

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

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Received and published: 18 September 2020

Please find below [our answers \(in blue\)](#) to the comments (in black).

Response to the comments of Referee #1

In this paper, the response of OH and HO₂ in the stratosphere and mesosphere to large

particle precipitation events – solar proton events and electron precipitation events – is investigated based on observations from the MLS satellite and model results from the WACCM chemistry-climate model. In particular, increases in both data-sets during periods of increased proton or electron flux are used to determine a threshold flux above which an observable response can be expected. The topic is of great interest as OH and HO₂ observations during such particle precipitation events are good indicators of an atmospheric impact, and of potential great use to evaluate the particle impact in chemistry-climate models used to study the climate feedback of these events. The paper is also very well written. However, in my opinion there is a problem with the methodology used to calculate the threshold which potentially leads to a high bias. I have summarized my concern below (specific comments), and am looking forward to a productive discussion of this point.

[Response to general comments: We would like to thank the referee for his/her positive comments and appreciate the time devoted to the evaluation of our paper.](#)

Specific comments:

Page 5, lines 20 and following, discussion of threshold determination: I have two comments on the determination of the threshold, which in my opinion could be improved considerably.

- Line 23: you use a linear fit between two datasets which have a very different range of variability: the particle fluxes vary over nearly six orders of magnitude, the anomalies vary by less than a factor of ten. I think this cannot work. Everything lower than

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about 10% of the maximum value of the x vector (particle flux) will be interpreted as essentially zero by the fitting routine, so whatever threshold value you derive here is probably within the uncertainty of the fit. You would see this clearly if you plotted the values on a linear scale – you lose the information about the lower flux values if you plot the flux on a linear, and not on a logarithmic scale, and the same is true if you do a linear fit on these values. You can see quite clearly in the left panel of Figure 4 that the fit did not work – just look at the black dots and black line (WACCM-D data and fit): for fluxes between 10 and 100, y-values are still rather high, but the dots are mostly to the left of the fitting line, that is, the lower flux values are not well represented by the fit. Only values with flux (SPE indicator) values above about 100 are well represented by the fit, as only those can be considered if the SPE indicator is used as a linear parameter. This also means that you overestimate the threshold value, and I think that this has to happen: the linear fit provides an artificial upper limit of about 10% of the highest value. If you just look at the black dots – there are a lot of dots between SPE flux values of 10 and 100 which are significantly above the fitting curve. If you just look at these dots, the threshold is probably around 10, not larger than 100 as your fit provides. I think using the log of the flux (SPE/RBE indicator) and a non-linear fitting function (polynomial) will provide a much better fit which also can account for the low flux values. If you do this with a multi-linear regression algorithm, you can still use the correlation coefficient as a measure of fitting quality.

[Thank you for the insightful comment. We have re-examined our threshold determination method. In addition to the suggested use of a logarithm of EPP flux values, we also tried our method using a square root of the EPP fluxes. The latter was inspired by the previous work of Verronen et al. \(2011\). As suggested, we tested a 2nd degree polynomial with the logarithm, and one standard deviation as limit of significant concentration enhancement \(see also the next comment and response\). We also used one standard deviation for the square root method, but used a first degree polynomial](#)

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like Verronen et al. (2011).

As suggested by the reviewer, the threshold values are indeed lower for SPEs for both tested methods when compared to our original approach. For electron precipitation, however, the tested methods resulted in higher threshold values, likely due to the use of one standard deviation instead of only a half.

In our analysis, the two tested fits were of similar quality and better than our original method. For SPEs, the square root method produces a factor of ≈ 2 larger threshold values than the logarithm method. After consideration, we have selected the results from the square root fitting method for the revised paper. This is mainly due to higher correlations produced by the square root method, leading to greater altitude-latitude extent of detected threshold values. This is somewhat to be expected, as the square root method uses a linear fit compared to the second degree polynomial used with the logarithm method. This difference in number of detected threshold values could therefore perhaps be accounted for by lowering the correlation limit, but we do not think there is much room to lower the current limit of 0.35 without losing in the screening effect. We also consider the chemistry-based reasoning behind the square root method to be in its favor (Verronen et al., 2011).

In the revised manuscript, Figures 4, 8–10, and 12–13 were updated (Figure 11 was removed as suggested by referee #2), and text was changed accordingly. Mostly this was changing the description of the method and the threshold values, since qualitatively our results and conclusions did not change.

- I think using half the standard deviation as a measure of significant enhancement is too low – this would be still in the noise floor. When you do the fit against $\log(\text{flux})$ as

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suggested above, you can probably afford to use one standard deviation, and still get a lower threshold.

We changed the method to use one standard deviation. As stated above, the detected SPE thresholds (STD + SQRT fitting) are still lower than the originals. However the electron precipitation thresholds are higher with the revised method, at least partly due to the change from a half to full STD. Using half STD with the square root results in threshold values similar or slightly lower than originally, but one STD raises these thresholds to be mostly around 1.5 times greater than with the original method. Regardless we still choose the revised method with one STD as the limit, since the method works clearly better with SPEs.

Technical corrections:

Page 4, lines 19-20: I think it would be more consistent to use the WACCM density for conversion, not the MLS HO₂ density. Because even if the output format for WACCM species may be mixing ratio, internally number densities are likely used for the calculation of photochemistry.

Both WACCM and MLS provide mixing ratios, although we use concentrations in the fitting. Conversion is made consistently for both, using MLS-derived total densities. Since we are aiming at EPP detection and not chemistry in detail, we do not think this is a crucial factor. Differences in total densities between WACCM-D and MLS should be relatively small in the middle atmosphere compared to the maximum changes in HO_x from EPP (from up to hundreds of percent to an order of magnitude).

Page 4, line 21: note formatting of HO₂

We have corrected the formatting in the revised manuscript.

Page 5, line 18-20: but would you not expect a difference in the HO_x loss rates between summer and winter which likely affect the observed increase?

We do not expect a large impact from the possible HO_x loss differences between summer and winter. HO_x loss is controlled mainly by HO_x recombination and reactions with atomic oxygen (Canty and Minschwaner, 2002). The chemical lifetime of HO_x is less than one day at altitudes below 80 km (Pickett et al., 2006). The larger impact will come through background HO_x production which has both seasonal and diurnal variability. However, our analysis takes this into account by using climatology-corrected anomalies and by considering day and night times separately.

Page 6, line 30: missing full stop after “levels”

We have corrected this in the revised manuscript.

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