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Interactive Comment

Interactive comment on "Validation of ozone measurements from the Atmospheric Chemistry Experiment (ACE)" *by* E. Dupuy et al.

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The authors have obviously put a great deal of work into this submission. But, the material is probably better suited for publication as a masters thesis or as an ACE technical note. Scientifically, the methodology is weak (being superseded by the approach of Rodgers and Connor, von Clarmann) and the paper is excessively long with little interpretation of the observed ozone differences.

As a positive, the paper is comprehensive in providing a first bias estimate for ACE-FTS/ACE-MAESTRO ozone profiles by intercomparisons with other remote measurements. Tables 5, 6, 7 adequately summarize the author's results.

On the negative, the paper is 144 ACPD pages long (with 20 ACPD pages of citations)

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and has 47 separate figures. It is worth noting that with the exception of 'Eureka (DIAL)', 'Lauder MWR', and 'Mauna Loa MWR' the authors average the ground-based results when summarizing their work. Accordingly, I question the relevance of tables (2, 3, 4, 5) as they simply describe the geographic location of ground-based research sites. Likewise, many of the 47 figures are repetitive; showing the percent ozone difference between the FTS or MAESTRO with yet another limb profiling instrument. No attempt is made to interpret the differences shown for these individual plots. This paper could be made considerably shorter if the authors removed many of these tables/figures and made them available to interested readers on the ACE website.

With regards to the analysis, the approach is rather simplistic and, in many respects, lacks the rigor one would hope to see in a modern and comprehensive review of ozone measurements. As the authors note, ozone is a rather important trace gas and even small changes in the VMR profile are significant. Their method can effectively be summarized as repetitively computing (x1 - x2)/x2 * 100% where x1 is an interpolated FTS/MAESTRO profile and x2 is some other interpolated profile. There is no discussion of how outliers are handled. More importantly, there is little discussion of the FTS/MAESTRO measurement characterization, i.e. (1) vertical resolution, (2) the expected random errors, (3) the expected systematic errors (see Rodgers, World Scientific, 2000; Rodgers and Connor, JGR, 2003).

Considering the work cited above, many authors have adopted Rodgers' view of validation,

"A full error analysis and characterization is needed as a basis for any comparisons to be made..."

and intercomparisons,

"the purpose of an intercomparison is to determine whether different observing systems agree within their known limitations."

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It is therefore disappointing that an neither an error analysis nor characterization were presented in this work. While very well cited, there is surprisingly little interpretation of the comparison results. For each instrument, VMR and difference plots are shown and explicitly described in the text (including both seems redundant unless you are highlighting a particular region for subsequent interpretation/discussion). A generic (1 page) summary suggests systematic and random errors (smoothing, interfering species, diurnal changes, spectroscopic linelists, atmospheric variability, etc) that may have contributed to the observed difference or the standard deviation but it is purely speculative. I am left to assume that this is a consequence of an inadequate characterization of the systematic and random error terms. The authors are quick to attribute the errors to spectroscopy and/or the retrieved temperature errors but offer no quantification or plots showing the sensitivity of ozone retrievals to these fields. All the statistics given in this paper arise from considering the ensemble mean and standard deviation (implicitly assuming a Gaussian PDF) for the given spatial/temporal coincidence sets. A more comprehensive definition of bias (i.e. one that includes the ex ante estimate of the systematic and random terms) is given by von Clarmann (ACP, 6, 4311-4320, 2006).

I have similar concerns regarding the interpolation, smoothing/convolution being applied to the retrieved profiles, and the comparison of profiles that contain explicit a priori information.

For limb comparisons, the FTS/MAESTRO profiles are interpolated quadratically (Boone et al., AO, 2005) and other profiles are linearly interpolated without any consideration of the measurement covariance (Migliorini et al., JGR, 2004; Calisesi et al., JGR, 2005). Likewise, when comparing with column amounts, FTS/MAESTRO profiles are integrated for a slant-column without any discussion/comment regarding the precision of the resulting quantity.

With regards to smoothing, the formalism for dealing with these effects is well established and used within the NDSC community (Connor, JGR, 1994; Rodgers & Connor,

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JGR, 2003). Ironically, the authors correctly smooth the ground-based FTIR/MW observations but do not feel compelled to rigorously handle the FTS/MAESTRO profiles. Vertical sampling is described as varying with a beta angle (?) from 1.5 to 6 km but a constant 3 km smoothing is somehow deemed appropriate. A variety of techniques can be used to estimate the actual vertical smoothing for FTS/MAESTRO (i.e. perturbation methods as done on MIPAS, http://www.ifac.cnr.it/retrieval/documents/AK_report.pdf).

Finally, no attempt is made to remove a priori information when comparing with profiles retrieved using optimal estimation. Again, a variety of techniques exist (i.e. von Clarmann and Grabowski, ACP, 2007).

Again, without a more comprehensive error characterization, it is difficult to assess whether the above methods are appropriate.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 2513, 2008.