Response to referee # 1:

We thank the reviewer for a useful examination of our paper. As the reviewer suggested, we have significantly increased the length of data used for this study, including model simulations. Also, we have changed to use the less biased MLS weekly zonal mean products (see below), to validate the model HCN. Below are our responses to specific comments (shown in italics).

1) The column HCN comparisons for Jungfraujoch in Fig. 3 are impressive. However, the Kitt Peak comparisons do not convince me of any model skill at simulating variability (beyond having an appropriate average value). What is the correlation coefficient for the model vs. observations in Fig. 2b? The Mauna Loa comparisons (Fig. 2c) are relatively useless in terms of model validation, because of the very limited amount of observations.

The Kitt Peak and Mauna Loa ground-based FTIR data are sparse but accurate so there is still value in using them to evaluate our model HCN simulation. These data represent the only long-term measurements of HCN available. Despite the sparse coverage at Kitt Peak and Mauna Loa, we find that correlations at these sites (mentioned in paper) are statistically significant.

2) Fig. 4 shows little to convince the reader that the model simulation is realistic. The MLS data in Fig. 4c have very large biases over most of the globe (and an unrealistic vertical structure), and comparisons with the model are very poor except for a small altitude range over the tropics (this simply demonstrates that the MLS HCN data have substantial bias problems). The comparisons with ACE data (Figs. 4 a-b) shows overall poor agreement in terms of detailed latitude or altitude gradients (vertical gradients in the tropics are very different, and there is little hemispheric asymmetry in the upper troposphere, despite such a reference in the text). Overall these comparisons are questionable for constraining the quality of the model simulation, and I think the authors should be much more critical in their assessments.

We have changed to use the MLS offline weekly zonal mean data with smaller biases out of tropical region (Pumphrey et al., 2008) to validate the model HCN mixing ratio. We have also included a more comprehensive discussion about data quality in the paper. With a modulated color interval, Figure 4 shows our model is able to simulate only the broad features of HCN mixing ratios, including the similar latitude or altitude gradients in UTLS as observed by MLS and ACE-FTS. We found both ACE-FTS HCN and model HCN show a hemispheric asymmetry with a higher mixing ratio in Northern Hemisphere although this asymmetry is stronger in the model. Similar as the comparisons between the ground-based observations and the model (see our response to the last question), the comparisons between the satellite observations and the model are still very useful for the evaluation of the model with some restrictions.

3) I think there are important limitations in the model simulation in the upper troposphere and stratosphere, and I am unconvinced by Figs. 5-6 that the model accurately simulates a 2-year cycle in the stratosphere. The model simulations in Fig. 6 suggest a strong increasing HCN trend in the stratosphere over the 6-year model simulation, and because the photochemical lifetime of HCN in the stratosphere is very long (> 5 years) I think this is evidence that the model is not equilibrated. Furthermore, the authors focus on comparing 2 years of observations from ACE and MLS with the model (Fig. 5), and propose to explain a 2-year cycle with only 2 years of data. Why not extend the comparisons to the > 4 years of data now available from ACE and MLS? The comparisons in Fig. 5 are also worrisome because of the focus on anomalies, rather than actual values; I understand that anomaly comparisons are most appropriate for the MLS data (which have large biases), but not so for the ACE measurements (with small biases, i.e. Fig. 4). Note that the ACE data also extend to altitudes lower than 100 hPa. Overall my feeling is that the model – observed comparisons are done in such a way as to minimize differences, rather than critically evaluate the model.

We have also run the model with a much longer term of spin-up (> 10 years) to equilibrate the model. We found the similar results as in Figure 6. Our model shows no increasing trend of HCN concentration in tropical UTLS over 2001 – 2008 in Figure 7. Using recently updated GFED v2 emission data and GEOS-5 meteorology fields, we have run the model to 2008/12 and have extended the comparison to > 4 years with the data available from ACE and MLS in Figure 5. Using the > 4 year data, we found two ~2-year cycle in UTLS for both satellite observation and model. Using the newer release ACE data, we have been able to extend the altitude of ACE data in Figure 5 down to 316hPa in this case. Along with the anomalies, the modified Figure 4 (see our response to last question) also improved the comparison of the actual values between satellite observations and model HCN.

4) I have to say that I like the sensitivity experiments in Fig. 7, where the simulations are run with constant emissions and constant meteorology to determine causes of the model interannual variability. I think this is convincing that emissions variations are most important. But the fact that the stratospheric data comparisons are only shown for 2 years makes the connection of the model simulations to the satellite observations less convincing.

Our previous calculations were limited because we only used > 2 years data. Now we have significantly increased the length of data to > 4 years. Using the > 4 years data, we found similar results. Using recently updated GFED v2 biomass burning emission data and GEOS-5 meteorology fields input, we have also significantly increased the length of our model simulations to > 4 years, and the length of the model sensitivity experiments to 8 years.