spectral regions in the UV. While wavelength ranges for both of the OMI UV windows are specified in the manuscript, no numbers are given for the Hartley and Huggins bands. Please indicate as this would help readers to follow the discussion.

4. There seem to be problems with the definitions of signs in some of the plots. For example, are the signs in Figure 7 correct? I find that local negative spikes in the total ozone column difference (BDM-BW) also correlate with cases where the tropospheric profile shows a tendency towards warmer colors (BDM > BW), which would indicate that either of the two scales (total ozone (TOC) vs altitude dependent ozone) should have a different sign. Another issue is the antarctic $+1$ % BDM-BW bias in the TOC. From Figure 4, one would estimate that the cross section bias is positive when integrated all over the $(270 - 346)$ nm wavelength range (despite some few local negative spikes at low temperatures). This should result in a negative BDM-BW bias of TOC. Anyway, the antarctic positive TOC bias needs to be discussed as compared to the lower latitude value around -1 % on the basis of the cross section data. In similar veins, the definition of the y-axis of Figure 4 shows that the room temperature BW cross-section is **negatively** biased with respect to BDM at low wavelengths. This is opposite to what is stated in line 254 of the manuscript *(Relative to the BDM data set, the BW data show systematic biases of 2$-$3 % in C_0 at shorter wavelengths below 300 nm, . . .)*.

5. In the comparison between BDM and BW in section 3, the BW data set is taken

as the baseline scenario. Because section 3 only provides a relative comparison and not an accuracy assessment, the authors should avoid the impression that BW is the truth (even though it compares more favorably with ozonesonde data presented in the next section 4). Instead of saying that BDM causes an underestimation or overstimation, it should just be stated that BDM estimates are lower or higher than estimates from BW.

6. Fig. 9 shows the OMI mean biases with respect to a common reference (ozonesonde). It would be nice to plot the reference profiles (or mean profiles with their sdev) along with the bias percentages.

7. TEMPO is not the only mission that will critically depend on refined ozone spectral data. IASI NG and UVNS are another example of combining retrievals in different domains. In the discussion, the authors need to mention/cite other ongoing or future activities on the synergistic use of different spectral regions that rely on the 9.6 μm region and the Chappuis band, eg. Costantino et al. (2017) and/or others.

2 Technical

l. 32 *th* $\rightarrow$ *the*

l. 95 indicate whether offset was assumed to be constant or wavelength dependent (for wavelength dependent offset specify dependence and range)

l. 97 *(<270.27 nm) > and* $\rightarrow$ *(<270.27 nm) and*

l. 106 *temperatures* $\rightarrow$ *temperature*

l. 107 Should use terms $(T - 273.15\,\mathrm{K})$ and $(T - 273.15\,\mathrm{K})^2$ including the unit of K in eq. (1).

l. 170 *0.015 in UV1* → *0.015 nm in UV1*

l. 254 *BW data show systematic biases of 2-3 % in* C_0 → *BW data show systematic biases of 2-3 % in the cross section at O°C* (C_0)

l. 255 *The difference in* C_1 *and* C_2 *implies distcinctly different* → *The differences in* C_1 *and* C_2 *imply a distinctly different*

l. 268 *200K* → *200 K*

l. 355 list all author names

l. 364 *J. Quant. Spectrosc. Ra.* → *J. Quant. Spectrosc. Radiat. Transfer*

p. 15 Panels (a) - (c) should use logarithmic scales for the coefficients as BDM and BW curves are indistinguishable from 0 at wavelengths $\gtrsim$ 325 nm.

p. 16 Legend to Figure 3 should contain hint on the factor of five different scales used in panels (a) and (b)

p. 17 Legend to Figure 5 should better describe what is on the plot.

p. 19 & 22 Degree symbol ° before *K* in x-axis legend of Figure 9 needs to be deleted. The same holds for the lower colour legend in Figure 7.

p. 21 Annotations *MB* and *MB* ± *SD* in upper right panel are misleading (there is no mean bias in the temperature plot). The 294 K temperature line for the BDM temperature point is drawn differently (thicker, other colour) than the other temperature lines.

References

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