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Interactive comment on "Odd hydrogen response thresholds for indication of solar proton and electron impact in the mesosphere and stratosphere" *by* Tuomas Häkkilä et al.

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Please find below our answers (in blue) to the comments (in black).

Response to the comments of Referee #2

The paper focuses on the impact of energetic particle precipitation (EPP, both proton

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and electron events) on the polar middle atmosphere. The overall goal is to determine the EPP flux thresholds at various altitudes and locations by using odd hydrogen species observed by MLS/Aura satellite and simulated by WACCM-D. Due to the significant uncertainties in satellite measurements of energetic particles, especially for electron precipitation, this study is useful. Although the study does not present critical advances, the paper is well written, the methodology is sound, and the results are in line with previous studies. Overall, I suggest publication subjected to address the following comments.

Response to the general comments: We thank the reviewer for the constructive comments. We also appreciate the time devoted to the evaluation of our paper.

Please try to make a more focused paper. Thirteen figures are too many for this study and make the reading difficult. It would be beneficial to try combining some of them. For example, three figures for showing the comparison of the climatology between MLS and WACCM are excessive. Other figures (at least Fig. 2 and Fig. 11) could be removed or included as supplementary information.

We have removed Figures 5 and 11 to make paper more focused. Figure 2 we kept, because it is needed to demonstrate our methods. Figures 6–7 were also kept, as we would like to show comparisons of both day and night HO_x. The text has been revised to accommodate the changes in the figures.

Abstract: I would like to see more emphasis on your new results. The current abstract mainly describes the state-of-the-art of this topic. A more in-depth discussion on the difference between observations and simulations at lower altitudes could be useful here as well as in the main text. The use of the improved MLS HO2 dataset should be mentioned in the abstract.

We agree that the abstract should have more details on the results and numeric information can be increased. We have thus revised the abstract in these aspects

Methods: Why did you not simply compute the HOx anomalies with respect to the previous days? The EPP-induced HOx enhancements are spikes lasting a few days at most. On the other hand, there is a consistent interannual variability, especially in the northern polar vortex. For example, how did you deal with the SSW occurrence? SSWs can affect the nighttime mesospheric OH layer for weeks (see Winick et al.,2009, Damiani et al, 2010). I think this issue should be at least mentioned. I know that it is common using the flux at energies >10 MeV, but what about the higher or lower energies? For example, I could expect a better correlation in the upper (lower)mesosphere with energies >5 (30) MeV.

We use a daily climatology in our analysis to calculate HO_x concentration anomalies in order to combine events from different seasons, as our aim is to define general threshold values for EPP detection in HO_x. We agree that this method may not be an optimal solution for some of the events, but argue that as a whole it is still appropriate. Day-to-day variability from sources other than EPP also exist in the data, and we believe the use of previous days would be more likely to include this type of uncertainty in the analysis.

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We did not take SSWs into account in the analysis, so this will add to the background variability. However, we show the results separately for the SH and NH, and in our analysis the SH data do not have SSWs. Although the SSWs related variability is included in our analysis, the results are largely consistent between SH and NH, which indicates that they are robust despite background dynamical differences. We have added discussion on this to the revised manuscript.

We do not expect significant differences to arise from the use of different proton flux energies, because the measurements from the GOES proton energy channels correlate well with each other. For electrons we are not aware of a good alternative for the indicator used in our analysis, while we note that the current one could possibly focus our capability to middle mesosphere detection only. We have added discussion on this to the revised manuscript.

Figure 10: By using the standard MLS HO2 dataset, Jackman et al. 2011 and 2014 showed evident MLS HO2 variability during the SPEs of January 2005 and January and March 2012. Here, it is puzzling that the nighttime northern hemisphere did not show any evident HO2 change (i.e., correlation < 0.35). If so, why did you use the new HO2 dataset of Millán et al. (2015)? What's the advantage of using this dataset for EPP-related studies? It could be good showing some comparison between the standard and the new HO2 dataset. If it is not possible to reproduce Figure 10 with the standard dataset, I suggest including at least a case study for a single event.

We use the offline HO_2 data because it offers better S/N ratio in general and an extended altitude range when compared to the standard MLS data.

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We checked the standard and offline data for the January 2005, as well as January and March 2012 events. Compared to WACCM HO_2, which shows a clear and extended response to SPEs, both the standard and offline data show only patchy responses around 0.1 hPa at high latitudes. Overall, in this case the differences in results between the standard and offline data are small, but the offline data provides a better altitude extent. We also analysed our full set of SPEs using the standard data, and there are no thresholds detected.

We decided not to include any comparisons between the standard and offline HO_2 data in our paper, because we see that to be outside the scope of this study and would be a distraction from the main points and focus. However, we have revised the text and added justification for the use of the offline data to the Data and models section.

Pag. 7, l30-34: In Fig. 8, you showed SPE-related changes down to about 35 km in WACCM-D and 50 km in MLS observations. This point deserves more discussion because the evaluation of the direct particle impact at these altitudes is very important. You correctly highlighted the issue of the MLS observations i.e., OH data become noisier in the stratosphere. Therefore, you cannot accurately evaluate the SPE thresholds in this region by using MLS HOx. Nevertheless, you could potentially do it with other MLS products (e.g., chlorine species) which are properly simulated by WACCM (Funke et al., 2011).

We have highlighted this matter in our conclusions and also discuss the use of other species in the detection of EPPs in the revised manuscript. Kalakoski et al. (2020) showed SPE-related increases in WACCM-D Cl_x down to 1 hPa lasting around a week following an event. They also showed a response in HNO_3 above 1 hPa with slightly longer duration (10 days), and enhancements lasting 20–30 days following SPEs below the 1 hPa level.

Pag. 7, l30-34: Why? Some issues with the data sampling? Perhaps SPE-related effects could be highlighted even better by using geographic latitudes.

We are somewhat unsure what this refers to. There may be an error in the comment, since the lines given as reference are exactly the same as for the previous comment, and the comment does not seem relevant to these lines.

This comment may be in reference to p. 8, lines 4–7 discussing differences in daytime threshold detection between SH and NH. The differences do arise from sampling of the data and the distribution of the magnetic latitudes and the geographic latitude coverage of the MLS measurements (82S to 82N (Waters et al., 2006)). These together lead to the daytime magnetic latitude bin 80N having more than 35% more MLS measurement points than the 80S bin. This is likely to cause the discrepancy between NH and SH in daytime SPE detection. Hence this is an issue of data availability at very high latitudes but does not mean that geographic latitudes are necessarily better suited to the study of SPE effects. Using geographic latitudes would not increase the amount of observations available for analysis, the data would simply be binned in a different manner. We believe the geomagnetic latitudes are a better choice, especially where the latitudes are evenly covered by the measurements, because HO_x impact is clearly seen along the geomagnetic latitudes (e.g. Andersson et al., 2014).

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