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Atmospheric Measurement Techniques Discussions

Interactive comment on "Validation of the Suomi NPP Ozone Mapping and Profiler Suite total column ozone using Brewer and Dobson spectrophotometers" *by* K. Bai et al.

Anonymous Referee #2

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This is a very straightforward comparison of the recently released NPP TC ozone mapper data with ground based Brewer and Dobson records. I find the analysis superficial. The comparison data need to be looked at more carefully in order to understand some of the odd findings - for example, why does the Brewer network see a strong total ozone dependence in the TC differences but the Dobson network does not? The analysis needs more depth for this paper to be publishable.

There is also an issue with English usage. I started out noting some of the sentences that need to be corrected, but the revisions needed are so extensive that the entire document needs to be revised by someone who can correct the language.

specific comments:

page 1 line 20 - The v8 OMI algorithm was the basis of this processing algorithm for the data used in this comparison, not v7. (See my second comment for page 6.)

page 1 line 23 - should read "Linear regression shows fair agreement ..."

page 2 line 2 - try "When compared with Dobson measurements, the OMPS TC averages are 0.86% higher..."

Page 2 line 5 - "... were analyzed as a function of latitude and time, as well as viewing geometry."

Page 2 line 7 - should read something like: "Brewer comparisons show an OMPS TC ozone dependent difference, with a large negative bias observed for OMPS TC ozone below 220 DU, and a positive bias for ozone above 460 DU." Note: the original wording was "... an OMPS TC ozone dependent error..." This wording implicitly assumes that the Brewer / Dobson observations are "truth" and the satellite observations are in error. This may be true, but such differences could also represent errors in the ground-based observations. This could be an issue of errors in single Brewers at high solar zenith angles for example and their known problems with stray light. Either way, it must be shown which instrument system is in error when differences are found. It cannot just be assumed that the ground based systems are "truth", especially when the two ground systems do not agree.

page 5 lines 23-25 - the difference between RDR, SDR, and EDR should be explained.

Page 6 line 6 - total column ozone is measured from the surface to top of the atmosphere, not 60 km.

Pages 6 & 7 - there is confusion here between the ozone algorithm as implemented by the NASA science team and the algorithm implemented by NOAA and the NPOESS project. The product being compared in this paper was retrieved using an algorithm very similar to the version 8 algorithm used for OMI retrievals. It is based on wavelength

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pairs. This is considered a "Science Product". The NPOESS algorithm implemented by IDPS for the NPOESS project is the one that uses multiple wavelength triplets. That is the algorithm described in the Baker ATBD. It is a different product from the science team product being compared in this paper and is the "official" EDR for the TC sensor. At this point the science team product is considered by NASA to be the more accurate, though the IDPS product is good and is being improved. [Possibly a comparison of the two products is in order?]

The NOAA product is available from NOAA STAR: http://www.star.nesdis.noaa.gov/icvs/PROD/proOMPSbeta.php

2.2 Ground Based Measurements - all but 3 of the listed Brewer stations are in the northern mid and high latitudes with 20 of 35 stations between 40N and 60N, and there are no Dobson stations near the equator. The point is that this station distribution must be considered in the interpretation. (You also might check to see if more current data are available from WMO.) Also, some of the affiliations listed seem to be in error - Amundsen Scott is a US station, not Austrian, and Halley Bay is a UK station.

Page 8 line 10 - you note that zenith sky ozone is included in this analysis. You quote an accuracy of 2-3% for Dobson zenith sky, but have no estimate of the accuracy of Brewer zenith sky observations except to say that it is similar to Dobson. The accuracy of the zenith sky ozone needs to be considered much more carefully if these data are to be used. To what extent are quoted discrepancies at very low ozone and at very high ozone driven by the zenith sky observations? It is probably a small effect, but it must be discussed. Suggestion: try analysis of DS only and compare.

Page 8 lines 16-19 - This sentence needs to be rewritten.

Page 9, lines 8-11 - you state that the TC observations agree better with Dobson than with Brewer. But the very different spatial distribution of stations could be the problem, since the Brewers are nearly all in the northern hemisphere while the Dobsons are a bit better distributed.

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Page 9 (Figure 3) - another interpretation is that all the zones agree within the uncertainty.

Figure 4 - the Dobson and Brewer plots should be plotted to the same scale so that they can be directly compared - maybe -5 to +7 for both a) and b) for example. Also refer to them as "top" panel and "bottom" panel, not up and down panels.

Page 10 - again, the differing time dependence and latitude dependence of Brewer vs Dobson could be related to the differing distribution of stations with latitude. This can be checked by doing the same plot for northern mid-latitude stations only. When TC comparisons are persistently different for Brewer and Dobson observations, the conclusion must be that there is a problem. It might be that one or all of the instruments are in error, or the problem might be in the comparison technique.

Page 10 line 22 - what do you mean by VZA (viewing zenith angle)? Is this the look angle from the instrument or the solar zenith angle at the FOV? Define please.

Page 11 - the reflectivity dependence will likely depend on whether direct sun measurements or zenith sky measurements are being analyzed. You should analyze separately.

Page 12 Conclusions - the conclusion that the TC ozone has a significant negative bias for low ozone (<200) and a significant positive bias for high ozone is a very soft conclusion, for the reasons already noted in your own discussion of Fig. 9 on the previous page. This should be made clear in the conclusion and in the abstract. This might also be an artifact of the statistical binning and natural geophysical variability. Try binning the x axis by the satellite data and see what happens. The curve will change shape. Why?

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 4577, 2013.