Atmos. Chem. Phys. Discuss., 9, C12162–C12164, 2010
www.atmos-chem-phys-discuss.net/9/C12162/2010/

Atmospheric Chemistry and Physics Discussions

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

S. Menon et al.

smenon@lbl.gov

Received and published: 19 April 2010

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 and the quality of figures included.

Below we include our responses to the questions raised.

1) Detailed model description More details regarding the model, including time steps used, model components, effective resolution for tracer transport are now included in Section 2 as requested. We do regret the fact that we did not include this discussion before.

2) Robustness of results and use of SST dataset and time period chosen. Why was 1993-2002 not used for all simulations We do include the statistical significance of the results for the diagnostics of main interest in the paper: Snow/ice cover and precipitation. As explained to reviewer 1 we have examined differences between a 12 year run and a 5 year run and find small differences of less than 2% between variables of interest for one particular simulation that had the 12 year period (D+I for Beig year 200 emissions). Global annual average differences between the simulations for the various diagnostics. (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.

Below we list 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%

Based on suggestions from Reviewer 1 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 include a general summary of differences we found for a 5 year versus a 12 year run.

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 1975-84 data set and thus we used that version. We include this statement in the revised

paper.

3) Unclear references to changes in snow/ice cover on page 26601 and 26603. Does fossil/bio-fuel BC contribute 30% or do we refer to 30% change in snow/ice cover? We agree that the wording needs to be clarified and have since revised it to state that fossil/bio-fuel has a 30% contribution to the change in snow/ice cover simulated.

4) Simulated decreases are less than observed decreases. Some explanation regarding this must be included. We include additional statements that indicates that due to the under prediction of BC amounts, and the fact that we may underestimate absorption efficiencies due to the external mixtures we assume, we may underestimate the simulated decrease compared to the observed decrease. Since we now include the observed decrease (at the request of reviewer 1 as well), which is 5.44% for the decade, we also show the quantitative difference between model and observations.

5) Better representation of regions in the figures so that patterns of changes may be observed more clearly in the figure. Yes, we agree. The other reviewer also requested better or magnified figures and we now include a regional map rather than the global map. We also maintain the change on the model grid as requested, without interpolating. Some of the changes within the regions of interest are hard to interpret more thoroughly than presented and so we restrict our analysis to results that pertain to the main point of the paper; that enhanced black carbon emissions from particular sources and physical processes represented may be responsible for some of regional climate change patterns observed over India over the last two decades.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 26593, 2009.