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Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrievals of Tropospheric Ozone Profiles from the Synergic Observation of AIRS and OMI: Methodology and Validation" *by* Dejian Fu et al.

Anonymous Referee #2

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The paper describes an algorithm for retrieving tropospheric ozone concentrations from a synergy of collocated AIRS and OMI spaceborne measurements, named AIRS+OMI. It deals with a challenging methodological problem. The results are presented and discussed only in comparison with TES retrievals and ozonesonde measurements. Overall it provides some evidence of the gain of using the synergetic observation and its performance, but in many occasions it is not precise enough. The paper lacks of sufficient discussion and critical analysis of the results. The improved performances of AIRS+OMI synergetic retrieval are not confronted with single-band IR and UV ozone profile retrievals from AIRS and OMI, which should be the first step of the analysis. The paper is suited for the AMT journal but it requires several major revisions in order to be

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publishable.

MAJOR REVISIONS:

1. Comparison of AIRS+OMI with other satellite retrievals: Since the new AIRS+OMI approach is the combination of AIRS and OMI retrievals and such synergism is a challenging task, it is essential to compare the multispectral approach against the single-band methods from AIRS and from OMI. In section 4.1, a comparison of the new AIRS+OMI retrievals is exclusively done with TES, considered as a validated approach. However, it is impossible to know what is the true gain of the multispectral synergy and which information on the vertical profile of ozone is either provided by AIRS or OMI measurements. I strongly recommend comparing the satellite retrievals from AIRS+OMI with AIRS only, OMI only and TES (section 4.1) and also each of them against ozone sondes (in section 4.2).

2. Comparison of AIRS+OMI with ozonesondes: The comparison of satellite retrievals against ozonesondes provided in section 4.2 is too weak to be convincing. The correlation coefficients between satellite and sonde data are not presented in the paper, even if they are essential for evaluating the performance of satellite retrievals. Differences between standard deviations of theses datasets are also an important diagnostic for such evaluations. Moreover, the gain of the multispectral synergism of AIRS+OMS with respect to single-band approaches should be shown in terms of comparisons against ozonesondes. I also recommend also showing the performances of the each of the satellite retrievals against the same ozone sondes and not different datasets, since otherwise we do not know whether the differences come from the chosen datasets or from the performances of the satellite retrievals. This major recommendation is to present a comparison with ozonesonde measurements against, AIRS+OMI, AIRS, OMI and TES, in coincidence for the same sondes (in addition to the full set of ozonesondes coincidences if additional information is provided), and using diagnostics including correlation coefficient and standard deviation comparisons.

3. Additional new multispectral ozone retrievals from CrIS+OMPS: Section 5 of paper presents in an extremely brief and incomplete form a completely new retrieval of ozone from different sounders, corresponding to CrIS+OMPS. I strongly recommend withdrawing this section 5 from the manuscript and all associated conclusions in section 6. The presentation of a new retrieval absolutely needs a thorough comparison against ozonesondes and other satellite retrievals in the same time basis as it is currently done with AIRS+OMI. The title of the paper only refers to AIRS+OMI and not to CrIS+OMPS. It would be much more appropriate to present this new product in a separate paper dedicated to show its performance and its thorough validation.

OTHER GENERAL RECOMMENDATIONS

4. Title of the paper: "synergetic observation of AIRS and OMI" is not clear. I recommend replacing it by " synergism of AIRS and OMI". Also capital letter may only be used at the beginning "Retrieval" and names as AIRS and OMI, but not intermediate words.

5. The good performance of TES ozone retrievals is thoroughly presented in the introduction. I suggest to also mentioning the performance of other tropospheric ozone satellite retrievals different from those from AQUA satellite. It is also stated in the paper that AIRS+OMI retrievals extend the TES record. I think that this association is awkward, since there are many other satellite databases of tropospheric ozone and AIRS+OMI is a different satellite retrieval, independent from TES. I recommend withdrawing the statement "extend the record of TES" of the paper since they are simply two different tropospheric ozone databases and only mentioning that the performance of both AIRS+OMI and TES is similar.

6. Clarity of figures: Many of the figures are difficult to read. It is very difficult to find the labels and quickly see what is shown. I strongly recommend to identify each of the panels of each figure with a letter (a), (b), (c), (d), etc (as the standard way and not A1, A2, B1) outside of the graphs themselves and with clear subtitles also outside of the

graphs (instead of mixing with colors of the plots).

7. Explanations and English language: Many of the panels of the figures are not or very briefly explained (e.g. a priori plots in Fig. 3). This is also the case for biases and RMS differences between satellite retrievals. In many cases, English language is not sufficiently clear or terms appropriately used. Please revise the text in terms of English language and add detail explanations of each of the elements of figures and tables.

8. The comparison of performances of satellite sensors in section 2 should also mention radiometric noise and signal-to-noise ratio and not also spectral resolution.

9. Multispectral retrievals from ozone and carbon monoxide are frequently mentioned together in a single sentence (e.g. conclusions section). These two retrievals are completely independent and different, using different measurements, spectral ranges and configuration of the retrieval techniques. Since the current paper concerns only ozone, I recommend mentioning these retrievals in two separate sentences, first ozone and then carbon monoxide, in a clearer way.

PARTICULAR POINTS:

10. In section 2, it is written that “The spectral resolution of TES is higher than the existing TIR and UV space spectrometers..” Since TES does not measure UV radiances, its spectral resolution against UV sensors is not comparable.

11. In section 2, it is stated: “around local noon time when the atmosphere/land thermal contrast is typically higher than other times of the day. ”. What is the evidence for this statement? Is there a reference? This might not always be the case.

12. Page 4 (line 10). The term “healthy” is not objective. Please replace.

13. End of section 2: How is the comparison against ozonesondes is done when several satellite pixels meet the coincidence criteria?

14. Page 4 (lines 23-34): This explanation of the algorithms is confusing and difficult to

read. Please detail separately each part of each of the algorithm. Clearly indicate and explain separately in each case: the radiative transfer codes, the retrieval codes, the a priori profiles and how they are chosen for each individual pixel.

15. Figure 2. Why is the tropopause pressure shown? Is it linked with the choice of a priori profiles for ozone? These aspects should be thoroughly explained.

16. Page 9 : The “species retrieval quality” should be thoroughly detailed in the paper, as a separate paragraph.

17. Section 4.1. The seasonal behavior of the correlation between AIRS/OMI and TES should be explained. Table 3 shows that for all three pressure levels the period of Septembre-Octobre-November coincides with the slight drop of the Pearson correlation coefficient values. Which part of the measurement (AIRS or OMI) is responsible for this? What could be the reason of this behavior? Correlations, biases and RMS between AIRS alone versus TES, and OMI alone versus TES would make it clearer. These comparisons should be added to the discussion.

18. Figure 3: comments of this figure are not precise not clear. They should better indicate the location, the vertical level and the panel to which they are refered.

19. Page 10: For clarity do not interchange the terms “error” and “uncertainty”.

20. Page 10 (lines 17-20): This paragraph is difficult to read. Please better explain in a more precise and detailed way.

21. Figure 4 and 5: Please indicate in a legend within the graphs the meaning of the line types.

22. Section 4.2: why number of coincidences between sondes and AIRS+OMI is lower than that for TES? Please clarify in detail the spatial coverage, pixels sizes, percentage of quality assured retrievals with respect to total pixels available, for all retrievals: TES, OMI, AIRS and AIRS+OMI.

23. Conclusions in section 6: “We have demonstrated multispectral retrievals . . .” is awkward. Please change by “We have SHOWN or PRESENTED . . .”

24. Conclusions in section 6. The statement “The product files of the joint AIRS+OMI 2006 ozone global survey retrievals, including a validation report and a reader program are available via the Aura Validation Data Center (AVDC) website (https://avdc.gsfc.nasa.gov/pub/data/satellite/Aura/TES/AIRS_OMI/O3/). ” should be corrected. This webpage contains the error message ‘This file you are trying to access was not found on the server.’

25. Page 16 (also before): the “global survey” and “regional mapping” modes are not clear. What is the meaning of these modes? Are they “gridding” or “sampling” of the retrievals? This should be better explain and detail.

26. Page 16 (line 8): the term “a TES daily global survey pattern of AIRS+OMI data” is not clear. What does it mean? If it is gridding or sample, I strongly recommend simply specifying the resolution of the AIRS+OMI data and then mentioning that is the same resolution as TES.

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