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Interactive Comment

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

Anonymous Referee #2

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The paper "Springtime warming and reduced snow cover from carbonaceous particles" by Flanner et al., addresses a very important issue both scientifically and potentially also politically on the role of carbonaceous particles on continental snow cover in the northern hemisphere. The topic is clearly within the scope of ACP.

The paper makes the first attempt to quantify the contribution of carbonaceous particles deposited on snow to the observed springtime warming and reduced snow cover through application of a global climate model that includes a fairly detailed description of snow physics and radiation.

The paper starts with results from a detailed column radiation model. Section 3.1 discusses the results, very instructively pointing out the effects of the carbonaceous aerosols with different SSA and how they affect radiative effects on the ground and TOA radiative forcing.

Then the NCAR GCM (CAM 3.1), including the SNICAR model for snow processes is used first for a series of equilibrium experiments to single out the effects of CO2, carbonaceous aerosols in the atmosphere and carbonaceous aerosols deposited in the snow. Finally two sets of 5 ensemble simulations for the period 1979-2000 are performed to attempt to single out the contribution of absorbing aerosols in continental snow to the observed continental surface warming and snow cover change.

The paper is clearly written, and I recommend publication in ACP after revisions and clarifications as described below.

Specific Comments: My first main point is on the trend analysis of the SCE and surface temperature data, and apparent contradiction to the conclusion by Dery and Brown (2007).

Statistical analysis of observed snow cover data: The paper show and discuss linear trends in surface temperatures and snow cover (page 19832, line 19). It is not spelled out how this trend analysis is done for SCE, but I assume that it is done as for the T1 and T2 trends, i.e. based on the least square method. From visual inspection of figure 5 it seems to be some degree of inter-annual autocorrelation in the data. This needs to be taken into account in the statistical analysis (cf. the method used by Dery and Brown, 2007), in particular in the uncertainty estimates for the trends. Cf. Weatherhead et al. (JGR, 1998) for details in statistical analysis of trends in geophysical data.

In their conclusions Dery and Brown (GRL, 2007) state: To summarize, strong negative trends in weekly SCE over the period 1972-2006 are observed in the NH, North America and Eurasia. The largest declines occur during spring over North America and, to a lesser extent, over Eurasia. This seems to be in contradiction to the analysis of the SCE data presented here (Page 19832, line 20, and Figure 4). This needs to be explained.

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My second main concern is about the set-up and interpretation of the results from the transient experiments. There are several points that need to be addressed.

The transient simulations T1 and T2 are forced by observed SSTs which in principle also includes a signal from the BC-snow albedo effect. This means that there is a potential danger that the influence of absorbing aerosols in the snow is underestimated. To test this I suggest that an additional analysis of the equilibrium simulation is performed. Based on the experiments PI5 and PI3 the difference in SSTs caused by FF+BF BC in snow can be calculated. If this difference is small compared to the SST trends that are used to drive the transient simulations (T1 and T2) then I believe that it can be concluded that the effect is minor. A hint that this may have an effect is the authors' statement on page 19834 (line 27-), that the SCE/temperature ratio is in better agreement with the observed ratio for the equilibrium experiments.

The transient simulations include a prognostic mineral dust source, using the DEAD model described in Zender et al. (2003). Since the source of mineral dust is a function of the meteorological conditions and the dust loading in Eurasia is much higher than in North America (Zender et al., 2003), a more detailed discussion of the potential influence of this source is needed. This includes possible trends and interannual variability in the dust source in the transient simulations.

Abstract: It is stated: "Darkening from natural and anthropogenic sources of BC and mineral dust exerts 3-fold greater forcing on springtime snow over Eurasia (3.9Wm-2) than North America (1.2Wm-2). Inclusion of this forcing significantly improves simulated continental warming trends, but does not reconcile the low bias in rate of Eurasian spring snow cover decline exhibited by all models". Since these forcing numbers include mineral dust, the authors compare apples and oranges here. If there is not a significant trend in the forcing by mineral dust one should not expect this to contribute to the observed SCE trend. This also affects the conclusions section.

Analysis of precipitation and peak snow cover/amount. The trend analysis of SCE fo-

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cuses on the role of absorbing aerosols in snow. Trends in surface temperatures are included in the discussion, but there is no discussion of possible trends in snow accumulation, i.e. in the snow conditions before the onset of spring melt. I understand that there is not a homogeneous dataset available for observations of snow amounts, but an analysis of trends in the date for peak snow cover could be used an indication about to what extent the observed SCE trends are caused by increased melting during spring or by reduced snow accumulation before the onset of the melting. The observations should be compared to the results from simulations T1 and T2.

Page 19836, lines 8-14. Here the possible impacts of uncertainties in aerosols on the discrepancy between observed and simulated trends in SCE are discussed. The discussion needs to include also the fact that T2 includes a forcing from natural sources of mineral dust (1.2 and 0.2 Wm-2 for NA and EA) that should contribute to an overestimation of the calculated SCE trends. While the possible role of brown carbon for the relatively low estimate of direct forcing is discussed, brown carbon is not mentioned in the discussion comparison of observed and modeled SCE trends

Technical comments:

Page 19821/22. The introduction should include a short description of the observed trends in surface warming and snow cover extent (with references) that forms the rationale for the study.

Page 19822, line 16: Does "semi-infinite" imply that the reflectance is never influenced by the ground albedo (for shallow snow)? If yes, a sentence on how that may influence the first conclusion on page 19827, line 20.

I think it would be nice to have a short description about how the transient simulations were initialized (Footnote e in Table 1 seems to indicate that they start in 1977, what about spin-up?)

Page 19823, line 21. The main text does not include information about the number of

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ensemble simulations performed for the transient simulation (only in the figure caption for figure 5. Should be added.

Page 19828, line 18. Misprint, should by 1.1um (not 1.1 m).

Page 19835, line 22. Period of SCE data. Dery and Brown (2007) use SCE data from 1972 based on the same NOAA dataset maintained by Rutgers University, while the authors claim that it is restricted to the post 1979 period. Why do the authors restrict their analysis to the post-1979 period?

Page 19834, lines 6-18: Figure 7 shows a striking difference between the warming patterns of T1 versus T2 in Northern Russia (and also to a lesser degree in NW parts of North America). Information about the statistical significance of this difference must be added.

Page 19837, line 27: Please explain what is meant by "other extinctive species".

Figure 9. The point from the eq. simulation PI5-PI1 is missing in the bottom panel.

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

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