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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.

Anonymous Referee #2

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The manuscript by Menon *et al.* adresses an important and contemporary issue regarding the impact of aerosols of the cryosphere. In particular, the authors attempt to resolve signals impacing the Himalaya region. Further, the authors couple the role of anthropogenic aerosol forcing the hydrologic cycle in the region. The paper examines the period from 1990 to 2010. The authors state to "quantify the impact of BC aerosol on snow cover and precipitation from 1990 to 2010 over the Indian subcontinental region".

A stated result indicates that simulated spatial patterns in snow cover and precipitation are similar to observations for the period 1990 to 2000.

1 Scientific Significance

The topic of aerosol interaction in the climate system, and in particular the relationship of aerosols to the cryosphere is certainly relevant today. The authors do a good job of introducing the topic and appropriately cite contemporary literature.

2 Scientific Quality

I feel the manuscript overall relies too heavily on prior acceptance of ModelE results and provides little discussion of why these results may be meaningful. Going through the manuscript, there was very little discussion on the physics in the model, underlying assumptions, or even basic model set up paramters. For instance, the reference give (Schmidt et al., 2006) provides only an overall description of the model in three different configurations. The authors state that the 4x5 degree, 20 vertical layer model is used, but provide no further information about the fundamental components of the model run. Perhaps a reader more familiar with the model would be comfortable to make the assumption the time step remained at 30 minutes, and other such parameters were maintained, but I think it should be explicitly stated in this paper.

As for references to implementations of other model components, it seems suitable. But again, some more detailed discussion regarding the model setup and limitations would be welcome. One point that merits further discussion is the fact that "The effective resolution for tracer transport is significantly greater than these nominal resolutions because of the nine higher-order moments that are carried along with the mean tracer values in each grid box." (Schmidt, 2006) For the present study, this bears relevance as the authors are attempting to resolve a regional-scale issue with a coarse model.

In the same way, the treatment of the model sensitivity seesm quite limited. There is virtually no discussion of the robustness of the results. For instance one of the central results (snow/ice coverage) seems to be impacted significantly by a change in the the SST dataset used, and in fact with the more contemporary SST dataset, the result is opposite ('snow-cover increases due to the reduced atmospheric forcing'). Why is the 1993-2002 SST dataset not used in the first place, with the earlier period used for the sensitivity test?

On page 26601 and on page 26603 there are references to a 36% and 31% decrease in snow/ice cover, respectively (both around line 25). However, the text and tables indicate the reduction is on the order of 1%. This is unclear as a reader, though I suspect the point is the fossil/bio-fuel BC contributes 30% of the total? This needs to be explained better.

There is also limited discussion of the fact that it seems the overall decreases simulated are far less than the observations.

3 Presentation Quality

The presentation of figures in this manuscript is quite poor. First, no where in the figures is an overview of the region explicitly shown. It is a challenge already that the authors are presenting results from a coarse global climate model to simulate dynamics in a complex region, but to show global figures when discussing the changes seen between simulations makes it very difficult for the reader to follow. The text and tables refer to percentage decreases in the Indian region, but often a global plot is shown. Furthermore, there are some strange features in the differences between simulations that merit further discussion. For example, in the region of interest (the Himalaya), on Figure 3 there is a complete reversal it seems going from the middle to right panels. Furthermore, the authors state that the "magnitude of the decreasee without BC is comparable to that obtained with BC", but the spatial patternsn change. This is hardly noticeable in these global plots. Certainly regional plots (not interpolated, but main-

tained on the model grid) need to be shown to give a better idea of the observed changes in 'spatial' pattern.

No further specific comments are provided for the manuscript at this time.

The importance of this work is considerable. Given recent media attention (NOTE: this article has already been cited on the web:http://www.scienceblog.com/cms/black-carbon-significant-factor-melting-himalayan-glaciers.html *as published in ACP*.) to the topic of Himalayan ice loss and precipitation changes, I feel it merits a more comprehensive treatment than given by the authors. A more in depth discussion of the potential short comings of the model being applied to such a region, and further discussion of the statistics of the results are necessary.

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