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Interactive Comment

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

E. Dupuy et al.

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We thank Anonymous Referee #2 for his/her valuable comments, which we feel have helped improve the manuscript. We hope to have replied to each comment to the satisfaction of the Anonymous Referee. We have made efforts to implement almost all the suggested changes, including possible additions to the manuscript, while trying not to add to its length. The responses are detailed below, with the original comments indicated in italics.

This is part 2 of our response to Anonymous Referee #2 addressing the Minor Comments (on the text). Because of the maximum length allowed, the General and Major Comments from Referee #2 and the Minor Comments (Figures and Tables) will be addressed in separate author comments.

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Minor comments:

To avoid lengthening the response, we have not listed all minor modifications (rewording or word removal). These have been addressed in the revised manuscript.

p.2521,l.2: 'pressure and temperature profiles are used to calculate': The VMR profiles are calculated from the spectra, not from temperature and pressure profiles, please reformulate and summarise the retrieval method for VMRs.

The retrieved pressure and temperature profiles are used to calculate the synthetic spectra to which the ACE-FTS measured spectra are compared during the retrieval of VMR profiles. Section 2.1 was reworded for clarification.

p.2523,l.4-11: The conclusions for MAESTRO profiles from Kar et al., 2007, are almost the same as the conclusions in this paper. What is the added value of the studies in the current paper?

This ACP special issue describes the validation of the current operational data products from the ACE mission, prior to the public data release. This paper focuses on the operational ozone retrievals for both ACE-FTS and ACE-MAESTRO. It is intended as a reference for the validation of these data products. For ACE-MAESTRO, it extends the comparison time period and expands the number of correlative datasets used beyond what was described by Kar et al. (2007). This has allowed us to increase our confidence in the ACE-MAESTRO results by increasing the number of comparisons with instruments other than occultation instruments or ozonesondes. It has also demonstrated that there was no degradation of the ACE-MAESTRO retrieval quality over the

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time period used in this paper.

p.2524,l.12:'time differences were calculated in Universal Time (UT)': time differences are in hours or minutes, it is irrelevant which coordinate system is used when talking about differences.

When calculating temporal differences, the value obtained can depend on whether the calculation is done for local times or for UT. This is why we specified how the differences were calculated.

p.2525,l.5-11: 'To test the sensitivity ... during these comparisons': Was this done for all correlative data or a subset?

This was done systematically for the statistical VMR comparisons with satellite instruments and with ozonesondes. The study performed for ozonesonde and lidars (described in Section 6.6) also included a detailed analysis of the time series of the mean relative differences, for each NDACC station.

p.2525,l.16-17: 'Careful examination ...': This is a vague sentence, should be clarified. It is not clear how one can examine 'time series' (i.e. relative differences as a function of time) as a function of distance. What is the 'observation geometry' quantitatively?

We apologize for the confusing wording. The vertical profiles of the relative differences for each instrument were examined as a function of distance and of observation parameters (such as the beta angle for occultation measurements or the solar zenith angle for limb-sounding instruments). No visible trends were found in the differences. We have clarified this paragraph in the revised version of the paper.

p.2528,l.1-2: What are partial columns reported on a grid? The partial column is the column between two altitudes. Are the ACE profiles reported in partial columns for the *study in 6.6? If partial columns are used for comparisons, than how does it affect the quantitative overall results?*

For the study of Section 6.6, the lidar and ozonesonde profiles were firstly integrated into partial columns calculated within layers centered at the ACE tangent heights, as mentioned in the manuscript. The partial column values obtained were converted to VMRs attributed to the corresponding tangent heights, subsequently interpolated on the comparison altitude grids. This method gave results in good agreement with the methodology applied in Section 6.5.

p.2528,l.12-18: Very good, but the filtering is mentioned only in a few cases. Please check and report for all correlative products which filtering is done.

We applied the same initial filtering for all comparisons as was described in Section 4. Therefore, we did not indicate the basic filtering to limit the length of the corresponding sections. Whenever information specific to a dataset was available, it was used as additional filtering criteria and this was mentioned in the appropriate subsection. This has been clarified in the revised manuscript.

p.2528,l.22-24: The filtering by 'visual examination' is not mentioned in any of the comparisons, please specify why profiles have been rejected.

Sometimes very odd profiles (no more than 1 in 500) occur which can skew the analyses but are not caught by our screening checks. These few anomalous profiles were identified during an initial comparison step, by looking at the absolute and relative difference plots. They were removed from the analyses in the final comparison, giving the results presented in the paper.

p.2530,l.4: should be: 'relative differences CAN become negative'

We used the subjunctive tense for this sentence ("it is possible that the differences become negative"). We have reworded this sentence for clarity.

p.2534,l.23: change 'in all cases' to 'for all latitudes'

This refers to both sunrise and sunset occultations for both instruments and has been clarified in the text.

p.2536,l.27-p.2537,l.3: 'There is no noticeable ... based on these comparisons.': You already have the error bars, giving you the information on significance (note the remarks made earlier). If it is not significant (please calculate!), it is enough to say: 'There is no significant difference between the ACE-FTS SR and SS comparisons.'

The mean relative difference values have been checked for all four cases. The measured biases remain within their standard error bars below 42 km. The corresponding sentences have been reworded.

Section 5.2.1. Odin/OSIRIS: Is v3.0 not validated? What are the expected differences with the older version(s)?

Validation studies for v3.0 have been published recently (Brohede et al. (2007); Jégou et al. (2008)), but were not available when this work was submitted. Relevant information is included in the revised paper.

p.2539,l.3: 'Since the comparisons': This is not a good reason, It could theoretically be possible that all occultation instruments have the same bias between sunrise and sunset measurements. Furthermore, Figure 6 shows a bias between SR and SS measurements above $\sim$40 km, which is not explicitly noted in the text.

This decision was based on an examination of all satellite instrument and ozonesonde

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comparison results. We have added this explanation in Section 3 and clarified this section of the text. We respectfully disagree with the comment on Figure 6 (Figure 5 in revised manuscript): When averaging all ACE-FTS SR (top panel) vs. POAM measurements and all ACE-FTS SS (bottom panel) vs. POAM measurements, the resulting mean relative differences for SR and SS do not differ by more than 2% except at a few altitudes. Therefore, we find that there is no strong evidence for a SR/SS bias of the ACE-FTS measurements given by this comparison.

p.2539,l.23-25: Most coincidences with OSIRIS are within an hour (table 1): what would be the expected difference? Can't you check this statement with narrower coincidence criteria?

Since detailed studies of the diurnal variation have not been completed, we have removed this comment from the paper.

Section 5.4.1 Envisat/GOMOS: Version IPF 5.00 is used, what is the expected difference to version 6.0a, which was validated?

The expected differences are negligible (lower than 1-2 %).

p.2545,l.16-26: This method should be described in Section 4. The reason given to use this method is a good one, so why are not all comparisons done with this method? For 1240 coincidences, is the result different from when the mean instead of the median was used?

This method is the preferred method used by the GOMOS team because it minimizes the effect of significant outliers in the comparison. For data with minimal scatter, no significant difference is found between the mean and the median (see, e.g., Figures 10 and 11 of Meijer et al. (2004) or Figures 4 and 5 of Cortesi et al. (2007)).

p.2546,l.4-25: Here a photochemical correction is used to account for the time difference between the GOMOS and ACE-FTS measurements. Why is this not also done for the other high-altitude comparisons?

As previously noted in the replies related to the comparison methodology, the comparison work was shared between the ACE team and our collaborators. Models to make photochemical corrections were not available for use in all comparisons. Note also that GOMOS is the only instrument for which comparisons extend above 70 km, where the photochemical cycle becomes the dominant factor in the observed differences.

p.2546,l.22-25: 'difficult to draw conclusions': If we a are talking about 1240 coincidences, it is very well possible to draw conclusions about the average difference between GOMOS and ACE-FTS above 60km, especially when estimating the uncertainty introduced by the model.

The difficulty to conclude does not arise from statistical considerations but because the photochemical correction is based on a model computing climatological values, without taking into account true (unavailable) mesospheric trace gas data.

p.2546,l.26-p.2547,l.2: this paragraph is better moved to before the paragraph starting at p.2546,l.16. What does the 10.5km width mean? Is this not expected from the GOMOS resolution? If not, is there another explanation?

This calculation was used as a test to optimize the agreement between the ACE-FTS and GOMOS datasets, but was not used in the quantitative analysis. Therefore we prefer to leave this paragraph at the end of Section 5.4.1. This test was done because ozone averaging kernels are not available for ACE-FTS. Empirical determination of the equivalent smoothing function indicates that the vertical resolution of ACE-FTS could be as low as 10.5 km in the upper mesosphere. This does not depend on the GOMOS resolution which is, as stated in the text, always better than 1.7 km. Text has been

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modified to clarify this discussion.

p.2548,l.27: 'which cannot be accounted for by the combined systematic uncertainty estimates': why not? First, there still is no FTS uncertainty estimate, this is to be derived in this paper, and second the uncertainty estimate of MIPAS was 10%, as stated in l.16.

This comment has been removed from the text.

p.2549: There is no information on uncertainty estimate / validation results for reduced-resolution mission ESA product.

A first study of the quality of the MIPAS reduced resolution ozone profiles is reported by Ceccherini et al. (2008). In general, the quality of the ozone profile retrieved from reduced-resolution measurements is comparable or better than that obtained from the full-resolution dataset. The only significant change in MIPAS performance is found at altitudes around 40 km, where a bias of approximately 3% is observed between full and reduced-resolution datasets. This information has been added to the text.

p.2550: most striking features in Figure 20 are the discontinuities in the average profile gradient of the IMK profile. Please explain.

Because of the discrete sampling of the MIPAS measurements, there are necessarily discontinuities in the retrieval results. The fine vertical sampling (1 km) of the observations below 44 km explains why the MIPAS-IMK profiles look smoother and "more continuous". Above this altitude, the sampling width is larger (2 km and higher) and the profiles look more frequently "discontinuous".

Section 5.4.3 Envisat/SCIAMACHY: Version 1.63 of the IUP profiles are used. What is the expected difference to the validated version 1.62?

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The difference between the two versions is the improvements in the pointing correction, the Tangent height Retrieval by UV-B Exploitation (TRUE) algorithm. Version 1.63 of the Stratozone retrieval code uses the newest and best TRUE correction (version 1.7), while version 1.62 Stratozone retrieval is based on TRUE version 1.4. This explanation has been expanded in the revised text.

p.2552,l.24-26: does this suggest a solar zenith dependent bias in the SCIAMACHY profiles?

At this time, the source of this bias is not clear. We do not think that it is entirely dependent on solar zenith angle since the same bias is not seen in the Antarctic. There may also be a contribution from the small scattering angles for the Arctic measurements or possible contamination by direct sunlight.

p.2555,l.14: 'constant solar zenith angle of 78°': how is that possible, do you mean 'constant zenith viewing angle'?

ASUR looks upward at a constant (stabilized) zenith angle of 78°. This has been corrected in the manuscript.

p.2556,l.18: 'very good agreement around the ozone VMR peak': not at all.

The mean relative difference is of about -3% at the peak, indeed larger than for ACE-FTS. We have removed this comment.

p.2556,l.19-22: Of course not: the mean of the individual differences is at each altitude exactly equal to the difference of the means:

$$\frac{1}{N}\sum_{i=1}^{N}(x_A(i) - x_B(i)) = \frac{1}{N}\sum_{i=1}^{N}x_A(i) - \frac{1}{N}\sum_{i=1}^{N}x_B(i)$$

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I guess that there is an error in the calculations, underlying this figure.

The discrepancy was due to an anomalous ACE-MAESTRO profile, used in calculation of the mean profile but not included for difference calculation. We have removed this profile from the calculation of the ACE-MAESTRO mean VMR profile and have corrected Figure 25 (now 24 in revised manuscript) accordingly.

Section 6.2 (FIRS-2): The comparison is between profiles which are more than 24 hours apart, this only makes sense if it has been checked if approximately the same air mass has been measured.

Scaled (Dunkerton and Delisi (1986); Manney et al. (1994)) PV values for the times and locations of both measurements indicate that both ACE and FIRS-2 measured airmasses inside the polar vortex. This comment has been added to the text.

p.2559,l.14-19: I don't understand this explanation. I don't see any evidence of an altitude shift in one of the other comparisons, so why here? ACE-FTS retrievals are starting at cloud-top, as stated in section 2, so why is there an ACE-FTS profile below 18 km if there are clouds?

Convective clouds can be highly localized, making it possible that the ACE line-of-sight did not encounter any clouds during the occultation used for comparison with SAOZ. We have removed the discussion of a possible altitude shift since it was a speculative explanation.

p.2560,l.20-25: Very good to use PV to confirm measuring the same air mass, in fact it can also be used to get better coincidence criteria. See also the remarks to Section 3 and Section 6.2.

We have attempted to use PV as a coincidence criterion or evaluation factor for those

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comparisons where the position of the polar vortex could influence the observed differences.

p.2560,l.27-p.2561l.3: No numbers should be given here. From Figure 29 and 30 it is clear that the ACE profiles are within the error bars of the smoothed SPIRALE profiles. This would be clearer if you would have plotted error bars over the difference profiles.

Error bars have been plotted on the difference profiles in Figures 29 and 30 (28 and 29 in revised manuscript) and the quantitative values have been removed from the text.

p.2561,l.25-27: Why average ACE profiles for one ozonesonde measurement? This only increases the spread. There should be one profile which has the best coincidence.

We averaged the coincident ACE profiles to maximize the information available from the satellite about the ozone profile within the area of coincidence. This was also the methodology employed for comparison of the POAM III ozone profiles with ozonesondes (Randall et al., 2003).

p.2562,l.16-19: I don't understand this statement. I don't see any significant difference in figure 32 between the SR (top,middle) and SS (bottom,middle) absolute difference profiles.

Thank you for this comment. We have corrected this sentence.

p.2562,l.19-21: I don't understand the message. It is true that the VMR values are small below 15 km and that therefore the relative differences can be high while the absolute differences are not. However, both the relative and the absolute differences seem to be significant from the plot.

We have clarified this sentence.

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p.2563,l.10: why are only stations included with at least three coincidences?

This was done to ensure a minimum level of statistical significance at all stations included in the analysis. The sentence in the manuscript has been reworded to clarify this point.

p. 2567,l26-29: The degrees of freedom are not used, since the whole useful FTIR altitude range is integrated into one partial column to be compared to that from ACE. Why not compare smaller altitude ranges?

The statement above is not correct. Only part of the FTIR altitude range is integrated to obtain the partial columns used for the comparisons. We have used a smaller altitude range that was determined from the optimum measurement ranges for both instruments. The partial column calculations were also checked for a common altitude range (18-36 km) and yielded essentially identical results (the mean relative difference values obtained with this reduced altitude range being almost always within 1-2% of the values found on the optimized altitude range).

p.2568: the correlation coefficients should be calculated with similar coincidence criteria for all stations

As explained in the text, we attempted to use consistent coincidence criteria for all of the FTIR comparisons. However, the final decision on the criteria was left to the individual site.

p.2570,l.22-24: a link to Table 6 would be in place here.

This table is referred to later in this paragraph when the specifics of the measurements are described.

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p.2573,l.11-21: The expected bias in the comparisons due to diurnal variation should be quantified, also a test should be performed with narrower coincidence criteria, and possibly using photochemical corrections to account for remaining time differences.

As mentioned above, the investigation of the persistent high bias in ACE-FTS results between 45-60 km is on-going and will include a more detailed study of the diurnal variation using photochemical correction.

p.2574,l.15-17: Not clear from Figure 47. It seems that the deviation between SR and SS average difference profiles starts around 35 km.

This correction has been made.

p.2574,l.26: I would expect that an offset of a few kilometers would be more apparent in the comparisons.

We do not expect to see a large impact in the comparisons from the offset due to the timing error between ACE-MAESTRO and ACE-FTS. This is because it is not a systematic error and it only occurs sporadically in the dataset.

p.2575,l.17-18: Any ideas why the SR/SS bias is not apparent in the SMR comparisons?

Currently we do not have any ideas on this.

p.2575,l.25-26: Why is it unlikely to account fully for this bias?

This comment has been removed.

p.2577,l.11-16: These studies were either not mentioned at all, or only briefly men-

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tioned, or only shown as one example. That is far too meagre to put it in the conclusions.

This discussion has been removed from the conclusions.

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