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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Possible effect of extreme solar energetic particle event of 20 January 2005 on polar stratospheric aerosols: direct observational evidence" *by* I. A. Mironova et al.

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We thank Referee #3 for helpful comments and suggestions. The Referee's Comments are noted first and then we give our Reply to the comments in italic font. All the changes in text of the paper are highlighted by bold face.

The paper correlates a major solar energetic particle (SEP) event in January 2005 with spatially resolved polar stratospheric aerosol/cloud data from satellites. It is hypothe-

sized that strong ionization in the polar stratosphere caused by the SEP event leads to strong changes in the observed stratospheric aerosol extinction. For the troposphere, similar correlations have been hypothesized for several years and there it is argued that the influence of galactic cosmic ray ionization facilitates aerosol nucleation and a fraction of the freshly nucleated aerosol particles may grow to CCN size influencing cloud microphysics and eventually climate. In the present paper, a connection is sought for polar stratospheric aerosol being influenced by ionization from a major SEP event. While such a correlation would be quite interesting I have major difficulties with the presented evidence from the interpretation of satellite data. Very similar to reviewer #2, I suspect that all the enhancements in the NH data from Jan 2005 can be explained by the occurrence of Polar Stratospheric Clouds (PSCs) without ions from the SEP event having to play a role. Therefore, the paper cannot be published in its current form and I am sceptic that it can be published at all.

We realize that our interpretation of data is somewhat speculative. However, some features of the observed effect can hardly be directly ascribed to PSCs, as we discuss below in our reply. In our approach we interpret possible uncertainties in favor of the SEP effect, trying to keep the assumption within a reasonable range. We thus provide a conservative upper limit to the effect, which remains quite low, as we discuss in the Conclusions. We state that the effect is only marginally observable for a severely strong SEP event and is expected to be negligible for the majority of weak-medium-strong events. The true effect can be even smaller, we accept it and reformulate the conclusions and abstract, but we still believe that providing a reliable upper limit is important, especially considering that it (the upper limit) is essentially smaller than considered in many previous studies.

Formation of PSCs is a question of vortex temperature, air mass history, abundance of HNO3, etc., and PSC formation processes are quite complex (see, e.g., Peter, 1997; Carslaw et al., 1997). Different types of PSCs exist. PSCs are expected to occur exactly in the low temperatures of late January 2005 and for the altitudes, latitudes

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and longitudes where enhancements in the SAGE III data were observed. Therefore it would have to be proven by detailed analysis that the SAGE III observations cannot be explained by PSC formation. The current discussion at the end of Section 4.1. (p 14013) is clearly not sufficient in this respect.

It is correct that PSCs can be largely (or even entirely) responsible for the observed AEC changes in the polar Eurasia region (30W - 90 E) since DOY 25. However, neither the increase of α in the entire NH polar region since DOY 22 nor the increase of AEC in the SH on DOY 20 are consistent with the idea of PSC being the main reason for the effect. Another aspect is that soon after the event we found an increase of α in the entire NH polar region, indicating reduction of the effective size of particles. The effect of PSCs after DOY 25 is the essential decrease of α (viz. large particle size). Thus, the observed phenomenon before DOY 25 is unlikely to be directly related to PSCs. We have modified the text.

The SH data from the OSIRIS satellite shows only one day of 30% enhancement, which is, to me, insufficient to validate the claims of the paper. Another alternative explanation for the enhancements in the satellite AEC data could be volcanic injections into the lowermost stratosphere. This possibility also needs to be discussed and checked with available volcanic emission inventories.

The claim is not robust, we agree (but see above), but it is consistent with the previous study by Mironova et al. (2008). As the Reviewer suggests, we have checked that there were no volcanic eruptions at that time. The stratosphere is known to be very clean until mid-2007. It was so clean that some satellite instruments struggled to reliably measure aerosols in such small quantities. We have also checked that there was no bushfires that could inject aerosols into, and above, the tropopause region.

Another fact that speaks against the offered interpretation is that in the stratosphere

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aerosol growth (except for situations of PSC formation or for fresh volcanic injections) is generally a slow process as only very small amounts of condensable material are present. H2SO4 from the photolysis and oxidation of COS is thought to be the most important condensable species. Even if the SEP would lead to the nucleation of numerous ultrafine particles, it would then take much longer than 3 days for such particles to grow to sizes observable by SAGE III as there is just not enough condensable material present to grow the aerosol. Furthermore, once formed, such particles should persist for months in the stratosphere and should not disappear after a few days (Figs. 2 and 3).

We agree with this statement and apologize for unclear writing. We have rewritten this part of text to emphasize the role of PCSs in modifying the particle parameters since DOY 25. We also agree that there are some difficulties in theoretical interpretation of the results, but the models may still be missing some aspects (e.g., Kirkby et al., Nature, 2011).

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 14003, 2011.