Response to reviewer #2

We thank the reviewer for his/her evaluation of our paper and useful comments that helped improve the manuscript. We appreciate reviewer's time and effort in reviewing the manuscript. Below are our responses to each comment. All reviewer's comments are in the standard font while the responses are in the italic font.

On behalf of the authors,

Alexander Vasilkov

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.

We agree that our manuscript could be classified as a technical note rather than a full-scale research paper. We changed the title in accordance with reviewer's suggestion, adding "Technical note:". However, we think that such technical note can provide important information to the UV atmospheric remote sensing community; that is why we feel that these types of manuscripts are worthy of publication and AMT is the appropriate journal.

In our case, we demonstrate that an algorithm first designed for a high spectral resolution instrument like OMI (FWHM=0.45 nm) can be applied to a lower resolution instrument like OMPS (FWHM=1.1 nm). The spectral resolution is critical for our algorithm because it is based on high-frequency structure in the TOA radiance caused by rotational-Raman scattering. We show in the manuscript that OMPS cloud products have sufficient quality to be used in operational trace-gas algorithms similar to OMI. We also document several instrumental issues that must be dealt with. This is important so that others who may process OMPS data do not have to reinvent the wheel as far as this understanding of the instrument.

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 algorithm is not sensitive to radiometric calibration errors if they are spectrally smooth because the algorithm makes use of a polynomial fit similar to DOAS algorithms. The algorithm is indeed sensitive to stray light. The manufacturer of the OMPS nadir mapper reported no measurable stray light in our fitting window 345.5-354.5 nm. Nevertheless, we attempted to use a *stray light correction for the OMPS level 1b data and found no significant differences in PDFs of cloud pressures as compared with the case of no stray light correction. We added this clarification to the text and removed the statement "Efforts are underway to better understand and compensate for the spectral features …; once a better Day 1 solar flux is available …" because this OMPS calibration and characterization work is actually beyond our scope. The NASA calibration group is trying to improve the Day 1 solar flux.*

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.").

The precise cause of this error has been identified. It is related to an insufficient number of OMPS pixels in the rightmost swath position available for soft calibration over the Antarctic plateau near solstice. We rewrote this sentence accordingly.

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").

We removed this sentence. An answer is provided by the authors that the use of a stray light correction does not lead to significant differences in PDFs of cloud pressures as compared with the case of no stray light correction.

The situation is the same for the validation with OMI-derived OCPs (p. 2700, l. 11-12).

In this part of the manuscript we briefly discuss results from Joiner et al. 2012. They compared cloud pressures from two different OMI cloud algorithms with a CloudSat-based simulator and found differences that could be attributed to errors in the instrument and algorithms. We refer to those results when discussing the OMI-OMPS differences. It is not a unique situation when different cloud algorithms (or even different versions of the same algorithm) produce differences in the cloud pressure product (see e.g. Sneep et al. 2008 and references therein). In contrast, it is an unusual situation when those differences can be fully explained. We rewrote this discussion in the text.

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 ... "

Changed.

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

A goal of the manuscript is to demonstrate that a cloud algorithm first designed for a high spectral resolution instrument like OMI can be applied to a lower resolution instrument like OMPS and that the OMPS cloud products have sufficient quality to be used in operational OMPS trace-gas algorithms. We also document several instrumental issues that are important for others who may process OMPS data. We added text in the introduction to make clear the goal of our study.

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

We agree that a BRDF climatology could be better than the Lambertian equivalent reflectivity (LER) climatology. However, MODIS does not have UV channels, that is why the MODIS BRDF climatology is not available for our spectral range.

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?

We say that OMPS is less sensitive to rotational Raman scattering (RRS) effects than OMI. However, OMPS radiances still clearly exhibit the RRS effects (see Fig.1); this allows for retrieval of OCPs. The current NASA total column ozone algorithm makes use of the OCP climatology derived from our algorithm applied to OMI. This climatology captures much of the OCP variability. However, the use of actual OCPs can improve OMPS total column ozone estimates locally in areas with large differences between climatological and actual OCPs. The ozone differences can be up to 5% in such areas (can be seen in Fig. 7). We now emphasize those local improvements of the ozone retrievals in the revised Conclusions section.

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.

The spectral smile is an inherent feature of the sensor. Since the philosophy of the developers behind the creation of the L1B dataset was to minimize modifications to the actual data taken by the sensor, the smile, as a matter of choice, was not artificially taken out in the L1B irradiance (or radiance) measurements. Along with the measurements, the developers also provide in the L1B dataset a map of the wavelengths for each pixel of the CCD. While the uncertainties of the radiance and irradiance measurements can be large, the uncertainty in the normalized radiance we (and others) use, where radiances measured from the CCD are divided by irradiance measurements taken from the same CCD, are much smaller. As for the spectral smile, as long as it is properly accounted for when using the data it has no effect on retrieval performance. We have modified the text to better explain these points.

In what layers of the 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)?

Cloud pressure is derived from the high frequency structure of the normalized radiance caused by RRS on molecules in the atmosphere. RRS results in filling-in of Fraunhofer lines in backscatter spectra. Clouds screen the atmosphere below and thus reduce RRS in the entire atmosphere and therefore the amount of filling-in of Fraunhofer lines. This reduction of the filling-in as compared with clear skies is related to the cloud pressure. Joiner et al. (1995, 2004) showed that the amount of the filling-in is approximately proportional to a pressure level at which a cloud, treated as a Lambertian surface in our approach, is placed. This proportionality is observed from the surface to top of the troposphere (from 1000 to 100 hPa) for solar zenith angles less than 80^0*. We added this explanation to Section 3.1.*

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.

We attempted to use the OMPS measured solar spectrum for the generation of lookup tables. However, cloud pressure retrievals with those lookup tables exhibited much more artefacts, e.g. negative values of retrieved cloud pressures, than with the tables generated using the synthetic spectrum. We added this to the manuscript.

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.

We removed "affected by interference from material outside the instrument" from this sentence because this is just a hypothesis even though it is somehow proven. We added to the text the following: The "row anomaly" affects the quality of the level 1b radiance data for particular viewing directions of OMI (cross-track positions) and consequently the cloud products for those cross-track positions. More information about the OMI row anomaly can be found at http://www.knmi.nl/omi/research/product/rowanomaly-background.php.

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?

That's right. A collocation of OMI pixels to OMPS pixels is not possible; therefore we do not provide scatter plots. OMI flies at 705 km altitude; OMPS is at 824 km altitude. Although they have, roughly, the same local equator crossing time, the difference in altitude means that they have coincident orbits on the earth's surface every eight days or so. And, even though the orbit can coincide, the times are still somewhat different. We added this explanation to the manuscript.

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?

We found that the overwhelming majority of pixels with cloud pressure retrievals slightly higher than 1000 hPa is located in two oceanic segments: the first is bounded by 95 E and 115 E and the second is bounded by 125 W and 135 W. Both segments are southward of 45 S. We checked reflectivity values in those areas and found them being in a narrow region around 0.5. We agree that the presence of ice/snow in those segments is unlikely and removed the sentence that speculates about snow/ice.

How does the algorithm perform in the presence of bright ground (i.e. australian desert)?

Deserts are not particularly bright in the UV as they are in the visible, so the algorithm behaves quite similar to other land areas.

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.

We agree that this is a subjective statement and removed "indicating excellent calibration of OMPS normalized radiance". However, we would like to emphasize that Seftor et al. (2014) have shown that the normalized radiance errors are less than 2%, well within instrument specification (see Section 3.3).

Spotted typos:

p. 2694 l. 7: minimizies -> minimizes

corrected

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

corrected, should be "iterations"

p. 2697 l. 4: normlaized -> normalized

corrected

p. 2702 l. 19: retrtievals - > retrievals

corrected