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Atmospheric
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Discussions

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

M. G. Flanner et al.

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We thank this reviewer for providing instructive comments, which have helped improve our manuscript. Below are our responses (’A’) to each reviewer comment (’R’), including descriptions of how we have modified the manuscript.

A current version of this manuscript, including revisions discussed below, is at: http://www.cgd.ucar.edu/~mflanner/ppr/ppr_FlZ08.pdf

R - The paper is clearly written, and I recommend publication in ACP after revisions and clarifications as described below. Specific Comments:

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 Fgure 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.

A - We have re-analyzed the observational data using the Mann-Kendall and Theil-Sen techniques to derive trends and confidence intervals. We applied the "trend-free pre-whitening" (TFPW) technique of Yue et al (2002) to account for autocorrelation (scaling the Z statistic for confidence interval accordingly). This appears to be similar to the approach of Dery and Brown (2007). The interannual lag-1 autocorrelation coefficients (1979-2000) were relatively small: 0.14 and 0.19 for Eurasian and North American MAM land temperature, and 0.35 and 0.27 for MAM snow cover extent, respectively. Consequently, the new technique had very little effect on estimated temperature trends, but larger autocorrelation in SCE data widened these confidence intervals and slightly reduced the trend estimates (slower rate of SCE decline). The new trend estimates do not alter any of our conclusions. Figures 5, 6 and 9 show the new trend estimates and confidence intervals, and we describe the new statistical approach in Section 2.4 (Methods).

R - 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.

A - The primary cause of this difference is the different time periods used in each analysis. Dery and Brown analyze trends over 1972-2006, whereas our analysis begins with 1979. Below are March-May snow cover trends, from NOAA/Rutgers monthly data, that we quantify over different time periods (using the statistical technique outlined above):

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N.Amer, 1972-2006: -2.7e+04 km2/yr (about -2.5%/decade)

Eurasia, 1972-2006: -4.3e+04 km2/yr (about -2.4%/decade)

N.Amer, 1979-2000: -3.3e+04 km2/yr (about -3.0%/decade)

Eurasia, 1979-2000: -9.9e+04 km2/yr (about -5.4%/decade)

N.Amer, 1979-2008: -2.2e+04 km2/yr (about -2.0%/decade)

Eurasia, 1979-2008: -8.2e+04 km2/yr (about -4.5%/decade)

Thus, from analysis over 1972-2006, one would conclude that (percentage) MAM snow cover decline was similar over both continents (or slightly greater over North America), whereas analyses beginning in 1979 shows significantly greater declines over Eurasia. The seasonal 1972-2006 SCE trends in Figure 2b of Dery and Brown actually appear to show greater (percentage) snow cover decline over Eurasia than North America during March and possibly May, but greater North American decline during April. Nonetheless, the results listed above demonstrate that SCE trends are sensitive to the period chosen for analysis - an issue which we carefully noted in the caveats discussed in section 3.3. We chose to begin our analysis with 1979 for two reasons. First, the SST-forced CMIP3 model experiments nearly all begin with 1979, when remotely-sensed sea-ice distributions became available. Second, the NOAA snow cover data is more homogenized from 1979 onward, when AVHRR came online, as discussed by Roesch (2006). Based on this and a later comment, however, we have modified Figure 4 to show the SCE timeseries beginning in 1972. We also added a statement in section 3.3 explaining that Dery and Brown find smaller Eurasian SCE decline over 1972-2006.

R - 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

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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.

A - This is an insightful point: The observed SSTs prescribed for the transient experiments already have some snow darkening influence "built into" them. As the reviewer suggests, this could lead to an underestimation of the transient BC/snow effect. We have followed the reviewer's suggestion to compare equilibrium SST changes caused by snow darkening (PI4-PI1) with the transient SST change. Northern Hemisphere March-May SSTs warmed by +0.06 K in experiment PI4, relative to PI1. The 1979-2000 March-May NH SST trend in our forcing (observational) dataset is +0.0094 degrees/year, translating into a 22-year warming of about 0.21 K. Moreover, the equilibrium change in SSTs caused by snow darkening (PI4-PI1) over-estimates the expected transient SST signal, as the system will not have fully equilibrated to snow darkening during the 22-year period. Therefore, we conclude that the influence of snow darkening on continental warming trends, via ocean warming, is small compared with the direct effect. The ocean effect could be larger at higher-latitudes, however, because of sea-ice-albedo feedback. Simulations using a full ocean model to study the transient snow darkening effect would be useful, but are beyond the scope of this study. We have added the following text to section 3.3 of the manuscript:

"Also, the influence of snow darkening on continental warming, via ocean warming, is not accounted for in these simulations because SSTs are prescribed. However, Northern Hemisphere MAM ocean temperatures warmed by only +0.06 degrees in equilibrium experiment PI4-PI1, smaller than the transient 1979-2000 SST warming of +0.21 degrees, suggesting this indirect effect is small."

R - 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

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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.

A - 1979-2000 trends in springtime dust emissions and deposition over Eurasia and North America were not statistically significant (at the 0.05 level) in the T2 ensemble. Eurasian emissions decreased slightly, while North American emissions increased slightly. Model trends, driven by prognostic emissions, may under-predict real trends because dust source regions and vegetation were fixed in these simulations. In reality, there is potential for an important feedback between dust and snow darkening: As snow cover declines, dust sources will increase, both in spatial extent and time duration, possibly increasing the amount of dust deposited on other snow surfaces and further reducing snow cover. We have added a statement to section 3.3 describing that model trends in emissions of dust and biomass burning BC were not significant.

R - 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.

A - Perhaps our description was confusing here. We included dust in the transient experiments to achieve a more realistic net snow darkening effect. Because contemporary climate models do not include darkening from either dust or black carbon, these experiments assess the degree to which total snow darkening (from both natural and anthropogenic aerosols) can reconcile model-observation trend biases. In response to the previous comment, dust trends are now mentioned. Regarding the last point: In spite of there being no significant trends in dust, the very presence of dust may influence the effect of black carbon forcing! The highly non-linear nature of snow-albedo feedback implies that the change in timing of snow ablation, caused by BC-induced albedo change, is influenced by the "background" snow albedo, and hence the amount of dust (a topic, perhaps, for a future study). We continue to report the net forcing of BC+dust in the abstract.

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

A - Although we initially conducted an analysis of snow accumulation using re-analysis data, we chose not to include it because of large uncertainties in snowfall data. But the reviewer raises an important point, and we have added the following paragraph to section 3.3 of the text:

"Reduced winter snow accumulation (preceding spring melt onset) could also drive reduced spring snow cover, but observations do not support a dominant role for this effect. First, Dery and Brown (2007) show increasing (but not significant) 1972-2006 Eurasian SCE trends during all months from October to January, suggesting early-winter snow accumulation has, if anything, increased. (February SCE decreased, but not significantly.) Analysis of streamflow in watersheds of Western North America indicates that the onset of spring melt has shifted earlier since 1950, and that these trends are driven largely by winter and spring warming (Stewart et al, 2005). Finally, Pierce et al (2008) found a decrease over the Western U.S. (during 1950-1999) in the ratio of April 1 snow water equivalent to year-to-date precipitation. Our own assessment of January-May snowfall trends over 1979-2000 from re-analysis data (Qian et al, 2006) does show a significant decline in winter-spring snowfall over North America, but no significant change in Eurasian snow accumulation. We acknowledge large uncertainties in these data, however."

A rigorous analysis of pre-melt snow accumulation is beyond the scope of this study, owing partially to the lack of a homogeneous dataset for snow accumulation, pointed out by the reviewer.

R - 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

A - The reviewer may be suggesting that inclusion of dust forcing in experiment T2 would contribute to an overestimate of the contribution of _anthropogenic_ snow darkening to calculated SCE trends. But this part of the manuscript addresses general causes of model-observation discrepancy, one of which is dust-induced snow darkening. (The dominant forcing agent and source of aerosol trends is, however, anthropogenic black carbon). As mentioned earlier, because snow darkening occurs from both dust and BC in reality, and because neither are included in modern climate models, both should be needed to reproduce observations. We have clarified in the text that dust forcing has been prescribed in the transient experiments simply to achieve the most realistic simulations possible. We have also added the following statement to the passage in question (p. 19836): "Also, as mentioned earlier, we neglect the role of absorbing “brown carbon,” which may be further darkening snowpack in Asia and elsewhere."

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Technical Comments:

R - 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.

A - We have added a sentence to the last paragraph of the introduction, citing Dery and Brown (2007) and the IPCC AR4 (Chapter 3) for reduced spring snow cover and rapid continental warming.

R - 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.

A - Yes, "semi-infinite" in this context implies that the snowpack is thick enough such that the underlying surface has (essentially) no radiative influence. We've added the following sentence to section 3.1:

"We note that the range of aerosol single-scatter albedo producing positive TOA forcing will be reduced over snowpacks thinner than ˜20 cm (Wiscombe and Warren, 1980), as exposure of the underlying surface reduces albedo."

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

A - We've added a couple of descriptive sentences to section 2.2 (which originally mentioned that the transient experiments began in 1977). As the trend analysis is over 1979-2000, there are two years of model spin-up. Most of the CMIP3 SST-forced experiments have only one year (or a few months) of spin-up. Simulations with prescribed SSTs require much less spin-up than fully coupled runs, although certain variables (like

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deep soil moisture) in SST-forced experiments do require long spin-up. The original text (p. 19825, line 22) mentions that T1 and T2 are each five-member ensembles.

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

A - Fixed.

R - 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?

A - We have addressed this issue above, in response to an earlier comment. The basic reason for this was to compare against CMIP3 SST-forced simulations that begin with 1979. We have modified Figure 4, however, to include SCE data from 1972-onward.

R - 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 signifcance of this difference must be added.

A - We have added stippling (black dots) in the bottom-most panel where the T2 ensemble trends are statistically different (at p=0.05) from those of the T1 ensemble. Temperature trends are statistically different over Northern Russia and Alaska.

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

A - We added: "(biomass burning and mineral dust aerosols)".

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

A - We chose not to include this point in the bottom panel because North American snow cover and temperature changes were not significant in experiment PI5-PI1 (Table 2). In fact, North American spring temperature actually cooled slightly in this experiment. Thus, including this point may introduce confusion.

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

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