

Interactive comment on “Trend Quality Ozone from NPP OMPS: the Version 2 Processing” by Richard McPeters et al.

Anonymous Referee #1

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This study provides interesting information on the new version of the NPP OMPS nadir total and profile data sets v2. The improvement is appropriately illustrated with a number of comparisons with independent measurements. The topic fits well in the field of interest of AMT. I have a number of recommendations that are worth to be considered before final publication.

Major comments:

- Soft calibration techniques are applied as a function of the cross-track to improve the measured reflectivity. Please mention if this is part of the new L1 version or if this soft-calibration step is applied as part of the ozone retrievals. Could you add figures showing the soft-calibration factors and also the impact on total ozone as

a function of the row?

- Generally, the comparisons are carried out using data until end of 2016. Please extend the time series up to now. This is especially important for drift analysis.
- Validation of total ozone columns from the nadir mapper (figure 3): There are systematic differences between Brewer and Dobson observations. I don't think it is appropriate to mix together those two networks for validating satellite data. Could you redo Figure 3 for each type of ground-based instrument separately? Please add as well V1 on those plots. Could you also give the numbers resulting from the drift analysis, even if they're small or not significant? For complete documentation of the product quality, it would be good to show also satellite/Ground-based differences as a function of other key parameters such as SZA, cross-track index,
...

- Figure 6: Please mention what is the upper SZA limit for which retrievals are performed. A similar plot for the ozone hole season would be welcome.
- For the profile comparisons, as it is stated, the vertical resolution of the NP retrievals is quite coarse, making a direct comparison with sondes or limb profiles not necessarily straightforward. Did you apply averaging kernels to do the comparisons? Could you give more details on how this has been dealt with.
- Lines 245-247: In which conditions are the longer wavelengths from the NM used? Is it at higher SZA/latitudes only? In that case, the low bias cannot be explained by the use of those wavelengths? Could you clarify this please?
- Figures 10/11: it seems that there is a drift in the upper stratosphere, which should be mentioned even its amplitude is not that large.
- Tropospheric Ozone: Can tropospheric columns be derived by combining the NM and NP retrievals (instead of using the LP product)? How would such columns



compare with the presented product? Or are there good reasons to prefer using LP measurements to remove the stratospheric part? Is it possible to add a comparison with integrated tropospheric columns from sondes?

Minor comments:

- Lines 181-183: please rephrase as you state at lines 195-197 that the drift likely originates from the NOAA 19 calibration.
- Figure 7: add longitude of Hawaii, add comparison with version 1 as well if possible and state the meaning of the two numbers in green and red.
- Please, add also the new L1 version and availability in section 7.
- Line 314: “of” instead of “or”
- Lines 318-319: Mention again the possible link between the negative bias in total ozone from NP and the observed negative bias in the 6-10 hPa region of the profiles.
- Figure 2: there is a half-cropped number at the bottom-right of the figure

Interactive comment

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