

Response to Chris Boone (Reviewer #1)

We would like to thank Chris Boone for his very helpful review and constructive comments and suggestions. We have considered all of them and are commenting below some of them. His comments are given below in black and our responses in blue.

This paper seems quite thorough and generally well written, except for a few patches where the English usage could be improved (discussed below). Detailed comments are provided below, none of them particularly concerning.

R. Thank you very much. Please see some responses below. We are also carefully reviewing the English usage, all over and particularly for Sec. 4.2 and 6.

> Line 106: "...along the LOS." The acronym LOS is not defined.
It was defined in a few lines above.

> Line 150: "and the apodization of calculated spectra used a wider frequency range."
What apodization is being used?

R. We use a Norton-Beer strong apodization. This has been added to the text.

> Line 369: "...information on the confidence limits of the error margins..."

I cannot decipher what this means. Are you saying some uncertainties (e.g., for intensities) are unavailable, or you don't know if the errors provided are 1-sigma or 2-sigma, or something else?

R. We meant that it is not clear if the errors provided are 1-sigma, 2-sigma or other. This has been clarified in the revised version.

> Line 333: "The uncertainties of the spectrally interfering molecules with ozone which are not jointly fitted (e.g., as in the case of water vapour), as well as their vertical covariances, are estimated from the error covariance matrices of previous MIPAS data version V5."

I do not see anywhere an indication of which interferers (other than H₂O and a passing mention of CO₂ laser lines) are being included in the analysis.

R. In addition to CO₂ and H₂O, the following interfering species were included:

N₂O, CH₄, NO₂, NH₃, HNO₃, ClO, OCS, HCN, CH₃Cl, H₂O₂, C₂H₂, C₂H₆, COF₂, C₂H₄, F-22, CCl₄, CFC-113, CFC-114, N₂O₅, HCFC-141B, HCFC-142B, ClONO₂, CH₃CCl₃, CH₃OH, Acetone and PAN.
A sentence has been added to the text.

> Line 384: "...ingoing uncertainties affecting the retrieval were reported..."

Perhaps "...various contributions to retrieval uncertainties were reported ..."?

Correct. It has been changed.

Starting in the last few paragraphs, the level of English seems worse than the text up to that point. The issue continues for this section (4.2). I will provide no further specific comments on English usage, but I would suggest this section be rewritten to improve grammar and phrasing. The level of English improves again following this section, although Section 6 could also use a mild rewrite for English usage.

R. Thank you very much for the corrections already given. We have carefully reviewed the English usage for the entire manuscript paying particular attention to Secs. 4.2 and 6. We hope it is better readable now.

> Line 396: “T-LOS” Terminology is not used consistently. Sometimes T-LOS, sometimes T+LOS (in the figures), sometimes TLOS. It is not made clear that LOS refers to the line of sight and relates to the tangent altitude determination.

R. Sorry about that. We have revised the whole manuscript and adopted the ‘T+LOS’ terminology, the same as in the O3 NOM retrieval paper (Kiefer et al., 2023).

LOS is explained in Sec. 4.1.2, the first time we mention it.

> Page 21, caption to Figure 7: Titles on the leftmost panels explicitly assume $m = 5$, MA measurements (i.e., they indicate 561-522), and the caption assumes $m=5$ for V8 (V8 561) but leaves m as a variable elsewhere. This creates some confusion. Are you comparing V8 MA results only to MA results from V5 or to results from all other V5 measurement modes? The same question applies to Figure 8.

R. This is a good point, sorry for the confusion. We are comparing only MA data. This has been clarified. Also, as pointed out by the other referee, we have fully revised the versions’ nomenclature. In particular all “V8” has been changed to “V8R”, as all the middle/upper atmosphere data were taken with the Reduced spectral resolution. The same applies to “V5”, changed to “V5R”.

> Figure 7: The color scale for the rightmost panels extends from -10 to +10 %, but there are contours in the plots labelled as 20 and -20. Is the expectation that the reader will mentally extrapolate the color scale (e.g., brighter red means even more positive)?

R. The figures have been redone with extended scales. Also, a note for the still 2 contours going beyond the colour scale in the upper/right panel has been included in the legend.

> Line 566: “...the differences are caused by inconsistencies between the spectroscopic data in those spectral regions.”

It has been shown that O3 intensities in HITRAN 2016 (the basis of ACE v4 retrievals) are too weak by about 3% (doi:10.1016/J.JQSRT.2019.01.004), consistent with this assumption.

R. Thank you very much for the reference. A sentence has been included in the revised version stating that although that difference does not fully explain the differences between ACE-FTS and MLS/SMILES, it goes in the right direction.

> Line 666: data availability should probably include all the data plotted in the paper: MLS, ACE-FTS, SMILES, etc. Some links are provided in the text but are more appropriately listed here. ACE-FTS has no data availability information provided. The link provided for MLS is broken.

R. Thank you for checking the links. They have been updated, the ACE-FTS link has been included and all the links are listed now in the “Data availability” section.

All other minor corrections and typos have been included/corrected.