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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Validation of OMI total ozone retrievals from the SAO ozone profile algorithm and three operational algorithms with Brewer measurements" *by* J. Bak et al.

Anonymous Referee #3

Received and published: 24 July 2014

The paper presents a detailed validation study for four different total ozone products from the OMI instrument, based on three operational algorithms (OMITOMS, OMI-DOAO3 and KOE) and the SOE algorithm. The paper is well written and structured and addresses in detail most of the aspects related to the validation of total ozone and therefore should be considered for publication in ACP after considering my comments below.

General comments:

The authors should make a comment on the usefulness of the total ozone products

as an integral from the ozone profile products, in comparison to the direct total ozone products. Is the consistency with the long-term series of the SBUV an objective of such a study and if yes, the authors should provide some relevant information. There are studies in the literature that compare column products with SBUV so the comparison of SOE and KOE with SBUV would be very interesting.

I agree with the other reviewer that the impact of the use of different absorption cross sections when comparing different total ozone data sets should discussed in more detail and respective comments should be included in the manuscript, especially concerning the observed biases.

A summary table with the main characteristics of the four algorithms (e.g. wavelength window, cross-sections, principle, calibration etc) would be very useful for the reader to follow the discussion and the figures.

Specific comments:

Page 4054, line 8, correct "Would" to "World"

Page 4056, line 8.Although the authors cite the paper by Liu et al., it would be very useful here to provide briefly some more information on the soft calibration since this is a major factor that affects the quality of the SOE data.

Page 4056, line 23. Please mention briefly what concerns the updates described by Kim et al.

Page 4057, line 13. I guess that the soft calibration mentioned here has no connection with the SOE one mentioned before, but as it is written can be very confusing for the reader, so please avoid using the same term for different corrections.

Section 3.1. Stations that seem problematic (and there are references for that) should be removed. The authors do that any way in a second step, but their inclusion in the discussion and then the exclusion can be confusing.

Page 4063. Line 6. Please write “trend of the differences”, since as it is written it can be confusing meaning trend in total ozone.

Page 4063, Line 13. Please write “the average difference of trends”. As it is written one can easily wrongly conclude that the average difference in total ozone is only 0.02%!

Section 4. This short paragraph could be merged with the section 3 when discussing the comparison results for the high latitudes as a confirmation. As it is written it hardly justifies to be considered a different section in the manuscript. A comment on possible differences between double and single Brewer comparisons should be added.

Section 5. The main conclusion from this comparison seems to be that the large KOE/SOE differences are mainly caused by other implementation details. I think that here the authors should try to be more specific. Which could be the other variables used in the fitting, what could be the source of differences in the RT simulations etc., and if there are plans to investigate these in more detail.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 4051, 2014.