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Interactive comment on “First results from a rotational Raman scattering cloud algorithm applied to the Suomi National Polar-orbiting Partnership (NPP) Ozone Mapping Profiler Spectrometer (OMPS) nadir mapper” by A. Vasilkov et al.

Anonymous Referee #2

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General comments

The paper by Vasilkov et al. describes an interesting topic, namely the adaptation of an existing algorithm for the analysis of OMPS TOA measurements. It aims at the derivation of OCP (pressure of the cloud optical/radiative center) from the filling-in of Fraunhofer lines and the ingestion of these actual OCPs for the calculation of total ozone columns.

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The topic is clearly suitable for publication in AMT. However, it seems that the manuscript isn't mature enough to be published as it is. It resembles more an internal technical report than a research paper. I must admit that the authors have spoiled their readership with past publications of higher quality and this work still lies far from those standards.

Several scientific and technical issues have been left unanswered and are of no help to any reader. For instance, the overall sensitivity of the algorithm to calibration errors hasn't been properly addressed, beside stating that "Efforts are underway to better understand and compensate for the spectral features" (p. 2697, l. 10 and ff). The reason underlying misfits in the OMPS-derived retrievals is still unclear (p. 2698, l. 18 and ff, "The precise cause of this error has not yet been identified."). No answer is provided by the authors about stray light (p. 2700, l 1-2, "We intend to examine this issue further in the future"). The situation is the same for the validation with OMI-derived OCPs (p. 2700, l. 11-12).

I'll try in the following to explain my concerns for improvement, overall agreeing with the 1st referee about a second round of reviews.

Specific comments

Title: if the authors' choice is to maintain the actual paper's structure and flow, then it is more appropriate to add upfront "Technical Note: First results from ... "

p.2692 l. 9: Overall aim of the investigation?

p.2693 l. 24: Why is R_g taken from TOMS climatology and not, for instance, from MODIS BRDF climatology?

p. 2694 l. 11 and ff: If OMPS shows to be less sensitive to OCP, due to worse spectral resolution, how comes that O_3 product improves?

p. 2697 l.8 and ff: So, the smile effect along the spectral-dimension of the CCD camera hasn't been corrected for. And the errors in I_m cancel out. In what layers of the

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atmosphere does RRS appear due to the presence of cloud? Other said: to what extent the presence of a cloud impact the harmonized ratio (Radiance/Irradiance)?

p. 2697 and ff: "We obtained superior results using the table generated with the synthetic solar spectra". I think that this statement should be somehow quantified and explained to the reader. It can't be accepted as it is.

p. 2698 l. 10-11: A sentence and reference should be added for sake of clarity about the row anomaly and the "material" causing interference.

p. 2698, l. 22-24: Could you provide details on the time gap between OMI and OMPS overpasses? No coregistration is possible of OMI pixels within a OMPS ground pixels, therefore no scatterplots?

p. 2699, l. 14-15: the PDFs for the southern mid-latitudes are cut off at 60°S. Are really ice or snow-covered surfaces present in this latitude range for January 7th (assuming a boreal season)? Have you inspected the longitudes of those pixels? How does the algorithm perform in the presence of bright ground (i.e. Australian desert)?

p. 2702 l.12: "... indicating excellent calibration of OMPS normalized radiances". I think this hasn't been sufficiently proven throughout the manuscript, especially in view of the discussion of Sect.3.3 and the concerns raised in the review provided by the 1st referee.

Spotted typos:

p. 2694 l. 7: minimizies -> minimizes

p. 2695 l. 2: interation -> interaction (?)

p. 2697 l. 4: normlaized -> normalized

p. 2702 l. 19: retrtievals - > retrievals

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 2689, 2014.

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