Review of **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, J. Joiner, and C. Seftor

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General Evaluation

Vasilkov et al. present work on the application of a rotational Raman scattering (RRS)-based cloud parameter retrieval scheme to observations of the Ozone Mapping Profiler Suite (OMPS) nadir mapper component. The algorithm is an adaptation of a technique that has been developed for, and is being applied to, Aura/OMI observations. The authors describe the general outline of the approach, modifications required to make it work for OMPS, and provide first results on cloud optical centroid pressure and cloud fraction. Those cloud parameters are compared to results from OMI. The manuscript concludes with the application of native OMPS cloud parameters in the operational OMPS Total Ozone algorithm.

Cloud retrievals using RRS is a tried and true method, primarily developed by the authors themselves, that is routinely used for satellite observations, particularly when no O_2 *A* band measurements are available. Thus, the manuscript does not introduce fundamentally new science. Its value, rather, lies in the prospect of obtaining native cloud measurements from OMPS. The recently closed NASA ROSES13 Suomi NPP Science Team call requested, among other things, proposals for atmospheric composition retrievals from OMPS. Cloud information will be an important element particularly in new trace gas retrieval algorithms, and the study by Vasilkov et al. presents the first viable report on cloud retrievals from OMPS.

The paper is generally well and clearly written and, for the most part, requires few corrections or clarifications. The main issue for this reviewer lies in the discussion (or lack thereof) of the apparently inadequate wavelength calibration of the OMPS (ir)radiances, and what its influence is on the retrievals. Related to this is a somewhat confusing discussion of Figure 2. All other issues are minor and should be fairly easily corrected. Specific and editorial/technical comments are listed below.

In summary, I find this manuscript appropriate for publication in AMT. It's topic is relevant, and the work is of high standard. I recommend publication, but suggest a quick, second round of review after the comments have been addressed.

Specific Comments

Wavelength Calibration

Judging from Figure 2, top panel, OMPS irradiances suffer from inadequate spectral calibration – at least, this is how I interpret the “loop” structure that arises when plotting the 36 different solar measurements with dots. There is some discussion on “spectral smile” and a 0.2 nm shift across the spatial domain, and how all this relates to the “loops”. The bottom line I take away from this is:

1. OMPS irradiance (and possibly also radiance) spectral calibration is insufficient
2. The measured spectra should not be used in this state; rather, the spectral calibration must be improved before a retrieval of any kind is attempted with them.
3. If the (ir)radiances *are* used “as is”, then a detailed error analysis is in order, on exactly how the error in spectral registration affects the retrievals.

If I interpret the two panels in Figure 2 correctly, i.e., the top being 36 observed irradiances and the bottom being

36 synthetic (simulated) irradiances, then the problem is obvious. Without better spectral calibration, a comparison between the 36 measured and 36 observed spectra is highly problematic. Since the synthetic spectra are being used to derive the RRS signature, that signature will be off relative to the measured irradiances, and hence likely also the radiances.

It is also important to note that one can't simply assume that measured irradiances and measured radiances do not have any spectral off-set between them. Any off-set in wavelength is likely to be smaller than 0.2 nm, but it may be enough to additionally worsen the “normalization” (in the sense used in the manuscript, i.e., the ration of radiance over irradiance), when identifying the position of solar Fraunhofer lines.

As it stands, the discussion on “spectral smile” and “loops” is confusing, and detrimental to instilling confidence in the retrieved cloud parameters. The whole discussion should be deleted, and replaced by one of the following:

(a) Ideally: an attempt to improve the spectral calibration of the OMPS observations at the outset of the cloud parameter retrieval; I would expect that the quality of the retrievals should improve.
(b) Alternatively: a quantification of the effect of the up-to 0.2 nm shift between measured and synthetic spectra on the retrievals. I am assuming here that no other correction is being made to reconcile the RRS signature derived from the synthetic data and used with the OMPS measurements.

If I happen to have completely misunderstood the points being made in this part of the manuscript, then this might serve as an indication that the “spectral smile” discussion needs revamping.

Effective Cloud Fraction PDFs

Figure 5 shows ECF PDFs from OMPS and OMI for the same day of observations. In the discussion on Page 7 the authors note that one would expect some differences to show, based on the difference in the instruments' ground pixels, e.g., OMI's smaller footprints may show larger cloud fraction. But then the authors go on to say “This comparison allows to state that there is good confidence in the OMPS ECF product and in the OMPS calibration since much work and validation has already been done for the OMI calibration (...)”.

First, I don't believe that the comparison allows such a far-ranging conclusion. Second, this glosses over the puzzling fact that OMPS doesn't appear to see a higher fraction of cloud-contaminated pixels at the middle to *lower* scale of the ECF range: A significant driver for current atmospheric composition instrumentation development is the decrease in ground footprint size, since it supposedly enhances the probability to observe under cloud-free conditions. OMPS, with its significantly larger footprint, should have a much higher probability of having *some* cloud contamination in the field of view, but the PDFs do not give any indication for that. The authors exclude include ECFs < 5% (below which OCP retrievals are infeasible), which might partly explain the “missing” higher percentage of cloud-contamination in OMPS. Still, it would go against all intuition if there was no difference at all in the overall fraction of cloud-contaminated pixels of OMI and OMPS.

The row anomaly in OMI removes a good fraction of the smallest-size ground footprints. It may be worth deriving a statistic on what range of ground pixel sizes make it into the comparison.

Smaller Issues

Lines 69/70: “; this” does **not** provide 36 cross-track positions. The OMPS L1 product from NASA contains 36 cross-track positions, while the NOAA L1 product contains only 35. The reason, apparently, is that the NASA L1 product does not bin across detector boundaries. Anyway, I recommend to rephrase the last part as “, and 36 pixels in the across-track dimension”.

Line 163: Why would the synthetic data have *any* loop patterns? Aren't they based on a synthetic wavelength

scale? There may be differences due to FWHM changes between cross-track positions, but those should not manifest as “loops”.

Lines 179+: Akima interpolation may be a fast alternative – more accurate than linear and less prone to oscillations than Spline.

Lines 237: The last sentence in this paragraph is confusing. Is it supposed to say that the delta in retrieved cloud pressure is approximately proportional to the (erroneously interpreted as) “filling-in” from the dark current?

Lines 288+: Are the changes in O3 retrievals due to using OMPS OCPs really statistically significant?

Line 302: There is not enough evidence to judge the calibration of the OMPS normalized radiance as “excellent”.

Lines 305+: The “slightly better agreement” is a 0.04% absolute reduction in the Gaussian spread but a -0.32% absolute increase in the overall off-set. It is a bit of a stretch to call that an improvement. “Inconclusive”.

Figure 1, caption: “filling-in”.

Figure 2, bottom: The description indicates that “dots are used for cross-track positions”. The bottom of Figure 2 appears to have two lines (dots and dashes), which could just be the effect of areas of dense “dot” accumulation. This is somewhat confusing. If the lower panel indeed contains 36 different synthetic irradiance spectra, can colors be used to make this more clear?

Figure 4: A suggestion – the OMI OMPIXCOR code can easily be adapted to work with OMPS. On that plot scale it will have little effect on the OMI image (aside from outer swath positions at high latitudes), but it will greatly improve the OMPS panel.

Editorial Comments

Title: “Ozone Mapping Profiler Suite” (not “Spectrometer”).

Line 15: “appears to improve OMPS total column estimates slightly” is a non-statement. Given the results from the analysis, I would call the effect “inconclusive”. Either quantify or rephrase.

Line 12: “The current NASA OMPS total ozone ...”.

Line 19: “The OMPS Nadir Mapper ...”.

Line 136: “normalized irradiances” (?).

Line 170: “normalized”.

Line 186: “snow-covered” (?)

Line 220: “close to each other”.

Line 249: Quantify “relatively good”.

Line 253: “to errors of one”.