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Atmospheric Measurement Techniques Discussions

Interactive comment on "Water vapor retrieval from OMI visible spectra" *by* H. Wang et al.

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The paper by Wang et al is one of two recent papers (the other paper is Wagner et al., AMT, 2013) on the retrieval of water vapour from UV/vis hyper-spectral instrumentation which is concerned with the exploitation of a spectral regions below 580 nm. Water vapour retrievals from the visible region of the spectrum contribute to the overall suite of available water vapour retrievals by offering a very good to good land- to a very acceptable ocean-sensitivity under day-light conditions, while like their companion retrievals from the thermal infrared region the retrievals are also are sensitive to clouds. Even though instruments like GOME-1 and 2, SCIAMACHY and OMI provided high resolution spectra covering a wide spectral domain their spectral resolution on the order of 0.2 to 0.4 nm is still not sufficient to resolve individual water vapour absorption lines, and therefore non-linearity effects ("saturation") especially for strong absorption

features, have to be accounted for.

As water vapour absorption bands are becoming weaker and sparse (better separated) towards the UV sensitivity of the retrievals naturally decreases, however, saturation effects are also less significant or even negligible. In addition the increased proportion of diffuse (scattered) light with respect to reflected light from the surface decreases the sensitivity to the surface albedo characteristics, which variability (over land) is also weaker in the blue spectral region, but the vertical profile sensitivity and the sensitivity to clouds has different characteristics, under some circumstances provides even higher sensitivity to the total water vapour column (TWVC).

The work by Wang et al exploits two water vapour bands in the region between 430 and 480 nm (standard setting) which are covered by the OMI spectral range. The paper is an important contribution the overall task in assessing the water vapour information in content along the full range of available spectral absorption signatures from the UV to the NIR. While the paper of Wagner et al. focussed on the sensitivity aspects of the overall sensitivity of slant column density (SCD) retrievals and their sensitivity in the vertical to different surface types and clouds for the absorption bands around 440 nm, the paper by Wang focuses predominantly on the use of different retrieval settings, like the choice of the spectral window, sensitivity to interfering absorbers, and the choice of line databases, and the related impact on the retrievals. Therefore it is an important scientific contribution also with respect to widening the scope of available sensors for the retrieval of TWVC from the visible to OMI, and the future sentinels 4,5p and 5, which all lack spectral coverage in the green to red part of the visible spectrum (above 500 nm).

Wang et al. also provide initial validation of vertical column densities applying the conversion from SCDs using radiative transfer air mass factor (AMF) calculations (AMF) which the study by Wagner et al has not yet carried out. This I find is however the weaker part of the study. The reason is that the transfer to vertical columns involving AMF RT calculations necessarily introduces biases, which in principle have to be taken

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into account for the interpretation of the comparison to ground based or other satellite measurements. Because of the “extreme” profile shape of WV (with scale factors close to two km) the sensitivity of the TWVC retrieval with respect to the mean scattering height for different to viewing geometry, surface type and due to cloud interference is significant. The authors are aware of this issue and refer to a future study providing these numbers. While I understand that adding a full study on the aspects of AMF induced errors on VCD and presenting a comprehensive validation of the latter would probably be a too brought scope for a single paper I find the order still questionable, since now the authors leave it to the reader to interpret validation residuals with respect to systematic biases from the SCD to VCD conversion or from the validation strategy or sources.

In addition the choice of MODIS in the validation part of the study together with the quite valid choice of Aeronet data, I believe, is not optimal. The problem with a MODIS/Aeronet to OMI comparison is that the oceans are not well validated since MODIS WV quality is expected to be worse than that from this kind of UV/vis retrievals over oceans (at least to the one from the 600 to 700 nm range), and Aeronet data is spares to not available there. A better combination would be SSM/I for ocean or any other microwave product (AMSU etc) and to restrict the use of MODIS/Aqua to comparisons over land. Since these two compromises made, from my point of view, compromise the overall quality of the otherwise very important scientific contribution the authors made, I would suggest that the paper could significantly benefit from adding either microwave data to the validation part for better validation of retrievals of ocean and for improving the confidence in the performance of the SCD to VCD conversion, or to remove the MODIS part and add some additional analysis of the AMF biases and sensitivity implication, which may be further substantiated with Aeronet data alone.

In case the authors would choose to put more emphasis on the validation it would be interesting to understand the sensitivity of the retrievals to aerosols, since with the weaker water vapour signature and the shorter wavelength I would expect potential

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sensitivity to e.g. dust absorption and fine mode (sea salt and aged dust) scattering aerosols with respect to retrievals in the 600 to 800 nm domain with much stringer water vapour signatures.

Specific comments:

p. 545, l.23: Aliwell reference is missing in the reference section

Section 2.2.2 on the retrieval setting sensitivity: I think there is a conclusion missing from this section on why the "standard" settings are used in the end as the standard. For example. why is Rothman et al 2013 not used even though the number of negatives are reduced with respect to the standard. Is the standard set with respect to the best performance in the validation results? This should be stated (or referred to) in this section.

Section 4. I think the overpass time of Aura (with respect to Aqua) is missing here or in the introductions to OMI.

Page 10, l.10f: “For Mauna Loa (Fig.7g) the large difference is partly related to AERONET measurements being on the mountain.” This seems an understatement. The differences are probably in toto related to the fact that the Aeronet station is at 4000 m.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 541, 2014.