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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Seasonal and interannual variations of HCN amounts in the upper troposphere and lower stratosphere observed by MIPAS" *by* N. Glatthor et al.

N. Glatthor et al.

norbert.glatthor@kit.edu

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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.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 8997, 2014.