Interactive comments on "Seasonal and interannual variations of HCN amounts in the upper troposphere and lower stratosphere observed by MIPAS"

This manuscript provides a comprehensive and in depth examination of the 10 years global HCN dataset in the upper troposphere and lower stratosphere, measured by the MIPAS instrument of ENVISAT satellite. Based on the nearly 10 years data, the authors looked into the seasonal as well as inter-annual variation of HCN in the UTLS region and addressed several crucial patterns, including tape recorder, strong vertical transport of Asia monsoon system. The main points are clear and the science questions are interesting. Then I recommended accepting the manuscript for publication with minor revision, focusing on making the manuscript more concise. Below are my minor comments:

P8998, line 5: almost unambiguous : changing into some simple words, such as "important"
P8998, line 15: peaks in July
P9002, Line 23: define IMK
P9003, Line15: define the height-constant a-priori file. Maybe a figure of a priori file
P9004, Line1: biomass burning plume

For the retrieved HCN profile, is there any evaluation based on surface station or aircraft observations? The authors may want to add one figure of comparing retrieved data with other observations.
P9004, Line 8: boreal spring, summer…

The authors attribute the seasonal and interannual variations of HCN to the biomass burning. It would be good to add a map of GFED emissions or a time series plot of GFED emissions over the burning regions.

P9005, Line23: The plume: please change to the enhanced HCN plume
P9006, Line5: which has been
P9008, Line 10: please give more details on HCN climatology of ACE-FTS such as the averaged time period, the spatial-temporal coverage of ACE-FTS
P9009. Lin26: from 300pptv to more than 500 pptv
P9010, line 3-5: additional sources of HCN at southern mid to high latitudes leading to an apparent meridional transport pattern only can be excluded: I had a hard time to understand this, please rewrite this sentence.

P9011, Line 12: Randel et al used the average result of MLS in latitude between 16km-23km. In your figure 5 at 18km, it also shows some strong signal of AMA pattern. It would be more precise to specify the difference between MIPAS and MLS in Randel's paper and give a short discussion on the possible reasons.

P9012, Line 9: what causes the decrease trend in the northern hemisphere? If the reason is biomass burning, the authors need plot the GFED3 emissions time series over this region.

P9013, Line 2: The emission over South America is strongest in Aug or Sep, not in Nov, although the HCN and CO signal is strongest in Oct or Nov in the UTLS over South America due to the combined effect of emission and southward shift of convections region. Liu et al 2010 has a detailed discussion on the formation of CO maximum in Oct or Nov at 215 hPa (Figure 8,9, 10)

P9016, Line 16: delete in this presentation
P9017, Line 26: How different? Please specify the values.
P9019, Line18-19: I agree with the authors that the interannual variation of emissions from different source region contributes to the biennial cycle. However, the authors cannot eliminate the meteorological effects without looking into the interannual variation of metfields during these years. It may or may not have effect on the IAV of HCN. Please modify this conclusion.
P9021, Line 24: is about 1.6 months at 10km from ** to **

Fig 5 and Fig 9, the missing data should be left as blank.
Fig 8, start the figure from Jan 2005

Liu, J., Logan, J. A., Jones, D. B. A., Livesey, N. J., Megretskaia, I., Carouge, C., and Nedelec, P.: Analysis of CO in the tropical troposphere using Aura satellite data and the GEOS-Chem model: insights into transport characteristics of the GEOS meteorological products, Atmos. Chem. Phys., 10, 12207–12232, doi:10.5194/acp-10-12207-2010, 2010