Atmos. Chem. Phys. Discuss., 10, C9160–C9165, 2010
www.atmos-chem-phys-discuss.net/10/C9160/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity studies on the impacts of Tibetan Plateau snowpack pollution on the Asian hydrological cycle and monsoon climate" *by* Y. Qian et al.

W. K. Lau (Referee)

william.k.lau@nasa.gov

Received and published: 27 October 2010

General comments: In this paper, the authors conducted numerical experiments with the NCAR CAM3.1 GCM to study the impacts of deposition of black carbon (BC) and dust on snowpack of the Tibetan Plateau (TP), and possible influence on the Asian water cycle and monsoon climate. They carried out a set of experiments using pre-industrial (PI) CO2 conditions without BC and dust deposition as control, and anomaly experiments including various combination of BC in atmosphere and in snowpack, under PI as well as present-day (PD) CO2 conditions. They found that aerosol-induced snow albedo effect can reduce spring snowpack over the TP, more than the CO2 increase, and heating by carbonaceous particle in the atmosphere. This is an important paper, documenting the first serious attempt at estimating the impacts of BC and dust deposition on TP snowpacks and their effects on Asian water cycle and climate. The authors wisely selected the approach of selecting monthly prescribed realistic forcing of aerosol loading, and limits to radiative effects only so as to narrows the uncertainty of the results due to interactive aerosol, and microphysics forcing, which should be left to future work. The paper is generally well written, and the key points and conclusions clearly stated. However, there are major concerns regarding the statistical significance of the quantitative results in the model and relevance to the real climate. Revisions are needed for clarification and strengthening some parts of the paper, before this paper can be recommended for publication.

Specific comments 1. The model clearly overestimated the snowcover over the TP, in many cases over 100%. As shown from Fig. 2, the observed snow cover are concentrated in narrow strips in the southern and southeastern , western and the northern slopes. Over the top of TP, snowcover is sparse and scattered, with many regions less than 10-20%. In contrast, partly due to its coarse resolution, the model snowcover is continuous, and large scale, with the excessive snowcover at the top of the TP. The model snowcover shows a large area of pronounced maximum (>80%) in southern central TP, where observation is actually a minimum (<10%). Also, the comparison of BC concentration with observations is not very meaningful, because the BC measurements were taken at isolated spots for specific ice-core in mountain glaciers at various depth, and various time of the year, whereas the model is dealing only with the seasonal snowpack over a large grid area about 300 km x300km. The authors recognized these facts, but still presented the results as if the model results are consistent with the ice-core measurements.

2. From the observed snowcover, it is mostly likely that in the real world, the BC -dust-in-snow effect will have impact on the wind-facing steep slopes, and much less at the top. In the model, the slope effect on deposition and sun-angle effects on radiative

forcing are not included, and all the BC and dust effects are plane parallel radiative effects on top of the TP, with greatly exaggerated snow cover. Thus, the model is likely to grossly over-estimate the BC on snow effect, compared to the real world. Such caveats have to be stated upfront in the abstract and in the conclusion, to make clear that the readers are aware that the results are model dependent, and should not be extrapolated to the real world nor beyond CAM 3.1.

3. Because of the large intrinsic variability of the monsoon water cycle and climate of Asia, ensemble simulations are necessary to increase the signal to noise level. The authors need to address the statistical significance of the results, especially for the evaluation of Asian monsoon cycle downstream of the TP. They did not say how long, many model integrations were conducted for each set of experiments. If only one model experiment was carried out, I would really question the robustness of the present results, and encourage the authors to carry out more cases. This would require more work, but will make this a much stronger paper.

Other comments : P1, Line 1-10. Abstract: The first paragraph of the abstract reads like an introduction and can be reduced. The abstract should say something about how the BC-snow effects affect the water cycle of the monsoon climate. The cited numbers should be qualified with statements stating the over-estimated of BC-snow effects, the level of statistical significance and possible model dependence of the results. P2, Line 4: OM is not defined yet. Line 5-9: Nigam and Ballasina (2010) erroneously used local correlation to imply causality. Lau and Kim (2010, JGR, accepted) has responded to their comments on the EHP. I suggest adding a statement. "Lau and Kim (2010) emphasized that validation of the EHP has to be based on the forcing and response of the entire monsoon system from pre-onset to termination, and not based on local correlation of aerosol and rainfall at one time." Lau, K. M. , and K. M. Kim, 2010: Comments on the paper " Elevated Heat pump" hypothesis for the aerosol-monsoon hydroclimate link: "Grounded" in Observations? By Nigam and Bollassino, J. Geophys. Res. (accepted) P.4, Line 9-10: Somewhere around here, reference needs to be made to recent

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papers that found from observations accelerated warming of the troposphere over the TP, attributed to atmospheric heating by aerosols (Gautam et al, 2009a, b, Prasad et al. 2009) Gautam, R., N. C. Hsu, K.-M. Lau, S.-C. Tsay, and M. Kafatos (2009), Enhanced pre-monsoon warming over the Himalayan-Gangetic region from 1979 to 2007, Geophys. Res. Lett., 36, L07704, doi:10.1029/2009GL037641. Gautam, R., C. Hsu, K. M. Lau and M. Kafatos, 2009: Aerosol and rainfall variability over the Indian monsoon region : Distributions, Trends and Coupling. Geophys, Annales, 27, 3691-3703, www.ann-geophys.net/27/3691/2009/ Prasad, A. K., K. H. S. Yang, H. M. El-Askary, and M. Kafatos, 2009 :Melting of major glacier in the western Himalaya: evidence of climatic changes rom long tterm MSU derived tropospheric temperature trend (1979-2008), Ann. Geophys., 27, 4505-4519. www.ann-geophys.net/27/4505/2009.

P.5, Line 15: Here, the author should include reference to Lau et al (2010) which showed from GCM experiment that atmospheric heating by black carbon and dust can induce a reduction of the Himalayas and Tibetan snowpack snowpack cover by 6-10%, without greenhouse warming.

P7, Line 10-15: Somewhere in this paragraph the authors have to state clearly how long was the integrations, and whether they are ensemble or single member experiments. If the former, what are number for each ensemble member? If the latter, they have to discuss the caveat, and the uncertainties associated with single experiments. Given the work already done, and the potential importance of the paper, I would urge the authors to conduct ensemble experiments of at least 4-5 members, to increase the statistical significance of their results.

P. 8, Line 8-9: I disagree with the statement that the overall large scale pattern of SFC over the TP is well simulated compared to observations. The statement has to be changed to reflect the large over-estimate of the snowcover, and hence the over-estimate of the BC-in-snow effect in the model.

P.8, Line 24 –P.9, Line 10: The comparison of BC-in snowpack and BC in-ice core are like “apple and oranges”. BC in ice-core is a measure of BC from ancient deposition events, that are not wash away by the seasonal melt, while BC in snowpack in the model are those that are deposited in the first 2 cm which are subject to annual melting and deposition. They can only be used as an order of magnitude estimate, and should not be construed as validating the model BC-in snow estimates.

P.9, Line 19: “Surface radiative forcing” is not a strictly correct term here, as the surface radiative forcing involve aerosols, and clouds feedback from dynamics. Better use “Surface radiative flux changes”.

P. 19-21: The results shown in Fig. 8 and in Fig. 16, clearly show that inclusion of dust and BC in atmosphere and in snow (PD1) has the largest impact on the surface heat budget, and accelerated snowmelt. However, the authors did not show any results of PD1 in Fig. 9 –Fig. 15. Although dust aerosols are less absorbing than BC, they tend to be present in large quantities compared to BC. In monsoon regions, dust can become even more absorbing when mixed with BC, and hence will contribute to more warming and snowpack melt. Although they have done the experiments, the authors did not show how the addition of dust aerosols accelerate the snowmelt in the TP, and alter the cloud, rainfall distributions for the Asian monsoon. Effects of dust aerosols have to be taken into account, when compared to model results to observations, because dust aerosols are always present and vary from year to year in the real world. They should include a discussion of impacts of dust aerosols in the atmosphere and in snow in the paper.

P. 17-21: Here the authors discussed the changes in the South Asian and East Asian monsoon, surface temperature, cloudiness, precipitation based on Fig. 12- 15. The intrinsic variabilities from weather to climate scales of these quantities are very large. To distinguish signal from noise, regions of statistical significance have to be highlights in the figures, and discussion of the statistical significance in conjunction with multi-member ensemble experiments have to be included. If the experiments were carried out for single member, it is likely that all the fields shown, except perhaps surface

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temperature, are not statistically significant.

Recommendation: Accept with revisions.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 22855, 2010.

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