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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Hydrogen cyanide in the upper troposphere: GEM-AQ simulation and comparison with ACE-FTS observations" *by* A. Lupu et al.

**A. Richter (Editor)**

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Unfortunately, one of the reviewers of this paper after providing a positive quick report has not submitted a full review in spite of numerous reminders. I'd like to apologize for the delay resulting from this and for the less thorough discussion the paper received as a consequence. Although I'm not an expert on HCN, its retrieval and modelling, I will add a few comments of my own below.

The paper "Hydrogen cyanide in the upper troposphere: GEM-AQ simulation and comparison with ACE-FTS observations" by A. Lupu et al. reports on a multiyear model run of GEM-AQ for HCN and compares the results for the upper troposphere with data

from a scientific retrieval of HCN on ACE-FTS data and FTS measurements from two stations. The results show good agreement for large regions and many seasons assuming only biomass burning as HCN source and a homogeneous oceanic sink. A clear underestimation of HCN is reported for the NH in summer and autumn which is attributed to uncertainties in the emissions or the emission height.

The paper is well written and reports the first quasi-global comparison of modelled and measured HCN in the upper troposphere. The topic is appropriate for ACP and the results interesting for atmospheric chemistry in general and interpretation of HCN data in particular. I therefore will accept the paper for publication in ACP after my comments below, as well as those from the anonymous reviewer have been addressed appropriately.

1) The uncertainties of the ACE measurements are only addressed by stating their random errors. Are there no systematic errors, e.g. from spectroscopy? The same question applies for the FTS data.

2) I was surprised that uncertainties in model transport are not considered at all - couldn't that also explain part of the observed differences?

3) The correlations between measurements and model data in individual latitude bands are not very large - more than half of them are below 0.5. Do you have any explanation for that? Could uncertainties in model transport be part of the reason?

4) Have you applied the averaging kernels of the measurements to the model data before comparison or is that not necessary, and if not, why?

5) Abstract: I'd suggest adding the comparison to ground-based FTS data

6) Abstract: "Our model results show..." I'd rather say that the comparison to the satellite data shows that the model performs well

7) Fig. 5: It would be good if you could reduce the y-scale to 0-600 to make the figures more clear



**ACPD**

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Interactive Comment



Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2165, 2009.



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Interactive Comment