**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Black carbon aerosols and the third polar ice cap" *by* S. Menon et al.

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We thank the reviewer for reviewing the paper and recognizing the importance of the topic and also for the comments raised to improve the paper. We have revised the paper accordingly and we hope the revisions have addressed any concerns of the reviewer regarding the results and conclusions presented in the paper.

Below we include our responses to the questions raised.

(1) We read with great interest the reviewer's explanation of why it is important to show the statistical significance of the results especially when evaluating changes shown in this work, as model internal variability may be a factor to consider and do agree with the suggestions. For the atmosphere-only simulations we performed, generally the 5 years (averaging over the last 60 mths of a 63 month simulation) provide robust results and has been used and reported in several prior papers by the group at GISS using the

same model version (e.g. Unger et al. 2009, ACP). Emissions are the same for each year but include GCM meteorological interannual variability. It is no longer possible to rerun the model with a different random number generator or to extend the simulations. However, we did have a simulation that was run for 12 years (D+1) for Year 2000 Beig emissions, and provide an analysis of results from examining a 5-year average versus a 12-year average for this run. Global annual average differences between these simulations for the various diagnostics listed by the reviewer are provided in the table below. (a) Aerosol Indirect effect (SW+LW Cloud radiative forcing (CRF)) = 0.05 -0.05 = 0.0 Wm-2 (b) Total cloud cover = -0.03% (c) Liquid water path = -0.06 gm-2 (d) Net TOA radiation = 0.08 Wm-2 (e) Precipitation = 0.003 mm/d (f) Low cloud cover = -0.06 % (g) Snow/ice cover = -0.05 %

Furthermore, we examine differences for the simulation domain of interest (4°–40° N and 65°–105° E) as shown in Table 2 for some of the diagnostics that the reviewer pointed out.

Below we refer to the Diagnostic, the 12 yr mean, the 5 year mean and the % difference given as [(5 yr – 12 yr)/5 yr]. (a) Net CRF (Wm-2) = -14.13, -13.95,1.29% (b)Total cloud (%)= 51.00,50.51,0.97% (c)LWP (gm-2) = 43.98, 43.33,1.5% (d) Net TOA Rad(Wm-2)= 34.48,34.69,0.61% (e)Net Sfc Rad (Wm-2)= 121.14,121.65,0.42% (f) Precip. (mm/d)= 3.93,3.89,0.45% (g) Low cloud (%)= 28.7,28.66,0.73% (h) Snow/ice cover (%)= 11.45, 11.52,0.61%

As can be observed, the 5-year run produces robust results and for the purpose of the rest of the paper, these averages over 5 years of simulations may be considered sufficient. We regret that we cannot extend all the simulations similarly. We agree that model internal variability may be an important parameter to consider and do include a statement to clarify the results are dependent on the model and could influence results obtained. We also summarise results from differences between the 5 year and 12 year mean runs. These additional statements brought up by the reviewer are included in the Methods section after the description of the simulations.

2) With regard to the statistical significance of the results; we calculated the statistical significance using the standard t test (as in Unger et al. 2009, ACP, Jones et al. 2007, JGR). The interannual standard deviation is calculated as (2/n)1/2*SDp, where n is the number of years in the simulation and SDp is the pooled standard deviation (Snedecor and Cochran, 1989). This method has been used for prior simulations as shown in Lohmann et al. (2010, ACP). We have rechecked our work and the results are consistent with what is reported in the paper (we did not find any bugs). We have since added the methodology by which we estimate the significance (mainly for the calculation of the interannual standard deviation) in the Methods section.

3) We agree that the model underestimates BC considerably and is a major problem for most models. Some of this may be due to the comparison between urban poor point measurements and those from a coarse-model. We include this and additional statements in the revised paper (since we also cite a paper by Koch et al. 2009, ACP that looked at differences between black carbon estimated by models versus observations for an AEROCOM study) in the section where we compare with the observed mass. We also include this point in the section where we compare the change in snow/ice cover between the model and observations.

4) We agree with the statements that it is hard to see the changes over a small region and have since revised the figures to mainly focus on changes over India. We included the whole globe previously since emission changes applied globally, but our focus is regional so we zoom into the region of interest as requested.

5) We have included a table that describes the simulations as suggested by the Reviewer. This is now Table 1. We also included additional description of the model at the request of the other reviewer in the Method section.

6) We did not have access to SSTs from 1984 to 1993. This is why we used SSTs from 1975-1984 and 1993-2002 to contrast emission changes between 1990 and 2000. The model climatology has been previously evaluated in Schmidt et al. (2006) for the 1975-

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84 data set and thus we used that version. We have included this additional statement in the paper.

7) The reviewer is right that we do not include any anthropogenic BC and this should not have a BC effect on snow albedo. Indeed that effect is 0. We include this revision in the revised paper.

8) For the heavily polluted region low single scattering albedos may be expected. However, we do not think the 0.57 value should be interpreted as the single scatter albedo since the model in general underestimates the AOD. This point has now been included in the paper.

9) The aerosol direct radiative forcing is calculated from the difference between the TOA radiative flux with and without aerosols at each model time step (Koch et al. 2009, J. Clim). The difference between the radiative flux for the 1990 aerosol emissions versus the 2000 emissions then represents the forcing for the particular decade. We have further clarified our description on pg 26597 with this additional statement to help clarify how these values were computed.

10) Forcings are calculated at the top of the atmosphere. We regret the error in Table 2 and have corrected it.

11) These values of $\sim$ -15 Wm-2 are for Net Cloud radiative forcing values for the region. IPCC values the reviewer refers to are from the change in forcings due to aerosols. Those values are given in the 3rd and 4th column of Table 2. Usually we get $\sim$ -0.9 to -1 Wm-2 for the indirect effect for aerosol emission changes from present-day to pre-industrial, similar to most other models used for the IPCC report. Our values reported in the 3rd and 4th columns are for differences between 1990 and 2000 and thus are a lot smaller.

12) These values are small since the model underestimates the BC amount and forcing. These changes alone do not explain the change in snow/ice cover and it is the

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total heating effects of BC including its snow albedo effect that causes the changes in snow/ice cover.

13) Yes, we will change as requested. We regret not being clearer.

14) We include the domain-mean value of observed snow cover change (5.33 % decline over the decade). Remapping the observed pattern on a coarser grid to compare with the model may not be as useful as it degrades the observations. However, we will try to remap them as requested.

15) We use "future" since we do include the reasons as to why we refer to 2010 as future, but we do note that for the current time period referring to 2010 as future does not seem helpful. Since our emissions were just for BC for 2010, we refer to it as a sensitivity run for the future.

16) We do note a change in wind direction as stated.

17) For 2010, SST's used were for 1993-2002. We recognize the cause for confusion in the statement. What we meant was that differences between snow/ice cover for 2010 versus 2000 was 0.16% for similar SSTs used (for the period 1993-2002) versus 0.05% for 2010 and 2000 (when emissions and SSTs were different; i.e. when 1975-84 SSTs were used for Year 2000 emissions). We clarified the statement in the revised paper to avoid confusion.

18) Yes, these were for trends from CRU. We have reworded to state as the reviewer suggested.

19) Yes. The colour bar scale has been multiplied by a factor of 10.

20) We did not combine Fig. 11 and 12 since one was global and the other regional.

Technical corrections: 1) We have added Indian BC emissions 2) This is an interesting fact we overlooked. Wikipedia suggests that a trillion is 10^12 and is the common meaning in English language usage. However, European use is different and we clari-

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fied our use of trillion as suggested. 3) We changed as suggested 4) We defined GHGs when we first use it. 5) We edited to state "thought" 6) We have rewritten to state BC-related 7) Yes, we have changed the subject accordingly. 8) A comma has been added 9) We have edited as suggested 10) We have edited the sentence to state that change in snow/ice cover between the Beig and Bond emissions from the enhanced BC emissions is 0.86% compared to 0.63% (with Bond). Difference between 0.86 and 0.63 is a 36% change. We regret the wording we used before.