

Interactive comment on “CO measurements from the ACE-FTS satellite instrument: data analysis and validation using ground-based, airborne and spaceborne observations” by C. Clerbaux et al.

Anonymous Referee #3

Received and published: 30 January 2008

The paper presents a very extensive validation of ACE-FTS CO profiles using various correlative CO data sets obtained from ground based, airborne and satellite observations. The validation exercise is carried out in a clear and systematic way and is in most cases very well described. The results show that ACE CO data are of high quality and the uncertainties for different measurement conditions and for different altitudes are quantified. The paper is well written, well organised and of great interest for the scientific community. My only concerns are that in some parts, the compared quantities seem to be not sufficiently well defined (for more details see below) and that in some other cases the shown profiles seem to indicate a higher information content than is actually present. The use of averaging kernel information is treated in a rather hand

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waving manner and should be presented more clearly. After these (and some other) minor points are addressed, the paper should be accepted for publication in ACP.

Detailed comments:

P3, line 21: Also SCIAMACHY is measuring in absorption

Figure 1: In the figure caption it is said that the profile below 6km is constructed from TES observations. What is the vertical sensitivity of TES? I have some doubts that it can really yield information for surface near layers?

Figure 1: In the figure it is indicated that for TIR nadir the sensitivity range is from 0 – 20km. Maybe one should better write 2-20km or something similar? TIR instruments are usually not sensitive to layers close to the surface.

Fig. 3: maybe the size of the Figure should be narrowed in x-direction?

Fig. 4, 5, 6: over which altitude range are the data actually averaged?

Fig. 4, 5: What do the crosses indicate? The location of the instrument or that of the tangent height?

Page 7, line 7: it is said that the CO profile 'should' be smoothed. This vague statement leaves it uncertain to the reader what is actually done in this paper. I suggest to mention in which cases a 'smoothing' was applied and in which cases not.

Table 3: Explain DOFS in the title of the table or below; what does it mean if no DOFS value is given?

Table 3: I think the altitude range of MOPITT and TES does not reach down to 0km?

Page 9, line 5: The statement 'is better than a column' is misleading. Actually, MOPITT can not measure the true total column, because it lacks sensitivity for layer directly above the surface. Often in this altitudes, the highest CO concentrations occur.

Page 11, line 6: Maybe step 1 should be combined with step 5

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Page 11, line 9: Step 3 and 4 should be interchanged?

Fig. 10: For the comparison of partial columns it is not clear to me what was actually done to the two data sets. This should be explained in more detail. I suppose that the ACE profiles are first convoluted with the averaging kernels of the GB instruments, and then they are integrated between two altitudes (lower altitude given by the lowest ACE data point; highest level given by the upper range of the GB data? (are these altitudes constant or changing for each pair of data?) For the GB data it is not clear to me which partial column or total column is taken? Which relative profile is taken for the integration of partial columns?

My feeling here is that in specific cases large errors can occur, e.g. if high CO concentrations close to the ground appear. In these cases, the GB instrument will 'see' these high concentrations, but ACE won't. Consequently, the GB data should be much larger. Could this effect explain part of the observed scatter?

What does it mean that the treatment with averaging kernels is performed 'when available'? What is done if not available?

Fig. 11: what causes the gap between 21.5 and 23.5km?

Fig. 12, 13: Was there no convolution applied? And why?

Page 13, line 20: Better replace 'good' with something else, e.g. 'small'

Page 13, line 17: I suggest to compare only partial columns, like for the GB instruments. The profiles shown in Fig. 14 indicate a vertical resolution which is much better than in reality. How is the relative profile shape of the TES measurements determined?

Page 15, line 4: Was a convolution with averaging kernels performed for MLS?

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 15277, 2007.

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