Author Comments as a response to Referee Comments #3, R. Loughman.

Thank you for the fruitful and constructive comments. We address all the comments in the notes below.

Specific Comments:

Page 3696, Section 2.1, first paragraph:
"trough a field" -> "through a field"

Response to comment: Thank you for the correction.

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.

Response to comment: The sentence might be a bit misleading. Better would be to write "line of sight intersects grid cell boundaries at at least two locations". In other words, if a line of sight intersects one grid cell boundary it has to intersect at least the same boundary, thereby twice. Also, by looking at Figure 3 it can be seen that the cells have both vertical and horizontal boundaries. Hence, a line of sight like the one illustrated intersects multiple boundaries.

"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).

Response to comment: Refraction has been ignored since it is not important at these altitudes due to the very low pressure.

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?

Response to comment: The grid has been optimized by trial and error to get the best possible match between input and output in the simulations. The sentence can be changed to "The grid cell sizes have been chosen with regards to available computing power and 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.

Response to comment: You are correct in that the first clause is a general statement; there is always a limit but we would like to make it as short as possible. Also the second clause deals specifically with OSIRIS; the 2 second limit is a technical limit of OSIRIS. We would like to use shorter exposure times if the sensitivity would provide it. This should be clarified in the text.

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.

Response to comment: It was actually truly arbitrary. There were no considerations on the shape of PMC’s when creating the input “test cloud”. The focus was on introducing shapes like wave patterns and different thicknesses to be able to compare the output structures with the input in the best possible manner and to assess whether there is a good match or not.

“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.

Response to comment: When it comes to an assessment of the retrieval quality, it is most important to judge the retrieval of cloud shapes. Difference plots between model cloud and retrieved cloud focus on comparing absolute magnitudes, which is of minor interest here. In the original manuscript, we therefore argued that “visual inspection” is the most appropriate way to judge the retrieval quality. In the revised paper, we will better quantify this by defining a “thickness of the blur” as a measure of the shape retrieval.

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.

Response to comment: Noise levels in typical OSIRIS data are 1e8-1e9 ph cm^{-2} s^{-1} str^{-1} nm^{-1}, which corresponds to about 1% of PMC peak radiances. This means that the noise levels that have been used to test the retrievals are very representative of the input data. The revised manuscript will discuss this further.

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).

Response to comment: You are correct in that we should be better in describing the units of the retrievals. As I also wrote to an earlier reviewer, we have made a substantial revision of this in the manuscript since "volume emission rate" is not a good quantity to use when describing scattering from PMC since this involves a phase function and is not isotropic. Instead, the values have been converted into a "volume backscatter coefficient", beta, in units m^{-1} str^{-1}. Inputs to the tomography algorithm are OSIRIS limb radiances in units ph cm^{-2} s^{-1} str^{-1} nm^{-1}. In applications of tomography to airglow or auroral studies, volume emission rate in units ph cm^{-3} s^{-1} is a convenient retrieval product (Degenstein et al., 2003). This is not the case for studies of PMCs as the scattering phase function introduces anisotropy to the radiance field. Therefore, we describe local scattering from a cloud element in terms of the volume scattering coefficient β_λ in units m^{-1} str^{-1} that includes the dependence on the scattering angle. β_λ is obtained by normalizing measured radiances to the incident solar irradiance in the spectral interval of interest. The use of β_λ also facilitates comparison to lidar studies that describe local cloud properties in terms of a volume backscatter coefficient in the same units. When interpreting cloud structures as shown in Figure 7 one has to keep in mind that the absolute value of the local scattering depends on the scattering angle of the observations and that the scattering angle slowly varies along the satellite orbit. This phase function effect on the retrieved PMC structures remains minor. During the PMC season poleward of 60°N, the solar scattering angle typically only changes from 70° to 100° from the ascent to the descent part of the flight.

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.

Response to comment: The "unpredictable" variability of molecular density at PMC altitudes is quite large, about 10% (see e.g. Rapp et al., 2001). However, for reasonable strong PMC the molecular Rayleigh scattering is less than 10% of the peak PMC scattering. Hence, the variability of the molecular density leads to a relative uncertainty in the Rayleigh subtraction that is less than 1%. This will be clarified in the revised manuscript.
Instrumental background can be caused by various effects such as insufficient correction of dark current, internal scattering, and sensitivity to radiation from below the nominal field-of-view (baffle scattering). In the ultraviolet, these effects are small. From the ordinary PMC analysis it is found that the instrumental background can simply be described as an altitude-independent offset. In the wavelength range used here, this background is 1.9e8 ph cm^{-2} s^{-1} str^{-1} nm^{-1} with a scan-to-scan variability of 0.4e-6 1.9e8 ph cm^{-2} s^{-1} str^{-1} nm^{-1} (standard deviation). The uncertainty introduced by the subtraction of this variable instrumental offset corresponds thus to an uncertainty of less than 0.1% of the peak PMC radiance plotted in figure 7. This is negligible as compared to the uncertainty of 1% of the climatological subtraction of the molecular scattering background.

Page 3705, Section 5, second paragraph:
"similar but noticeably different", "relative magnitudes … are different" – same concerns as cited earlier (vague language).

Response to comment: See earlier response.

Page 3705, Section 5, last paragraph:
"this time scale is considered not to provide any complications" – can you elaborate on this statement?

Response to comment: The temporal scale for sampling a specific cloud volume at 85 km is about 5 min, i.e. the time it takes to collect data from all the regions used in the reconstruction of the cloud structure. PMC's are affected by gravity waves less than this time scale, but those waves described in the reference (Witt, 1961) are of much smaller scales than the current algorithm can resolve and are therefore not considered to provide any complications. Thank you for the comment. This should be clarified in the manuscript.

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?

Response to comment: This is simply to avoid upwelling radiation from lower atmospheric altitudes. This should be clarified in the text.