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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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General comments

This paper addresses an important topic: it tries to evaluate the role of anthropogenic black carbon emissions on the decline of Himalayan glaciers as well as precipitation changes from year 1990 to 2000 and onwards to year 2010.

In principle, several factors could contribute to the observed changes: anthropogenic emissions of black carbon and other aerosols, through their direct and indirect impacts as well impacts on surface albedo, increases in greenhouse gas concentration, and changes in sea surface temperatures, as well as natural climate variations.

The authors address this issue by performing a large set of GCM simulations with the GISS ModelE coupled with an aerosol chemistry/transport model. Most importantly, the paper shows that including the effects of black carbon on snow albedo contributes to a decline of snow/ice cover in the Himalayan region.

Although the approach is, in principle, sound, I have some major concerns about this paper. Most importantly, I am concerned about the robustness of many of the results. The problem is that the response of GCM simulations to any forcing is influenced by internal variations of model climate. For the relatively short (5-year) simulations and rather modest forcings considered here, these (essentially random) variations could impact the model response substantially, either masking or exaggerating the model response to forcing. This is especially true for regional details. The signal-to-noise issue is exacerbated by the fact that the model (apparently) underestimates the BC concentrations and optical depths. Incidentally, it appears that the statistical significance testing included in this paper may have been done incorrectly (see specific comment 2). Finally, the presentation could be improved; in particular many of the figures are far too small to read. These issues and several smaller points are discussed in more detail below.

In my opinion, this paper still requires subtantial improvements before it can be possibly published in ACP. In particular, the authors have to be able to show that their results are robust.

Specific comments

The most important comments are listed first (comments 1-4). Smaller comments follow page-by-page.

1) As noted in the general comments, I am concerned about the statistical significance of the results. Any change in model formulation or forcing, however small, will cause essentially random changes in the simulated weather patterns, which results in “ran-

dom" changes in the simulated climate statistics. The role of randomness is the larger the shorter is the simulation, and in general, it is larger when considering statistics for smaller regions. Only little attention is paid to this in the paper.

I have myself performed an extensive set of five-year simulations with the ECHAM5 GCM (Räisänen et al. 2007; J. Climate vol. 20, p. 4995–5011). I revisited this set of simulations to evaluate the impact of model internal variability on five-year mean values averaged over the Indian region as defined in this paper (65–105°E, 4-40 °N). Based on ten 5-year simulations with the REF model version considered in that paper, with identical model versions but different seed numbers for the random number generator, the standard deviations of five-year mean values for India were as follows: net radiation at the TOA 0.58 Wm^{-2}; net radiation at the surface 0.69 Wm^{-2}; atmospheric forcing (radiative convergence/divergence) 0.47 Wm^{-2}; SW radiation at the TOA 0.48 Wm^{-2}; net cloud forcing at the TOA 0.44 Wm^{-2}; low cloud fraction 0.26%, total cloud fraction 0.34%; cloud liquid water path 1.28 gm^{-2}, precipitation 0.050 mm d^{-1}. For the JJA precipitation, the standard deviation of five-year JJA mean values in India was around 0.3 to 1.5 mm^{-1} depending on location. (Unfortunately, snow cover was not saved in these runs). Obviously, these figures are model-dependent, but they may provide some guidance on what to expect also for the GISS simulations.

Based on these values, here is a list of some results that might be influenced substantially by model internal variability.

- the changes in aerosol indirect effect, cloud cover and liquid water path between years 1990 and 2000 (p. 26600, lines 9–18, and Table 2)
- the TOA net radiation changes from year 1990 to 2000 related to the changes in Bond/Beig emissions (p. 26600, lines 23–25, and Table 3)
- changes in precipitation and low cloud cover in Table 3. Possibly the snow/ice cover changes too

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- spatial patterns of snow cover change (p. 26602, lines 1–7, 13–18 and Figs. 3 and 4). Either the Beig emissions are indeed needed for a "realistic" pattern, or alternatively, you were lucky.
- the smaller decline in snow/ice cover when the increase in GHGs is taken into account (p. 26602, lines 10–12).
- the "reversed" geographic pattern of snow/ice cover change from year 2000 to 2010 (p. 26603, lines 16-21). Curiously, the "red spot" in northeast India in the upper left panel of Fig. 8 turns into a "blue spot" in the lower left panel. It seems as if there was something anomalous with the year 2000 simulation?
- the patterns of precipitation change (section 3.4, Figures 8 and 10), temperature (Fig. 7) and wind change (Fig. 9) are worth checking too.

I strongly recommend that you evaluate systematically the statistical significance of your results. When considering the mean difference of two five-year simulations, this could be done by performing a two-sided t-test, and by treating each simulated year as an independent sample (not strictly true as there could be some year-to-year autocorrelation, but usually good enough). Another way to get an idea of the role of random variations is to repeat (at least some of) your runs with different initialization. For example, start the runs one month earlier, and run them for 64 months instead of 63, and again analyze the last 60 months. Then check which findings remain valid. Obviously, the discussion of the results should focus on those features that are found robust.

If feasible, the statistical significance of the results could be boosted by running the model longer, or by performing ensemble runs. By doing so, more robust estimates of (e.g.) the impact of BC in the model could be obtained. One caveat should however be noted: we obviously have only a single realization for the real climate system, so internal variability can also influence the observed changes substantially. The implication is that even with a "perfect" GCM, it might not be possible to

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separate unambiguously the contributions of various forcings to the observed changes.

2) A related point is that the statistical significance has most probably been computed wrongly in this paper. According to Figures 2 and 8, the differences in five-year-mean annual-mean snow cover and June-July-August precipitation are statistically significant at the 95% confidence level almost everywhere. Since precipitation and snow cover are quantities that show substantial interannual ("random") variations, this would be very surprising even for relatively large forcings. For the rather small forcings applied here, it plainly seems too good to be true.

3) The comparisons of BC concentrations (Table 1) and aerosol optical depth (p. 26599, lines 17–23) suggest that the model underestimates BC, possibly severely. Do you think this is a representative result, or could the disagreement be partly artificial, related to comparisons of a coarse-resolution model with localized, mostly urban measurements?

The realism of simulated BC fields is a major issue for this study, so some more discussion on this question seems warranted. Presumably, an underestimate of BC might contribute to the smaller-than-observed changes in snow cover.

4) This study focuses entirely on the Indian and Himalayan regions. Still, many of the figures show global fields. It is extremely difficult to try to follow the discussion on regional features, when India is a millimeter-scale feature on the maps! The figures should show only the region that is relevant for this study (like Figs. 6 and 7, or even more tightly cut).

5) p. 26597–26598: It is a bit difficult to get a good grasp of this set of simulations (i.e, what exactly are the differences between them). A table might help. For each

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simulation, there could be entries like simulation name, direct aerosol effects (yes/no), cloud droplet number concentration (fixed/interactive), effect of BC on snow albedo (yes/no), greenhouse gases (years), sea-surface temperature (years).

6) p. 26598, line 3: What is the reason for using the average SSTs for years 1975–1984? This does not sound like the most self-consistent choice, when the emissions are for year 1990.

7) p. 26598, lines 6–8: In the NBC(D+I+SA) simulation, there is no anthropogenic black carbon. How can there be a BC effect on surface albedo, as the letters SA suggest?

8) p. 26599, lines 19–21: An AOD of 0.14 and an absorption AOD of 0.06 is reported for the simulation using the Beig emissions. This implies a very low single-scattering albedo of (0.14-0.06)/0.14 $\approx$ 0.57. Is this a plausible result?

9) p. 26600 and Table 2. How were the various forcings evaluated? Diagnostic calls to the radiation scheme during the model run, off-line radiation calculations afterwards, or just based on the radiative flux differences between the GCM runs? While there are some notes on this (e.g., p. 26597, line 8), it would be worth adding a summary of this. This question is also linked to the role of model internal variations: forcings evaluated using diagnostic or off-line radiation calculations are more robust than those based on radiative flux differences between two GCM runs.

10) Also, were the forcings evaluated at the tropopause level (as stated in Table 2), or at the top of the atmosphere (as stated on p. 26597, line 8)? I recognize that this is a minor issue in practice.

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11) Table 2: Why are the "aerosol indirect effect" and "total aerosol effect" for year 2000 extremely large (around -15 Wm^{-2}). What do this quantities represent? They are surely not compatible with the IPCC values (around -1 Wm^{-2}) for the aerosol indirect effect.

12) p. 26601, lines 12$-$14. The changes in BC forcing through snow albedo changes between years 1990 and 2000 appear very small (within 0.03 Wm^{-2}). Even though the values for snow-covered regions are probably larger, I wonder if these minor forcings can really explain the simulated changes in snow cover?

13) p. 26601, lines 25-27, and elsewhere: Please use the notation (D), (D+I) etc. to list the simulations. It is not reasonable to expect the reader to remember which is simulation (1), (2) etc.

14) p. 26602, lines 1-7, and Fig. 5. For comparison with model results, what is the domain-mean value of observed snow cover change? Perhaps it would be good to also show the observed pattern of snow cover change averaged to the much coarser model grid.

15) p. 26603, line 12: it would appear simplest to use the term "year 2010" simulation instead of the "future" simulation.

16) p. 26603, lines 24-25: Your wording here could be interpreted so that for the year 2000 simulations the mean wind is easterly, for the year 2010 westerly. But is this what you actually mean?

17) p. 26603, line 27: How were the SSTs defined for the year 2010 experiment? You mention climate-driven changes, so apparently the SSTs were modified too.

18) p. 26604, line 12 : "observed trends" = trends from CRU TS2.0 data?

19) Fig. 6: "Differences in annual average forcing (0.1 Wm^{-2})"... Does this mean that the colour scale on the map actually goes from -0.9 to 0.9 Wm^{-2}?

20) Figures 11 and 12 could be combined.

Technical corrections

1) p. 26594, lines 11-12 "Indian BC from coal and biofuel are large ...": something missing? "Indian BC emissions"?

2) p. 26594, line 19: Be careful with the Indian GDP. ACPD is an European Journal; thus 1 trillion = 10^{18}, 1 billion = 10^{12}.

3) p. 26594, line 21: it should be "sixfold".

4) p. 26594, line 24: the acronym "GHGs" should probably be defined.

5) p. 26594, line 26: it should be "thought".

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6) p. 26595, line 17: it should be "BC-related".

7) p. 26596, lines 19-21: the subject is "an increase", so the verbs should be singular: increases, reduces, inhibits.

8) p. 26597, line 5: add comma: "snow/ice albedo, BC concentrations ..."

9) p. 26601, line 21: this should be (e.g.) "the corresponding values".

10) p. 26601, line 24: "the enhanced fossil/bio-fuel BC from the Beig emissions causes a 36% decrease in snow cover". Do you mean: "a 36% larger decrease in snow cover"?

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

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