October 18, 2013

Author response to the comments of Reviewer #2 – Manuscript ACP-2013-457 *Longitudinal hotspots in the mesospheric OH variations due to energetic electron precipitation* by Andersson et al.

We would like to thank Reviewer #2 for the comments. We greatly appreciate the time and effort spent by the reviewer on our paper.

Response to the general comments of Reviewer #2

1. **Comment:** Page 19901, line 15 –17: here, I disagree with your description of the spatial distribution of the OH enhancement in the Southern hemisphere. As I see it, OH is always maximal at high latitudes (probably South of –70°, though this is difficult to see because there is no tick for –70° on the axis). There are strong local maxima between 120°E and 60°W, which apparently correlate to geomagnetic latitudes of 55–72°S, which strengthens your assumption that the maxima are related to particle precipitation. However, a similar local maximum is not observed where geomagnetic latitudes of 55–72°S extent to lower latitudes, e.g., at 40–60°S / 60 -180°E. Of course you can discuss that this might be related to the SAMA, but the structure that you see in Figure 2 in the SH is that of a strong geographic gradient, with some longitudinal structure at high geographic latitudes. The relation to geomagnetic latitudes of 55–72° is not clear from this figure in the Southern hemisphere. It is quite clear in the Northern hemisphere, and also becomes clearer when looking at anomalies directly, and with the EOF analysis later on in the paper. However, at this point you should describe accurately what is observed in Fig 2, not what you derive from later analysis.

   Response: We agree with the Reviewer that the lines 15–17 was not formulated adequately. We revised the text as follows: "The geographic distribution of the OH high values in the NH is very similar to the distribution of precipitating electrons, i.e., OH follows geomagnetic rather than geographic latitudes. The highest OH values in the NH are confined to the longitudes from 180°W–30°E (NAm hot–spot). In the SH, there are strong local maxima at longitudes between 180°W–30°E (AP hot–spot) which partially correlate to geomagnetic latitudes 55–72° and could be connected to the radiation belt electrons. However, OH yearly median data do not show similar enhancements at other longitudes of the radiation belt range. We investigate this matter further in Section 5. "

2. **Comment:** Page 19914, second paragraph (discussion of Fig 2): you should give an estimate of the significance of the observed maxima in your hot–spots. As far as I can see, your hot–spots have amplitudes of 1–2 ppb, compared to a background of 0.6–1 ppb (depending on hemisphere). I appreciate that it is probably not trivial to derive significance levels because the underlying distribution is probably not gaussian - however, you could get an idea of the significance of the median values by comparing histograms of the single values in the hot–spots and low spots.

   Response:

   We now give an estimate of the significance of the observed maxima in OH hot–spots. Because the distribution is non gaussian we use non-parametric statistics, i.e. the bootstrap method.

Lines: ”In order to estimate the significance of the observed maxima in the NH and SH we used the bootstrap method. For each hemisphere, we have selected two 5 (latitude) $\times$ 30 (longitude) degree bins inside radiation belts (geomagnetic latitude 55–60° N/S) – one with low OH medians (90–120° E) and one with high OH medians (60–90° W). For each bin we calculated the median values 200 times for a random distribution of all available data points. Then the standard deviation (SD) and 95% confidence intervals (CI) were calculated. As an example, for year 2005 calculated median values are robust with SD$<$ 3% in both hemispheres. CI are: [0.94; 1.10] in the NH and [1.65; 1.81] in the SH for high OH medians bins and [0.67; 0.75] in the NH and [0.83; 0.95] in the SH for low OH medians bins. Because the 95% confidence intervals error bars between low and high OH bins do not overlap, the difference between the two estimated medians is statistically significant with p value less than 0.05.” were added to the second paragraph (discussion of Fig. 2.)

3. **Comment:** Page 19902, line 14 ff, discussion of Fig. 3: why do you only show results from one model run here, but then mark NH and SH? Is this model run from the NH or SH? The atmospheric background could be quite different in the two hemispheres, so model runs for both hemispheres should be shown here.

   Response:

   We are showing results for one model run at 60° N and 0° E. This example is enough to demonstrate the general decrease of OH mixing ratio during nighttime. The NH and SH marking does not indicate location of the model runs, but simply the local times related to measurement made in the NH and SH. The reviewer is right that background atmospheric conditions could be very different in the two hemispheres for a certain day of year. However, because with Fig. 3 we are trying to explain the yearly median asymmetry in OH concentration between SH and NH, we want to avoid having differences due to atmospheric conditions (which should more-or-less average out when looking at yearly median data). This is why we demonstrate the local time effect on the OH mixing ratio with an example. We have revised the figure text to make the modeling location and the NH/SH marking clearer as follows: ”A model run was made for the 5–6th of March 2005 at 60°N and 0°E, using MLS/Aura monthly mean values of H_2O and temperature. This setup (single location instead of contrasting NH and SH) allows us to focus on the LST effect in general without interference from, e.g., seasonal variability. Note, that no electron forcing was applied to the model in order to get the general behaviour of the OH during nighttime. Fig. 3 gives an example of the OH mixing ratios from a SIC model run averaged between 70–78 km. The modeled OH mixing ratios at LST of the satellite passage (gray areas) are of about 30–40% higher in the SH than those in the NH”.

4. **Comment:** Page 19903, line 14: ... OH clearly peaks in the AP sector. In this figure, unlike Fig. 2, OH also clearly peaks within the radiation belt region at Southern lower latitudes, e.g., in the 40-60°S / 75-165°E region. I find this quite convincing, and you should mention this here, even if the amplitudes in the AP region are much higher.

   Response: We revised the text on page 19903 as follows: ”Note that OH enhancements are also observed at other longitudes within radiation belts, i.e., 75–165° E, but the amplitudes of these enhancements are lower than in the AP sector. This cannot be seen from yearly medians presented in Fig. 2.”

5. **Comment:** Page 19903, lines 7 -16: again, the amplitudes of the anomalies in the HEEP case are in the range of 1–2 ppb in the radiation belt region compared to 0.7–1 ppb outside - can you please give an estimate of the significance of these enhancements?

   Response:

   We now give an estimate of the significance of the OH enhancements in the HEEP case using the bootstrap method (see comment 2). Lines ”In order to estimate the significance of the observed HEEP–enhancements in the NH and SH we used the bootstrap method in the same way as in case of yearly OH medians (see description of Fig. 2). In this case, we have selected 5 (latitude) $\times$ 30 (longitude) degree bins inside radiation belts (geomagnetic latitude 55–60° N/S and 60–90° W) and outside the radiation belts (40–45° N/S and 0-30° E). Inside the radiation belts bins, SD$<$ 12% and CI=[1.05–1.42] in the NH and SD$<$ 8% and CI=[1.80; 2.24] in the SH. Outside the radiation belts latitudes, SD values are the same and CI=[0.68–0.97] in the NH and SD$<$ 8% and CI=[0.62; 0.99] in the SH. Again, the 95% confidence intervals for bins inside and outside the radiation belt latitudes do not overlap, which suggest that the difference between the two estimated medians is statistically significant with p value less than 0.05.” were added to the description of the Fig. 4.

6. **Comment:** Page 19903, lines 19ff: please clarify why you only show values for the SH in Fig 5, or show both hemispheres.

   Response: We show only the SH plot because we are interested in OH elevated values in the AP region during LEEP condition (not seen in the NH). This could be connected to the steady drizzle of the electrons as well as different atmospheric conditions. Therefore we want to have a closer look at the Southern Hemisphere. We revised the text for better clarity as suggested by the reviewer. Lines ”Because the differences in $_2$ and temperature could cause some of the observed OH longitudinal variability in Fig. 4, we examine their possible role in the observed OH enhancements in the AP sector. ” was replaced by ”As it was mentioned in the previous paragraph, the enhanced values in the SH (see Fig. 4) could be connected to the steady drizzle of radiation belt electrons but also the differences in H_2O and temperature could cause some of the observed OH longitudinal variability. Therefore, we examine their possible role in the observed OH enhancements in the AP sector.”

7. **Comment:** Page 19904, line 14 ff: again, it would be good to give an estimate of the significance of the ECR$>$100 anomalies shown in Fig 6 here, especially as the anomalies themselves are very small (see line 28).

   Response: We now give an estimate of the significance of the ECR$>$100 anomalies using bootstrap method. Lines: ”In order to estimate the significance of the OH enhancements for the ECR$>$100 case, we again used the bootstrap method and calcluated SD and CI for each of the longitudes presented in Fig. 6. In the NH, SD varies between 7–12% with CI between [0.7–1; 1.0–1.35] for all longitudes. In the SH, SD varies between 3–8% with CI=[0.8–1.0; 1.0–1.3] for longitudes 45° E–165° W and CI=[1.4–1.6; 1.7–2.2] for longitudes 135° W–15° E. For all longitudes except 45° E in the NH and 45–75° E in the SH, 95% confidence intervals error bars calculated for ECR$>$100 do not overlap with 95% confidence intervals error bars calculated for ECR$<$5 and the estimated medians are statistically different with p value less than 0.05.” were added to the description of the Fig. 6.

8. **Comment:** Page 19905, line 20: as I understand this, you have removed the monthly mean for each individual month, to erase both the annual variability, and possible interannual variabilities. Is this correct? Please clarify. This probably leads to the jumps you see in PC1 as given in Fig. 7, right? This should be mentioned in the discussion of Fig 7. However, I think the EOF analysis would be applied more correctly if instead of a mean of every individual month a global mean (for a certain latitude) of all months considered was subtracted. In this case, PC1 and PC2 would probably reflect the annual and interannual variability instead of the particle contribution. However, the particle contribution should still be there, in PC3 if you are lucky. Have you tried this? If this does not give useful results, you should at least discuss this.

   Response: Yes, we do remove the monthly mean for each individual month in order to remove the possible variability which is not connected to the electron precipitation. The obtained result were similar, however much more noisy and not so clear as the current approach. The OH increases due to electron precipitation are small in magnitude and difficult to identify. By subtracting the mean from all considered months we could quickly end up with removing to much for the NH (we are closer to the summer season in average) or to little for the SH (we are closer to the winter season in average) as we consider only 6 selected months. This makes the results noisier. By removing monthly mean for each individual month we make sure that the OH anomalies are probably connected to the electron precipitation and not influenced by differences between background levels of the selected months. We agree with the reviewer that proposed approach would give us annual and interannual variability if our data set would be complete. Here, the PC1 and PC2 do not show annual and interannual variability, because the data selected do not cover one full year, neither one full season. The only two criteria we have is 1) months with electron precipitation, 2) full global coverage in both hemispheres.

   Here, we would like to also point out that EOF analysis was repeated. We have realized that equatorial regions should be removed from analysis in order to avoid possible impact from other factors that could affect the OH variation (for example tides). The obtained result is the same with higher variance explained, i.e. 9% instead of 6%.

9. **Comment:** Page 19905, discussion of Fig 5: you can also emphasize here that EOF 1 follows the radiation belt areas much more closely than the absolute OH values shown in Fig 2, at least in the SH.

   Response: We revised the text as suggested by the reviewer as follows: "The spatial patterns of the OH changes do not extend to the other latitudes and follow the radiation belt areas much more closely than the yearly median presented in Fig. 2."

10. **Comment:** Page 19906, line 12: r is probably a linear correlation coefficient. A correlation coefficient of 0.6 is not that high. However, giving the comparatively large number of datapoints, it is probably highly significant. Instead of giving p, you should give the significance here (which should be easy at this point if you already did a t–test analysis for the correlation coefficient): is this significant at 90%, 95%, 99%? Please clarify.

    Response: The correlation is significant at 99%. We now modify sentence on page 19906 line 12 as follows: "The statistical robustness of the correlation was determined by calculating the p value (t-test). Because $p < 0.01$ the random chance probability of getting such correlation for the data sets when the true correlation is zero is less than 1%."

11. **Comment:** Page 19897, line 9/10: this is also discussed in Sinnhuber et al., Sur Geo, 2012

    Response: We added the reference as suggested by the reviewer.

12. **Comment:** Page 19900, line 17: appears to contain ... I think what you mean is, that enhanced particle fluxes are apparently observed in the SAMA region, which however are more likely due to contamination of the particle detectors than precipitating electrons. However, this only became clear after reading the discussion of the OH results. Maybe you can clarify this sentence so the meaning of the appears ... becomes already clear at this point.

    Response: We revised the text for better clarity as suggested by the reviewer as follows: "The data inside the SAMA region, i.e., around 30°E–90°W and 0°–45°S, appears to contain an increased particle background due to a local minimum of the geomagnetic field. This however, is more likely due to contamination of the particle detectors than electron precipitation (we will discuss this in the next paragraph)."

13. **Comment:** Page 19900, line 26/27: with HIGHER electron fluxes observed between 150-30°W in the NH, you mean. The minimum appears to be around 60–180°E in both hemispheres.

    Response: Yes, there was clear mistake in the text which was corrected.

14. **Comment:** Page 19901, line 6: Please clarify what is meant with this region – the SAMA?

    Response: Here we meant AP region. Page 19901, line 6 was clarified as follows: "In the AP region the magnetic field is weaker, such that the angular width of the bounce loss cone increases and electrons which were mirroring just above the atmosphere at other longitudes will be lost inside the atmosphere in this longitude region."

15. **Comment:** Page 19902, line 1: please clarify which data are contaminated by the SAMA POES or MLS? I assume that POES data are meant. However, I know that some atmospheric sounders are also affected by the strong particle fluxes when crossing the South Atlantic Anomaly. I dont know whether MLS is one of those, though.

    Response: Only MEPED data are contaminated. The MLS OH data are not contaminated, although the observations from the SAMA region seem to be noisier (statistically). Lines on page 19902 was modified as follows: "In the SAMA region, however, we observe no enhancement in OH. This indicates that in this region there is no significant $>$ 100 keV electron precipitation, even though precipitating fluxes generally appear to peak in this region. This is consistent with our suggestion that the signal above South America is due to the POES data contamination by protons, and in reality little precipitation is taking place, consistent with the very low geomagnetic latitudes relative to the locations of the inner and outer radiation belts."

16. **Comment:** Page 19903, line 10: you should write Antarctic Peninsula (AP) first time it appears on this page

    Response: We define Antarctic Peninsula hot–spot in Page 19900 line 22. However for better clarity we corrected the text as suggested by the Reviewer.

17. **Comment:** Page 19904, line 9 –13: I found this sentence confusing. Maybe its meaning would be easier to understand if it was stated without the negation

Response: Lines 9-13 ”Therefore, the stronger OH response in the AP sector (80 % higher than at the other longitudes) can not be explained only by different atmospheric conditions but is most likely also connected to the peak in electron precipitation forcing seen to occur in the same spatial region.” were replaced with ”Therefore, the stronger OH response in the AP sector (80 % higher than at the other longitudes) can be partly explained by different atmospheric conditions, but it is also likely connected to the peak in electron precipitation forcing occurring in the same spatial region.” as suggested by the Reviewer.

18. **Comment:** Page 19899, line 4: by GOES-11 in 5-10 MeV channel → by GOES-11 in the 5-10 MeV channel

    Response: We modified line 4 on Page 19899 as follows: ” by GOES–11 at 5–10 MeV energies”.

19. **Comment:** Page 19902, line 18: from SIC model run → from a SIC model run

    Response: Corrected as suggested by the Reviewer.

20. **Comment:** Page 19902, line 24: e.g., amount of H2O → e.g., the amount of H2O

    Response: Corrected as suggested by the Reviewer.

21. **Comment:** Caption of Fig3: NH nad SH → NH and SH

    Response: Corrected as suggested by the Reviewer.