October 18, 2013

Author response to the comments of Reviewer #3 – 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 #3 for the comments and the effort that the reviewer have put into this paper.

Response to the general comments of Reviewer #3

1. **Comment:** The recent works on the enhancements of nighttime mesospheric hydroxyl in response to electron precipitation from the radiation belts by Verronen et al. (JGR, 2011) and Andersson et al. (JGR, 2012) already show that those enhancements are mainly confined to the geomagnetic latitudes in both hemispheres. The major aim of this manuscript is to show that those enhancements are not longitudinally homogeneous but have some structure including some "hot–spots". Since the geomagnetic latitudes do not coincide with the geographic latitudes (centered at different poles), it is obvious that some longitudinal (geographical) structure will appear if plotted in geographic coordinates. Also,since the magnetic field, and hence the electron precipitation, has some longitudinal dependence, it seems also obvious that the OH enhancements produced by the energetic electron precipitation will also show longitudinal variations associated to the geomagnetic field. Hence, I cannot really see anything new in this manuscript over the papers mentioned above, or even actually regresses at some points (see below). In addition, the presence of the so–called "hot–spots" (which btw are only about 1 ppb and not really so "hot") is not neatly shown. For example, after a quick look at Fig.1, one would expect a real "hot" spot like that in the OH, but there is not. Conversely, the fact that such spot does not appear is used to conclude that the MEPED measurements are corrupted. This is an indirect argument and wonder if there are not other methods to reach that conclusion. This, on the other hand, posses some questions to the reader: are the ERC measurements outside of that region, as assumed in the work, really accurate?

   Response: We agree with the Reviewer that the electron precipitation and hence OH longitudinal structure are expected due to the geomagnetic field longitudinal dependence. However, it has never been shown before using the atmospheric measurements. We feel that the experimental validation of the theoretical assumptions forms the basis of today's science. Another thing is that there are 2 types of geomagnetic events – CME-driven storms and HSSWS-driven storms. These events differ from each other considering the strength, duration and radiation belt dynamics and therefore the atmospheric response can be different. We now clarify this in the introduction and discussion.

   The aim of the previous studies (Verronen et al. 2011 and Andersson et al. 2012) was to show the connection between energetic electron precipitation and OH enhancements by analyzing OH zonal means. This paper bring the analysis forward by analyzing in details the impact of the longitudinal structure seen in electron precipitation to the OH which is important not only from point of view of the POES measurements validation but also atmospheric modeling. We agree with the reviewer that geomagnetic latitude selection as well as the other factors can explain part of the OH variability but that is exactly the reason why we do investigate it in details in the paper. We provide the whole paragraphs dedicated to the role of water vapor, temperature and the other factors which can influence the OH in order to differentiate them from the EEP effects. Please see: page 19896 line 20 (abstract), page 19902 lines 10–27, page 19903 line 12–13, page 19903 – the whole paragraph from line 17, page 19906-19907 line 27 and 1–3.

2. **Comment:** If one really want to show the latitude/longitude variation, why not showing Figs. 1 and 2 as maps as in Fig. 4? Also, in my opinion, they should be shown including only the days with high ERC, otherwise they are somehow ”contaminated” with the signal of the atmospheric variability not associated to ERC.

   Response:

   Replacing maps from Fig. 1 and Fig. 2 with polar plots would only double the number of Figures in the paper. The maps presented in that way are much more clearer because they shows both NH and SH at the same time allowing the reader to pick up main features and behavior as moving from 2005–2009. Polar plots will not introduce any new information only different angle of view. Showing yearly medians allows us to introduce the ECR structure as well as and general view on to the OH including the atmospheric variability. Later on in the paper we do include only days with high electron count rates as well as with low electron count rates in order to check how much of the total variability is connected to the electron precipitation. We also investigate in details the possible effect caused by the different atmospheric conditions (see comment 1).

3. **Comment:** Figs. 2 do not clearly show a good correlation of the OH enhancement with the geomagnetic latitudes in the SH (cf, at longitudes 60E-150W). This actually regresses from that shown previously by Verronen et al., 2012. They just seem to show enhancements at polar geographic latitudes.

   Response: Figure 2 shows yearly medians of OH which present only general view onto the OH including the signal of the atmospheric variability as it was pointed by the reviewer himself in comment 2. Therefore, we do not expect very good correlation with ECR, only some features which can possibly be connected to the electron precipitation (the OH latitudinal structure in the NH, higher OH enhancement in the Antarctic Peninsula region, the OH decrease with decreasing geomagnetic activity as moving from year 2005–2009). At this point we are not certain if reviewer refers to the Verronen et al. 2011 or Andersson et al. 2012. The analysis presented in both of the papers is carried out for OH zonal means and the correlation is calculated for each month separately using daily means in order to check the connection between the ECR and the OH. In neither of them the correlation is calculated for the whole year as the signal from electron precipitation would be strongly affected by seasonal and annual variability. Also, the map presented in Verronen et al. 2011 (Fig 3.) shows 5 days average during strong electron precipitation not yearly median. Note, that this map is in very good agreement with the HEEP case in the present paper i.e., longitudinal structure in OH enhancement.

4. **Comment:** Fig. 4. Why not showing the corresponding ERC plots for these conditions? By direct comparison one would see if they have the same structure. Additionally, in the manuscript (abstract, body, conclusion) is discussed about the OH ”hot-spot” in the NH in the NAm region. In this figure (top left), where the days with larger ECR have been selected, the larger values

are not associated to that longitudinal region. Hence, it seems that one of the conclusions of the manuscript is not clearly supported by the shown data, at least w.r.t the NH. About the SH, the fact that the same OH structure (although weaker) appears for the low ECR than for the high ECR, hints at that it might not be produced (at least totally) by electron precipitation. However, instead of attributing this OH enhancement to atmospheric processes (transport from the illuminated side, others) the authors mentioned that it might be connected to "steady drizzle of radiation belt electron". If so, should not that "drizzle of radiation" appear in the ERC? Why attributing this enhancement to that radiation and not seeking for causes based on atmospheres processes? Which are the selected days? Depending on the particular days, and because of illumination conditions and transport, these processes might be the responsible for the found lat/long distribution.

Response: The corresponding plots of ECR are very much the same: for the HEEP conditions the structure in electron precipitation is preserved in the SH, while in the NH ECR are equally distributed through the whole longitudes. Similar for the LEEP case – longitudinal distribution of ECR is the same, the magnitude of the fluxes is smaller. That is why we think that it is only the redundant information which do not show anything new apart from what is already shown in the paper. However we clarified the discussion of the Fig. 4

We do not discuss about the NAM hot–spot in the abstract. We clearly state that in the NH, the electron precipitation is more homogenous. We do use NAM and NAs hot–spot as the enhancement in that region is the highest. We do agree that it is not enough clarified in the paper and we now modified the description of the Fig. 4.

We attribute OH enhancements to the electron precipitation only partially. Through the whole paper (see comment 1 and 2) we do seek the causes based on atmospheric processes and explain in details how much variability they can account for.

5. **Comment:** Local time. The authors have shown with model calculations the important role played by the local solar time on OH distributions. Actually they demonstrate that the larger values in the SH, w.r.t. the NH, (e.g. Figs. 2 and 4) are caused by the different local times at which the atmosphere was sampled, and, additionally, by SZA effects. That is, a fraction of the OH enhancements in the SH seems not to be induced by electron precipitation but by atmospheric effects. Then, this OH enhancement should not be called a "hot-spot" induced by energetic particle precipitation, or at least state clearly that not the whole enhancement is produced by EEP. In this line, because MLS is in an sun–synchronous orbit, I guess the atmosphere is sampled at the same local time at all longitudes. However, any small change around the poles might induced some effects. This point should be discussed in the manuscript.

   Response: The overall OH amount is higher in the SH and this partially can be explain by the time of the satellite measurement. The longitudinal differences have nothing in common with the hemispheric differences. However, also in case of longitudinal differences we asses the role of solar zenith angle as a part of the OH variability (see page 19902 line 25–27). As we pointed out in comment 1, 2 and 4 our paper contains detailed analysis of other atmospheric processes which can influence OH variations with exact percentage estimation of such effects.