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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Characterization of ozone profiles derived from Aura TES and OMI Radiances" *by* D. Fu et al.

**Anonymous Referee #1**

Received and published: 14 January 2013

In this manuscript, D. Fu and co-authors report on the application of a combined multi-wavelength retrieval of ozone profiles from two nadir viewing satellite instruments, OMI and TES, with a focus on the lower troposphere. This study is based on previous theoretical work by Worden et al. and shows first results on 22 individual real observations. The retrieval set-up is described in some detail, the information content and uncertainties of the retrievals are discussed and comparisons with ozone sonde profiles are shown. The main conclusion of this study is that also on real data, an improvement in sensitivity to the lower troposphere is observed when using a combined retrieval, and that the variability in surface near ozone can be retrieved even when using a constant a priori.

**General comments**

The paper is well organised and reports on an interesting study with relevance for ozone research, better use of existing satellite data sets, and the definition of the upcoming generation of geostationary satellite instruments aiming at air quality observations. From the focus of the paper, AMT would have been a more appropriate journal but the study is also relevant for the ACP audience.

Overall, the paper could be shortened in several places, and not all parts are written clearly (see examples in the detailed comments below). Thorough proof reading by a native speaker would also help to improve readability. On the other hand, clarification is needed for some points and the paper would benefit from a more detailed discussion of the uncertainties to be dealt with in real data (see below). I therefore recommend this paper for publication in ACP but only after the points listed below have been dealt with.

**Detailed comments**

1. One of the main conclusions given in the abstract and summary of the paper is the statement:

   "We find that the vertical resolution of the joint TES/OMI ozone profile estimate is sufficient for quantifying variations in near-surface ozone with a precision of 26% (15.6 ppb) and a bias of 9.6% (5.7 ppb)."

   I think this statement is not supported by the study. As I understood, the precision and bias given are taken from the results shown in Fig. 5 (although the numbers do not fully agree). In this analysis, the averaging kernels were applied as well as varying a priory profiles, so this uncertainty applies to the smoothed results. The real analysis of variations in "near surface" ozone (if 700 hPa is considered to be near surface) are shown in Fig. 8, and here much larger uncertainties are found. As an aside I note that TES retrievals alone perform better than the combined retrieval in Fig. 5 which illustrates that this is not the right metric to judge the quality of the retrieved surface ozone.



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I suggest to remove this sentence from the abstract and to replace it by a quantitative statement on the uncertainty based on the evaluation shown in Fig. 8.

2. I'm surprised by the lack of discussion of the problems that are relevant when using real data as opposed to synthetic spectra as was the case in previous studies. Some obvious examples would be

   - Inconsistencies in the spectroscopic parameters
   - Differences in air volume probed
   - Differences in the cloud parameters retrieved
   - Differences in the additional parameters retrieved as compared to what is found in OMI / TES only retrievals – this could be indicative of calibration or spectroscopic problems

   I suggest to add some discussion on the effects of using real data as this is the strong point of this study.

3. I do not fully understand Fig. 8. Why are the O3 mixing ratios from the ozone sondes different in the three panels? As no averaging kernel is applied, I would have expected that all points have the same O3 value from the ozone sondes. Please explain.

4. In Fig. 9, the caption claims "that joint TES and OMI observations have better capability of capturing the variations of ozone concentration than each instrument alone in the region from 700 to 200 hPa". Apart from the fact that in the figure, it says that O3 from 700 to 100 hPa is shown, I think that this plot illustrates that the TES only retrieval gives the best results for this altitude range, as is also evident from Figs. 5 and 6. This should be corrected in the caption and also in the text.

5. There is a change in colour scheme between Figs. 5-7 and Fig. 8. In addition, the caption in Fig. 7 refers to the wrong colours. Please homogenize and correct.



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6. In the caption of Fig. 6, reference is made to OMI data which is not shown.
7. Abstract: DOF defined twice
8. p 27591, l16, check closing brace of Browell reference
9. p 27592 / 27593, section on satellite instruments: difficult to read and not clear if really needed. Note that S5 is a polar orbit satellite.
10. ) p 27594, what is 1B2 and 2A1 for the TES bands?
11. ) p 27595, l 6, remove “those”
12. p 27596, l 18: check grammar of sentence
13. p 27595, l 12: characterization => characteristics
14. p 27598, l 27, obtaining => obtained
15. sections 3.2 and 4.1 – reconsider how much is needed for the paper and what can be dealt with by a reference to Rogers or some previous work on TES retrievals
16. p 27605, l 7: troposphere => tropospheric
17. p 27607, l 3: For the unmeasured => The unmeasured
18. p 27609, l 13: the bias increases but the precision decreases. . .
19. p 27610, l 3: does => do
20. p 27610, l 16: that => those
21. p 27610, l 24. Check grammar of sentence
22. Why are the tables repeated in the supplement?



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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 27589, 2012.



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