

Interactive comment on “Application of tomographic algorithms to Polar Mesospheric Cloud observations by Odin/OSIRIS” by K. Hultgren et al.

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General comments on Hultgren et al. [2012]:

This paper presents useful technique, as far as it goes. The material presented appears sound, but I often felt that the most interesting parts of the analysis were skimmed over or excluded entirely from the text. The text suggests that the authors have already completed the analysis necessary to respond to all of my comments, and I hope they will consider adding more detail to this paper. See below for specific thoughts along these lines:

Specific comments:

Page 3696, Section 2.1, first paragraph:

“trough a field” -> “through a field”

Page 3698, Section 2.2, second paragraph:

“line of sight intersects at least two grid cell boundaries” -> Couldn't a line of sight intersect just one grid cell boundary, by passing through the same grid cell boundary twice (if the tangent point lies within that grid cell)? I also can't imagine how a path could intersect more than two boundaries (if the cells are spherical shells), but that may be a failure of my imagination.

“Finding these intersections and path lengths is a straightforward geometrical problem.” I see no mention of refraction in the text. Is it correct to assume that it is ignored in this analysis? If so, then this statement is supportable; if not, then more should be said about the handling of refraction (especially for those without easy access to the Degenstein dissertation cited).

Page 3701, Section 3, second paragraph:

“The grid cell sizes have been optimized based on the observation geometry.” Could you say more about this? Using the word “optimized” implies to me that some quantitative analysis was done to determine the grid used, but perhaps your true meaning is simply that the grid cell sizes were guided by the observation geometry?

Page 3701, Section 3, third paragraph:

“Since there is a limit on the exposure times when observing the limb from a satellite platform, the read-out frequency cannot be made shorter than two seconds for OSIRIS.” Could you clarify this statement? The first clause is a general statement about satellite limb observations, and the second deals specifically with OSIRIS, so I'm not sure whether the limit that you cite is Odin/OSIRIS-specific or general.

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Page 3701, Section 3, fourth paragraph:

“arbitrarily created input emission” – Was it truly arbitrary? I assume it was guided somewhat by past observations of PMCs.

“structures . . . are very similar and thus considered satisfactory for the purpose of this work.” This is the first of a number of instances in which vague words take the place of quantitative analysis. It would be helpful to explain your criteria for considering something “similar” or “satisfactory”, more helpful to report the degree of similarity quantitatively, and most helpful of all to include a difference plot for Fig. 5.

Pages 3701-3702, Section 3, fifth paragraph:

“slightly misplaced”, “resembles . . . very well”, “noticeable” – same concerns as previous comment.

Page 3702, Section 3, seventh paragraph:

“successfully reproduce” (should be “successfully reproducing”), “large uncertainty” – same concerns as previous comment.

Also, the degradation of performance as we go from zero noise to 4% noise is one of the most interesting aspects of the analysis you performed, so please say more about it here.

Page 3703, Section 4, second paragraph:

The term “emission” is used liberally throughout the text, but at this stage it is revealed that the wavelengths considered are actually scattered (primarily single-scattered) by the PMCs. This should probably be mentioned earlier, and I concur with an earlier reviewer who notes the increased difficulty of interpreting scattered radiance (relative to isotropically emitted radiance).

Page 3701, Section 4, third paragraph:

“The instrumental background is taken as the mean value of the background obtained from ordinary limb scans during the days before and after the tomography scans.” This sounds dangerous to me, given that significant properties of the scene change from day to day (ozone profile, illumination conditions, underlying scene, etc.). We are assured that the impact of these uncertainties “can be estimated” to be $< 1\%$, but again, no supporting figure or analysis is given.

Page 3705, Section 5, second paragraph:

“similar but noticeably different”, “relative magnitudes . . . are different” – same concerns as cited earlier (vague language).

Page 3705, Section 5, last paragraph:

“this time scale is considered not to provide any complications” – can you elaborate on this statement?

Page 3706, Section 6, second paragraph:

“from several wavelength intervals in the ultraviolet” – can you explain why longer wavelengths (outside the UV) are not considered for this analysis?

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 3693, 2012.

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