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Atmospheric Measurement Techniques Discussions

Interactive comment on "Four Fourier transform spectrometers and the Arctic polar vortex: instrument intercomparison and ACE-FTS validation at Eureka during the IPY springs of 2007 and 2008" *by* R. L. Batchelor et al.

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The authors would like to thank Clare Paton-Walsh for her very helpful comments and suggestions. These have been addressed in a point-by-point manner, as detailed below.

Specific Comments

(1.) Page 2891: clarify the timing of simultaneous spectra: The current description

implies that the PARIS spectrum will sample for only half the time that the “coincident” DA8 spectrum samples. If this is correct then why not compare 2 averaged PARIS spectra to each DA8 spectrum, and 1 PARIS spectrum to each HR125?

The current implication is correct, in that PARIS measurements do only measure for half the time of DA8 ones (and for the same time as 125HR ones). The measurements are not started simultaneously, so do not necessarily exactly match the measurement period, but are within a few minutes, as outlined in the text. Rather than co-add PARIS spectra to match the DA8 12 minute measurements, we have specifically chosen to compare standard measurements in all cases, as these are the measurements that are generally quoted in other situations. We believe this makes the validation more useful for our general understanding of the measurements.

(2.) Page 2894-2985, last sentence of results section 4.2, the differences are comparable in magnitude to many (but not all) previous intercomparisons around the globe. Exclude at least the reference to Meier et al which yielded a much greater level of agreement than that shown in this intercomparison.

We have excluded the Meier reference, as suggested.

(3.) Agreement within one standard deviation is not the most rigorous statistical test of agreement between two sets of data. The “paired t-test” would be quite easy to apply to these datasets and would perhaps help justify (or not) claims that there was no significant differences (as stated for the comparison between ACE and the HR125 partial columns on line 17 page 2900.) The authors should consider applying this or a similar statistical test of significance or otherwise refrain from using the term "no significant bias".

As suggested by Reviewer 2, we have added “standard error”, i.e. standard deviation/square root(n), to the tables and the comparison, as this indicated the significance of the bias in the mean. This demonstrates that in most cases, agreement after smoothing is within standard error, but in some cases it is not. As such, we have removed the

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description of “no significant bias” and clarified the text to reflect this change in the statistical test of agreement. See Reviewer 2 comments for further details.

(4.) It is not clear how the improved coincidence criteria between HR125 and ACE-FTS were determined. Were the values of 10K for temperature difference and 0.3 x10-4 s-1 for PV chosen arbitrarily or did application of these tightened coincidence criteria result in a much better level of agreement than say values of 12K and 0.4 x10-4 s-1 ? The authors should clarify this point in the text.

As explained in the text, plots of each individual measurement were made with the criteria thought to influence the measurements in order to select the coincidence criteria for sPV, temperature and distance at all altitudes. The selected values of each of these criteria were based on these plots, with these values providing a compromise between tight matching to ensure separation of different air masses and having enough points remaining for meaningful comparison.

An additional sentence has been added to the text to address the reviewer’s concern. This reads:

The values of these criteria were selected to limit the likelihood of the measurements sampling different air masses, while ensuring that there were sufficient pairs of measurements remaining for a meaningful comparison.

Technical corrections

(1.) Line 1, page 2886 - A pedantic point: the description of the measurements being from the ground to 100km sounds odd to me. Perhaps from 610m to the top of the atmosphere would be more accurate?

The text has been changed to read: Atmospheric measurements made at PEARL sample the atmosphere from the ground to the top of the atmosphere.

(2.) Line 6, page 2886 - Even more pedantic the satellites don’t pass nearby but sample nearby air.

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This has been changed to read: In addition, PEARL is ideally situated for the validation of polar-orbiting satellites, which typically sample nearby air several times a day

(3.) Page 2887: The descriptions of the instruments are not entirely consistent. For instance, why are the corner-cube mirrors mentioned for PARIS but not for ACE or HR125, nor are the dynamically aligned flat mirrors of the DA8 described.

Both the Bruker 125HR and the Bomem DA8 are commercially available instruments with well-documented layouts. As PARIS-IR is a custom-built instrument, with an unusual layout (that matches that of ACE-FTS) we have chosen to provide a little extra information regarding the way that it measures.

(4.) In the data analysis section the phrase “atmospheric profile” is used without first explaining that this means the changing volume mixing ratio with altitude above the ground.

This has been clarified. The text now reads:

Total column densities of O3, HCl, ClONO2, HNO3 and HF covering the region between the ground and 100 km have been determined from the recorded spectra of each of the ground-based spectrometers in a consistent manner. Altitude-dependent atmospheric volume mixing ratio (VMR) profiles were retrieved from the spectra using SFIT2 (Pougatchev et al., 1995), a profile retrieval algorithm based on the optimal estimation technique of Rodgers (1976, 2000) whereby a calculated spectrum is fitted to the measured one by adjustment of the VMR profile.

(5.) Section 3.2: ACE-FTS v2.2 is mentioned twice first with updates in O3, N2O5 and HDO and then just with O3 updates.

This has been fixed so that both mentions include N2O5 and HDO.

(6.) Line 11, 2891 : “measurements were made to maximise the agreement between similar measurements” – it is not clear what is meant by this.

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This referred to the filter-matching already described. We agree that the line mentioned by the reviewer made this more confusing than it needed to be, thus have improved the text by removing the following sentence entirely (as it was already described in the previous paragraph):

This was made possible because the three instruments shared a solar tracker, thus were looking through identical or nearly identical air masses, and the measurements were made to maximize the agreement between similar measurements.

(7.) Page 2892-2893: "PARIS-IR consistently underestimates the total column of this gas" compared to the other instruments- (the true value is not known). Again in next sentence about HF - the bias is compared to the other instruments.

We agree. This has been clarified:

Through both years, PARIS-IR consistently underestimates the total column of this gas relative to the other instruments, with the bias being greater when the HNO3 column is large. In HF, there are high biases in the PARIS-IR data relative to the higher-resolution instruments at the highest solar zenith angles, early in the campaigns, but these disappear by about day 75 when the sun is slightly higher in the sky.

(8.) Line 11, page 2893 – not the cause of the discrepancy but a major cause or a significant contributor to the discrepancy.

Corrected. The text now reads:

If this was indeed a major cause of the discrepancy,

(9.) Page 2894, line 12. Rephrase – the smoothing error masked differences caused by other uncertainties.

This has been rephrased as suggested, and includes suggestions by Reviewer two. The revised text reads:

For the two gases for which the biggest biases were seen in Fig. 1, notably HNO3

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and HF, the agreement between the three spectrometers is substantially improved by accounting for the differences in vertical resolution. Unfortunately the difference in O3 columns increases following smoothing with the PARIS-IR averaging kernels as the smoothing error had masked other sources of difference in the measurements. These measurement differences may include extra sensitivity of PARIS-IR retrievals to saturated features (caused by the extremely long path length at this time of year), detector linearity differences and real differences in the vertical distribution of the gas in parts of the atmosphere along the line-of-sight which are not well captured by the lower-resolution instrument (but are, as shown by the very high DOFS in Table 1, well represented by the high-resolution instruments, resulting in substantial changes to the column after smoothing). Fortunately the difference in total columns between the instruments is still fairly small, and, as for all of the compared gases after smoothing, well within the error estimates of the individual measurements.

(10.) Section 5.1: presumably the HR125 was chosen as the sole comparison instrument because of its lower uncertainties? Clarify this in the text.

The text has been amended to read:

validation of the satellite-based ACE-FTS has been carried out using just the new 125HR, which has high spectral resolution, low uncertainties and more measurements than the DA8.

(11.) Line 15, 2896. The description of the partial columns as “comparable” is confusing here since you go on to determine strict coincidence criteria to determine comparability. Perhaps use the term “similar “ to avoid confusion.

We have replaced “comparable” with “similar” as suggested.

(12.) Not all of the references are in proper alphabetical order, i.e. Wunch et al, Fast et al.

All references are listed in the text in chronological order, which is consistent with other

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papers published in AMT. (If this is incorrect, please could the editor advise?)

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