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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Longitudinal hot-spots in the mesospheric OH variations due to energetic electron precipitation" *by* M. E. Andersson et al.

Anonymous Referee #2

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The paper 'Longitudinal hot-spots in the mesospheric OH variations' by Andersson et al. investigates the response of mesospheric OH in one altitude region (70-78km) to precipitating energetic electrons. The paper builds on results of similar studies from the same authors, which have already shown that OH in this altitude region clearly responds to precipitating electron forcing. Th focus of the present paper is on the spatial patterns observed in mesospheric OH related to this electron forcing. A major result of this study is that OH at these altitudes clearly follows the radiation belts in both hemispheres, with distinct hot-spots of higher values at certain regions within the radiation belts. Contrary to reviewer #3, I found this a new, very interesting and important result. Verronen et al 2011 have already shown that the OH response is largest at geomagnetic latitudes of 55-65°. However, a clear pattern of electron precipitating in the

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radiation belt region as shown in Fig 4 and Fig 7 of the present paper has never been shown before for any constituent in the middle atmosphere. A similar structure with similar hot-spots in the North Atlantic magnetic anomaly region and over the Antarctic peninsula has been observed in thermospheric NO around 107 km (e.g., Barth et al., JGR, 2003), where they are also attributed to inhomogeneities of the geomagnetic field in the South Atlantic anomaly. I find it very important to show that such patterns indeed can exist also in the middle atmosphere down to 70km. Whether the hot-spots might be derived at least partly because of the latitudinal inhomogeneities of the radiation belt area is of course debatable, but this is also investigated in the paper at least for the Southern hemisphere, the conclusion being that up to 55% of the observed variability might be accounted for by inhomogeneities of the background atmosphere and the changing sza of the observation at different latitudes.

To conclude, I found this paper very interesting, and the results well suited for publishing in ACP. It is also reasonably well written. I have some comments which should be addressed before publishing listed below. My main concern is that while I found the figures showing the spatial distribution of OH, especially of the high and low EEP scenarios and the result of the EOF analysis, quite striking, the amplitudes are small, and it would be good to give an estimate of the significance of these amplitudes somehow.

Comments:

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

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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.

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.

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.

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.

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?

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

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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).

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.

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.

Page 19906, line 12: r ist probably a linear correlation coefficient. A correlation coefficient of 0.6 is not that high. However, giving the comparatively large number of data-points, 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.

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

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

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this point.

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.

Page 19901, line 6: Please clarify what is meant with “this region” – the SAMA?

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 don’t know whether MLS is one of those, though.

Page 19903, line 10: you should write “Antarctic Peninsula (AP)” first time it appears on this page

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

Page 19899, line 4: “by GOES-11 in 5-10 MeV channel” → “by GOES-11 in the 5-10 MeV channel”

Page 19902, line 18: from SIC model run → from a SIC model run

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

Caption of Fig 3: NH nad SH → NH and SH

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 19895, 2013.