Review of the manuscript "*The Impact of a Solar Extreme Event on the Middle Atmosphere, a Case Study*" submitted by Reddmann et al. for publication in ACP

Reddmann et al. describe results from simulations with the KASIMA 3D atmospheric circulation and chemistry model for the impact that an extreme solar particle event may have on atmospheric chemistry and surface UV radiation under present day conditions.

The model is forced by ionization rates estimated for an extreme event that has likely occurred in 774/5 CE. Tropospheric and stratospheric circulation of the KASIMA model are nudged to a specific meteorological situation from boreal winter 2008/2009 which includes a sudden stratospheric warming event that is considered to favor a strong impact of the particle event.

The topic is well suited for publication in ACP, the paper is in general well written, the methodology and the explanation of atmospheric chemistry responses seem sound to me. However, I think what the paper crucially lacks to be considered for publication is that the simulation results are put into context a) of an earlier simulation of such an extreme event, b) of the response to weaker, frequently simulated solar particle events, and c) of the role of the atmospheric background conditions.

a) There is an earlier simulation of impacts of the same 774/5 CE event by Sukhodolov et al. (2017) that is mentioned several times in the manuscript. However, I think the manuscript requires a very clear discussion of the question what we learn from the new study that we haven't learned from Sukhodolov et al. (2017). I understand that the new manuscript focusses much more on atmospheric chemistry than the earlier study, however, also Sukhodolov et al. show the response of NO_x and O_3 for a period of half a year after the main event. Why not even put a figure that compares the two results with the same color scale and discuss the differences? Also Sukhodolov et al. briefly mention the likely minor impact on surface UV, admittedly using a different metric, but also here a discussion of similarity and differences would be very useful. Sukhodolov et al. go beyond the current study in the sense that they are not nudging to observations but run their model freely such that also the circulation responses can be analyzed. I think that this paper needs a short discussion of possible feedbacks from suppressed circulation and temperature responses on the chemistry. I know that the authors state this "needs more simulations … and will be addressed in a follow-up paper", but I guess there is experience from earlier studies which allows to estimate the potential importance of this.

b) The authors mention several other studies where effects of contemporary particle events have been observed and simulated. However, I don't think they sufficiently discuss differences between weak and strong particle events. For all their discussion of the response of atmospheric chemistry I would like to know if these responses are typical for particle events and just stronger because the ionization rates are larger. There is some discussion of this in Section 5.1 where for the total extra NO_y it is said that it approximately scales to contemporary events as the ionization rates do. I would consider this the central result of the study. At least for total NO_y, we can estimate the

effect of events by simply scaling simulation results by the ratio of ionization results. Very useful, because we may not need new simulations for new events. Is this true beyond just total NO_y increase? Are there non-linearities in other parameters? I guess there should be for example in ozone, but I'd like to see this discussed.

c) The question if the background conditions matter is partly related to a) because Sukhodolov et al. point out that they tried to do their simulations for historic 774/5 CE conditions. However, it is also prompted by the authors emphasizing already in the first sentence of the abstract that effects are "studied for the present-day climate and geomagnetic conditions". If this seems so important, I think some discussion is necessary about the implications of this assumption.

I'm listing a few further minor issues ordered by appearance in the text:

L10: "The results show a strong enhancement of NOx which causes a substantial decrease of ozone in the mesosphere and stratosphere, and a significant decrease of total ozone." I think the same sentence could have been written for a contemporary strong particle event due to the vagueness of the terms strong, substantial, and significant. I would like the authors to be more precise.

L23, figure headings, etc.: "additional ozone destruction". I guess that the authors want to point out that this ozone destruction is additional to the annual destruction by halogens, or only the anthropogenic destruction. However, as it isn't said anywhere and not quantified either how much the original destruction is, I find "additional" rather misleading.

L68: L* is not defined.

L72, and many other places: GMS, not GSM

L76, and several other places: "ionization production rate"? Rather "ionization rate" or "ion pair production rate", I guess.

L84: "ozone is the most important radiatively active gas for solar radiation". I think this depends on the metric.

L105: What are "basic equations"?

Fig.2 and others: I'd suggest to use color scales where not large parts of the signals are outside of the scale.

L171 "a kind of optimum" sounds very vague.

L176, and other places: Is "NO_y input" or "injection" really an appropriate term? The additional NO_y is produced in the atmosphere.