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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Springtime warming and reduced snow cover from carbonaceous particles" *by* M. G. Flanner et al.

Anonymous Referee #1

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This study considers the impact of aerosols above and upon snow, with particular focus on the role of absorbing aerosols to enhance snowmelt in Eurasia during springtime. The combination of models and measurements leads toward the direction of constraining the aerosol-albedo effects. The study is innovative and informative and overall a valuable contribution to the literature on aerosol-climate effects. Listed below are some major issues that should be addressed before the paper proceeds to ACP, followed by minor points. Overall the paper is quite well written. (Note: my comments are based on the original version of the manuscript.)

Major points

1) The authors note that the AR5 models failed to simulate the amount of snow-melt

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and increased temperature observed in Eurasia from 1979 to 2000, and that including the effects of BC/dust on snow albedo might have improved their results. However it is not clear to me that the BC trend in much of the region is even in the right direction. In the final section there is some mention of the BC trend increasing in southeast Asia but decreasing in Europe. The authors should a) do a more careful analysis of the sub-regional trends using Bond et al. 2007. I suspect that ONLY south-southeast Asia has increasing BC during 1980-2000, thus the analysis of the AR4 models should be limited to that area. Europe and northern Asia should actually have increased snow from (decreased) BC changes. Related to this, I suggest adding figures showing the change in snow cover and BC deposition for models T1 and T2 (like the bottom 2 panels of Figure 7). Do BC deposition and snow cover increase or decrease over northern Asia and Europe?

2) Some aspects of the experimental design should be clarified:

a) Why do the PD experiments have biomass burning active in snow but not in air (according to Table 1)? Or maybe they do, but the table does not say so.

b) It seems unnecessary to do an ensemble of simulations for prescribed SST experiments. How much variability is there among the ensembles?

c) Bottom of 19830-top of 19831: A larger response is seen for the PI experiments than the similar PD experimental pairs. Indeed the snow response would be sensitive to the initial conditions. A warmer climate (higher CO2) reduces snow-cover so that there is less additional contribution from the BC-forcing. The point should be made that a realistic study requires a transient and fully coupled simulation. By the way, P19831 top line, instead of PI5-PI1 say what the experiments are.

Detail points

1) p 19823 line 2, How representative is 200um grain size and how sensitive is result to this assumption?

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2) P 19827 line 15, Are results sensitive to assumption of 0.2 extinction optical depth?

3) Figure 1 discussion. Since readers are used to "surface forcing" being from atmospheric aerosol suspensions only, please specify somewhere that surface forcing includes effects from aerosols in atmosphere and in snow.

4) Figure 1: Does the SSA change in both atmospheric and snow aerosols?

5) Figure 3: Instead of using labels "MAM snow fraction" on each panel, replace with the forcing (e.g. CO2; BC-snow; etc).

6) P 19831 line 22, clarify the meaning of "adjusted radiative forcing".

7) I think that the GISS-ER model did include a parameterized BC-albedo effect, so in this model it did not apparently help.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 19819, 2008.

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