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***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 #1 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.

General:

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This manuscript analyses the influence of a major solar energetic particle (SEP) event on aerosol properties in the polar stratosphere. The paper is original, relatively well-written, and it appears scientifically sound. I have a few comments that should be addressed before the paper can be accepted for publication.

Scientific issues:

The changes in aerosol properties due to this SEP event are observed above 10 km, yet the authors discuss potential changes in cloud condensation nuclei (CCN) concentrations (Abstract, section 4.1, section 5). CCN are relevant for liquid clouds, not for ice clouds. In polar (Arctic) areas, pure liquid clouds are observed only below some 3 km, whereas above 7-8 clouds tend to contain only ice (see Shupe, J. Applied Meteorology and Climatology, 50, 2011, p. 645-661). As a result, one should concentrate on ice nuclei rather than CCN when discussing the potential influences of SEP events.

We agree with this comment and modified the text referring to ice particles rather than CCN.

Page 14013, lines 7-11. The authors mention that they checked the mid-winter/summer periods for years 1998-2003. For what purpose? The authors never return to their findings in the text.

In order to exclude a possibility that the observed phenomenon is a typical mid-(local)winter effect due to, e.g., a change in insolation of the polar atmosphere, we have checked the period of mid-winter (January in NH and July in SH) for other years 1998–2003 using POAM data (Cora Randall, personal communication 2010). No similar change of the aerosol index have been found in all other years. We are aware that the winter 2004–2005 was not quite usual, but this at least suggests that such a change is not a typical mid-winter feature. Unfortunately, the POAM data cannot be

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directly used for January 2005 because of a calibration error (Cora Randall, personal communication 2010). We have clarified the text accordingly.

Page 14013, lines 13-21. The authors observe i) formation of small particles, and ii) formation of polar stratospheric clouds in one region. I suppose these two findings are not related to each other in any way. Or could they be? The authors should discuss this issue a bit. Now a reader may easily get the impression that both these observations result from the SEP event.

We agree that these effects are not directly related. We claim that the formation of small particles occurred in the entire Northern polar region on DOY 22, i.e. the 2-nd day after the event. PSCs were formed in a limited region (30 W–90 E) on the DOY 25, following a T drop. Thus, PSCs are a result of the T drop, which may or may not be indirectly related to the aerosols due to SEP event. We have clarified the text.

Section 4.2. The authors plot an altitude profile of AEC for SH, but have not done that for NH. Why? Although measured with a different sensor and different vertical resolution, it would be interesting to see the comparison of SH and HN profiles for AEC.

We don't show an average AEC profile for NH at a fixed wavelength, but all the information on variable profiles can be obtained from, e.g., Fig. 2. In NH we analyze the Ångström exponent, which is not affected by the vertical profile of AEC. However, for the SH we cannot build the AE and have to use AEC instead. Therefore, we normalize the AEC to the mean profile (in order to focus on relative changes), and this was the purpose to show it in Fig. 8.

Page 14014 to 14015. The authors give 3 potential reasons for the appearance of small particles. I completely agree with the authors that the reasons 2 and 3 are more likely

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than the reason 1. I do not, however, completely understand the reasoning behind this conclusion provided by the authors. Furthermore, can the authors exclude a fourth possible reason: transportation of smaller particle from somewhere else. Probably yes, but this possibility might be worth to be mentioned.

We could not exclude this possibility. However, the polar vortex was quite stable during that particular period to prevent effective latitudinal transport of particles from elsewhere (we observe the effect in the entire polar region). We have modified the text.

Technical issues:

Page 14015, line 27: CNN? Should be CCN, event though ice nuclei would be the more relevant quantity to discuss in this regard (see the comment presented earlier).

We have changed this.

Figure 3, Y-axes: Longitudes span from 180 W to 180 E. The authors should avoid using the notation -180 to 180, especially as they use a different notation in the text (page 14011, lines 8-12)

We believe these notations (-180 and 180 W) are both correct, similar to latitude notations (-90 is the same as 90 S). However, following the Reviewer's suggestion we have modified the Y-axis units for consistency.

Figure 5 legend: I suppose the Ångström exponent is equal to 1.58, not -1.58.

We have corrected this.

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