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Comment

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.

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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

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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 necessary 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

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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

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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

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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 criticises, that we "cannot eliminate the meteorological effects without looking into the interannual variation of meteorological fields during these years." We will

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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.

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