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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of January 2005 solar proton events on chlorine species" *by* A. Damiani et al.

Anonymous Referee #1

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In their paper "Impact of January 2005 solar proton events on chlorine species" Damiani et al. describe and discuss satellite observations and model simulations of chlorine species during a solar proton event. I very much like the approach of the study where "nudged" model simulations are compared directly to observations, and model simulations with and without proton forcing are compared to each other. This enables a discussion if anomalies in species concentrations are related to the proton event or to the specific meteorological conditions. The presentation and discussion of results is exhaustive, in places, I feel too exhaustive. The readability of the paper may benefit from an attempt to present results in a more condensed way. It is very difficult to judge from the paper which of the results are details and which represent important and new scientific findings. I think the authors fail in providing motivation and goal of their study in the beginning and in concluding on scientific progress achieved in the end of their

manuscript. In the introduction, it is written that "the variability of chlorine species is a further interesting consequence of solar protons" and that "this has been investigated only in a few studies". I would rather like to know why it is interesting, which questions have been left open by the previous studies and why it can be expected that the chosen approach will help in answering these questions. Later in the introduction it is said that "the study of these changes is very useful for validating the chemical schemes used in current atmospheric models". I agree. However, if this is the aim of the paper it should be said explicitly. The conclusion section in the end is for my understanding mostly a summary of observations and simulations and their differences. There are very few concluding remarks. As a conclusion I consider that some results are "further corroborating the hypothesis of chlorine activation under SPEs". If this was the main goal of the study it should be stated and I think would need a somewhat more extensive assessment. Because, as said earlier in the manuscript, there are also hints for a deactivation later after the SPE. I would like to see clearer statements in the conclusion on the main points that can be learned from this study.

I would classify the changes that need to be made to the study to deserve publication as "minor" because I do not see the need for redoing experiments or cosidering many further observations. But I would like to encourage the authors to better put their work into the context of existing open questions and let the reader know more explicitly about the scientific progress obtained in the study.

Specific comments:

- P1938L11: "short-term O3 depletion ... principally caused by NOx" Isn't HOx fairly important for the short-term depletion?

- P1938L18 to P1940L4: This paragraph seems important to me. It could be used to make the existing knowledge and potential gaps and the possible role of this study even clearer. Some suggestions for specific changes in this section: a) I understand that it is generally assumed that the influence of SPEs is indirect, i.e. via the increase of NOx and HOx. This should be stated in the beginning. Could there be a direct influence? And how would the mentioned ion reactions work? b) "first experimental confirmation of (R1)" Do you mean, a hint that this reaction may be important for the SPE influence? c) (R4) Later in the paper the chain HCl->ClO->HOCl is often mentioned. It would be good to state already here that increased HO2 may lead to a shift of the balance between ClO and HOCl towards the latter, so that the observed decrease of ClO becomes easier to understand. d) (R5) Was this not included by Jackman et al. (2008)?

- P1944L20: "Due to ..." I disagree. The identification of HOCl signals does not depend on the absolute concentration but on the noise.

- P1944L27: It is claimed that the SPE effect on HCl in December 2006 "is not as evident as in 2005". I have the impression that it is comparable in magnitude (0.2 ppbv?) but just less well identifiable because of the thin cyan line.

- P1947L21: "finer resolution" than what?

- P1948L22: "MLS shows that the vortex is well defined ... whereas ..." I disagree, smaller latitudinal gradients may also occur with a well defined but displaced vortex.

- Chapters 6 and 7: I know that it is common to split the description of the results from their discussion. I would prefer to combine sections 6 and 7 in order to avoid repetions in chapter 7 and the necessity for the reader to jump back an forth. But of course, this is my personal preference, only. Even if separate chapters 6 and 7 are kept I would suggest to better structure them by introducing subsections.

- P1950L17: "... HCl depletion below 4-5 hPa is not ascribable to SPE impact" But also the (p-b) comparison shows a decrease of HCl close to the SPE date for all latitudes. It’s just comparably small at the lower levels.

- P1951L1: "The decrease of ClO starts before the occurrence of the SPEs and therefore it masks the influence of solar forcing." I agree that this is a likely possibility, but can it be excluded that there is simply no SPE influence?

- P1951L5: "Therefore, active chlorine exists mainly in the form of HOCl ..." What is the "therefore" referring to? I would guess that the reason for having active chlorine mainly as HOCl is the HO2 increase caused by the SPEs. Or am I wrong?

- P1952, discussion of the "model-data" HCl discrepancies: It is interesting that no clear reason can be found. But can we be sure that the depletion below 4hPa is not SPE-related? Would it be useful to look at other events, e.g. December 2006 which is included in Fig. 1?

- P1953L14 "does not indicate a significant SPE-induced ClONO2 response" Looking at Fig. 7, top middle and right panels, I would guess that there is a significant response. Maybe it is small. To judge if a change is small or large, occasionally it would be good to provide changes not only in terms of absolute mixing ratios but also as a percentage.

- Fig. 7, lower panels: Why is the ClONO2 response strong only close to 1hPa?

- P1955, discussion of HCl depletion below 5hPa. Can we be certain, that it is unrelated to the SPE? See above.

- P1956, discussion of the relevance of ion chemistry: I do not agree to the comment by H. Winkler. I do not see that the WACCM results indicate that ion chemistry is unimportant. The HCl response is underestimated at 58-70N. In several places it is mentioned that lateral mixing maybe underestimated by WACCM. If this is true than the realistic reproduction of the HCl concentrations at polar latitudes may be the result of compensating errors: too little depletion plus too little mixing.

- P1957L7: I guess the "neglection" of ion chemistry is meant.

- P1960L14: "models tend to underestimate the actual isentropic mixing" Is that true for models in general? Is there a reference for this statement?

- P1962L1: "For the first time ..." I guess this is thought as evidence for the originality of the work. Comparison of observed and simulated (even multi-model) chlorine species have been presented e.g. also by Funke et al., 2011. I would rather judge the originality of a study by its outcome and not that something was done for the first time.

Technical comments:

- Figs. 4 to 7: Please use always a color scale where green is indcating zero change. It is difficult to identify increases or decreases when zero change is indicated by yellow in some and blue in other plots.

- P1936L8: "HCl decrease ... with the lowest values (of less than 0.25 ppbv)" This can easily be misunderstood. I would talk about the "strongest decrease". Similar issues with the wording pop up in several places of the manuscript, e.g. P1950L8, P1962L10,

- P1938L12: "In this sense ..." Unclear. In which sense?

- Fig. 1: It is not clear to me why two vertical levels are shown that are fairly close to each other (about 3km), in particular given the relatively coarse vertical resolution of MLS data. A level in the middle stratosphere may also be helpful concerning some discussion in the later parts of the manuscript, e.g. the question if the observed depletion of HCl below, say 5hPa, is SPE induced or not.

- P1946L26ff. It would be nice to rewrite the comparison of the proton fluxes for the different events in an easier to understand way, not to jump from one altitude to another event and back to some altitude.

- P1947L15: For which altitudes are these numbers valid?

- P1947L29: "ionization rate".

- P1950L13: "p-b"

- P1956L24: It sounds funny that depleted molecules originate somewhere.

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