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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.

M. I. Heggkin et al.

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Reply to reviewer 2

We thank the reviewer for his/her helpful comments and suggestions. We address the reviewer's major concerns as follows. (The comments of the reviewer are given in italics)

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

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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.

We agree that we did not really establish the validity of the methods or quantify the errors that are introduced by considering non-coincident measurements. This is because the methods are already widely used to validate models, on the assumption that the 'instantaneous climatologies' reduce not only day-to-day but also longitudinal and interannual variability. However the reviewer's point is well taken and we have now provided a detailed assessment of the validity of our approach in our new Section 3, using model results as suggested by the reviewer, and including several new figures.

When comparing our results to the results from standard validation techniques, the main benefit of our validation technique becomes obvious. Standard validation techniques generally yield high uncertainties in the measurement errors (around 50%) in the UTLS (i.e. the region between 300 and 100 hPa, or 6 to 16 km). The uncertainties in our error assessment lie rather between 5 and 30% (depending on the level relative to the tropopause height). This is due to the fact that our method allows us to include more measurements, which helps beating down the standard errors in the mean from which we calculate the uncertainties in the measurement errors.

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 a somewhat arbitrary function. I un-

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derstand 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 not too big?

We now show how the smoothing effect influences the error assessment for all tracers. Unfortunately, averaging kernels for ACE-FTS are not available, therefore we use the triangular convolution function to smooth the higher resolution reference data sets. This technique has been used in several other validation papers within this special issue, for example E. Dupuy et al. (accepted for publication in ACPD).

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

We now use the terms precision and accuracy in a consistent way throughout the revised manuscript (see also reply to specific comment 10).

Replies to specific comments:

1) We changed the sentence to '...and even profiles taken no further apart than 500 km exhibit large differences in tropopause height.'

2) We agree and have reworded the text accordingly. Indeed, one of our main results is that the oversampling provides much higher resolution than would be suggested by the FOV.

3) We agree with the reviewer that the choice of calculating the relative differences by

(1-spurt_mean / ace_mean)*100 (see Eq. 1)

is not appropriate. We now use rather relative differences calculated according to

(1- (ace_mean-spurt_mean)/(0.5*(ace_mean+spurt_mean)))*100. (revised Eq. 1)

While the aircraft measurements are known to be accurate to within 2-5% (depend-

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ing on the species) and therefore should be taken as the trustworthy reference, the limited measurements introduce an uncertainty in the determination of the mean atmospheric tracer profile especially in the troposphere and at highest measurement altitudes. When using 0.5*(ace_mean+spurt_mean) in the denominator, this uncertainty is being accounted for.

We further improved the evaluation and use now the standard errors of the mean to indicate the uncertainty in the relative differences rather than the measure suggested in Eq. 2.

4) We added more information about how many years were used and which longitude section for both sections 4.1 and 4.2. We also added information about the used height ranges (see also reply to comment 5).

5) We disagree with the reviewer. There is no inconsistency between the evaluations using tracer-tracer correlation or vertical profiles in tropopause coordinates. The high ozone mixing ratios at very low CO are just representing the fact that the satellite measurements shown cover a larger altitude range than the SPURT measurements. While the SPURT measurements were restricted to below 14 km, we show ACE-FTS that go up to around 16 or 17km. This is now pointed out explicitly in the text. We do, however, not want to remove these data, since they show nicely the specific characteristics of the used tracer-tracer correlations. These are a very tight stratospheric branch due to low variability in CO and H2O in the stratosphere (both tracers have no relevant sources in the middle stratosphere and their concentrations are expected to be in a well-known chemical equilibrium), a compact relation in the troposphere (here O3 is about constant compared to the large changes in the lower stratosphere), and a well-confined transition layer with intermediate CO, H2O and O3 values. The statement that the transition layer is relatively well-confined is a relatively new result and based on aircraft measurements and model simulations of aircraft observations (e.g. Hegglin et al., 2005).

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6) These values changed throughout the manuscript with changing to the more standard way to calculate the relative differences. We now also use the mean errors for the UT and LS respectively.

7) See reply to specific comment 2.

8) No averaging kernels exist for the ACE-FTS retrievals (see reply to general comment). However, we added evaluations of smoothed profiles also for the tropospheric tracers.

9) These numbers have all changed, please see revised manuscript.

10) We really meant precision here. We note that we did not use the terms ‘precision’ and ‘accuracy’ in a self-consistent way throughout the manuscript, and have improved this deficiency in the revised manuscript. From the changes made it should become clear that ‘precision’ really means ‘reproducibility’ of the measurements, which is the degree to which further measurements show the same or at least similar results. ‘Accuracy’ on the other side means how close the measurements are to the ‘true’ value. The tracer-tracer correlation method is a way to gauge the precision of satellite measurements, and to our knowledge there are no satellite evaluations that show anything comparable in the UTLS. The numbers the reviewer quotes refer rather to the accuracy, which we assess using vertical profiles in tropopause coordinates. We do not claim ‘unprecedented accuracy’ of the ACE-FTS.

11) We changed the figure caption to: ‘...the location of two independent profiles that fulfill the spatial coincidence criteria of being taken no further apart than 500 km.’

12) The revised Figures are improved.

References:

Dupuy, E., et al.: Validation of ozone measurements from the Atmospheric Chemistry Experiment (ACE), ACPD, accepted for publication.

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Hegglin M. I., Brunner, D., Peter, T., Staehelin, J., Wirth, V., Hoor, P., Fischer, H.: Determination of eddy diffusivity in the lowermost stratosphere, Geophys. Res. Lett., 32, L13812, doi:10.1029/2005GL022495, 2005.

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