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Atmospheric Measurement Techniques Discussions

Interactive comment on "Assessment of Odin-OSIRIS ozone measurements from 2001 to the present using MLS, GOMOS, and ozone sondes" *by* C. Adams et al.

Anonymous Referee #2

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The manuscript presents results of the validation analysis of Odin-OSIRIS ozone profile measurements against MLS, GOMOS, and ozone sondes. Authors estimated mean differences and drifts between OSIRIS and validation measurements. Biases in the OSIRIS data were estimated for various measurement parameters. The results of this study will be used to improve the OSIRIS retrieval algorithm for future versions. This paper fits into the scope of AMT. However, some results presented in the manuscript need additional clarification. I would recommend the manuscript for publication after the listed comments/remarks are addressed.

Specific Comments:

p. 3820, l. 19-22. " and within error of 3% " . I would suggest to re-phrase this statement because in reality the drifts +/- the error bars intercept +/-3% lines. This statement is also repeated on p. 3835, l. 20-21 and in Conclusions, p. 3838, l. 4-6.

p. 3823, l. 8. I would suggest providing specific details instead of saying "very little variation".

p. 3826, sec. 4, par. 2. I would suggest providing necessary clarification on multiple conversions of the OSIRIS profiles for comparisons with MLS. First, OSIRIS profiles were converted from number densities on the altitude grid to mixing ratios on the MLS pressure scale. Then they were converted back to number densities on the altitude grid. The first step was made to account for differences in the vertical resolutions of the instruments, and it would be reasonable to compare OSIRIS and MLS profiles on the MLS pressure scale. But authors made another step and converted MLS and OSIRIS profiles to number densities on the altitude grid. The motivation for this additional transformation is not clear. If it is a desire to make all comparisons in number densities on the OSIRIS altitude grid, then the original OSIRIS profiles could have been compared directly with the interpolated MLS profiles without the smoothing, because authors noted that the smoothing of OSIRIS profiles with the MLS averaging kernels "had a minor impact on the results".

p. 3828, l. 9-10. "The variability ... is small". I would suggest providing specific numbers instead of saying that variability is small.

p. 3828, par. 3. Could you, please, explain how the correlations have been calculated. Did you calculate correlation for the time series of daily zonal means? Please, explain your method in Sec. 4 "Methodology".

p. 3829, l. 16-19. I would suggest replacing the term 'air mass'. The term 'air mass' is associated with the dynamical processes. The diurnal ozone variation is driven by the photochemistry and depend on the solar zenith angle rather than on dynamical movements of air masses. It would be appropriate to re-word this sentence as: "This

suggest that discrepancies at high latitudes can be partly explained by the diurnal variation of ozone."

p. 3830, l. 1-5. Authors mentioned large positive biases below 20 km at SH high latitudes in fig. 3 relative to MLS. Though I can see large positive and negative differences in this region. Do you refer to another figure?

p. 3831, sec. 5.2.2 par. 1. There is a contradiction between the analysis of Fig. 7 and the results shown in Fig.7. Authors reported: "In the tropical troposphere, descending node measurements are biased low against MLS and ozone sondes, with a larger low bias in the Southern Hemisphere." However, I can see in Fig 7f that biases in the southern tropical troposphere are positive. This paragraph needs some work. It would be helpful if authors can put a reference to the specific figure (for example, Fig. 7a etc.), when they describe and analyze results in the text.

p. 3834, sec. 5.3, par.1. The first paragraph of Sec. 5.3 describes the methodology of calculating drifts. Wouldn't it be better to move this paragraph to Sec.4 "Methodology"?

p. 3847, Fig. 2. This figure shows the global mean results. Could you, please, explain in the text or in the figure caption how the global mean results were obtained. Did you calculate the mean differences for each latitude bin and then average them? Did you weight results by latitude or number of coincidences?

p. 3857, Fig. 12. There is a positive and significant (within the shown error bars) drift in the altitude range between $\sim$15-20 km and 25 km (see Fig 12 a, b, c and d). I would suggest to discuss this result in Sec 5.3.

Technical notes:

p. 3830, l. 9. Should be "at".

p. 3849, Fig. 4c. Please, change the range for x-axis in order to fit 1-sigma lines.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 3819, 2013.