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Atmospheric Measurement Techniques Discussions

Interactive comment on "Quantitative comparison of measured and simulated O_4 absorptions for one day with extremely low aerosol load over the tropical Atlantic" *by* Thomas Wagner et al.

Anonymous Referee #1

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This manuscript addresses an inconsistency persistently reported in several past studies (some of them by the same authors) between observations of the O4 absorption in atmospheric spectra and radiative transport simulations attempting to reproduce these observations. This inconsistency represents a major issue for the interpretation of MAX-DOAS measurements of aerosol properties, which are based on O4 slant column measurements. Results from past studies indicated that, to reconcile observations with simulations, it is often (but not always) necessary to apply a scaling factor of typically -20% to observations. The reason why this correction is needed remains unknown, but some authors suggested that it might be related to uncertainties in the knowledge of aerosol properties in the atmosphere, which can possibly affect the light path of the

solar radiation in a complex way. In the present study, the authors try to overcome this difficulty by concentrating their analysis on observations performed under very low AOD conditions, therefore minimizing uncertainties due to aerosols. Even in such particular conditions, they find that simulations underestimate measurements by about 20%, which confirms that a fundamental inconsistency - not related to aerosols - exists between observations and simulations. Although the study is limited in coverage (only one day of measurements is presented), the proposed case is fully pertinent as it suggests that at least for the conditions of the study inconsistencies cannot be resolved by uncertainties in aerosol properties. The mystery remains however unresolved, since no valid explanation can be proposed. The suggestion that systematic errors on the O4 spectroscopy could be an explanation is on the one hand in contradiction with known uncertainties on laboratory measurements, and on the other hand also in contradiction with published results indicating that a scaling factor is not always required to bring measurements and simulations in good agreement. It would of course be interesting to multiply measurements in similarly low AOD conditions but this is clearly beyond the scope of the paper. From an editorial point of view, the manuscript is concise, well written and well organized. I therefore recommend its publication in AMT, after attention to the few comments listed below.

Specific comments

Pg. 5, l. 3: please justify the use of 0.05 as appropriate value for the albedo of the sea at UV wavelengths. A reference would be enough here. Also indicate at which wavelength the radiative transfer calculations have been computed.

Pg. 5, l. 205: I have the impression that the use of lidar backscatter ratio profiles as a proxy for aerosol extinction profiles involves more assumptions than stated here. E.g., one also has to assume that the aerosol phase function does not vary much with altitude, and maybe more important that the backscatter profile shape measured at 1000 nm is also valid at 360nm. But despite all uncertainties, I agree that using ceilometer profiles makes sense in the absence of real extinction values. Maybe the

system could be improved by adding a device to measure aerosol surface extinctions (if possible).

Pg. 5, l. 221: add a reference to justify the Angström exponent of 2 used for the conversion of the stratospheric AOD (unless this would be documented in Thomason et al., 2018)

Pg. 7, sect. 7.2: considering the very low aerosol content, and the comparatively large uncertainty of the assumed stratospheric AOD (basically a climatological value at 525 nm converted to 360 nm using a not well established Angstrom exponent), I think that the AOD values retrieved by MAPA are highly uncertain. The fact that the retrieved scaling factor matches the values empirically derived in the previous section is not really surprising, since this scaling is already necessary to bring clear-sky simulations in agreement with observations. Inspecting more closely Fig. A13, it seems that the retrieved AOD values are very unstable. Comparing e.g. results derived using SF=0.8 and SF=0.85, we see that AOD values differ quite substantially although RMS values are similar. I am not really convinced that MAPA inversions add a lot of information in the study. At least they are not inconsistent. Something that would be very interesting would be to test whether the discrepancy depends on the O4 wavelength used for the retrieval. Unfortunately, this is not possible using the current setup due to the limited spectral range of the spectrometer, but it should be considered for future studies. Finally, one may also wonder whether this particular day was really the only clean day (during the ship cruise) allowing for a comparison of measured and simulated O4 slant columns. If other similarly clean days were encountered, it would be nice to know whether similar inconsistencies were found.

Spelling, typos:

Pg. 1, l. 21: remove ‘variation’

Pg. 1, l. 33: remove ‘mainly’

Pg. 3, l. 101: add ‘at’ between ‘are not’ and ‘the identical location’

Pg. 4, l. 151: add ‘dry air’ between ‘For the’ and ‘mixing ratio of O2’

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