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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Possible effect of extreme solar energetic particle events of September–October 1989 on polar stratospheric aerosols: a case study" *by* I. A. Mironova and I. G. Usoskin

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Reply to Comment of J.R. Pierce

We are thankful to Jeffrey Robert Pierce for his interest to our paper and for useful comments and suggestions. We have revised the manuscript addressing his concerns with all the changes being highlighted in italic below and in bold face in the text of manuscript.

There are several issues with this ACPD manuscript.

1. In Figure 3a, the surface area density increases from $\sim$ 3 to $\sim$ 12-20 $\mu m^2 cm^{-3}$ at altitudes 12-13 km around day 274. In Figure 3b, the effective radius doubles from about 0.25μm to 0.5 μm.

For fixed aerosol number, the surface area scales with the square of the radius, so a jump in surface area density from 3 to 12 $\mu m^2 cm^{-3}$ (a 4x increase) can be explained by the doubling of the radius of the particles without any increase in aerosol number (for a fixed distribution shape). There would, however, have to be an increase in aerosol mass to make this particles larger. On the other hand, a jump in surface area density from 3 to 20$\mu m^2 cm^{-3}$ could not be explained by holding the number fixed(for a fixed distribution shape, some additional aerosol number would be necessary), but regardless there would need to be an increase in total aerosol mass.

If the "ion-aerosol clear sky" mechanism were acting alone, the aerosol mass would be fixed. An increase in nucleation due to the increase in ions would lead to a *decrease* in the effective radius because the aerosol mass would be distributed across more particles, so they would all be smaller (on average). Thus, the addition of aerosol mass here (which could possibly be due to the decrease in temperature if this caused additional material to condense) is an *extremely* important result.

The authors do not highlight this additional-mass issue and appear to attribute most of the changes to the "ion-aerosol clear sky" mechanism in Section 3.2. In lines 11-16, "One is the homogenous theory of the new aerosol particle formation after a decrease of the temperature and another scenario is related to additional ionization that also can lead to the notable effect from the exact day of GLE. The latter can be illustrated by Fig. 3 and upper panels of Fig. 4, where one can see formation of additional small aerosol fractions and/or growth up to the CCN size exactly after the day of the GLE on 29 September 1989." In the first sentence, the authors say there may be 2 sources of changes occurring, but in the 2nd sentence draws most of the attention to the ion-aerosol clear sky mechanism. However, while there may be som formation of additional small particles (e.g. if the SAD goes from 3-20$\mu m^2 cm^{-3}$ but the Reff only goes from 0.25-0.5μm), this is not discussed quantitatively in the paper, and the main driver in the changes in the aerosol properties is still the additional mass of aerosol – there is no way that both the surface area density and Reff could have both increased without additional aerosol mass. Finally, it is not clear how one can "see formation of additional small aerosol fractions and/or growth up to the CCN size exactly after the day of the GLE on 29 September 1989." From Figures 3 and 4. Please add quantification of this using the SAD and Reff numbers.

In the conclusions, lines 15-16, "Based on the present investigation we conclude that ionization plays a role additional to the temperature in formation of clouds over the polar stratosphere." Again, it is possible, but the authors have not quantitatively isolated the ion-aerosol clear sky mechanism (which generates additional aerosol number) from the additional aerosol mass that occurs.

In the abstract, the wording is better, "we found that an extreme major SEP event might have led to formation of new particles and/or growth of preexisting ultrafine particles in the polar stratospheric region. However, the effect of the additional ambient air ionization on the aerosol formation is minor, in comparison with temperature effect, and can take place only in the cold polar atmospheric conditions.", but the abstract/paper could still benefit from an attempt to separate and quantify the additional aerosol number from the additional aerosol mass.

This extra mass was also apparent in Mironova et al. (2012). In figure 4 of their discussion paper (*http: //www.atmos-chem-phys-discuss.net/11/14003/2011/acpd-11-14003-2011.html*) (removed from the final version, not sure why , but reproduced below), there is about and order-of-magnitude increase in aerosol extinction at all wavelengths at altitudes of 18 km and below. Again, there is no way that this behavior could occur without additional aerosol (or polar stratospheric cloud) mass, which shows that if the ion-aerosol clear sky mechanism is occurring, there is also another, more dramatic change occuring that is increasing the aerosol mass. Although the figure below was missing from the final ACP version of this paper, the large increases in the extinction coefficient are evident in Figure 3 of the published paper.

We appreciate the comments and suggestions presents here, as well as close attention to our works. The noticed remarks are very significant from the physics point of view and highlight the importance of the obtained results. We have added some sentences in the text of the paper where we have tried to include all suggestions mentioned above and to make a point of "the additional- mass issue".

2. Please add some discussion as the how the temperature change might affect the aerosols (e.g. allowing more vapors to condense).

We have added a discussion on formation of polar stratospheric clouds in the low temperature conditions and how the temperature affects the aerosols. We did not add some discussion on the microphysical processes like as nucleation, condensation, evaporation, coagulation and sedimentation as soon as taking into account these processes we need to add some assumptions on chemical compositions of the atmosphere, that is not a task of this work.

3. Pierce and Adams (2009) (cited on line 1 of 5413) is not a statistical correlation study as is implied by the sentence following the citation. It is a global aerosol microphysics model study to test the sensitivity of CCN concentrations to the changes in nucleation that might occur due to changes in cosmic rays (i.e. the “the ion-aerosol clear sky mechanism”). Additional related modelling approaches have been done by (1) “Kazil, J., Zhang, K., Stier, P., Feichter, J., Lohmann, U., and O’Brien, K.: The present-day decadal solar cycle modulation of earth’s radiative forcing via charged H2SO4 /H2O aerosol nucleation, Geophys. Res. Lett., 39, L02805, doi:10.1029/2011GL050058, 2012.”, (2) “Snow- Kropla, E. J., Pierce, J. R., Westervelt, D. M., and Trivitayanurak, W.: Cosmic rays, aerosol formation and cloud-condensation nuclei: sensitivities to model uncertainties, Atmos. Chem. Phys., 11, 4001– 4013, doi:10.5194/acp-11-4001-2011, 2011.”, (3) “Dunne, E. M., Lee, L. A., Reddington, C. L., and Carslaw, K. S.: No statistically significant effect of a short-term decrease in the nucleation rate on atmospheric aerosols, Atmos. Chem. Phys., 12, 11573-11587, doi:10.5194/acp-12-11573-2012, 2012.”, and (4) “Yu, F., Luo, G., Liu, X., Easter, R. C., Ma, X., and Ghan, S. J.: Indirect radiative forcing by ion-mediated nucleation of aerosol, Atmos. Chem. Phys., 12, 11451-11463, doi:10.5194/acp-12-11451- 2012, 2012.”

Thus, the statement, “However, a quantitative model enabling an assessment of the cosmic ray influence on the aerosol properties still does not exist, even in the form of a simple empirical parametrization.” is incorrect (at least for the troposphere). It would be appropriate for the authors to say that no one has modelled the stratosphere, or that no one has modelled other potential connections between cosmic rays and climate.

We are sorry for our unclear writing of the sentence that did not quite correctly reflect the study following the citation. Now we have changed the text according to the suggestions presented above and added the mentioned references.

4. How does the variability in SAD and Reff during the period after the SEP compare the typical variability of SAD and Reff (in long periods without and SEP) in this region of the stratosphere?

We agree that the variability in SAD and Reff during different periods should be discussed. We have added some sentences on variations of these parameters (based on the data of SAGE II instrument) during time intervals with more or less the same atmospheric conditions as in our studied case, but without increasing ionization rates. The study of periods with increasing ionization rates during other SEPs is another task that will be considered in the future papers.

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Figure 4 of the discussion paper Mironova et al. (2012)
(http://www.atmos-chem-phys-discuss.net/11/14003/2011/acpd-11-14003-2011.html)

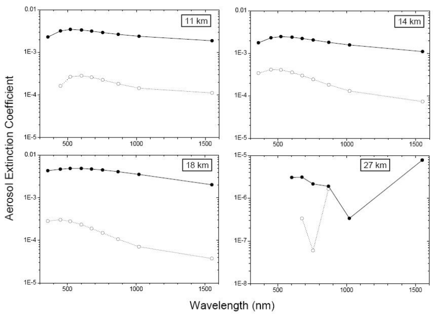

Fig.4 Aerosol extinction coefficient measured by SAGE III in the selected NW Eurasian region (see text) at four selected altitudes. Dotted line with open circles and solid line with filled circles correspond to the time-averaged aerosol extinction before (13--19 January) and after (21--27 January) GLE event, respectively.

Fig. 1.