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Interactive comment on "Statistical response of middle atmosphere composition to solar proton events in WACCM-D simulations: importance of lower ionospheric chemistry" *by* Niilo Kalakoski et al.

Anonymous Referee #1

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This paper analyses the impact of D-region ion chemistry on the middle atmospheric composition responses to solar proton events by means of superposed epoch analysis of standard WACCM and WACCM-D simulations covering 1989-2012. The authors identify important differences of simulated responses for NOx, O3, HOx, and Clx, highlighting the importance of including ion chemistry reactions in models used to study EPP. This is a relevant result, particularly when considering that EPP is increasingly considered in climate models as a part of the solar forcing. The paper is written in a clear and concise manner. Overall, I recommend publication after addressing my

comments below, most of them being minor.

General comments:

1) I would have liked to see an analysis separating for seasons instead of (or in addition to) the analysis for SH and NH. This is motivated by the strong dependence of the SPE responses on the prevailing illumination and dynamical conditions (i.e., photochemistry, polar winter transport etc.), being particularly relevant for chlorine responses. I understand that the main purpose of the paper is to identify the impact of explicit D-region ion chemistry - being probably less affected by seasonal impacts (though much of the discussion is dedicated to gas phase chemistry impacts) . I'm further aware that such analysis implies additional problems (e.g., different ionisation levels during different seasons due to the uneven distributions of SPEs). In this sense, I'm not insisting in such additional analysis. However, if not included, the authors should at least be more quantitative about the prevailing seasonal conditions in the NH/SH. It is not sufficient to only mention that "the strongest SPEs occurred during NH winter".

2) It is very difficult to compare the presented results quantitatively with other studies dealing with individual events. This could easily remedied by providing a number that expresses the epoch ionisation level as fraction of that of a well-studied event such as the Halloween SPE.

Specific comments:

p1 l18: geomagnetic latitudes above 60 deg

p2 l8-9: none of the cited studies deals with SPE impacts.

p2 l11-16: This paragraph is confusing as it mixes up the different aspects "direct vs indirect ozone impacts" and "depletion by HOx/NOx chemistry vs increases due to chlorine buffering". I recommend to reorder this paragraph in the following manner: 1) simulated TOC decreases (Jackman et al. 2014) and reported local depletions in the lower stratosphere (Denton et al. 2018) 2) Lower stratospheric decreases are indirect effects (Jackman et al, 2011) 3) On the other hand, local chemical impacts (chlorine buffering) may compensate indirect effects in the lower stratosphere (Jackman et. al., 2008).

p2 l28: Differences

p3 l2: . . .led to AN improved. . .

p3 l12: . . .to A number of. . .

p3 l12-13: The responses are not studied here in dependence of background atmosphere or illumination. In this sense, the statistical analysis performed here allows only for an evaluation of the CLIMATOLOGICAL response. Also, the “timing” (l14) is not studied explicitly in this paper.

p6 l24: Interestingly, there are apparently SPE-related short-term increases in the NH (not visible in the SH) which, however, occur slightly BEFORE the SPE onset. Any explanation?

p6 l25: Since 5hPa ozone responses are seen throughout the epoch period they are likely not caused by SPE. Instead they might be related to UV-induced solar cycle effects.

p6 l26: It might be possible that TOC decreases as reported by Denton et al. would be visible in an analysis restricted to polar winter (as done in the cited study).

p7 l2: Isn’t the lowering of the peak altitude related to the increasingly (with altitude) smaller availability of water vapour during solar maximum conditions? How can HOx then increase at below 0.001 hPa? Please explain!

p7 l7-8: Short-term decreases (during nighttime conditions) as observed in 2005 in the NH are likely caused by conversion of ClO to HOCl in the presence of enhanced HO2 (see e.g. Funke et al., 2011). An increase after 30 days, as seen here, is not a short-term increase! A more plausible explanation - at least for the SH negative anomaly

- appears to be the descent of NOx and subsequent formation of ClONO2 (note that the NOx contours at 5 hPa seem to decrease with time more pronounced in the SH compared to the NH).

p7 l12: Isn’t it more relevant in this context that HOCl is converted to ClO during DAYTIME?

p7 l16: While the few strongest SPEs occurred in NH (early winter), it is not easy to infer if this is also true for the epoch average.

p7 l20: THE response

p7 l22: This is not easy to infer from Fig. 4. Do the contours within the white areas (with low significance) indicate decreases or increases? If they indicate decreases, then it would look more like the NH events being more short-lived compared to the SH events.

p7 l24: If winter conditions - allowing for descent of thermospheric NOx - were prevailing in the NH, shouldn’t the anomaly be more pronounced there compared to the SH?

p8 l8-9: Why only in the SH? reduced HOx responses in WACCM D occur in both hemispheres. Could it be that, in the NH, Clx increases outweigh HOx increases in contrast to the SH?

p9 l9: a stronger

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