

Dear Prof. Haynes,

please find below our responses to the referee comments on our paper "Seasonal and inter-annual variations of HCN amounts in the upper troposphere and lower stratosphere observed by MIPAS". We performed practically all suggested changes. In the marked-up manuscript, passages deleted on request are denoted in blue and requested updates in red. General shortenings as requested by referee 1 are denoted in green and incorporated in braces. Some additional editorial updates are denoted in green without enclosing braces.

Reply to General Comments of referee 1:

The referee finds the arguments on meridional transport of high HCN from low to high latitudes in the stratosphere rather weak. This is stated more specifically in the Specific Comments (P10, L21), where the referee does "not see significant transport from NH mid-latitude (AMA) to high latitudes in Fig. 3b." However, our statement was related to Fig. 2c, in which to our opinion there is clear indication of NH meridional transport from the low latitude TTL into the high latitude lowermost stratosphere. Second, she/he suggests shortening of some of the descriptions in the text to make the main idea of this work better understandable for the readers. Respective shortenings are denoted in green.

Reply to Specific Comments of referee 1:

P2, L17, P2, L22: The suggested changes were performed.

P3, L20: "in the 1990s" was changed into "in later years".

P4, L1: Changed as suggested by referee 3.

P4, L2: In our opinion this sentence is complete.

P7, L11: The suggested change was performed.

P10, L9: "Compared to the previous season" was changed into "Compared to boreal spring".

P10, L21: The referee does not see significant transport from NH mid-latitude (AMA) to high latitudes in Fig.3b. However, as mentioned above our statement is rather related to Fig.2c, where to our opinion the discussed transport pattern is clearly visible. To illustrate more clearly, which Figure we are talking about, we added "Fig.2" in referencing the individual graphs of Fig.2 in the paragraphs at pages 9–10.

In the same context the referee states that there is a little bit of confusion in the description of the HCN maximum in the NH summer. Since at P10, L21 we already discuss NH fall, we are not quite sure, if this comment is related to NH summer or rather to NH fall. Therefore we slightly restructured both paragraphs.

P10, L23 and P17, L29: The citation was changed into "Randel and Jensen (2013)."

P15, L3: We have no further comments on why ACE-FTS sees lower minima in the tropical upper troposphere and lower stratosphere.

P20, L3: The suggested change was performed.

P24, L29: The double citation was removed.

Fig. 3: The referee suggests to include Fig. S1 of Randel et al. (2010) in the description of Fig. 3. Therefore we added the sentence "A similar presentation of HCN measured by

ACE-FTS can be found in Randel et al. (2000, Fig. S1)” at the beginning of the discussion of Fig. 3. However, since the referee also requests shortening of some of the descriptions we leave the comparison to the reader.

Fig. 4: As requested, the same color scale was used for all the regions and seasons in the OLR-plots.

Fig. 7: We used ACE v2.2 data although it is not the current version to avoid inclusion of implausibly high or low HCN values not flagged in versions v3.0 and v3.5. The HCN climatology from ACE-FTS looks noisier than the MIPAS climatology, because much less values are available for calculation of the averages.

Fig. 8: We added the standard errors of the mean of the displayed data. For reasons of consistency and adequateness, we also replaced the standard errors in Fig. 9 by the standard errors of the mean

Fig. 10: The blank (white) regions do not contain HCN values lower than 100 ppbv. They are all data gaps either caused by operational shutdown, by missing measurements or by discarding measurements contaminated by polar stratospheric clouds. To make this clear we added the phrase ”and data gaps at high latitudes are caused by polar stratospheric clouds” in the figure captions.

Reply to specific substantive comments of referee 3:

P10, L24–P11, L2:

(1) We added a sentence stating that the minimum in northern tropics to midlatitudes is weaker than its southern hemispheric counterpart in boreal spring.

(2) The referee remarks that Antarctic HCN amounts below 10 km in SON were not as low as in JJA. However, our statement is related to Antarctic stratospheric HCN amounts while Antarctic altitudes below 10 km are mostly tropospheric. To account for the referee’s observation we added the subordinate clause ”but the tropospheric minimum has been filled up again.”

(3) We replaced the expression ”retention” by ”withholding”.

(4) Actually the minimum at low latitudes discussed in the previous sentence is in the troposphere. To make the sentences better understandable, we changed ”stratospheric HCN” into ”mid- and upper stratospheric HCN” and added ”tropospheric” in the previous sentence.

P11, L11: We removed the part ”insufficient fire-induced convection for uplift of the pollutants into the upper troposphere and” from the sentence.

P19, L19–20: We also removed the part ”insufficient fire-induced convection for instantaneous uplift and” from the sentence and changed the rest into ”delay of effective lifting until the onset of deep convection”.

P14, L2–3: We changed ”as well as in amplitude” into ”and in fairly good agreement in amplitude”.

P15, L18: We changed the passage ”Generally, there is rather good agreement between the averaged profiles. The HCN VMRs of MIPAS are slightly higher than the ACE-FTS values, but the shape of the profiles is very similar.” into ”There is rather good agreement in

the shape of the averaged profiles, but the HCN VMRs of MIPAS are slightly higher than the ACE-FTS values.”

P16, L9–10: We removed the sentence ”There is good agreement between the MIPAS and INTEX-B HCN profiles.”, which is a subjective estimation, and changed the wording of the following passage from

”Except for the uppermost INTEX-B value at 11.5 km, which exhibits a larger uncertainty and appears like an outlier, there is nearly no bias between the two datasets in phase 1.”
into

”Except for the uppermost value at 11.5 km, which exhibits a larger uncertainty and appears to be an outlier, the INTEX-B data between 7.5 and 10.5 km oscillate closely around the MIPAS profile. Thus there is nearly no bias between the two datasets in phase 1.”

P16, discussion of Fig. 9: We changed the colours of MIPAS and ACE-FTS data in Fig. 8 for better consistency with Fig. 9.

P18:

(1) L11: We added ”(no El Niño year and no outstanding biomass burning in South America)” to make clear that these years were typical of the climatological mean.

(2) L15–16: We replaced the adjective ”subsequent” by ”consecutive”.

(3) L17:

We removed the word ”real”.

(4) L19-20: We added the sentence ”However the longer time series of MIPAS contains a larger portion of periods in which the contributions from the southern hemisphere were larger than during the timeframe 2004–2009 observed by Randel et al. (2010).”

P22: We added ”than in the southern hemispheric biomass burning plume” to specify, compared to what the HCN amounts in the AMA were more regular.

Minor wording and grammar comments of referee 3:

P19, L1–2: The phrase ”are expectedly shorter” was replaced by ”as expected are shorter” instead by ”are shorter as expected”.

P24, L23: The apparent redundancy in ”and denoted at the top of Fig. 16 (top)” was replaced by ”and the numbers in Fig. 16 (top)”.

All other suggested changes were performed exactly as requested..