Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-431-RC1, 2018

Atmospheric Measurement Techniques Discussions

Interactive comment on "Validation of ozone profile retrievals derived from the OMPS LP version 2.5 algorithm against correlative satellite measurements" *by* Natalya A. Kramarova et al.

Anonymous Referee #1

Received and published: 15 January 2018

General comments:

This paper deals with a new algorithm version for the OPMS LP data. The paper is well written, concise and contains interesting and important observations for users of OMPS LP data. I would like to recommend its publication in AMT. I have the following comments and questions.

Specific comments:

1. p.4, line 18: During past few years there has been activity to harmonize the ozone cross sections used in ground-based and satellite remote sensing instruments (see Oprhal et al., Journal of Molecular Spectroscopy, 327:105– 121, September 2016).

Printer-friendly version

Discussion paper

What is the attitude of the OPMS algorithm team towards this activity? 2. p.4, line 23: Normalizing by the high altitude radiance: If I understand right, you must do the same operation in the radiative transfer model i.e., dividing by the upper altitude model radiance. Please, provide some information how to calculate the reference upper level radiance. Whar do you assume about the atmospheric state? 3. p.4, line 29: Wavelength pairs and triplets: Are you doing flat field correction before pairing? 4. p.5, line 1: Why do you perform the ozone retrieval separately in UV and VIS wavelengths? There is only unique ozone field, so the results should agree within error limits inside some altitude interval, If not, something is wrong. Please, comment? 5. p5, line 20: You are moving from the Tikhonov smoothing to a full a priori covariance matrix. This adds an active a piori to the ozone density and is, therefore, at least in principle a stronger constraint for the solution (but this depends on used parameters). Please add reasons for this decision. If you compare your earlier Tikhonov and the new a priori regularisation, how do ozone profiles differ? 6. p.5, line 20: A priori information is a part of your solution. How are you following the amount of a priori information in your product and do you apply some maximum limit for the a priori contamination? 7. p.5, line 28: The discussion of the SNR is somehow confusing to me. What is measured now and what was assumed earlier? 8. p5, line 28: How do you measure SNR for unique measurements? Every situation is new. 9. p5, line 28: If the measured SNRs do not agree with the calculated SNR, is something wrong in the error propagation or in the estimation of the instrumental noise or what? 10. p 5, line 6: If I understand right, when you detect a cloud below your LOS you are still retrieving ozone above the cloud top. But how are you handling the cloud contribution to radiance in your radiative transfer model? 11. p5, line 23: The new aerosol mode: Are the artificial structures now removed? How do your aerosol profiles compare with aerosol profiles from other satellite missions? 12. p12, line 13: You are saying that altitude independent factors cancel out when using normalized radiances. In my mind only strictly multiplicative altitude independent factors cancel out. For example, stray light residuals and other additive factors are not cancelled. Also if the factors are inside spectral integral (point spread)

and/or integration time integral, they are not cancelled. Do you agree? 13. p16, line 18: You explain the observed UV-VIS differences by remaining radiometric calibration differences. But using normalised radiances and wavelength pairs or triplets should reduce the calibration sensitivity. Why these tricks do not work? 14. Sec 4.2: I wonder if you have calculated Chi2-values for the old and new version. Is the fitting quality improved? 15. Regarding the difficulties related to the instrument: What improvements for a future similar instrument can now be foreseen?

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-431, 2017.

Printer-friendly version

Discussion paper