

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

Anonymous Referee #2

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The paper presents a global analysis of atmospheric hydrogen cyanide, relying on model simulations (GEM-AQ) and satellite observations in the upper troposphere (ACE-FTS). An additional model assessment is made by comparisons with ground based FTIR measurements. Sources, sinks as well as spatial and temporal (seasonal, inter-annual) variations of HCN are described. The paper is very clear, well written and structured. It provides extensive and relevant references to previous work.

The results presented are extremely useful, giving for the first time a global picture of HCN distribution in the troposphere and its relation to biomass burning in particular. They are for instance critically analyzed in terms of fire injection heights, seasonality

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etc. The value of the ACE-FTS satellite observations to capture regional and seasonal variations of HCN are nicely demonstrated for this tracer, which has a medium atmospheric lifetime. In addition to its geophysical relevance the paper also assesses well the capabilities of GEM-AQ for tropospheric applications. For the above reasons I strongly recommend publication of this paper in Atmospheric Chemistry and Physics.

Minor corrections:

- Page 2170, line 13: "The fires in Africa exhibit..."; would avoid possible misunderstanding.
- Page 2171, line 13: All sources quantities are provided in Tg N yr⁻¹. What the 41 % variability refer to is unclear in that respect and should be specified.
- Page 2173, line 26: Although it is true that SCISAT-1 orbit provides coverage from tropical to higher latitudes, the measurement distribution is very uneven. The authors should say that the orbit was optimized for high latitudes, also providing some coverage of the tropical regions.
- Page 2174, lines 10 through 13: Labeling heavier molecular isotopologues with numbers may not sound familiar for many readers. The authors should indicate which isotopologues these number refer to.
- Page 2174, line 26: The authors should add the extent of signal to noise reduction between 3300 cm⁻¹ and 1400 cm⁻¹.
- Page 2175, lines 24-25. The author state here that the time series where separated between sunrises and sunsets for clarity. However, I don't see any strong argument for the separation and would think the discussion would even be simplified if there was no distinction between the two datasets. In fact, the seasonal behaviors and in particular the peak concentrations would likely appear better. The authors could consider revising the Figure and the associated text on page 2176.
- Page 2176, line 3: It is said that "the high UT values in October are the result of peak

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fire activity in South America, South Africa, insular SE Asia and Northern Australia, as shown in Figure 1". This is difficult to apprehend without discussing simultaneously Figure 3, which shows the actual measured and modeled zonal distributions. It would probably be more sensible to say the "the results are consistent with peak fire activity"; but that even is not fully clear. Indeed from Figure 1, wouldn't one expect larger concentrations in these regions (South America and South Africa) earlier in the summer? In light of this the authors could consider revising somewhat the text around Figure 2 and maybe the Figure itself (see above).

- Page 2177, line 10: How is the subtropical megaplume characterized and where exactly is it to be seen?

- Page 2179, line 6: For the ground-based measurements the HITRAN 1996 database has been used. Does it differ from more recent versions for HCN and does that have any impact on the retrieval?

- References: Some Journal abbreviations are probably to be corrected (Journal of Quantitative Spectroscopy and Radiative Transfer)

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

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