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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.

Anonymous Referee #2

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

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.

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.

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.

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 MillaÌĄn 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.

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).

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

References: Funke, B., et al., Composition changes after the "Halloween" solar proton event: the High-Energy Particle Precipitation in the Atmosphere (HEPPA) model versus MIPAS data intercomparison study, Atmos. Chem. Phys., 11, 9089–9139, https://doi.org/10.5194/acp- 11-9089-2011, 2011.

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