

Review of “Intercomparison methods for satellite measurements of atmospheric composition: application to tropospheric ozone from TES and OMI”, L. Zhang et al., 2010.

This paper presents different approaches to comparing tropospheric ozone profiles retrieved from two satellite instruments: the Tropospheric Emission Spectrometer (TES) and the Ozone Monitoring Instrument (OMI). Three approaches are presented: an in situ method which compares the retrieved ozone profiles against ozonesonde measurements; a CTM method which compares the retrieved profile against global output from a chemical transport model; and an averaging kernel method which smooths the retrieved profiles of one instrument with the averaging kernels of the other. The authors apply each of these approaches to TES and OMI data retrieved throughout 2006 and show the in situ and CTM methods to be comparable with the CTM method capable of comparing the two retrieved products globally over the sparse spatial sampling inherent to the in situ method. The authors also demonstrate the potential of the CTM method described in the paper for evaluating chemical transport models with satellite data with GEOS-Chem CTM.

Specific comments:

Page 1420, line 10: should “tropospheric ozone” be “tropospheric ozone distribution” as it is later in the paragraph?

Lines 16-20: I think this sentence is better suited to the next paragraph in order to distinguish between empirical and direct approaches to determining ozone abundances in the troposphere.

Line 21: isn't “another approach” the same approach just applied to a different part of the spectrum? the formulation of the retrieval process is still the same.

Line 25: “A multi-year record” - please can you specify which years?

Page 1421, line 2: the context for this sentence does not seem to be all that clear - it may not be clear to a non-expert reader why this would be the case, I suggest you need to add a reference to clarify this.

Line 16: “vs. with” should be “vs. that with”.

Lines 24-25: this sentence could potentially be misleading to the reader - are you following the same methodology as Kopacz et al? please clarify.

Line 29: the statement “smoothing the retrievals of the higher-resolution instrument” is unclear - I assume this refers to the vertical resolution of the retrieved profile? please clarify.

Page 1422, lines 2-3: please can you clarify that Luo et al compared CO profile retrievals, also you need to define what MOPITT stands for.

Line 4: “as intercomparison” should be “as an intercomparison”.

Line 8: should “the in situ method...” be “an in situ method...”? the same goes for methods (2) and (3).

Line 18: “ascending equator crossing” should be “an ascending equator crossing”?

Lines 22-25: potentially misleading, please can you clarify that the Nassar et al validation against ozonesondes was “global”?

Page 1423, line 3: “spatial resolution” is not clear - do you mean instrument field of view? this is also inconsistent with the TES description.

Line 10: not clear what “along the TES sampling tracks” means - do you mean “sampled at the TES observation locations”? Later in the manuscript you also use “TES orbit track” which needs to be consistent with what is written here.

Line 18: “as described by Bowman et al. (2006) and Liu et al. (2009a) respectively”?

Lines 24-26: I suggest adding a sentence to briefly describe the retrieval process to put these sentences into better context - the first sentence seems like unnecessary detail otherwise.

General comment on equations - italicising the vectors seems to be unnecessary and does not follow the cited Rodgers notation.

Line 27: doesn't TES retrieve the natural logarithm of vmr? as you describe in Appendix A?

Page 1424, lines 2-3: this sentence is repetitive, suggest indicating this in the previous sentence - also, what about units?

Line 7: missing space - “x 60”

Line 14: are the OMI a priori profiles already described? what are they derived from? also it might be useful to specify 30S-30N is latitude band.

Page 1425, line 11: should “sensitivity peaks” be “peaks in sensitivity”?

Line 14: suggest specifying “shown in the central panel of Fig. 1”

Line 17: would the “weaker assumed a priori error constraint in TES” not propagate into the comparison in the next section as well?

Page 1426, line 1: the title for section 3 is misleading as there isn't really a description of the TES and OMI ozone distributions included - I suggest adding a couple of sentences to highlight the main features shown in Figure 3, differences between them are then described in the following sections.

Lines 5-6: the interpolation of the TES profiles has already been stated in the previous section.

Line 8: what are the “significant differences”?

Line 15: “as will be discussed in Sect. 6”.

Line 20: please can you clarify if “simply explainable by instrument sensitivity” is due to the averaging kernels as in eqns (5) and (6)?

Page 1427, line 2 and eqn 7: this is the difference from directly comparing TES and OMI - I suggest adding a sentence outlining the contents of this section before starting with the equation.

Line 5: how many retrieved profiles are there typically in a grid cell?

Lines 17-18: "but is limited by the sparsity of ozonesonde observations" looks like a conclusion and therefore not needed right here - I suggest rewording that this is what might be expected, or else add a reference that shows this.

Page 1428, line 2: "Previous validation against...?"

Line 6: please can you clarify that the interpolation is to the pressure grid for the OMI retrieval?

Line 11: please can you clarify if by "measure the retrieval error" you mean the actual retrieval error (i.e. \hat{S}) or the error term from equation (1) (i.e. $e + b$)?

Equation 10 (also 13 and 15): the delta symbol is used for differences calculated using each technique - I suggest subscripting each delta to distinguish between the different techniques.

Page 1429, line 11: please clarify that "higher resolution" refers to the vertical resolution of the retrieved profile.

Line 18: on the previous page, $b_{TES-b_{OMI}}$ is stated as being the "true difference" whereas here it is "internal consistency" between the two instruments - please check the consistency of this term throughout the manuscript.

Page 1430, line 10: suggest changing "small statistics of ozonesonde coincidences" to "limited number of ozonesonde coincidences"

Page 1431, line 3: are the OMI biases smaller than the TES biases because there are more of them? what if the OMI data were sampled at the TES measurement locations as well? would the bias be more comparable to that for TES?

Line 18: suggest changing "The successful comparison" to "The close agreement" - the comparison is also successful whether the outcome of the comparison is positive or negative.

Page 1432, line 1: it is not clear from Fig. 7 that this statement is completely true - TES also has higher values than OMI in winter at northern mid-latitudes, however, there does appear to be a larger area where TES is lower than OMI for the winter months in the northern hemisphere - please can you clarify this?

Line 3: please clarify sparse spatial sampling?

Line 9: "TES bias b_{TES} " - please clarify that this is the systematic bias.

Line 10: suggest changing "smaller than..." to "less than..."

Page 1433, line 6: are the "mean positive biases" globally averaged biases?

General comment on section 6: while I agree that it is important to highlight what the possible contributing factors are to the discrepancies between the GEOS-Chem and retrieved ozone distributions, I think it is necessary to add a sentence stressing that a comprehensive analysis of these discrepancies is beyond the scope of this paper.

Page 1434, line 13: “atmospheric composition” should be “tropospheric ozone profile retrievals” - while these methods could also be applied to satellite retrievals of atmospheric composition, you only describe tropospheric ozone profile retrievals in this manuscript and should clarify this here.

Page 1435, line 7: is the “noise term due to error in the a priori profile”? in the formulation that you use, the noise term is determined by the a priori profile used and not the a priori error (e.g. S_a) - do you mean the noise term will vary depending on how representative the a priori profile is of the true atmospheric state?

Line 12: “is general smaller” should be “is generally smaller”.

Line 20: missing punctuation - “For the in situ method, using...”.

Page 1436, line 7: the GEOS-Chem underestimate is also true for the ozonesonde comparison.

Caption for Fig. 1: is the description of the TES and OMI retrievals necessary in the caption? they should be described in the main text. “dot line” should be “dotted line”. Are the colours in the TES case consistent with OMI? please clarify in the caption.

Caption for Fig. 3: suggest changing “measurements” to “distributions”.

Caption for Fig. 4: the description of the four seasons in the first sentence seems unnecessary - point the reader to Fig. 3, where they are also described. The 4th and 5th sentences do not appear to be necessary in the figure caption, suggest moving this to the main text.