

Interactive comment on “Retrieval of daytime mesospheric ozone using OSIRIS observation of $O_2(a^1\Delta_g)$ emission” by Anqi Li et al.

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General comments: This manuscript presents a generally well written study on a new MLT ozone data set retrieved from O₂ IR A-band airglow emission measurements with the IR imager on the Odin satellite. The calibration of the IRI measurements is described, as is the retrieval approach and a first comparison with independent satellite measurements. The study presents an interesting and relevant contribution to the field and should eventually be published. I ask the authors to address the following comments, many of which are really minor. I have two general comments: 1. The section on the calibration of the IRI measurements should be more specific and detailed. I guess this paper will be THE paper on the calibration of these measurements and will be used as a reference for future papers. The description

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is not detailed enough to understand the details and to reproduce the individual steps. I'm not asking for every little detail to be explained, but more information on the vague parts should be provided (see also the specific comments below). A more detailed description on calibration will be included in another paper, which is aimed for submission in several months by Saskatoon authors. 2. The agreement of the IRI O3 retrievals with some of the other data sets is not very good or rather poor at some altitudes, latitudes and/or time of the year. The authors offer different explanations for these differences, but issues with the photochemical model are not discussed. I think this is an obvious candidate to investigate. I'm not asking for new analyses etc. but suggest mentioning that the model may be an issue here and may / will be tested in a future study. In my opinion the data sets (IRI and co-located SMR measurements are a unique opportunity to test and improve the photochemical model. The issue with the uncertainty in the photochemical model is mentioned in the introduction section as well as in the ozone retrieval section. But this will be re-emphasized again in the result/discussion section in the updated version of the manuscript. Indeed, IRI and SMR measurements are a unique opportunity to test and improve the photochemical model in a future study. In the course of this study we noted some instability in the mesospheric part of the SMR profiles. While the average profiles are fine in order to properly tune the model individual profiles of good quality are needed. We therefore reserve this for a later. We would like to thank referee #1 for the valuable input to help us improve the manuscript. Specific comments: Title: "OSIRIS observation" -> "OSIRIS observations"? Corrected in the revised version. Line 12: "19 years-long mission" -> "19-year mission" ? Corrected in the revised version. Line 38: "affect the inferred ozone distribution, especially whose lifetime is comparable to the transport timescales." Something is missing / wrong here. Please correct. Corrected in the revised version as "Furthermore, the photochemical timescales of the airglow species critically affect the inferred ozone distribution, especially species whose lifetime is comparable to the transport timescales." Line 54: "Degenstein et al. (2005b)" I suggest changing the order of the papers in the reference list such that Degenstein et al.

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(2005a) is cited first. Degenstein et al. (2005a) is cited in line 26 before Degenstein et al. (2005b) is cited in line 54. Line 64: “sample .. have been processed.” -> “sample .. has been processed.” Corrected in the revised version. Line 70: “we also include MIPAS and ACE-FTS ozone profiles, measurements retrieved from other satellites” This is only a really minor thing, but “measurements retrieved” sounds somewhat strange. I tend to associate “measurements” with the initial radiance spectra measurements. Perhaps you could write, e.g. “.. ozone profiles, i.e. data sets retrieved from measurements with instruments on other satellites”. I leave it up to you to decide, whether you want to change this or not. Corrected in the revised version. Line 83: “emissions with“ -> “ emissions with a” ? Corrected in the revised version. Line 91: “dark current and electronic offset” -> “dark current and electronic offset correction” Corrected in the revised version. Line 93: “version of (Bourassa, 2003).“ -> “version of Bourassa (2003).”, i.e. wrong cite command used. Corrected in the revised version. Line 116: “The fitting process is a periodized” Please explain what “periodized” means in this context. It is unclear to me. The fitting process is split into temporal chunks that span several calibration periods of the IR instrument (which are roughly every 50 orbits for the bulk of the mission). In this way, small changes in the calibration parameters can be tracked as the satellite instrument ages. This will be clarified in the upcoming paper Line 131: “The in-flight curves closely resemble the pre-flight curves with notable differences towards the edges of the arrays.” It would be interesting to show this comparison, because this paper will probably serve as a description of the calibration process also to be used for future studies. This will be clarified in the upcoming paper. Line 141: “The shape of the stray light is then extrapolated to lower tangent altitudes” Please describe, how this is done. There are many different ways to extrapolate data. This is a big section with a lot of discussion, thus it will be left in the upcoming paper. Line 148: “or photons per second from a unit area” from a unit area? I think it’s photons passing through a unit area, right? Corrected in the revised version. Same line: “The usual per nm wavelength dependence of the radiance” This would then be “spectral radiance”. The quantity with your units is

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simply “radiance” The usual unit of spectral radiance is not used in this formulation as the spectral information is effectively lost by integration of the signal across the passband. The radiance or brightness units are then photons per second. Line 162: “The final reported error also incorporates the error in the pixel electronics offset” How is the final error determined based on the individual error components? This should be explained in more detail. This will be clarified in the upcoming paper Figure 1, left panel, x-axis label: “Radiance” Units missing. Corrected in the revised version. Line 187: “The value of phi is relatively insensitive to the emission temperature.” Can this be quantified? If you have tested that it is relatively insensitive to T, you should be able to easily provide a rough quantitative estimate. In the updated retrieval procedure, we have implemented the temperature dependent fraction of the optical filter overlapping the emission band, i.e. the value of phi, being temperature dependent, based on the temperature at the tangent point. The description is added to the revised version of the manuscript. Caption, Figure 2: “Every two rows” -> “Every second row”? Corrected in the revised version. Line 181: space missing in “B-band(688nm)” Corrected in the revised version. Line 300: “are only sensitive below 90km or below.” ? Corrected in the revised version. Line 330: “Eq.9” -> “Eq. 9” Corrected in the revised version. Caption, Figure 6: “Every two rows” -> “Every second row”? Corrected in the revised version. Caption, Figure 7: “scaled with their corresponding a priori profiles.” -> “divided by the corresponding a priori profiles.” This figure is deleted. However, the updated/added figures which show the relative error are written ‘error size relative to the individual a priori profiles’ in the captions. Line 347: “the 20 years data” -> “20 year data set” Corrected in the revised version as ‘the 20-year-dataset’. Line 386: “the thermal emission line of ozone” Line? It’s many, many lines, right? No, it is a single line Line 388: “Van Der A” -> “Van der A” Corrected in the revised version Line 406: “SMR ozone measures from” -> “SMR provides/measures ozone from” Corrected in the revised version Figure 8, IRI data: Why is O3 negative over such an extended altitude range? It would be good to discuss potential reasons in more detail. What about problems with the photochemical model? Please also mention,

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whether the VERs are also already negative in these regions. VERs are not negative but very low for this region. The main reason for such an extended region of negative values mainly comes from the fact that the photochemical equilibrium assumption is used in the model, while this assumption is hard to be considered valid in that region. On top of that, the model includes the contribution from O₂ ground state to produce O₂(1Δ), therefore the inversion tries to force O₃ to be very low, even negative, to overcompensate the low VERs being observed. In short, the steady state assumption will underestimate O₃ concentration when this assumption is not valid. We have added this discussion in the revised manuscript, as well as a novel approach to address this issue in an updated procedure for reprocessing IRI O₃ data (see the newly added Sect. 2.3.3.) Line 425: “every 20th orbits .. have” -> “every 20th orbit .. has” Corrected in the revised version Line 343: “and therefore blanked out in Fig. 9.” Please check grammar of this sentence. Something is missing here. (perhaps Line 434) The section and figure have been rewritten in the revised version therefore this comment is no longer relevant. Line 437: “MIPAS observes a deeper trough in the winter hemisphere as in IRI and SMR data, but a relatively even distribution in the MLT region.” I can’t really see that in the figure. What does “deeper” refer to here? The ozone values or altitude? This is not clear. The section and figures have been rewritten in the revised version therefore this comment is no longer relevant. Lines 441 following: The authors discuss differences in SZA as a cause for the differences between the different datasets. This is certainly a possible reason, at least for part of the differences. But are the differences between the data sets consistent with the diurnal variation of O₃ and the different SZAs of the measurement shown in Fig. 10? This could be easily addressed qualitatively. This figure and the discussions about differences in SZA sampling are no longer in the revised manuscript, as the reprocessed IRI data show significantly closer to MIPAS zonal mean data compared to the version before. Figure 11, left panel: x-axis label is wrong -> “cm⁻¹” This figure is no longer in the revised manuscript. Line 454 following: It should also be mentioned that the differences can be significantly larger at other latitudes. We have expanded

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the difference figure to include all overlapping latitude bins (see Fig. 14 in the revised version), excluding ACE-FTS as referee #2 provided the reason not to include this dataset in the paper. Line 474: “Overall, IRI agrees very well with SMR” Looking at Figures 9 and 12, I think this statement is not justified. Relative differences between the two data sets reach very large values, right? With the newly added Fig. 14 in the revised version, it is shown that the updated IRI data has generally 20-50% positive bias compared to SMR and MIPAS. Line 477 following: “The differences between IRI, ACE-FTS and MIPAS in Fig.12 may be explained ..” There may also be issues with the photochemical model used to retrieve O3. I think this should be explicitly mentioned. The dataset should be used in future studies to attempt to improve the photochemical model used. We acknowledge this comment. This is emphasized in the revised version. Figure 12: I suggest that negative values are more clearly indicated (e.g. in black). The current depiction makes it difficult to identify negative values. After the IRI data being reprocessed with the modification in measurement uncertainty scaling with the equilibrium index, the negative values in such plots are mostly replaced by a priori value with low measurement response. Data with low measurement response and low equilibrium index are filtered out before making an averaging profile. Table A2: “ACE-FTS tmospheric” Thank you for pointing out the typo. However, ACE-FTS will be removed from this paper for ozone comparison as pointed out by referee #2.

Please also note the supplement to this comment:

<https://amt.copernicus.org/preprints/amt-2020-56/amt-2020-56-AC1-supplement.pdf>

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-56, 2020.

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