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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Validation of ACE-FTS satellite data in the upper troposphere/lower stratosphere (UTLS) using non-coincident measurements" *by* M. I. Hegglin et al.

Anonymous Referee #2

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Review of

Validation of ACE-FTS satellite data in the upper troposphere/lower stratosphere (UTLS) using non-coincident measurements

by M. Hegglin, P. Bernath, C. Boone, W. Daffer, P. Hoor, G. Manney, C. Schiller, K. Strong, and K. Walker

General comments:

This paper deals with the validation of ACE-FTS profiles of CO, ozone and H2O in the UTLS region using non-coincident (in time and space) aircraft measurements. Be-

cause the validation measurements were performed in earlier years than the ACE measurements, standard validation techniques are not applicable. The authors therefore use two novel techniques in order to compare these non-coincident data sets: tracer-tracer correlations and the comparison of profiles relative to the (thermal) tropopause. The concept of using these methods for the validation of satellite data is original (I believe, or has it been applied before?), but I think that major revisions are required before this manuscript should be published in ACP. There is two main points of criticism:

1) I think that the methods presented are not really mature enough to really assist the validation of satellite data, because the intrinsic uncertainties have not been quantified. For the method using the vertical profiles relative to the tropopause it is not clear what the 'intrinsic uncertainty' introduced by the method itself is. Assume both measurements (SPURT and ACE) are accurate to within 1%, but because of the intrinsic variability of the atmosphere it will never be possible to demonstrate this with your technique. Until this intrinsic uncertainty due to atmospheric variability has not been established, the validation results provided are not really useful, I believe. I suggest to use, e.g., output from a CTM and compare the trace gas profiles (relative to tropopause height) for a range of different conditions (seasons, latitudes) and determine a quantitative measure for the variability found. Perhaps this has been done already, and has been published elsewhere. If yes, this paper should be cited.

2) The limited vertical resolution of ACE-FTS compared to the SPURT data should not only be considered for ozone, but also for the other trace constituents. As you correctly point out, the across-tropopause gradients of these species are quite large. Therefore, it should be mandatory to include the differences in vertical resolution between the measurements in all comparisons, not just for ozone. I also would like to see the use the correct ACE-FTS averaging kernels, instead of an somewhat arbitrary function. I understand that measurements at different latitudes and times may have different vertical resolutions, due to the varying beta-angle. But if you only compare measurements for the same seasons and a limited latitude band (as you did) then this effect is perhaps

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not too big?

Another general point is the use of 'precision'. Do you really mean precision or rather accuracy? This should be clarified.

Specific comments:

1) Page 13864, line 2: '... fulfilling the coincidence criteria...'

It is not clear what coincidence criteria are meant here.

2) Page 13865, line 20/21: 'Note, however, that the vertical resolution of the ACE-FTS is limited by its field of view'

I don't think this statement is true. If the vertical FOV is 4 km, but the vertical sampling is 1 km or better, than the vertical resolution of the retrieved profiles is not FOV-limited, but will generally better than the FOV. It should be stated what the true vertical resolution of the retrieved profiles is (e.g., what is the half-width of the averaging kernels).

3) Page 13869, equation (1): I don't think it is a good choice to normalize the relative differences between the measurements to the ACE measurements. To my understanding this is not the standard way to do it. the basic approach should be to use the SPURT data as a trustworthy measurement (whose quality is known) in order to quantify the accuracy (and/or precision?) of the new satellite data product. Therefore, the SPURT data should be used in the denominator of equation 1. This will of course directly lead to significantly larger relative differences in ozone in the UT (and perhaps you used the definition in equation 1 to avoid these large differences?) that will be on the order of 60 - 80 %.

4) Page 13869, sections 4.1 and 4.2: Apart from the latitude range nothing is mentioned about the ACE-FTS data used. More information is required here. Did you use data from different years? All available years?

5) Page 13869, section 4.1: What altitude range was used for these tracer-tracer cor-

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relations? I also don't understand why the ACE ozone mixing ratios for low CO (i.e., in the stratosphere) are so much larger than the SPURT values (Fig. 4). This is indicative of ACE ozone values being systematically larger than the SPURT values in the lower stratosphere, isn't it? This, however, contradicts the results shown in Fig. 6. Perhaps I'm missing a point, but I think there is an inconsistency here, which needs clarification. The same phenomenon is apparent in the ozone-H2O correlations.

6) Page 13870, line 5: You state that the agreement between SPURT and ACE-FTS is within 5% in the LS, but in Figure 6 a) three of the four points above the tropopause are right on the 10% line. Therefore '5%' should be replaced by '10%'.

7) Page 13870, lines 16 - 18: 'Recall that the vertical resolution is limited by its FOV. Due to this effect, it is difficult to resolve structures at better resolution than 3 km.'

Again, I am not sure, these statements are correct. Even with a large vertical FOV smaller-scale structures can be resolved if the vertical sampling is high enough. Please state, what the true vertical resolution is.

8) Page 13870, equation (3): Rather than this relation, the averaging kernels of the ACE-FTS data products should be used to smooth the aircraft measurements. I think this is a critical point, particularly considering the large gradients at the tropopause. Besides, if the vertical resolution is not better than 3 km, why did you use a smoothing with a half-width of only 2 km? Smoothing with the averaging kernels should not only be done for the ozone comparisons, but also for the other species.

9) Page 13871, line 23: '5%' should be replaced by '10%' (see comment above).

10) Page 13872, line 6: '... offers unprecedented precision ...'

I'm not sure this statement is justified considering the 40 % differences in the UT. If the relative differences are determined w.r.t. SPURT the differences will increase to ca. 70%, and this is not really unprecedented precision (I think it should be accuracy), is it?

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11) Fig. 1, caption, last line: '... fulfilling the coincidence criteria.'

What criteria do you refer to?

12) Fig. 5, 6, 7, 8: The labels 'a)' and 'b)' are misplaced in these Figures.

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