

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.

Anonymous Referee #1

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General:

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,

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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 km 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.

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.

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.

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.

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 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.

Technical issues:

Page 14015, line 27: CNN? Should be CCN, event though ice nuclei would be the

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more relevant quantity to discuss in this regard (see the comment presented earlier).

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)

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

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