Response to the review by T. Kurosu

We thank the reviewer for his 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

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

The prelaunch system specification for the OMPS nadir mapper sensors allowed for errors in the individual radiance and irradiance measurements of 8% and 7%, respectively. More stringent constraints were not required because it was assumed that the algorithms for this sensor would utilize normalized radiances (i.e., radiance divided by irradiance) in the retrievals. The specification for normalized radiance was therefore set to a more stringent 2%. Analysis of the post-launch performance of the instrument has validated that the instrument is performing to within this specification (Seftor, et al., 2014) as noted in the manuscript. Furthermore, analyses of the resulting Level 2 ozone products validate their performance as well (Kramarova, et al. 2014) as also noted in the manuscript. Because the instrument meets its normalized radiance specifications, we would not qualify the spectral calibration as insufficient. Please also see additional responses below related to spectral calibration.

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.

We address comments 2 and 3 regarding wavelength and other calibration issues below.

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.

To clarify:

1) *The spectral smile, as shown in the top part of Figure 2, 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.*

2) *The existence of a spectral smile in no way reflects on the calibration of those measurements or how accurate the wavelength map is.*

3) *To investigate the accuracy of the irradiance measurements shown in the top of Figure 2, the measured bandpasses for the CCD were used in conjunction with the wavelength map to create the synthetic irradiance measurements that are shown on the bottom of Figure*

2. The structure seen in this figure is caused by the change in bandpass across the CCD mapped onto the spectral smile. If there were no spectral smile in the wavelength map, the synthetic calculation for each of the 36 FOVs would be performed for the same 196 wavelengths. Furthermore if, for a given wavelength, the bandpass was the same for each FOV, the calculation for that wavelength would be the same for each FOV, and we would end up with the same 196 synthetic irradiances from 300 to 380 nm. Now, if we introduce the spectral smile, the calculations are no longer performed at 196 specific wavelengths but are spread across an interval corresponding the smile (+/- 0.2 nm) about each of the 196 wavelengths at the center of the CCD; the result would be a synthetic spectrum spread out over the entire 300 to 380 nm range. Finally, if, for a given wavelength, the bandpass changes across the CCD, we would calculate a different irradiance for that wavelength for each of the 36 cross-track FOVs, and the combination of bandpass change and spectral smile results in the looping pattern seen in bottom plot of Figure 2.

4) *The top plot of Figure 2 shows the actual measured irradiances; the difference in the size of the loops between the measured and synthetic irradiances is a measure of the error in the irradiance measurement. Equivalently, the error of the absolute radiance measurements is similar.*

5) *While the errors in the radiance and irradiance measurements are apparent, the error 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 (for example, using a normalized radiance, where the effect of the cancels) it has no effect on retrieval performance.*

6) *Analysis performed by the NASA calibration group indicates offsets on the order of 0.06nm or less between radiance and irradiance measurements; our fitting term in the retrieval produces similar shifts and effectively accounts for them, though as noted in the manuscript a spline interpolation is used rather than a linear interpolation as was used for OMI that has higher spectral sampling.*

We have rewritten this section of the paper to emphasize the points in the discussion above.

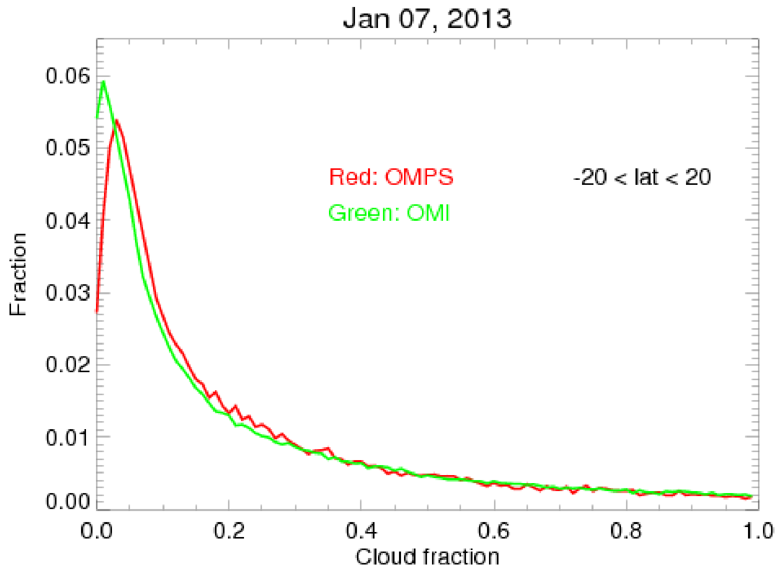
Effective Cloud Fraction PDFs

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.

We particularly thank the reviewer for this comment and agree that it is counterintuitive if there was no difference at all in PDFs of cloud fraction. The reviewer's speculations could explain the small difference in the PDFs of ECF: (1) pixels with ECFs < 5% are not included in the comparison; (2) a significant fraction of the OMI smallest-size pixels are indeed excluded from the comparison due to the row anomaly that is located mostly near nadir smallest pixels. Therefore we actually compare PDFs of ECF for OMPS and OMI pixels with effectively similar sizes. To clarify further, we replaced Fig. 5 with a new one that shows PDFs of ECF starting from zero (even though OCP retrievals are infeasible for ECF<5% as the reviewer correctly stated). The new figure (see below) shows the expected behavior: (1) noticeable differences are observed for low ECFs; (2) there are presumably more clear scene pixels for OMI than for OMPS because OMI has more smaller pixels. We added this explanation to the text and removed the conclusion that is mentioned by the reviewer in the first paragraph of his comment.



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".

Done.

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".

The synthetic solar flux is based on an instrument wavelength scale that has the "spectral smile". This "spectral smile" is reflected in the synthetic data; it essentially provides oversampling in the spectral domain, but does not produce loops for a constant bandpass. As explained above, the changes in the bandpasses across the swath that were measured prelaunch on the ground (and are accounted for in the synthetic data) in fact do manifest as the loops. We have clarified this in the text.

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

Thank you. We will consider this option in the future.

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?.

We rephrase as “Cloud pressures retrieved from Raman scattering are approximately proportional to the amount of filling-in of solar Fraunhofer lines (Joiner et al., 1995). Stray light increases the filling-in; if not properly accounted for, stray light can thus lead to erroneous cloud pressures.”

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

The changes in O3 are statistically significant in areas where the retrieved OCP deviates noticeably from the climatology. The ozone differences can be as large as 5% in such areas. See additional comment below explaining clarifications made in the revised manuscript.

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

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).

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”.

Agree. We removed the statement of “slightly better agreement”.

Figure 1, caption: “filling-in”.

corrected

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?

The bottom panel of Fig. 2 does have dots only. There are so many dots (due to the spectral smile) that they fuse together to simulate a line. We added this clarification to the Fig.2 caption. The use of colors did not improve the plot significantly.

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.

We agree that the OMI OMPIXCOR code could improve the OMPS image, but it is not clear that the code can be easily adapted to work with OMPS data.

Editorial Comments

Title: "Ozone Mapping Profiler Suite" (not "Spectrometer").

corrected

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.

Agree. We rephrase as follows -" changes OMPS total column ozone estimates locally (presumably in the correct direction), i.e. only in areas with large differences between climatological and actual OCPs. The ozone differences can be up to 5% in such areas."

Line 12: "The current NASA OMPS total ozone ...".

corrected

Line 19: "The OMPS Nadir Mapper ...".

We mean both OMPS nadir sensors .

Line 136: "normalized irradiances" (?).

We rephrased as follows: "... a representative set of solar irradiance values.

Line 170: "normalized".

We meant diffuse radiance coming from a diffusor. However, we decided to remove this sentence because it provides unnecessary detail of measurements.

Line 186: "snow-covered" (?)

"ice-covered land" in Antarctica.

Line 220: "close to each other".

corrected

Line 249: Quantify "relatively good".

It is hard to quantify differences in PDFs (used in e.g. Joiner et al. 2012). We replaced "relatively good" by "qualitatively good".

Line 253: "to errors of one".

corrected