# “Seasonal and interannual variations of HCN amounts in the upper troposphere and lower stratosphere observed by MIPAS” *by* N. Glatthor et al.

**Author’s Response**

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Dear Prof. Haynes,

today we have uploaded a new version of the paper acpd-2014-124 on MIPAS HCN observations. As outlined in our referee-replies (see below), we performed most of the suggested changes, from which we think the update benefits considerably.

As suggested by referee 1 we start the discussion with a presentation of timeseries of fire carbon emissions of the Global Fire Emission database (GFED) of the regions most relevant for the MIPAS HCN observations, which makes reference to GFED emissions much more straightforward than in the ACPD-version of the paper. Comparison with these GFED-timeseries corroborates the observed upper tropospheric MIPAS HCN distributions and their interannual variations. In response to the comment of referee 3 we also compare the temporal relation of maximum fire emissions, maximum HCN observed in the upper troposphere and of deep convection by inclusion of plots of the outgoing longwave radiation provided by the NOAA/ESRL Physical Science Division. As already shown by Liu et al. (2010, 2013) we find time lags between maximum fire emissions and maximum upper tropospheric HCN and suggest meridional movement of deep convection as potential reason for the delay. Further, inclusion of two plots of the upper tropospheric windfield as proposed by referee 1, also provided by NOAA/ESRL, corroborates our conclusions on plume expansion.

In addition to the changes suggested by the referees, we decided to present an updated, improved version of the MIPAS HCN dataset in the new version of the manuscript. This dataset was created with a new microwindow selection, designed to gain as much information as possible on tropospheric HCN. The reason for doing so was comparison with independent data during the last weeks, in particular INTEX-B aircraft data, which demonstrated a high bias of our older data set at the lowermost altitudes, and initiated the re-assessment of our retrieval set-up. As a consequence, we found the reason for the high bias and could remove this artefact.

First of all we want to note that changing the HCN data version has only little influence on the discussion and conclusions of the ACPD version, except for the comparison to ACE-FTS. The updated dataset is in better agreement with ACE-FTS distributions. For better comparison we now include additional figures containing latitudinal cross sections of ACE-FTS HCN, which is obviously favoured by referee 3 anyway. Moreover, this update makes the comparison with the ACE-FTS much more straightforward than in the ACPD-paper, in

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which we refer to other publications describing the ACE climatology. The presentation of ACE-FTS data is carried out in agreement with the ACE-team. Because of helpful comments we included K.A. Walker from the ACE-team in the authorlist. Further we present a figure, in which we compare MIPAS HCN with INTEX-B aircraft data as suggested by referee 2.

In summary, we believe that the updated HCN data version in a quantitative respect is a real improvement in the UTLS-region compared to the previous version in ACPD, and - since it does not affect the main conclusions of the paper - we hope you could agree with us that it would not be sensible to keep the previous dataset but to present the updated version.

Best regards,
Norbert Glatthor

## Reply to referee 1

We thank referee 1 for her/his helpful comments.

**Reply to general comments:**

1) The referee states that attribution of regional and seasonal HCN maxima to biomass burning "without providing specific reference (or figure) of biomass burning emissions" is not convincing, and that a "climatology of biomass burning ... should be provided." For this reason we will add time series of fire carbon emisssions of the Global Fire Emissions Database (van der Werf et al., 2006, 2010) from the regions most relevant for the subsequent discussion at the beginning of Sect. 3. By referring to these plots, assignment of the upper tropospheric HCN maxima presented in the subsequent subsections to fire emissions will be much clearer.

2) The referee states that the vertical resolution of the MIPAS HCN data is coarse and that potential caveats in interpreting features in the data should be mentioned. First of all we would like to note that in our opinion the vertical resolution of MIPAS HCN (4–5 km in the altitude range 8–16 km) is comparable or not much worse than the vertical resolution of HCN from other spaceborne experiments, and that determination of location and height of HCN plumes is not extraordinarily degraded. Further, comparison of plume heights derived from MIPAS HCN should be possible anyway, since the height-dependence of the width of

the averaging kernels is similar at different geographical positions.

3) Unfortunately we were not aware of the Park et al. (2013) paper when discussing the MIPAS HCN dataset. We will include it in the comparison with the HCN climatology of ACE-FTS. Further, we will compare its long time series of equatorial HCN (Fig. 14) with the MIPAS HCN tape-recorder signal. We will also include the paper of Tereszchuk et al. (2013) as reference for the biomass burning signature of HCN.

**Reply to specific comments:**

1. P9000, L7:

We mean biomass burning plume in general, of which HCN is one constituent. We will make this clearer in changing the preceding sentence into ”HCN and other pollutants ...”

2. P9000, L16:

We will ad the reference to Park et al. (2013) here and also on P9008, L11.

3. P9000, L22:

We will refer to the HCN tape recorder plots in Park et al. (2013) at the end of the paragraph on P9001, L11.

4. P9003, L15:

Use of a height-constant apriori profile does not mean we assume HCN mixing ratios to be constant regardless of height. This kind of profile only serves to avoid any influence of the apriori on the shape of the retrieved profile. Maybe here our retrieval approach is misunderstood. We do not apply optimal estimation but the first order Tikhonov operator, i.e. our retrieval is not constrained towards the apriori profile itself, but to its vertical slope. We will make this clearer in the revised paper.

5. P9003, L24-26:

A joint-fit of a continuum profile and of a calibration offset in each microwindow is a relatively common approach in evaluation of limb scans of infrared emission measurements. We will make the sentence better understandable.

6. P9003, L29:

We will give more information on how the errors are calculated. Profiles with biomass burning signatures (enhanced HCN) generally have smaller relative errors than background profiles. We will account for the referee’s question by giving errors for enhanced and background HCN.

7. P9004, L23:

We will add references (Duncan et al., 2003; van der Werf et al., 2010) for springtime northern hemispheric biomass burning.

8. Fig. 1:

Yes, we also have noticed that the minima in the tropical upper troposphere in Fig. 1 are not as low as in the ACE-FTS climatology. We mention the difference in the comparison with ACE-FTS in Sect. 3.2 and discuss the consequences in Sect. 3.5.

9. P9005, L5:

We will change the sentence ”The lowest stratospheric HCN amounts ... occur” into ”The low stratospheric HCN amounts ... at”. On P9005, L17-18, we already mention the ”fully developed SH vortex” during June to August. We will make the situation clearer by changing this sentence, which according to Referee 3 is not understandable, into ”Due to subsidence of mesospheric air masses in the Antarctic vortex, this season exhibits the lowest stratospheric HCN amounts, observed at high southern latitudes.”

10. P9005, L11-19:

We will cite Figs. 1 and 2 of Randel et al. (2010) here.

11. P9006, L13:

We will add an additional Figure with wind vectors from the NCEP/NCAR reanalysis, which confirms eastward transport of parts of the plume.

12. P9006, L20:

We mean that compared to boreal winter and spring HCN released by biomass burning has increased in boreal summer, which counteracts ocean uptake and thus leads to less distinct HCN minima above the tropical oceans. We will make this clearer by rewording the sentence.

13. P9006, L28:

”This process” means biomass burning in Indonesia and tropical Australia. We will reword the sentence as follows: ”... because during most of the years fire emissions from this region were rather low only."

14. P9007, L14, 16:

We will exchange "curves" by "time series".

15. P9007, L16:

As mentioned above, we will add time series of GFED fire carbon emissions, which will be compared with the observed seasonal cycles at 10 and 14 km.

16. P9008, Section 3.2:

We are aware that the ACE-FTS dataset is restricted by sampling issues and will address this problem by two additional sentences. Among others, the sampling issues can partly explain the differences in horizontal gradients at high southern latitudes. Due to the bias of ACE-FTS observations to certain months, the comparison with the ACE-FTS datasets in Lupu et al. (2009), Randel et al. (2010) and Park et al. (2013) is qualitative only and not a full validation.

17. P9009, L14:

We will change "time series of monthly zonal averages" into "time versus latitude cross sections of monthly zonal averages".

18. P9009, L27:

The southern hemispheric plumes were particularly weak in 2003 and 2008, because in these years fire emissions from Southern Hemispheric South America and from Equatorial Asia were very low. This will be visible in the time series of GFED fire carbon emissions added in the revised paper.

19. P9010, L3-4:

We mean that a temporally delayed contribution from sources at higher southern latitudes can be excluded and thus the observed feature most likely is a meridional transport pattern. We will change the wording into "Meridional transport of HCN is the only obvious process to explain this observation, because there are no further sources of HCN at high southern latitudes.” to make the sentence clearer.

20. P9010, L20:

We will replace the outdated website for the ENSO index by the suggested update.

21. P9011, L13:

The difference between MIPAS and ACE-FTS/MLS in the equatorial UTLS region is not related to different vertical averaging. For closer comparison to ACE-FTS and MLS data, a time versus latitude cross section of MIPAS HCN averaged over the altitude region 16–23 km was also produced (not shown). This representation of MIPAS data leads to essentially the same conclusions.

22. P9011, Fig. 5:

For better readability of horizontal axes, we will update Fig. 5 with one graph below the other and x-axes extending over the whole page. Further, we will mark the Southern Hemispheric maxima of the different latitude bands by solid symbols, which will make the time lags clearer.

Second, the referee does not think the transport is purely horizontal. Anyway, inspection of Figs. 3 and 4 shows that the postulated Southern Hemispheric transport pattern is widely restricted to the altitude of 10 km and thus is tropospheric or near the tropopause. We just want to show that tropospheric HCN maxima from low southern latitudes after about 1.6 months appear at Antarctic latitudes. Further, she/he wonders if the vertical resolution of MIPAS HCN being 4-5 km is appropriate to use in this diagnostics. We think, especially the widely disappearance of the pattern at 14 km shows that the vertical resolution is sufficient.

23. P9012, paragraph (L5-):

Since referees 1 and 3 critisise the trend calculation and referee 3 doubts the value of the derived trends, if their statistical significance is not known, we will omit presentation of trends.

24. P9016, section 3.5:

We will include Fig. 14 of Park et al. (2013), which will improve the discussion of the HCN tape recorder.

25. P9017, last paragraph (L22-):

Here we do not understand the referee's argument. Generally the main entry path of trace gases into the stratosphere is thought to be through the tropical tropopause. Randel et al. (2010) argued that ”broad tropical upwelling cannot be the main source for the stratosphere”, because the tropical upper tropospheric HCN values measured by ACE-FTS are too low. We concluded that according to MIPAS HCN the usual pathway via the tropical tropopause might well be sufficient, since the tropical upper tropospheric HCN values measured by MIPAS are considerably higher than the stratospheric HCN values. Provided the MIPAS HCN data are correct, we do not see a general weakness in this argumentation.

26. P9018, Fig. 10:

We will change ”time-height series” into ”time-height cross section”.

27. P9019, L20:

We will include discussion of Fig. 14 of Park et al. (2013) here.

28.:

We will perform the suggested improvements of Figs. 4, 5, 9 and 10.

29.:

We will leave the measurement gap in 2004 in Figs. 5 and 9 blank.

## Reply to referee 2

We thank referee 2 for her/his helpful comments.

**Reply to comments:**

P8998, line 5:

The intended statement is, that HCN is nearly exclusively released by biomass burning. Most of the other tracers of biomass burning have also additional sources. Thus we think that the proposed change of ”unambiguous” into ”important” is not quite appropriate. We did not find more simple words to express the intended statement and decided to stay with

”unambiguous”, which is also favoured by referee 3.

P8998, line 15:

We will change the text as suggested.

P9002, Line 23:

We will perform the explanation of IMK here instead at P9003, lines 1–2.

P9003, Line15:

The height-constant HCN a-priori profile just has the same volume mixing ratio at each altitude. If applied in first order Tikhonov regularisation, this kind of profile has no influence on the shape of the retrieved HCN profile. We think it is not neccessary to provide a plot of this simple profile.

P9004, Line1:

We will correct the phrase as suggested.

The referee likes to have an evaluation based on surface stations or aircraft observations. Since we already perform a qualitative comparison with ACE-FTS data, we think another comparison with surface stations or aircraft observations is beyond the scope of the paper.

P9004, Line 8:

We will add the adjective ”boreal” as suggested.

Like referee 1, referee 2 would also like to have a map of GFED emissions or a time series plot of GFED emissions over the burning regions. Thus, we will add a time series plot of GFED fire carbon emissions from the most relevant regions. This update will indeed make the interpretation of upper tropospheric HCN observed by MIPAS more straightforward.

P9005, Line23:

We will change ”plume” into ”plume of enhanced HCN” as suggested.

P9006, Line5:

We think the phrase ”which have been” should remain in plural form, because it is related to ”remnants”.

P9008, Line 10:

We will extend the description of the HCN climatology of ACE-FTS as requested by giving the time periods discussed in the different publications we are referring to and by adding some sentences to the instrument’s spatio-temporal coverage.

P9009. Line 26:

We will change the sentence as suggested.

P9010, line 3-5:

To make this sentence better understandable, we will rewrite it as follows: ”Meridional transport of HCN is the only obvious process to explain this observation, because there are no further sources of HCN at high southern latitudes.”

P9011, Line 12:

As already mentioned in our reply to referee 1, integrating MIPAS HCN values between 16 and 23 km, which makes our dataset better comparable to Randel et al. (2010, Fig 3), does not much change our findings. In any case we will add a sentence that the referenced Figure (Fig. 3 in Randel et al., 2010) shows averages over 16 to 23 km.

P9012, Line 9:

The referee asks for the reason of the decreasing trend in Northern Hemispheric HCN. However, because of the concerns of referees 1 and 3, we will omit the trend analysis completely.

P9013, Line 2:

The referee is right. According to the GFED time series, the emission over South America (and southern Africa) is strongest in August/September. The strongest upper tropospheric HCN signal, however, is in October or November. To illustrate this time lag, we will add HCN distributions of September in Fig. 6. Further, we will add a plot of deep convection from the NCEP/NCAR reanalysis and discuss the interplay of maximum emission and southward shift of deep convection. We will also present the findings of Liu et al. (2010), who discuss the role of South American deep convection in lifting biomass burning emissions into the upper troposphere.

P9016, Line 16:

We will delete the phrase ”In this presentation”.

P9017, Line 26:

We can not give exact values for troposphere-to-stratosphere transport through various pathways from the simple estimation performed here. We just like to mention that, different to the findings of Randel et al. (2010), the tropical upper tropospheric HCN amounts measured by MIPAS seem to be sufficiently high to establish the observed stratospheric HCN values via tropical upwelling.

P9019, Line18-19:

The referee critisises, that we ”cannot eliminate the meteorological effects without looking into the interannual variation of meteorological fields during these years.” We will inspect the meteorological fields for a potential contribution to the biennial cycle.

P9021, Line 24:

We will add ”from Southern Hemispheric low to high latitudes”.

Fig 5 and Fig 9:

We will leave the missing data as blank.

Fig 8:

As requested by the referee, we will start Fig. 8 from Jan 2005.

## Reply to referee 3

We thank referee 3 for her/his helpful comments.

**Reply to general comments:**

The referee criticises that there are a ”number of places throughout the manuscript, where the authors make unsupported assertions in explaining the behavior observed in their data.” Therefore we will try to back up such assertions in the revised paper: We will add time series of GFED fire carbon emissions from different regions, which will facilitate the interpretation of the observed upper tropospheric HCN distribution. Secondly, we will show upper tropospheric windfields from the NCEP/NCAR reanalysis to confirm the postulated eastward transport of the observed African plume. Further, we will add plots of the outgoing longwave radiation (OLR) from the NCEP/NCAR reanalysis to explain the time lag between fire emissions and upper tropospheric HCN maxima observed by MIPAS.

**Reply to specific substantive comments:**

P8999, L25:

We will change the wording into ”an almost unambiguous tracer” here and in the conclusions.

Section 2.2:

Contrary to the referee’s impression, the retrieval results shown in this manuscript were not produced using a revised algorithm, but with the algorithm described in Wiegele et al. (2012) and in Section 2.2 of this publication (RR-mode retrievals). We will make this clearer in the updated manuscript by focussing the description of the retrieval to the setup actually used. Further the referee criticises that the bias between MIPAS and ACE-FTS HCN data is mentioned in the Conclusions, but not discussed in Subsection 2.2. However, this bias is discussed in the comparison to ACE-FTS in Subsection 3.2. We think, we should leave the discussion at this place. A dedicated comparison with MLS data was not performed, since the standard HCN product of MLS is restricted to the stratosphere and the focus of this paper is the UTLS region. The discussion of the retrieval error for a single MIPAS scan will be performed more precisely by giving error estimates for the UTLS region for enhanced and background HCN. In quoting stratospheric error estimates the impression will be avoided that HCN plumes extend into the middle stratosphere.

Section 3.1:

The referee feels it is not appropriate to call climatological features of enhanced HCN ”plumes”, because this term should be used only to describe particular events. We will introduce another term like ”enhancements” or perform a wider definition of plumes.

P9004, L23:

We will add the suggested citations (Duncan et al., 2003, 2007; van der Werf et al., 2010) to show that northern hemispheric biomass burning typically occurs in this season.

P9005, L17-18:

We will add ”boreal” in front of ”summer” at the beginning of this paragraph and at other occasions. Further we will rewrite the sentence ”Stratospheric HCN reflects the fully developed Antarctic vortex.” as follows to clarify its meaning: ”Due to subsidence of mesospheric air masses in the Antarctic vortex, this season exhibits the lowest stratospheric HCN amounts, observed at high southern latitudes.”

P9005, L23-26:

We will cite, e.g., Edwards et al. (2006) and Glatthor et al. (2009) as reference for southern hemispheric biomass burning during this season and discuss elevated HCN values at 40–60N.

P9006, L1-8:

We will reorder the paragraph as suggested.

P9006, L15-17:

We will explicitely refer to the bottom left graph of Fig. 7 (May 2005) and outline that the northern subtropical HCN anounts of May 2006 (bottom right graph) are untypically low.

P9007, L17-19:

The referee criticises the unsupported hypothesis of northward transport of northern hemispheric HCN in the discussion of Fig. 3. To support the hypothesis, we will refer to Fig. 4 or omit the statement at this place completely.

Section 3.2:

(1) The referee calls the qualitative comparison between the MIPAS and ACE-FTS by reference to plots in previously published papers instead of actually showing some comparisons not very convenient, but then suggests to add ”(not shown)” at the end of the second sentence in this section. We will follow this suggestion.
(2) The referee asks, which Figure of Randel et al. (2010) confirms poleward transport of enhanced HCN following the southern hemispheric biomass burning season. This transport can be seen in Figure S2 of the supporting material. The confusion is caused by combined listing of references for two processes (low HCN amounts over the tropical oceans and poleward transport). We will separate the references to these two processes to make things clearer.
(3) The referee believes that the different underlying time periods will not lead to substantial discrepancies between the MIPAS and ACE-FTS HCN climatologies. But since some of the

referenced ACE-FTS climatologies cover considerably shorter time periods (e.g. Lupu et al. (2008) analyse the period 2004–2006 only) this difference can cause discrepancies at least to a certain extent.

(4) She/he is puzzled about suggesting the use of different spectral regions as possible reason for systematic deviations of about 20% between HCN VMRs observed by MIPAS and ACE-FTS and feels that more explanation is needed. According to the error codes in the HITRAN database, the spectroscopic uncertainties of the strongest HCN lines in the spectral regions used for MIPAS and ACE-FTS retrieval are 5–10%, both for line intensity and for pressure broadening. These systematic error contributions have not been included in the error estimation presented in Section 2.2. If they are added to the upper tropospheric MIPAS HCN retrieval error of 5–15%, the deviations between MIPAS and ACE-FTS are within the error bars. Thus, both instruments capture the HCN distribution within the combined uncertainties. We will add some sentences explaining these issues both in Sections 2.2 and 3.2.

(5) The referee states that a systematic bias can not explain the differences in the vertical extent of the northern and southern hemispheric tropical and subtropical enhancements. She/he is right. This is a basic difference between HCN observations of ACE-FTS and of MIPAS.

(6) We agree that the difference in sampling of ACE-FTS and MIPAS is another explanation for differences and had already checked this issue. For example, the much sharper gradient of the autumnal (Sep–Nov) southern hemispheric plume observed by ACE-FTS towards high southern latitudes (Randel et al., 2010, Fig. S2) can at least partly be explained by sampling of regions south of 70°S in September, only. MIPAS HCN data restricted to September also show a sharper gradient than HCN data averaged over the period September to November.

P9011, L10-13:

The referee notes that the statement ”entry of enhanced HCN into the lower stratosphere seems to be somewhat more effective in the northern than in the Southern Hemisphere, but the dominance of the Asian Monsoon is not as distinct as shown by Randel et al. (2010, Fig. 3)” needs more explication. First, with our statement we are not only referring to 22 km in Fig. 4 of our manuscript, but also to 18 km. For better comparison to the ACE-FTS/MLS-plot, we also averaged the MIPAS time series over the altitude region 16–23 km. But this does not lead to different conclusions. One reason for different interpretation of MIPAS HCN in the tropical UTLS region is analysis of a longer time series than in the reference papers. The MIPAS time series additionally contains the years 2010–2012 and especially 2003 with larger contributions from the Southern Hemisphere.

P9011, L24-27:

The referee criticises that our explanation for longer meridional transport times in the years 2002 and 2006 is not backed up by some references. We did not give any reference here, because we thought the statement is explained by the MIPAS measurements shown in Fig. 4 (10 km) itself. She/he wonders, why we did not look at GFED emissions. As mentioned above, we will introduce time series od monthly GFED fire emissions in relevant regions for better confirmation of our statements at the beginning of Section 3.

P9012, L11-17:

The referee criticises, that it is not meaningful to calculate HCN trends in the northern and southern hemispheres without presentation of error bars. Since referee 1 also criticises the discussion of trends, we will omit the presentation of trends.

P9012, L26 - 9013, L16:

(1) The referee is right. We will change the wording into ”... in October most enhanced HCN amounts were observed above northern Australia and Indonesia, indicating strong biomass burning and effective upward transport in this region.”

(2) The referee criticises that our discussion takes no account of the time lag between surface emissions and the appearance of HCN enhancements in the upper troposphere. We will address this problem by referencing the findings of Liu et al. (2010), by comparison of maxima in MIPAS HCN with GFED emission data and by investigating the seasonality of tropical deep convection.

(3) The referee criticises the wording in referencing the GFED fire emissions presented by Li et al. (2009). This sentence will be completely omitted in our revised manuscript,

because we will refer to the GFED time series, which will be included in a revised paper.

P9015, L5:

We will change the citation to ”(Randel et al., 2010, and references therein)”.

P9015, L23:

We will change the wording as suggested.

P9017, L3-4:

Although the levels of 14, 17, 20 and 23 km are not completely separable, they were chosen to localize the transition from semi-annual to longer cycles and to track the time lag of the tropical HCN variations with increasing altitude.

P9017, L3-21:

The referee misses the discussion (here and elsewhere) of the importance of seasonal variations in the strength and location of convection. To account for deep convection, we will introduce plots of the outgoing longwave radiation (OLR) from the NCEP/NCAR reanalysis and discuss the interplay of deep convection and enhancements in upper tropospheric HCN.

P9017, L20 and P9018, L9:

We will compare our time scales for vertical transport more closely to time scales for vertical transport of $H_2O$ given by Mote et al. (1996) and Schoeberl et al. (2008), for vertical CO transport derived by Liu et al. (2013) and with HCN time scales estimated from plots presented in Pumphrey et al. (2008) and Li et al. (2009).

P9017, L26-29:

The referee thinks that our statement ”... Randel et al. (2010) ... from analysis of ACE-FTS and MLS data conclude, that the HCN amounts at the tropical tropopause are too low for effective supply of stratospheric HCN” is too strong. We will reread the cited publication and update our statement. In case our statement was right, the requested implication is that according to observations of HCN by MIPAS troposphere-to-stratosphere transport via the AMA is not as important as with respect to HCN observed by ACE-FTS and MLS.

P9018, L3:

Right, the averages are slightly different, because they were calculated over the time periods covered by the respective plots.

p9018, L16-18:

We will change ”strong AMA of 2005” into ”strongly polluted AMA of 2005”.

P9018, L26-29:

As pointed out be the referee, we will emphasise that low HCN was already observed at the beginning of 2008. The positive anomaly of autumn 2009 probably could not propagate into the stratosphere, because it was not followed by subsequent pulses in 2010. We will add this explanation in the revised paper.

P9019, L5 and 16:

The referee misses a reference for the biomass burning at the end of 2011. We will reference Fig. 4 (14 and 18 km) for the relatively strong biomass burning at the end of this year. Further, she/he states that the maximum of the tape recorder signal in 2011 is missing in L16. We will update the manuscript accordingly.

P9019, L26-28:

The referee states that the statement made here more or less agrees with the conclusions of Randel et al. (2010) and contradicts the statement made earlier that our findings are in opposition to those of Randel et al. But here we list three sources of positive HCN anomalies: Extensive southern hemispheric and Indonesian biomass burning followed by a strong AMA containing large amounts of HCN.

P9020, L6-9:

We will cite publications of the water vapor tape recorder by Mote et al. (1996) and Schoeberl et al. (2008). Further, we will make the vertical propagation of HCN and $H_2O$ anomalies better comparable by plotting the slope of the HCN anomalies into the $H_2O$ time series.

P9021, L27-29:

We will give a concrete number for the time shift and weaken the differences to Randel et al. (2010).

P9022, L8-9:

Yes, the conclusion that the periodicities of the HCN tape recorder is similar to the findings of Pommrich et al. (2010). We will mention this fact in the revised manuscript.

P9022, L17-19:

As mentioned above, we will compare our ascent rates to rates of Mote et al. (1996), Schoeberl et al. (2008), Liu et al. (2013), Pumphrey et al. (2008) and Li et al. (2009).

**Minor wording and grammar comments:**

The suggested changes will be performed.