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Atmospheric Measurement Techniques Discussions

Interactive comment on "Characterization and Correction of OMPS Nadir Mapper Measurements for Ozone Profile Retrievals" *by* Juseon Bak et al.

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I found this paper to be well written an organized, and the scientific relevance clearly indicated. The scientific arguments are substantiated through analysis and presented in a fashion that is mostly understandable. Since this paper reports on a technique that has already been published, its value is in describing how the performance varies with different instruments. The paper accomplishes this objective. It also provides an important independent evaluation of OMPS Level 1 product performance.

I do have several technical questions/criticisms. I don't think they represent major problems, but I would like to see them addressed in some way prior to publication.

Section 3.1

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Discussion paper

It is not entirely obvious that the discussion in this section is necessary. The authors fail to provide an estimate of their sensitivity to slit function shape that justifies the investigation. Given that they use sun-normalized radiances in their retrievals, much sensitivity to the shape goes away in the ratio. While some sensitivity remains, it is not clear that this represents an error comparable to other error sources. For example, the large OMPS footprint means that most scenes are partially cloudy. The resulting signal gradient across the slit width not only shifts the weighted mean of the function, but also distorts its shape. The effects of this distortion do not cancel in the sun normalization. Surely this is a larger source of error than small shape errors, one that the authors have not accounted for.

Lines 225-226

This is not a correct assumption. OMPS NM is known to have slit widths that change with temperature. The result is Earth-view slit functions that are broader at the swath edges than their irradiance counterparts, by about 4 percent.

Lines 263 and 268

These two lines of text seem contradictory. In the v2 L1B product separate wavelength scales are reported for radiance and irradiance data. These scales differ by at most 0.05 nm. Line 263 implies that the reported radiance band centers are in error by 0.05 nm on average, which is a very large number. But Line 268 states that the derived difference between radiance and irradiance scales is 0.05 nm. Both of these statements cannot be true unless the authors are using the irradiance wavelength scale to represent radiance data. At the very least, the authors should state which parts of the L1B product are in error.

Section 3.3

I fail to understand what is gained from the common mode correction described here. It appears that the end objective is to reduce fitting residuals and standard deviations

along the orbit. When this is done independently for each spectrum, without identifying and correcting the underlying cause of these residuals, it's not clear there are any gains in product accuracy. It would be beneficial if the authors can discuss up front the objectives for these corrections. What types of physical errors will this correction address? Also, I would appreciate a clearer description of how the correction is derived (the explanation in the conclusions is better than in this section).

In addition, I have several editorial comments.

Line 22

. . .resulting in serious. . .

Line 29 (and throughout document)

The typical phrase is "noise floor" rather than "floor noise."

Line 188

The OMPS preflight slit functions were characterized for each CCD pixel . . . (they were not measured for each pixel)

Line 207

"super" instead of "supper"

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