

***Interactive comment on* “Global distribution of upper tropospheric formic acid from the ACE-FTS” by G. González Abad et al.**

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General Comments

Inclusion of results from in situ measurements (rather than mentioned in text).

Values obtained by Herndon et al., Rinsland et al. (2004, 2006, 2007) and Coheur et al. (2007) will be included in the manuscript.

Comparison of HCOOH results from Rinsland et al. (using same instrument, but different spectroscopy).

We have made a more detailed comparison with previous work by Rinsland et al. and our recent work.

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Use of model data for the seasonal/latitudinal variability.

We are currently working with Geos-Chem to include HCOOH as a tracer but the work is not finished yet. Our plans are to obtain that functionality and then use it to compare the model output with the satellite data.

Use of other ACE-FTS products, eg CO, for source attribution.

We include a new figure, with 8 plots. Here occultations sr6539 and sr6582 have been taken as representative of fire plumes and HCOOH, CO, HCN, SF6 concentration profiles and are plotted.

Specific Comments

The paper mentions that they use the Q-branch of the v6 mode, which I imagine is a fairly narrow feature, within a microwindow of 10cm⁻¹ width. I'd like to see a plot of actual and simulated data for this region to get some visual impression of the signal-noise and interferences from other gases.

We include a plot with the synthetic spectra, the observed spectra and the residuals for that particular microwindow.

The “Validation” against the MkIV instrument retrievals is unconvincing. The data from the two instruments may lie within 1SD, but the SD is so large that it is not clear that either instrument is telling us anything useful. This is just qualitative, rather than quantitative, agreement. Given the difficulty of the comparison, including lack of spatio-temporal collocation of the two instruments, it is probably the best that can be done, but I would just describe this as a “comparison” rather than a “validation”.

The suggestion will be included in the revised manuscript.

Section 4 also mentions that the latitudinal profiles “are consistent” with published aircraft profiles, but I would have liked to see those profiles also overplotted so I can judge this for myself. Assuming that the aircraft profiles are more accurate than the

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MkIV instrument, a key question would be: is the level of consistency such that the ACE-FTS retrievals agree better when taken at the same time-location as ACE-FTS or is the level of disagreement so large that it swamps the seasonal/latitudinal variation of ACE-FTS?

These profiles were mentioned in the paper by Ito et al. (2005) who were using data from Emmons et al. (2000) who refer to the GTE (Global Tropospheric Experiment) data. Therefore we have processed data from the GTE TRACE-A field campaign, one of the few experiments with data for HCOOH, and produced a new figure where we have plotted a median profile obtained from the TRACE-A data together with an appropriate ACE profile from the same latitude.

Given the lack of suitable collocated measurements, I would therefore have expected to see some model results which might at least reproduce the observed seasonal/latitudinal variability.

We are currently working with GEOS-Chem to include HCOOH as a tracer but the work is not finished yet. Our plans are to obtain that functionality and then use it to compare the model output with the satellite data.

The calculated HCOOH/CO ratio is described as in "good agreement" with the values of Rinsland et al. Their result is quoted as 0.0114 ± 0.0076 compared to your value of 0.0051 ± 0.0015 . First of all, their error bar is so large that "consistent with" is probably a better description. Secondly, given that you both use the same data, why is their error bar so much larger than yours? And thirdly, how do your actual retrieved HCOOH profiles compare with theirs (given the adjustment for different spectroscopy)?

We agree with the suggestion that "consistent with" is probably more an accurate term and have changed the text accordingly. The difference in error bars is due mainly to two factors. First, the new spectroscopic data produces smaller residuals in the retrieval, and secondly, we have filtered the available data rejecting profiles with very large retrieval errors.

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This also leads to a further question of why is more use not made of ACE-FTS data to understand your HCOOH climatology? For example, correlation with CO seems a good method of distinguishing HCOOH associated with biomass burning from that associated with other sources. Would anticorrelations with H₂O show anything useful (you mention that HCOOH is highly soluble? CH₄ is related to vegetation growth?

A new figure (Figure 10) has been included with tracer-tracer correlations between HCOOH and CO, HCN, C₂H₂, C₂H₆ and SF₆ concentrations. High correlations of HCOOH with CO, HCN, C₂H₂ and C₂H₆ suggest that HCOOH production is particularly linked with biomass burning. No correlation with SF₆ indicates that HCOOH has little industrial origin.

Fig 3 presents the latitudinal profiles as a series of small, separate plots. However, since the purpose is to compare these profiles, this would be easier if they were all plotted on a single, large graph, eg. colour-coded for different latitudes.

We tried to plot all profiles in a single large graph (colour-coded) but there are too many profiles and the plot was very confusing. Although we agree with the referee, we have left the figure unchanged.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 12465, 2009.

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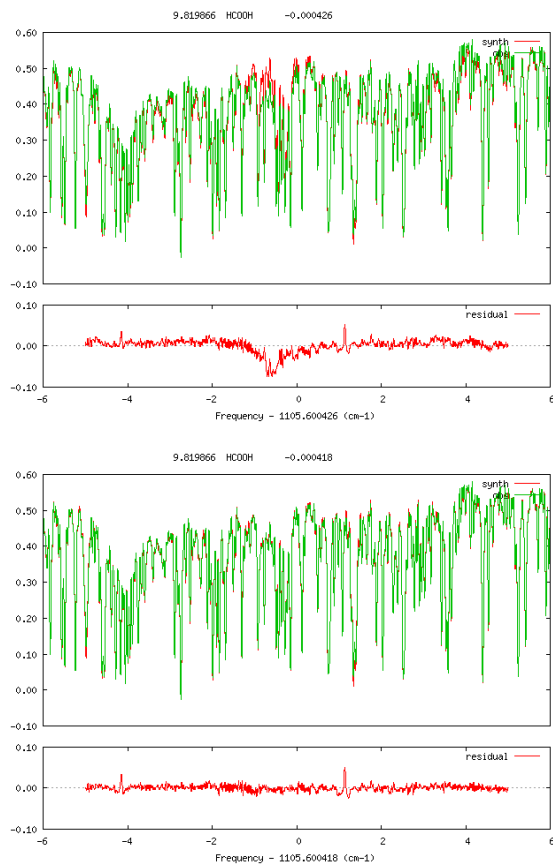
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Fig. 1.

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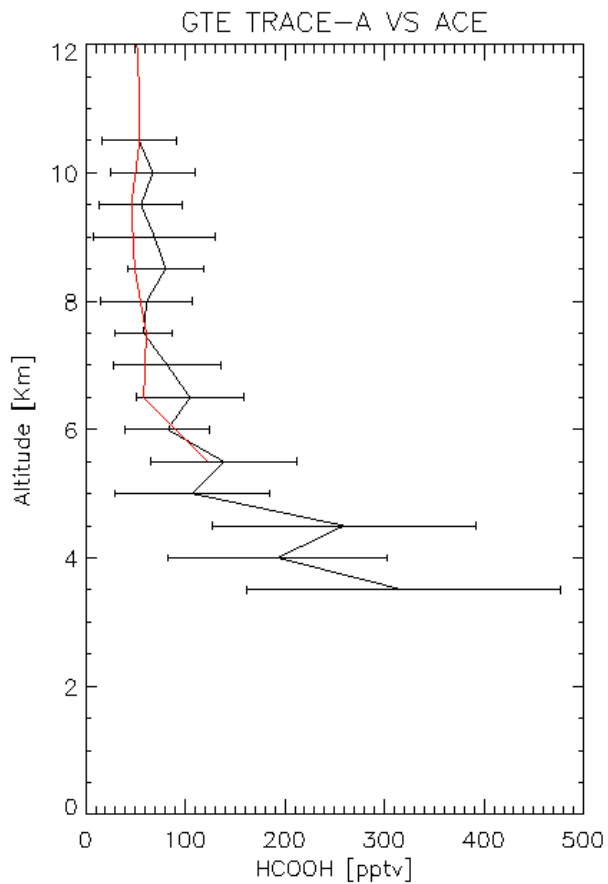


Fig. 2.

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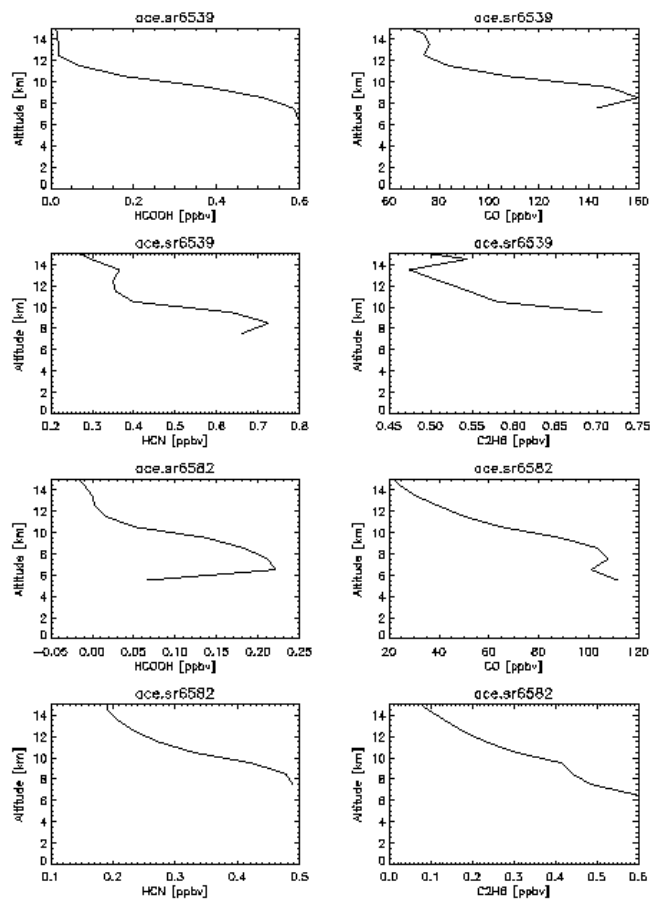
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Fig. 3.

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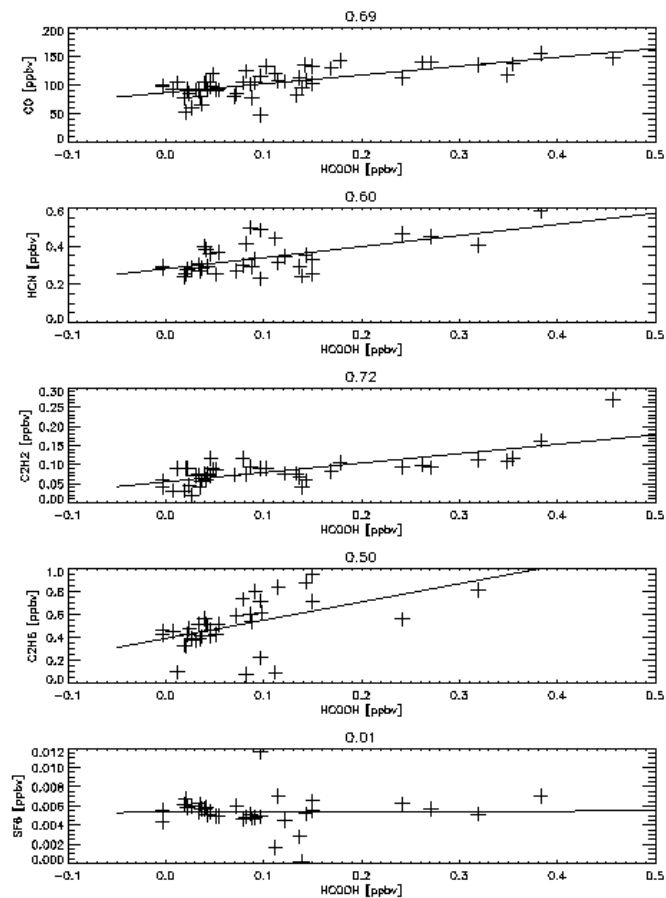
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Fig. 4.

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