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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison and synergy of stratospheric ozone measurements by satellite limb sounders and the ground-based microwave radiometer SOMORA" *by* K. Hocke et al.

Anonymous Referee #2

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1 General recommendation

This ACPD manuscript by K. Hocke et al. presents a variety of aspects of the comparison of ground-based ozone profiles from the SOMORA radiometer with profiles from satellite instruments. The topic is well suited for publication in ACP. Data and assumptions are sound, and the conclusions are generally supported by the evidence in the paper. In fact, the authors are to be commended for the wide range of aspects presented. However, I feel that the manuscript would benefit greatly from a much more concise and logical presentation, and from the ommission of secondary material. In its

present form the manuscript has almost enough material for three papers (as is indeed indicated in the introduction, page 5055). The material is presented very much like a potpourri, many aspects are mentioned, but much remains unresolved. I feel that the paper is acceptable for publication in ACP only after substantial revision and some shortening.

Since the manuscript contains much valuable information and is based on many detailed investigations, I would encourage the authors to make the required effort and submit an improved, especially condensed, manuscript for ACP.

2 Major comments

A crucial point that needs to be addressed is the major step in SOMORA data after the change of the SOMORA front-end in July 2005. After this change, the level of the SOMORA data is shifted up by 10 to 20% below 32 km (top panels of Figs. 10 and 11), and possibly also at 52 km for noon (bottom left panel of Fig. 11). This change in the SOMORA data cannot be ignored, as is the case in the present manuscript. For example, in Figs. 1 and 2, the SOMORA reference is different for the 2004 to 2006 AURA data, than for all the other instruments, where the data end in or before 2005. (By the way the authors are to be commended for presenting this information in Table 1!!). The upward shift of the SOMORA data (roughly half of the entire data set), can explain why the AURA results lie to the left of the other data sets in Figs. 1 and 2. The authors should address that!! I would recommend to either correct the SOMORA data for the change, or to omit the data after the change.

The patterns of the AURA-SOMORA differences in Fig. 8 and 9 are very interesting. If they are presented, however, they should be made much clearer by the authors: Where do they come from? Are there two types of AURA and/or SOMORA profiles? If so, what differentiates these types? Time, orbit, view direction..? Could the patterns

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be caused by the July 2005 level change of the SOMORA data? E.g. because one set of satellite orbits comes from the time before July 2005, and the other orbits from the time after? I think an ACP paper should not leave such questions unanswered.

3 Detailed comments

2.1 page 5056: Please also state the expected precision/ accuracy of the SOMORA ozone profiles for day- and nighttime.

page 5059, line 23. I don't see a 5% positive bias of the SAGE II data against HALOE from 20 to 50 km in Fig. 1. Please correct your statement.

page 5059, line 25 ff, page 5060, lines 1 to 2: I would shorten this paragraph to 1 or 2 sentences, and omit Fig. 2. Not using the averaging kernels essentially gives the same results.

page 5061: I would strongly recommend to move Figs. 10 and 11 (and their discussion) to the front (e.g. around this page, before Fig. 3). These are the basic data used in the intercomparison!! Everything else is derived and second order! If the authors had looked at these time series first, they would probably have realized the jump in the SOMORA data level, and would have accounted for it properly in their further analysis.

Fig.3 What are the R^2 (squared correlations) between the AURA and SOMORA ozone values? What are the slopes and offsets? Please mention at least the R^2, or add them to the plots.

4.1 page 4061, Figs. 4 and 5. It may be a good idea to check if the differences have a Gaussian distribution. This is, however, what everybody expects - finding that it is "true" is not a major result. I think one sentence and no Figures might be enough. At least drop a few panels from the two Figures. One smaller Figure is enough to convince the

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readers.

One more point. Are the mentioned large positive differences at 52 km due to the low SOMORA values in 2004/2005 (bottom right panel of Fig. 11)? Don't just drop "outliers". Think about them, and explain why they are outliers!

4.2 This is a good and important aspect. One thing the authors should add, is the behaviour of $\sigma/\sqrt{n}$ as match distance increases (n would be the number of statistically independent measurements, maybe not an easy number to find). $\sigma/\sqrt{n}$ gives the standard error of the mean difference - essentially the error bar of the mean bias. An important tradeoff in this type of comparison is always the tightness allowed for a match in time and space versus the number of matches. If the match criteria are very tight, there are no matches - so we learn nothing. If the match criteria are very loose, there are many matches, but the two instruments observed different ozone profiles (σ goes up). Somewhere in between, there should be an optimum, probably where $\sigma/\sqrt{n}$ reaches a minimum. The authors should consider plotting $\sigma/\sqrt{n}$ and discussing this optimum (or lack of it).

4.3 Fig. 7. I think that allowing very large time differences up to 20 days CANNOT produce meaningful results. Ozone in winter/over the year changes too much (as the authors then find out). I would suggest to omit Fig. 7 and shorten 4.3 to just two or three sentences. Alternatively restrict the analysis and the plot to +- 2 or 3 days time difference, and use much more data, not just 10 days in January 2005. The results should be symmetric around 0 days - if they are not you have picked an episode where the atmosphere behaves differently before and after.

4.4 This is very interesting. It may be an entire, different paper, unless it is something very simple. As mentioned before: make sure the patterns are not due to the July 2005 level change of the SOMORA data. Go deeper! What is causing these differences? Can we really believe them. As presented now, Figs. 8 and 9 are VERY UNSATISFYING! There is nothing that makes them plausible or understandable to a reader. You

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need to convince the reader that the patterns are true, and not an artefact.

4.5. As mentioned before: Move this section, and Figs. 10 and 11 to the front: They are the foundation of everything else!

4.6. For the sake of conciseness: Omit this section and Fig. 12. Two sentences are enough: You checked it, and there was virtually no effect of tropospheric attenuation.

4.7. Same as for 4.6: You checked it, and there was virtually no effect. Omit Fig. 13 and condense to a few sentences!

4.8. I would also move this section and Figs 14 and 15 to the front, near Fig. 1 and 2 (which show the same for the other satellites). The mean difference is important, many of the other things are secondary.

page 5067, lines 24 and 25. As discussed before: When do the outliers occur? Why? Can they be ommited or not? If so, what do the results look like without outliers?

page 5067, line 26 to page 5068 line 8. Omit summary here - summary in the conclusions section of the paper is enough.

5.1 I find this section really blown up. The "double differencing method" is nothing new, and is quite obvious/ straightforward. People have been using "transfer standards" forever (e.g. Wild et al., 1995). There is no need to develop a "theoretical framework" here (Eqs. 4 and 5 could easily be omitted). The entire section should be condensed to two or three paragraphs. Fig. 16 and its dicussion can be omitted. A key point is that the "transfer standard" must not change over time. According to Figs. 10 and 11, that is not the case for SOMORA.

page 5073, lines 15 to 21: What about the obvious differences in Fig. 1? They should be discussed as well.

page 5073, lines 22 to page 5074 line 19: This is a very nice and concise summary!

page 5074 lines 20 to page 5075 line 5: The key point is that the reference needs to

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be "constant", whether it is ground-based or satellite-borne. I would suggest collapsing the two paragraphs into one.

4 References

Wild, J.D., et al., Comparison of stratospheric temperatures from several lidars, using National Meteorological Center and microwave limb sounder data as transfer references, *J. Geophys. Res., 100(D6), 11,105–11,112, doi:10.1029/95JD00631, 1995.*

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