Atmospheric Measurement Techniques Discussions

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Reply on RC1

Kimberlee Dubé et al.

Author comment on "An improved OSIRIS NO_2 profile retrieval in the upper troposphere–lower stratosphere and intercomparison with ACE-FTS and SAGE III/ISS" by Kimberlee Dubé et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-133-AC1, 2022

Thank you for taking the time to review our manuscript and for providing helpful suggestions. Our responses to each comment are included here in bold text.

Page 2, Section 2.2: This Section is a bit difficult to follow. I would recommend the authors to summarize all those validation results in a table including the following entries (or something similar): OSIRIS retrieval version, ancillary data + version, local time/SZA used for the comparison, altitude range, comparison results.

A table summarizing these results has been added to the manuscript.

Page 3, Section 2.3: The authors should describe in the first paragraph how the temperature dependences of the O3 and NO2 cross-sections are treated in their retrieval. Also, are the absorptions by O4 and water vapour included in the retrieval?

Again, a table summarizing the main retrieval settings could be helpful for the reader here.

The temperature dependence is handled in two different ways. The forward model radiative transfer calculation includes the full temperature dependence at all altitudes. For the regression for each line of sight we use the temperature at the tangent point to compute the cross section. This is now mentioned in the manuscript. A table of the main retrieval settings has also been added to the manuscript

No, the absorptions by O4 and water vapour are not included in the retrieval. Both water vapour and O2-O2 are spectrally uncorrelated with NO2 and weakly absorbing in the spectral region used, so we do not expect biases from neglecting them. However, it is something to consider for future data versions.

Page 4, lines 80-82: The threshold value of 1.01 should be justified.

We have added the statement "which results in profiles that have converged to a level that is orders of magnitude less than the estimated precision". The 1.01 here is actually a very strict convergence criteria for the values it is testing, it is not comparable to something like a 1% chance in chi2 between iterations which would be a much weaker criterion.

Page 5, lines 112-113 and page 6, lines 126-127: the new retrieval version allows to retrieve negative number density values. Is any quality-control criterion applied on these profiles with negative values? For instance, do you reject profiles with negative values which are below a given threshold?

Negative values are not filtered out, and in fact should be used in computing quantities such as a monthly zonal mean or in the comparisons presented here or else the results will be biased high. This is now mentioned in the manuscript.

Figure 3, page 7: Why the 10°S-30°S and 10°N-30°N latitude bands are not considered in this figure? Also, nothing is said about the probability densities of both retrieval versions at high (>50°) latitudes. If not included in Figure 3, both aspects should be at least discussed here. Another option would be to put the figures with the missing latitude bands in an annex.

The figure has been updated to include all latitudes.

Figure 4(a): Why not showing an example of averaging kernel peaking at an altitude below 15km, i.e. with a difference between the nominal altitude and the altitude derived from the Gaussian fit which is larger than the threshold value of 1.5km? This would better illustrate your kernel filter approach.

Thank you for the suggestion, the example altitude in the figure has been changed to 15.5 km.

Page 11, lines 216-227: the photochemical correction applied to all data sets and which consists in shifting all of them to 12:00 pm is a critical point and to my opinion, the uncertainty associated to this correction should be better characterized. The 1% uncertainty on NO2 obtained by perturbing the main input of the model is likely correct but this is clearly a lower estimate of the photochemical correction uncertainty. In order to get a better estimate, I recommend to make some sensitivity tests on the rate constants (and their respective uncertainties) of the main reactions involving NO2. Also, nothing is said about the stratospheric aerosols. Did you include them in your photochemical box model simulations? If yes, are they those simultaneously retrieved from the OSIRIS measurements (see page 3, lines 67-70) ?

Thank you for the suggestion. It would be worthwhile to test the effect of varying the rate constants in PRATMO, however it is not feasible to do within the scope of this study as the PRATMO software is not designed to have the rate constants altered. It is also worth noting that the reaction rates used PRATMO are the same as those used by larger models like WACCM, so they are likely well quantified (the rate constants are taken from Burkholder et al (2015)).

We do not include aerosols in the box model calculations. It would be interesting to investigate their effect in the future.

Burkholder, J. B., Sander, S. P., Abbatt, J. P. D., Barker, J. R., Huie, R. E., Kolb, C. E., ... & Wine, P. H. (2015). *Chemical kinetics and photochemical data for use in atmospheric studies: evaluation number 18*. Pasadena, CA: Jet Propulsion Laboratory, National Aeronautics and Space Administration, 2015.

Figure 7, page 12: Why not including also mid-latitude bands?

Mid latitudes have been added to the figure.

Page 13, line 251-252: The lower bias in the SH is attributed to the sampling of coincident profiles. I think this point should be further discussed in a quantitative way, i.e. how different are the SH and NH samplings?

This is just a theory because each latitude is sampled mostly in specific months. We have removed the statement from the manuscript. The hemispheric difference seems to just be a feature of the ACE data as it does not appear in the coincidences with SAGE. The hemispheric difference is also present in the comparisons between both OSIRIS v6.0 and v7.2 with ACE. As the difference is not caused by the OSIRIS retrieval, determining its origin is outside the scope of this work.

Page 14, lines 255-257 + Figure 9: The application of the kernel filter can have a huge impact on the retrieval results below the tropopause. What is at the end the official v7.2 product? Is it with or without applying this filter?

The official v7.2 product includes a variable for the averaging kernel filter altitude so that users can apply it if desired. This is now mentioned in the conclusion of the manuscript.

Figure 12, page 17: How did you select the altitude and latitude ranges shown in this figure? Are they representative of other altitude and latitude ranges? Maybe you could show the plots for all the altitude/latitude range combinations in an annex? This could be useful for those readers interested in stratospheric NO2 trend analysis.

We chose bins that are generally representative, but also focused on the lower altitudes as this is where the OSIRIS retrieval changed the most. Several more bins have been added to an appendix for the interested reader.

Technical corrections:

The date format is not consistent throughout the manuscript (e.g. we can find 06:30 am, 06:30 a.m, 06:30 AM). Please check.

This has been fixed.