Atmos. Chem. Phys. Discuss., 7, S2956–S2957, 2007
www.atmos-chem-phys-discuss.net/7/S2956/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The influence of biogenic emissions on upper-tropospheric methanol as revealed from space" *by* G. Dufour et al.

Anonymous Referee #1

Received and published: 9 July 2007

*Summary

The authors report some new and interesting data from the ACE FTS satellite instrument. While I think the data are extremely exciting from the standpoint of improving fundamental understanding of source and sinks of oxygenated organic compounds I believe the model analysis could be improved. The subject matter is clearly suitable for ACP publication after the following comments are addressed.

*Major comments

1) The Jacob et al 2005 paper provides a CH_3OH budget based on measurements but their paper clearly highlights the wide range of source/sink magnitudes that easily fit the measurements. The authors here have quite rightly taken best estimates from Jacob

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but perhaps they might also consider running additional calculations with alternative source magnitudes (not just alter the biogenic source)? The model CH_3OH lifetime of 9 days is on the high side of the range provided by Jacob.

2) How is the model sampled to be consistent with the ACE instrument? At least the authors should acknowledge that they have sampled the model at the time and location of the measurements.

3) Figs 2 and 3 are useful but I am left wondering the authors did not report 2-D maps (height vs lat) of relative and absolute fitting uncertainty, and/or perhaps the $1/\sqrt{n}$ error, where n is the number of observations used to calculate a mean error.

4) I remain unconvinced that those elevated CH_3OH signals described in this paper are from biogenic emissions. Comments such as "The measured distribution at 8.5km confirms a higher VMR and a stronger seasonal variation in methanol over the continents...., in agreement with the biogenic origin of the species." should be rewritten, unless the authors are lumping biomass burning with natural sources. Better evidence of biogenic activity is needed for this reader and the authors are sitting on the data to do just that, e.g., provide plots of CO/HCN vs CH_3OH? Alternatively, the model could be run with biomass burning switched off so that this reader could be convinced that elevated concentrations in the UT over and downwind of regions with active burning (e.g., Asia during MAM, North America JJA, Africa, SON) can be attributed to biogenic emissions. On a related note, instead of the model underestimating biogenic emissions have the authors considered that the model might be underestimating the vertical transport of CH_3OH into the UT?

*Minor comments

Page 9185: "Medium range lifetime"? Please remove.

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Interactive Comment