

Interactive comment on “Validation of water vapour profiles from the Atmospheric Chemistry Experiment (ACE)” by M. R. Carleer et al.

Anonymous Referee #1

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The paper "Validation of water vapour profiles from the Atmospheric Chemistry Experiment (ACE)" presents the results of a necessary and important validation effort assessing the ACE-FTS water vapor dataset. The comparisons shown here are complicated due to less than ideal coincidence and other compatibility problems between ACE and the other comparison instruments, and due to various suspected problems in the measurements made by those instruments. While these complications make it difficult to draw very strong conclusions in some cases, they serve to highlight the need for high quality ongoing atmospheric water vapor measurements. Some of the results seen in this validation effort are robust, and some still leave open questions. Nevertheless, the current investigation is necessary and should be published, in my opinion, after some changes as outlined below.

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Specific comments: The organization of the paper is clear and easy to follow. The introduction is thorough and provides a good basis for the discussion that follows.

My main objection is that many of the conclusions about individual comparisons throughout the paper are overstated. For example, the authors find a bias between ACE-FTS and HALOE and then state "Given the possible low bias in HALOE data, this suggests that the ACE-FTS measurements are highly accurate in this altitude range." While the authors are correct to point out that other studies have found a bias in HALOE, it is a leap of faith to draw the conclusion that ACE-FTS are "highly accurate" because of it. Similarly the authors cite "very good agreement with corrected POAM III profiles" when in reality what they have shown is a bias with uncorrected POAM III profiles consistent with bias previously observed with POAM comparisons (I don't think there is actually a data set of corrected profiles). Differences seen between ACE-FTS and ODIN are waved away with a citation of personal communication. While I think it's important to point out the known or previously suspected issues with these other instruments, and that they don't invalidate the ACE-FTS measurements, I believe it is a fallacy to suggest that these biased comparisons are strong validation of the accuracy of ACE-FTS measurements. It seems like most of these objectionable comments occur in the individual comparison sections; in the "Summary and Conclusions" section, the language is much more reasonable.

The authors point out that some of the difference seen with SAGE II might be attributed to aerosol clearing problems in SAGE II data. In that case, was an attempt made to filter the SAGE II data based on the aerosol measurement as suggested by Taha et al (2004)?

A map of where the coincidences occur for the SAGE II, HALOE and POAM III comparisons should be included. I'm curious about why the HALOE comparisons show much smaller variability (even in the ACE-FTS data). Is it because the small number of coincidences all occur at one location and season?

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I'm surprised that no separation was made between sunrise and sunset coincidences in the POAM III comparison, since it was pointed out that POAM III measurements exhibit SR/SS differences (and splitting them was discussed in the SAGE II and HALOE comparisons). Why not split them in this case?

The comparisons with satellite instruments suggest a wet bias in ACE-FTS below about 15 km. The cause for this is unknown, but the coarser vertical resolution of ACE-FTS is given as a possible explanation. I don't understand this. I believe ACE-FTS measurements are individually sampled, not averaged over a vertical bin, so the sampling resolution should not cause a bias, assuming the altitude registration is accurate. Can you add a few words of explanation? Also, it would be helpful if you included information about the vertical resolution of the other instruments.

The coincidence criteria used in the ODIN comparison is much broader and the number of coincidences found is correspondingly larger than the first three comparisons. Were you unable to find sufficient coincidences using the more strict criteria?

Please quantify "good agreement" from the Gattinger et al. (2006) citation at the end of 3.1.5.

The comparisons with the Frostpoint Hygrometers should include quantitative information. It would be better to create figures in the same format as the earlier comparisons, showing means and variability with percent differences, rather than (or in addition to) these figures showing a large number of profiles overplotted on a log scale with no percent difference information. It is difficult to tell in this format just how good the comparisons are. And again I think using the phrase "perfect agreement" is an overstatement.

In general, I would like to see more consistency in the figures from one comparison to another. For example, there is no figure to show the variability of the ACE-FTS and ODIN comparison, like Figures 2, 5, 8 and 11. The variability in the differences is shown in figures 3, 6, 9 and 12 as both the standard deviation and the uncertainty in

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the mean; however, I'm not sure which quantity the "uncertainty of the distribution" in Figures 14 and 15 indicates. Some of the water vapor mixing ratio profiles are shown on a linear scale and some on a log scale. If they can't all be made on a linear scale, it might be helpful to make note in the figure captions that the scale changed.

Please double check that the descriptions of each figure in the text match what's shown in the figures. For example, I don't see the pattern in Figure 14 that is described as an "almost constant [bias] at a value of around 0.4 ppmv over the entire altitude range." Nor is it true that the difference in Figure 15 does not exceed 10% below 82 km (though it's close). The difference in Figure 6 is described as "only about 5% from 20-50 km," but there is a peak at about 23 km that looks to be about 9%.

The observation that there could potentially be errors in common between the two ACE instruments (pg 4519, lines 25-27) is a good point, which should probably be made first in section 4. Also, can you please add quantitative information about differences in geophysical location due to the refraction at the different wavelengths observed by the two instruments (pg 4518, line 2)?

Technical comments: I wonder if the title should include Fourier Transform Spectrometer (FTS). The ACE-MAESTRO water vapor profiles are not validated here.

The sentence "These processes become even more important in the mesosphere, so that water vapor is increasing in the stratosphere" (in the introduction) might be better if reworded. The link between the two clauses doesn't seem clear.

Page 4511, line 6, add "with" to make "the same bias as with SAGE II and HALOE"

Page 4516, line 12, please consider changing "no temporal variation" to "minimal temporal variation"

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 4499, 2008.

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