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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Intercomparison methods for satellite measurements of atmospheric composition: application to tropospheric ozone from TES and OMI" *by* L. Zhang et al.

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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 mea-

surements; a CTM method which compares the retrieved profile against global ouput 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.

Response: We thank the reviewer for the helpful comments. All of them have been implemented in the revised manuscript. Please see the itemized responses below.

Specific comments:

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

Response: We changed the latter phase “tropospheric ozone distribution” to “tropospheric ozone”.

2. 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.

Response: As suggested, we put this sentence in the next paragraph.

3. Line 21: isnot “another approach” the same approach just applied to a different part of the spectrum? the formulation of the retrieval process is still the same.

Response: We now state “Tropospheric ozone has also been directly retrieved from nadir measurements of thermal infrared (IR) emission in and around the 9.6 mm absorption band at high spectral resolution.”

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

Response: We now state “A multi-year record (2004 -) of thermal IR observations of tropospheric ozone is available from the Tropospheric Emission Spectrometer (TES) also aboard Aura.”

5. 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.

Response: We removed the sentence to make the context clear.

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

Response: We changed the text to “versus that with”.

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

Response: We now state “Kopacz et al. (2010) used the GEOS-Chem CTM to test the consistency of multiple satellite CO datasets. Here we investigate the theoretical basis of the method with satellite retrievals of tropospheric ozone.”

8. 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.

Response: We state “Rodgers and Connor (2003) presented a general method to compare measurements from two satellite instruments with different averaging kernels, by smoothing the retrievals from the instrument with higher vertical resolution using the averaging kernels of the instrument with lower vertical resolution”.

9. 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.

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Response: We now state “The method was applied by Luo et al. (2007) to compare retrieved CO profiles from TES and the Measurements Of Pollution In The Troposphere (MOPITT) instrument”.

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

Response: Changed as suggested.

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

Response: We keep the word “the”. The three methods have been introduced in the earlier text.

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

Response: We state “with an ascending equator crossing time of 13:45”.

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

Response: We now state “Nassar et al. (2008) and Richards et al. (2008) presented the validation of TES V002 ozone retrievals with ozonesondes globally and with aircraft data over the Pacific”.

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

Response: We state “OMI has a large field of view of 114° with a swath of 2600 km. It has a spatial resolution of 13 x 24 km2 at nadir and daily global coverage”. We also clarify TES measurements have “a spatial resolution of 5x8 km2”.

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

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Response: We now state “we select OMI observations at the TES sampling locations for comparison”. The latter “TES obit track” in the manuscript is also replaced with “TES observation locations”.

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

Response: Changed as suggested.

17. 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.

Response: We add in the text “The retrieval algorithm minimizes the differences between observed and simulated radiance spectra subject to constraints from a priori profiles x_a.”

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

Response: This is following the ACP format that vectors need to be bold and italic.

19. Line 27: does not TES retrieve the natural logarithm of vmr? as you describe in Appendix A?

Response: We state “TES retrieves natural logarithms of ozone volume mixing ratios (VMR) in ppbv at 67 pressure levels up to 0.1 hPa, while OMI retrieves partial ozone columns in Dobson Units (DU) for 24 layers with thickness of approximately 2.5 km.”.

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

Response: We deleted this sentence. Units are also specified in the previous sentence as addressed in the comment above.

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21. Line 7: missing space - “x 60”

Response: Added the space.

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

Response: We state that “OMI a priori profiles are based on a latitude- and month-dependent ozone profile climatology (McPeters et al., 2007) derived from 15 years of ozonesonde and Stratospheric Aerosol and Gas Experiment (SAGE) data (Liu et al., 2009a).” We also specify that “averaging the original OMI a priori profiles within the latitude band of 30°S-30°N”.

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

Response: Changed as suggested.

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

Response: Changed as suggested.

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

Response: We state in the text “The OMI averaging kernel matrix shows weaker sensitivity than TES with DOFS = 1.0 in the troposphere, although this is partly due to a weaker assumed a priori error constraint in TES. The different sensitivities result in large differences between TES and OMI observations as will be discussed below.”.

26. Page 1426, line 1: the title for section 3 is misleading as there isnÊijt 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.

Response: We added the following description in this section: The general geographic

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features and seasonal variability observed by TES and OMI are very similar. They both observe the zonal wave-one pattern in the tropics, with higher ozone over the Atlantic than the Pacific. They both show enhanced ozone pollution in the summertime (JJA) of northern mid-latitudes due to increasing photochemical production, and high ozone over southern Africa in fall (SON) due to biomass burning [Duncan et al., 2003].

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

Response: We deleted the sentence “TES profiles have been interpolated to the OMI pressure grid as per Appendix A”.

28. Line 8: what are the “significant differences”?

Response: We removed in the sentence “although there are significant differences”. The differences between TES and OMI are described later in Fig.4 and section 5.

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

Response: Changed as suggested.

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

Response: We state, “we find that most of the observed differences are explainable by instrument sensitivity (different averaging kernel matrices in Eqs. (5) and (6))”.

31. 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.

Response: We now start this section with “Intercomparison of TES and OMI ozone profiles needs to account for their different vertical resolutions. The difference from directly comparing TES and OMI is given by:”

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

Response: We state in line 5, “5-25 retrieved profiles over each 4x5 grid”.

33. 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.

Response: We now state “but it is limited by the sparsity of ozonesonde observations (see Sect. 5.1)”

34. Page 1428, line 2: “Previous validation against...”?

Response: Changed as suggested.

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

Response: We state “Ozonesonde vertical profiles for a satellite viewing scene are interpolated to the OMI retrieval pressure grid”.

36. Line 11: please can you clarify if by “measure the retrieval error” you mean the actual retrieval error (i.e. S-hat) 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.

Response: We state that comparisons with satellite retrievals measure “the retrieval error (ε + b)”. We now subscript each delta (Δ_1, Δ_2, Δ_3) to represent differences calculated with the three methods.

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

Response: We state “an intercomparison method in which the profile from the instru-

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ment with higher vertical resolution is smoothed by averaging kernels of the instrument with lower vertical resolution.”.

38. Line 18: on the previous page, bTES-bOMI 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.

Response: We changed the term “internal consistency” to “true difference”.

39. Page 1430, line 10: suggest changing “small statistics of ozonesonde coincidences” to “limited number of ozonesonde coincidences”

Response: We changed the text to “is limited by the available ozonesonde coincidences”.

40. 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?

Response: We state in line 3, “The OMI validation statistics are similar if we subsample the OMI/sonde comparisons at the TES observation locations”.

41. 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.

Response: Changed as suggested.

42. 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?

Response: We now state “At northern mid-latitudes TES is generally higher than OMI, but in winter there is a broad region within 20°N-40°N where TES is lower than OMI”.

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43. Line 3: please clarify sparse spatial sampling?

Response: We now state “The CTM method shows that TES is generally lower than OMI in the tropics, which is not apparent from the in situ method because of the sparse spatial sampling of ozonesonde measurements.”

44. Line 9: “TES bias bTES” - please clarify that this is the systematic bias.

Response: We state “TES systematic bias b_{TES} ”.

45. Line 10: suggest changing “smaller than...” to “less than...”.

Response: Changed as suggested.

46. Page 1433, line 6: are the “mean positive biases” globally averaged biases?

Response: We state “the TES and OMI data for the global mean positive biases of 5.3 and 2.8 ppbv respectively”.

47. 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.

Response: We now state in the end of section 6 that “Further investigation of these model errors is warranted but is beyond the scope of this paper.”.

48. 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.

Response: We keep “atmospheric composition” to emphasize that the methods can be generally applied to satellite retrievals of atmospheric composition. The next sentence states “The three methods were illustrated and compared using a full year (2006) of

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tropospheric ozone data".

49. 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. Sa) - do you mean the noise term will vary depending on how representative the a priori profile is of the true atmospheric state?

Response: We now state "a noise term due to error in the a priori profile (the difference between the a priori profile and the true profile)".

50. Line 12: "is general smaller" should be "is generally smaller".

Response: Changed as suggested.

51. Line 20: missing punctuation - "For the in situ method, using...".

Response: Changed as suggested.

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

Response: We now state "Both satellite (TES and OMI) and in situ measurements show that GEOS-Chem underestimates ozone at 500 hPa in the tropics".

53. 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.

Response: We deleted the description of the TES and OMI retrievals in the caption. They are described in the main text. The term "dot line" is replaced with "dotted line". We clarify the color for the three panels that "Red colors represent high pressure levels while blue colors represent low pressure levels."

54. Caption for Fig. 3: suggest changing "measurements" to "distributions".

Response: We changed "Mean tropospheric ozone measurements" to "Tropospheric

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ozone distributions".

55. 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.

Response: As suggested, we deleted the description of the four seasons in the first sentence, which has been described in Fig. 3. We also deleted the 4th and 5th sentences, as they have been described in the main text.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 1417, 2010.