Atmos. Chem. Phys. Discuss., 13, C6251–C6253, 2013
www.atmos-chem-phys-discuss.net/13/C6251/2013/


Atmospheric Chemistry and Physics Discussions

ACPD

13, C6251–C6253, 2013

Interactive Comment

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

**Anonymous Referee #3**

Received and published: 28 August 2013

General comments

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 en-



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ergetic 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?

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.

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.

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.

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.

I have also a number of less important specific comments but, at this point, I think it is not necessary to be detailed.

For all these reasons, I find this manuscript not acceptable for being published in a high impact journal as ACP, at least in its current form.

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