acp-2011-920 - Final Response

We thank Anonymous Referee #1, Referee #2 and Dr. H. Winkler for their good opinion on our work and for the useful recommendations to improve the work.
Here we first report their comments then we provide our responses (in our replies, Pages (p) and lines (l) refer to the revised manuscript).

Anonymous Referee #1

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 considering 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.
- Reply: In the introduction of the revised manuscript we have made clear the goals of this study in the context of the previous open questions. The discussion section has been rearranged. Moreover we have inserted in the conclusions additional statements on the main points that can be learned from the present study.

Specific replies included in the revised manuscript:

- why it is interesting, which questions have been left open by the previous studies
- Reply: none of the few past studies, investigating SPE-induced impact on chlorine components, reported on variations in all the main components (i.e., HCl, ClONO2, ClO and HOCl) of the chlorine family during a single SPE. This is an essential condition to correctly analyze SPE effects.
In prior studies (Jackman et a., 2008; Winkler et al., 2009; Funke et al., 2011), which focused on data vs. models comparison, a good agreement was found mainly for HOCl but some uncertainties still remained for HCl (Winkler et al., 2009) and ClO and ClONO2 (Jackman et al., 2008; Funke et al., 2011).

A limitation in these studies (i.e., Jackman et al., 2008; Funke et al., 2011) has been the lack of measurements of the most important chlorine reservoir (i.e., HCl). On the other hand, when HCl has been investigated (Winkler et al., 2009), satellite observations of other components were missing.

- why it can be expected that the chosen approach will help in answering these questions

- Reply: - The impact of the SPE on HCl has been investigated only during the SPE of July 2000 by means of HALOE observations recorded at 62°-69° N (Winkler et. al. 2009). The modeling findings of Winkler et al. (2009) show the necessity to consider the influence of the ion chemistry on HCl mainly in mesosphere. However they have not been able to investigate the temporal evolution of SPE-induced changes since the employed satellite dataset includes only three days after the event. In our approach we did not consider ion chemistry so we indirectly tested its importance in the lower mesosphere. On the other hand we had the possibility to study in detail the temporal evolution of the chlorine changes by a larger dataset from polar orbit satellites, which provides a more representative picture of the polar cap region with respect to the prior work (i.e., Winkler et al., 2009).

- Some difficulties in reproducing the effects of the SPEs of Oct-Nov 2003 (von Clarmann et al., 2005; Jackman et al., 2008; Funke et al., 2011) are likely due to the early winter vortex being partially illuminated. In January 2005, the vortex, well established and coincident with the terminator, should allow better modeling results.

- Some evidence has been provided (Funke et al., 2011) that the model vs. data discrepancies could arise also from the underestimation of the ClO "background" (i.e., ClO VMRs before the event) in the upper stratosphere at higher latitudes. The close reproduction of the observed ClO "background" before the SPEs of January 2005 should improve the modeling of SPE-induced ClO changes. Because ClONO2 is strictly dependent on ClO, this is expected to be valid also for ClONO2.

- goal of their study

- Reply: - to test the ability of WACCM4 to reproduce changes induced by the January 2005 SPEs

- to validate chemical schemes used in current atmospheric models

- to investigate the temporal evolution of HCl changes without the inclusion of the ion chemistry in the model.

- to check if the favorable conditions of the vortex, roughly coincident with the terminator, allows the reproduction of changes in ClO and ClONO2 better than in prior studies;

- to corroborate the hypothesis of chlorine activation under SPEs

scientific progress achieved

- Reply: - The difficulties encountered by Winkler et. al. 2009 in reproducing the HCl depletion in stratosphere have been confirmed. This data-model discrepancy could suggest that deficiencies in chemical schemes involving chlorine species in the upper stratosphere could be present.

- Despite the lack of ion chemistry, WACCM4 reproduces (underestimates) mesospheric HCl changes induced by the SPE of 17 January at latitudes higher (lower) than 70 N. Since lateral mixing is probably underestimated by WACCM4, the realistic reproduction of the HCl concentrations at higher latitudes may be the result of compensating errors i.e., too little depletion, caused by the lack of ion chemistry, plus too little mixing. Therefore we cannot exclude the importance of the ion chemistry in the mesosphere. Addition investigations, taking into account the energetic electrons ionization, are necessary.

- Mesospheric HCl changes induced by the SPE of 20 January appear to be overestimated at higher latitudes. Since prior work (Verronen et al., 2011) point out the importance of the horizontal mixing in the mesosphere on 21 January, this is a further hint suggesting that horizontal mixing could be underestimated in WACCM4 during this period.

- The chlorine activation under the SPEs of January 2005 has been shown. In addition, an HOCl enhancement accompanied by a similar decrease in HCl was observed by MLS also during the SPEs of December 2006 (i.e., under similar seasonal conditions), further corroborating the hypothesis of chlorine activation under SPEs.
Hints for a deactivation later after the SPEs of January 2005 have been highlighted; however, WACCM4 did not fully reproduce this, so no clear conclusions can be drawn.

Specific comments:
- P1938L11: "short-term O3 depletion ... principally caused by NOx" Isn't HOx fairly important for the short-term depletion?
-Reply: (p.2, l.30) you are right; we have added HOx.

- 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.
Reply: (p.2,l.39-46) Agreed

Could there be a direct influence? And how would the mentioned ion reactions work?
- Reply: (p.2, l.101 to p.3,l.5) To our knowledge SPE-induced changes in chlorine species can occur only via reactions with HOx and NOx (i.e., indirect influence); the direct SPE-induced influence on chlorine species has not been reported. However, several negative ions react with HCl and form negative chlorine species (e.g., Cl-) mainly in the mesosphere. Then they can react with H to reform HCl, or with positive ions releasing e.g., Cl, ClO. The increasing ion production rates during a SPE can change the concentration of charged chlorine species and it results in a final net HCl depletion (additional to the depletion induced by OH) and an increase of other neutral chlorine species (e.g. Cl, ClO) (see Winkler et al., 2009). We added this information in the revised manuscript.

b) "first experimental confirmation of (R1)" Do you mean, a hint that this reaction may be important for the SPE influence?
- Reply: (p.2, l.54) Modeling findings of Solomon and Crutzen, 1981 highlighted the importance of this reaction under SPE conditions. Lopez Puertas et al., 2005 and von Clarmann et al., 2005 reported that SPE-induced ClONO2 changes were recorded by MIPAS during the SPEs of October-November 2003. Accordingly, we have rephrased the sentence.

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.
- Reply: (p.2, l.70-74) In the revised manuscript we have mentioned in the introduction the importance of SPE-induced HO2 enhancement for the balance between ClO and HOCl.

d) (R5) Was this not included by Jackman et al. (2008)?
- It was included.

- P1944L20: "Due to ..." I disagree. The identification of HOCl signals does not depend on the absolute concentration but on the noise.
- Reply: You are right, we have removed this sentence.

- 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.
- Reply: (p.5, l.57-60) Yes, you are right. Indeed chlorine changes occurred in December 2006 are a further corroboration of the presented results. HCl depletion in December 2006 is not very evident in Fig. 1 because the SPE-induced variability is comparable to the HCl climatology; in contrast SPE-induced variability in January 2005 comes out from the climatology. At least at this pressure surface, the depletion in both events is comparable; however due to the different proton energy spectra of the SPEs of December 2006 compared with the January 2005 ones, no SPE-induced changes in chlorine species are present roughly below 3 hPa. We have rephrased the sentence.

- P1947L21: "finer resolution" than what?
- Reply: (p.6, l.78) Jackman et al., 2008; 2011 computed the ionization on daily basis. In the present work the ionization is computed on hourly basis. We have changed the sentence accordingly.

- 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.
-Reply: (p.7, l.24-26) In the revised manuscript we have stated that smaller latitudinal gradients may also indicate a 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.
- Reply: Since in the discussion section we often refer to prior achieved results, we feel that the separation is necessary; putting results and discussion in the same paragraph could be misleading. Nevertheless, following the reviewer's advice, we have rearranged the discussion section and introduced 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.
- Reply: (p.7, l.97) we have changed the sentence as follows: "…HCl depletion below 4-5 hPa could be only slightly influenced by the SPE impact".

- 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?
- Reply: It is not straightforward to recognize if ClO changes are entirely caused (or not) by SPEs. However it is improbable that ClO was not influenced by SPEs. Fig. 6 shows that strongest MLS ClO depletion occurs at higher latitudes. Time series like Fig. 4, performed at higher latitudes (i.e., 75-82 N; not reported in the paper), clearly show two definite MLS ClO depletions soon after the SPEs occurrence (i.e. on 18 and 21 Jan). Moreover, also the b-p model results corroborate a SPE-induced ClO depletion.

- 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?
- Reply: You are right; we have removed this sentence.

- 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?
- Reply: it is unlikely that HCl changes below about 4-5 hPa are due to SPE occurrence alone, but we cannot exclude that some small SPE-induced HCl depletion could occur also there. We have pointed out that in the revised manuscript. SPE-induced HCl changes in December 2006 are not evident at and below 3 hPa; however, in December 2006 the proton flux at higher energies, which affects lower altitudes, is less than the January 2005 events.

- 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.
- Reply: (p.9, l.7) Thank you for pointing this issue out. Even though the caption indicates "model SPE response (perturbed – base)", the right panels of Fig. 7 accidentally show differences in ClONO2 background (i.e., WACCM4 simulation without proton forcing). The error has been fixed and now right panels of the new Fig. 7 show the actual model SPE response (perturbed – base). Now WACCM(p-b) differences do not show any SPE-induced ClONO2 variation in the upper panel. In contrast, in the lower panel the SPE-induced ClONO2 maximum, located at latitudes higher than 70 N, is roughly between 1 and 2 hPa and degrades slowly toward lower altitudes. Note that the new Fig. 7 now includes also the original model result without the application of MIPAS averaging kernels. Accordingly, in the revised manuscript, we have somewhat corrected this section; however conclusions remain unchanged.
Absolute ClONO2 VMRs in top middle and right panels are about 1 ppbv below around 6 hPa so changes of about 0.02 ppbv in the top middle panel do not seem to be significant when compared with the background variability.

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

- P1955, discussion of HCl depletion below 5hPa. Can we be certain, that it is unrelated to the SPE? See above.
- Reply: As stated in the text, we cannot exclude that some small SPE-induced HCl depletion could occur also there but it is unlikely that the HCl depletion below 5hPa is mainly related to SPEs.

- 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.
- Reply: We agree that one possible explanation of the realistic reproduction of the HCl concentrations at polar latitudes may be the result of compensating errors and we have stated it also in the conclusion. Therefore we cannot exclude the role of the ion chemistry in mesosphere.

- P1957L7: I guess the "neglection" of ion chemistry is meant.
- Reply: (p.10, l.36) Yes, you are right.

- P1960L14: "models tend to underestimate the actual isentropic mixing" Is that true for models in general? Is there a reference for this statement?
- Reply: (p.12, l.7) Funke et al., 2011 showed that the majority of models (including WACCM4) present a pronounced ClO gradient at latitudes greater than 70 N before the October 2003 SPEs occurrence, whereas MIPAS showed only a weak decrease of ClO

VMRs from 40 to 90 N. This is why we mentioned this possibility. We slightly changed this sentence in the revised manuscript.

- 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.
- Reply: (p.12, l.23-26) We agree that the originality of a study should be judged by its outcome. The sentence is meant to point out that, differently from prior studies (e.g., Funke et al., 2011), the present work analyses SPE impact on chlorine species, HCl included. Since HCl is the most important chlorine reservoir, and many uncertainties (confirmed by the present study) are related to it (Winkler et. al. 2009, 2011), it is important to point this out since the originality of our study arises also from this. In the introduction and in the conclusions of the revised manuscript, we have explicitly mentioned the importance of analyzing the response of chlorine species to SPEs taking into account also HCl.

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.
- Reply: You are right but this is a limitation of the adopted software. To indicate zero change with the same color in every plot, we should put all color scales symmetric (e.g. from -0.2 ppb to 0.2 ppb) or similarly ranged, but in this way SPE-induced changes would not be evident.

- 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,
- Reply: (p.1, l.11-12) Agreed.

- P1938L12: "In this sense ..." Unclear. In which sense?
- Reply: (p.2, l.30) We have rephrased the sentence in the revised manuscript.

- 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.
- Reply: (p.5, l.21-25) despite the relatively coarse vertical resolution of MLS data, we showed two vertical levels fairly close to each other and near the upper limit of the MLS data use because chlorine changes, caused by the SPEs of December 2006 and by SSWs, are visible mainly there. On the other hand, it is not interesting to show levels around 5 hPa because nighttime ClO and HOCl VMRs and SPE-induced changes are almost zero there. We have added this information in the revised manuscript.

- 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.
- Reply: (p.6, l.50-51) Agreed

- P1947L15: For which altitudes are these numbers valid?
- Reply: (p.6, l.67-68) In the mesosphere, roughly between 1 and 0.01 hPa (about 50-80 km). We have added this information in the revised manuscript.

- P1947L29: "ionization rate".
- Reply: Agreed

- P1950L13: "p-b"
- Reply: Thank you.

- P1956L24: It sounds funny that depleted molecules originate somewhere.
- Reply: We have removed it.

Anonymous Referee #2

The paper presents the results of the MLS and MIPAS observations of the chlorine species during the SPEs of January 2005 which are compared to WACCM4 model results. The paper is interesting, it gives some new insights and deserves publication on ACP. My comments are below:

COMMENTS

1) p. 1936 lines 8-9: Please explain more clearly the investigated altitudes.
- Reply: (p.1, l.11) In the revised manuscript we have explicitly indicated that the investigated altitudes are between 10 and 0.5 hPa.

2) p. 1938 line 6: Please explain why there are only a few HOCl observations available.
- Reply: (p.2, l.16-23) The reason of the small amount of HOCl observations is twofold. First, this is due to the fact that it is not a very useful indicator of the atmospheric chlorine content from human made CFCs. Traditionally, long-term increases in the gaseous inorganic chlorine content of the upper atmosphere have been more easily observed recording HCl, that is the main stratospheric reservoirs of chlorine, and/or recording ClO. Then, there is also a technical difficulty due to the weak nature of HOCl transitions in the microwave/millimeter wave region and in the infrared windows, and the low concentration of HOCl and low signal/noise ratio. We have added this information to the revised manuscript.

3) p. 1939 lines 16-18: But HCl goes via R2 to Cl which is also a ODS, could you please say which of the reactions, i.e. R2 or R5, is faster.
-Reply: (p.2, l.82) Values from Sander et al., 2011:

	A-Fact.	E/R	k(298 K)	f(298 K)	g
R5a OH + ClO → Cl + HO_2	$7.4x10_{-12}$	–270	$1.8x10_{-11}$	1.2	50
R5b OH + ClO → HCl + O_2	$6.0x10_{-13}$	–230	$1.3x10_{-12}$	1.7	100
R2 OH + HCl → H_2O + Cl	$1.8x10_{-12}$	250	$7.8x10_{-13}$	1.1	50

The branching ratio for R5a and R5b is around 7 %. Please note that in Sander et al., 2011 error limits for R5b have been reduced with respect to Sander et al., 2006 but the recommendation is unchanged.

4) p. 1944 lines 14-16: You just can say by making (not shown) one sentence out of the two.
- Reply: Agreed

5) p. 1944 lines 16-19: It is not clearly visible the HCl decrease, please ameliorate Fig.1
- Reply: The small reproduction of Fig.1 is due to the particular format of ACPD online journal. Fig. 1 will be ameliorated in the final ACP paper.

6) p. 1945 lines 10-11: Please say in which figure we see the HCl peak at 2 hPa in late February.
- Reply: (p.5, l.75) In the revised manuscript we have indicated that the HCl peak at 2 hPa, occurred in late February 2008, is visible in the upper-left panel of Fig. 1.

7) p. 1946 lines 10-11: Why did you not implement the energetic electrons? In the mesospheric regions these particles can have a impact which should not be underestimated.
- Reply: (p.6, l.10-20) We agree with the reviewer that the impact of energetic electrons could be significant in the mesosphere. However the paper focuses mainly on the middle/upper stratosphere where, generally, the electrons-induced ionization is somewhat small. We showed results up to 0.5 hPa only for HCl. However Figs. 4, 5 and 6 clearly show that WACCM4 does a good job in reproducing MLS HCl at the upper investigated altitudes, excluding (or at least reducing) the possibility of an underestimation of the

ionization induced by the significant electrons impact on this altitudinal range. In a future work we will implement the ionization induced by energetic electrons. We have explicitly stated this in the uploaded manuscript.

8) p. 1950 lines 4-5: You show the temporal evolution with respect to January average, why not take some days before the solar proton event started? Please explain your decision.
- Reply: (p.7, l.76-77) When studying changes connected to SPEs it is usual to show time series starting from one or a few days before the event and reporting the data as differences with respect to these days. In the present case, we investigate not only the period affected by SPEs, but also the period before the events (i.e. 1-15 Jan). Showing temporal evolutions with respect to January average provides the best representation not only for SPEs but also for the meteorological evolution. We have explicitly stated this in the uploaded manuscript.

9) p. 1950 line 8 and p. 1962 line 10: Saying 'of less than 0.25 ppbv' could mean everything, i'd rather say 'of up to xy ppbv'.
- Reply: You are right, in the revised manuscript we simply say "about 0.25 ppbv".

10) p. 1955 line 16: The sentence '...this long lasting tail of HCl decrease seems to be not connected...' it is better to say '...this long lasting tail of HCl decrease seems not to be connected..'
- Reply: Agreed

11) p. 1956 lines 20-21: Why not showing the plot where we see the even more intense HCl depletion, i would appreciate this.
- Reply: For congruence with other atmospheric components, available only up to about 1 hPa, we prefer not to show HCl VMRs at altitudes higher than 0.5 hPa in the manuscript. Since we did not implement electrons to compute ionization and it is know that they can produce ionization mainly in mesosphere (see reply to comment n. 7), the model vs. satellite data comparisons could be misleading there. The new MLS HCl V. 3.3 presents an upper limit of data use lower (at 0.3 hPa) with respect to the prior MLS V. 2.2 (at 0.15 hPa). MLS V. 2.2 data, not adopted in the present work, are probably more suitable to show changes at upper altitudes.

Below (Fig. 1) you can see the temporal development of daily HCl for (top to bottom) MLS data, WACCM results and the model SPE response (perturbed -- base) between 10 and 0.2 hPa averaged over 60-82 N. (Mixing ratio values where the a priori information has a strong influence are flagged with negative precision for MLS data, and they should not be used in scientific analyses. In our case HCl profiles are flagged positively up to 0.1 hPa so in this region there is not a strong influence of the a priori). The SPE of 17 Jan is reproduced quite well also in the mesosphere. The SPE of 20 Jan is underestimated by WACCM likely due to the underestimation of the intense horizontal mixing occurring in these days and region (see Verronen et al., 2011a).

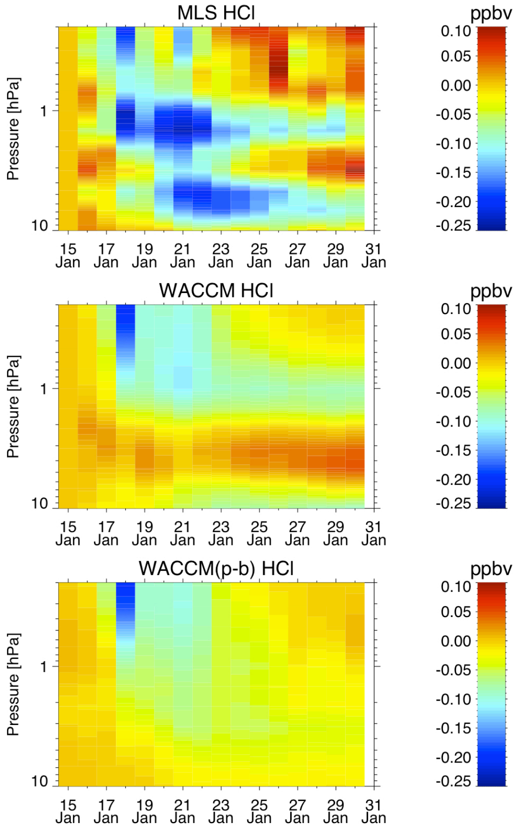

Fig.1 - Temporal development of daily HCl for (top to bottom) MLS data, WACCM results and the model SPE response (perturbed -- base) between 10 and 0.2 hPa averaged over 60-82 N.

12) p. 1957 lines 13-15: It is just written that on 27 January we see ClONO2 changes, you don't say when the first changes are visible, please add this information to the text.

Reply: As stated in Sect. 2.2, during this period MIPAS measurements were performed on 10–13 January (middle atmosphere observation mode), 16–18 January (upper troposphere-lower stratosphere (UTLS) observation mode) and 27–28 January (nominal observation mode) and these data provided ClONO2 profiles used in this study. Additional upper atmospheric observations with a scan range of 40–170 km in terms of tangent heights, taken on 21–24 January are not considered in this study since they provide only marginal information on ClONO2. So, unfortunately, ClONO2 changes, likely starting a couple of days after the SPEs, cannot be documented by MIPAS data. Please note that even though the caption indicates "model SPE response (perturbed – base)", the right panels of Fig. 7 accidentally show differences in ClONO2 background (i.e., WACCM4 simulation without proton forcing). The error has been fixed and now right panels of the new Fig. 7 show the actual model SPE response (please see above our reply to referee 1). Moreover the new Fig. 7 now includes also the original model result without the application of MIPAS averaging kernels. Accordingly, in the revised manuscript, we have corrected this section.

13) p. 1960 line 21: It is written that the modelled ClO VMRs are similar to experimental values, but what experimental values are meant? Please explain.

Reply: (p.12, l.17) In the revised manuscript we have clearly stated "…modelled ClO VMRs are similar to MLS ClO values".

14) p. 1963 line 11: Again, please explain what you mean when you write 'experimental data'.

Reply: (p.12, l.71) In the revised manuscript we have changed "experimental data" with "satellite data".

15) p. 1972: Figure 1 shows a good idea but it is to small and there are to many lines, in this figure i would say less lines gives a better understanding.
Reply: Fig. 1 will be ameliorated in the final ACP paper (see the reply to comment n. 5).

16) p. 1974 Figure 3: It is written '.. with respect to the average...' average of what, please be more precise what you mean.
Reply: New sentence: "Zonal mean relative anomaly of N_2O, with respect to the N_2O average computed over 40–82 N, expressed in percent. "

Dr. H. Winkler

I think this is a really nice paper. It is an important step improving our understanding of chlorine disturbances after solar particle events.
There are only a few minor comments I would like to make:
As far as I understand, all model results shown are convoluted with the averaging kernels of MLS and MIPAS, respectively. When it comes to the satellite/model comparison this is of course reasonable. But I think it would also be interesting to see the raw model output, maybe for one selected case in order to get an impression of what the AKs do to the "real" atmosphere? On the other hand, if the effect of the AKs is small, this could maybe simply be mentioned in the text?
- Reply: (p. 6, l.25-29) We did not show the impact of the MLS AKs applied to the WACCM output because their effect is small. We have pointed that out in the revised manuscript. On the other hand the effect of the AKs is more evident for MIPAS data and we have shown this in the revised Figure 7.

Page 1937, Line 18: Rohen et al. (2005) report on O3 measurements, not on "other chemical components" as implied in the text.
- Reply: (p.1, l.56-58) Thank you for pointing this out. Indeed, the paper of Rohen et al., 2005 focused on ozone. The reported references refer to some recent studies about the impact of SPE of Oct-Nov 2003 on the atmospheric chemistry but few of them focus on "other chemical components". In the revised manuscript we have rephrased the sentence, accordingly. Moreover, the reference of Seppala et al., 2006 have been moved to p.3, l. 17 since it refers to SPEs of Jan 2005.

Page 1956, Line 20: The finding that there is no need to include any further ion chemistry but that HOx production does a good job to reproduce the observed mesospheric HCl depletion here is an important result (at least it appears to me). If I may, I would suggest to mention this in the conclusions again or event the abstract.
- Reply: We agree with Dr. Winkler that this is a significant point. It is important to highlight that we obtained substantial good reproduction of the mesospheric HCl depletion without ion chemistry. However one possible explanation of the realistic reproduction of the HCl concentrations at polar latitudes may be the result of compensating errors (see referee 1). Therefore we cannot exclude the importance of the ion chemistry in the mesosphere. We have stated this in the conclusions.

And finally, a question: HOCl from MLS is shown down to 1hPa. I do not claime that these values are "wrong" and apparently there is something going on during the SPE, but the recommended pressure range is 10-2.2hPa only. Is there any filter criterium applied to the MLS data for p<2.2hPa?
- Reply: (p.4, l.30-40) Studying chemical changes during the SPE occurrence is a "special" application of MLS data, hence data quality recommendations do not consider these particular conditions. In general the recommended pressure range reported on Version 3.3 Level 2 data quality and description document is conservative and often referred to a single MLS profile. Mixing ratio values where the a priori information has a strong influence are flagged with negative precision for MLS data, and they should not be used in scientific analyses. In our case HOCl profiles are flagged positively up to 1 hPa (comprised) so in this region there is not a strong influence of the a priori. This is the main filter to be applied to data. Moreover, in order to study SPEs, it is roughly common to extend the useful limit of satellite data use slightly upward (e.g., Verronen et al., 2011 for MLS data; Seppälä, et al., 2007 for GOMOS data; von Clarmann et al. 2005 for MIPAS data). This is because, under these circumstances, most species (e.g. NOy, HOx) greatly increase their abundance in the upper stratosphere/mesosphere reaching atypical high

VMRs and showing a higher than usual signal-to-noise ratio which allow the possibility of exploiting the data also at upper altitudes. In our case, the HOCl VMR under usual conditions is very low in the upper stratosphere and this leads to a poor signal-to-noise ratio. However, precision can be improved by averaging hundreds of profiles inside the region of interest like in this study. During the space weather storm of January 2005, HOCl VMR increases by a factor of 3 in the upper stratosphere with respect to typical pre-SPE conditions. In this way the increased signal-to-noise ratio allows us to get information related to the upper stratosphere. We have inserted these notions in the revised manuscript.

Additional references:

- Seppälä, A., P. T. Verronen, M. A. Clilverd, C. E. Randall, J. Tamminen, V. Sofieva, L. Backman, and E. Kyrölä (2007), Arctic and Antarctic polar winter NOx and energetic particle precipitation in 2002–2006, Geophys. Res. Lett., 34, L12810, doi:10.1029/2007GL029733.