[Referee Comment # 1]

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.

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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 preindustrial (PI) CO_2 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) CO_2 conditions. They found that aerosol-induced snow albedo effect can reduce spring snowpack over the TP, more than the CO_2 in-crease, 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.

Thanks for the general comments. In the revised version we have made significant changes by adding the statistical significance tests and other analysis to strengthen the paper. We have also clarified more clearly the caveats in this study. Please refer to our response for more details.

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.

(1) Snow Cover Overestimation

We recognize the significant overestimation of snow cover, especially over central TP. In the revised version we have added a comparison of monthly snow cover fraction (SCF) averaged over the TP for model and MODIS observation so we can more clearly estimate how much the SCF is overestimated and the potential impact of the overestimated SCF. To understand the positive SCF bias, we have added a comparison of simulated and observed precipitation over the TP. Results show that overpredicted precipitation during winter and early spring is a leading cause for the excessive snowpack in the model (Figure 3b). These comparisons are helpful for future model improvement in predicting precipitation and snowpack in GCMs.

We have correspondingly modified the texts to emphasize the uncertainty of this study related to the excessive snowpack and probably overestimated BC content in snow as well over the TP, and added more discussion of the caveats of this study. We have emphasized in the Title, Abstract and Conclusions that this is just a model sensitivity study and the results could be model dependent. We also pointed out that our current results likely represent only the upper limits of snow impurity effect and therefore should not be extrapolated to the real world.

In fact, as summarized in IPCC (2007), uncertainties and inter-model inconsistency in predicting precipitation are still pretty large among IPCC AR4 GCMs. The confidence in predicting snowfall is even lower, especially over mountainous regions. Roesch (2006) evaluated the snowpack simulation in a dozen of IPCC AR4 GCMs and found that most GCMs predict excessive snow mass (by 20-100%) in spring due to excessive snowfall during winter and spring. Our results are consistent with this finding and highlight the limitations of current GCMs for use in assessing aerosol effects on snowpack because of common problems in simulating the hydrological cycle.

Roesch, A. (2006), Evaluation of surface albedo and snow cover in AR4 coupled climate models, *J. Geophys. Res.*, 111, D15111, doi:10.1029/2005JD006473.

(2) Comparison of BC content in snow with ice-core measurement

We agree with the comment on the comparison between modeled BC content in snow and ice-core measurements. In fact, BC in-ice core can only be used as an order of magnitude estimate. We have added more sentences here to clarify the uncertainty in this kind of comparison in this section.

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.

We have included this comment and stated these caveats upfront in the abstract. We have also added a paragraph in the Discussion section to discuss model uncertainty based on this comment.

The results of this study could be model dependent, and should not be directly extrapolated to the real world or beyond CAM 3.1. Also see the response in (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.

This is an important suggestion, not only for this work but also for any model sensitivity studies. Please note that the climate simulations we performed in our study are all equilibrium runs, not transient simulations. In each run, the forcings (BC, CO2, etc) are kept constant each year. We allow 35 years for the simulations to equilibrate with the forcings and estimate the effects of BC, CO2, etc, by comparing the last 15 years of different 50-year simulations. Hence each year of the last 15 years can be treated as independent samples used to assess statistical significance of the various effects. If we had performed transient simulations with time dependent forcings instead, it would require multiple ensemble members with the same forcings to be treated as independent realizations to establish statistical significance because each year in the transient simulations cannot be treated as independent samples due to the time dependent forcings.

We have more clearly explained how long the simulations are integrated and added the significant tests for variables such as surface air temperature, skin temperature, snow cover fraction, cloud fraction, and precipitation. We have modified all related figures by highlighting the areas passing the 90% significance level and revised the discussion correspondingly. Overall the conclusions are not changed based on the 90% significance level.

Here are the paragraphs we added at Section 2.1:

In all equilibrium experiments, results were averaged over the last 15 years of the 50-year simulations, during which the TOA net energy flux showed no significant trend. So we are giving the system 35 years to equilibrate. Statistical difference between the simulations was determined with two-sided pooled t-tests, using realizations from each of the 15 analysis years. Since the climate had equilibrated by the beginning of the 15-year period, each year is treated as an independent sample of the equilibrium state. Thus, significance is defined relative to inherent inter-annual variability in the model.

[Determining significance from ensembles of multiple simulations (with perturbed initial conditions) offers a greater sampling of the natural model state, but increases the required computational expense substantially. Because we conducted multiple experiments (i.e. 6 experiments) for this study, the added expense of multi-member ensembles (e.g. 6 experiments multiplying by 5 ensemble members for each experiment = 30 experiments totally = 600 model years even if only run 20 yrs for each experiment) would be much excessive than what we can afford.]

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.

We have revised the Abstract based on this comment by removing the introduction-like sentences and adding a paragraph describing the limitations and uncertainties of this study.

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)

We have added the definition for the Organic Matter (OM). Also we have added a statement "Lau and Kim (2010) argued 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., in press.

P.4, Line 9-10: Somewhere around here, reference needs to be made to recent 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, 2009a: 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, 2009b: 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 from long-term MSU derived tropospheric temperature trend (1979-2008), Ann. Geophys., 27, 4505-4519. www.ann-geophys.net/27/4505/2009.

We have added a statement regarding the observations of accelerated warming of the troposphere over the TP, attributed to atmospheric heating by aerosols, by citing three suggested papers.

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 cover by 6-10%, without greenhouse warming.

Done.

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.

Please refer to our response to the General Comment #3 above.

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 overestimate of the BC-in-snow effect in the model.

We have changed this statement to reflect the large overestimate of snow cover, and consequently the overestimate of the BC-in-snow effect in the model. We have also added one more plot here (i.e., monthly model and MODIS SCF, figure 3a) to more quantitatively address how much the SCF is overestimated in the study.

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.

Yes we agree the comparison of BC-in snowpack and BC in-ice core can only be used as an order of magnitude estimate. We have added more sentences here to clarify the uncertainty in this kind of comparison and to urge more in situ measurements for BC content in snow over the TP.

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”.

Good point. We have changed most of “surface radiative forcing” into “surface radiative flux changes, SRFC”.

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.

The focus of our study is on black carbon. We have included dust in the present-day simulation (active in both the atmosphere and snowpack) to improve the realism of the simulation, but we did not design an experiment to isolate the role of dust (i.e., by removing dust). In experiment PD1 dust is included together with Biomass Burning (BB) BC and OM, so we cannot look at the dust effect alone in the current experiment design. As the reviewer notes, dust may in fact be the dominant absorber in this region, but the uncertainties associated with modeling dust are probably even greater than those associated with BC, largely because the emissions of dust are prognostically simulated, whereas BC emissions are prescribed based on bottom-up emission inventories (Bond et al, 2004). That's another reason why we are not attempting to interpret the real world based on the results of this study.

In the revised version, we have added one paragraph and a few references introducing the dust treatment in the model and a new plot showing the seasonal deposition of dust, but we are not able to present more details on the impact of dust alone based on the current experiment design. We would investigate the impact of dust on snowpack in future study.

References:

Mahowald, N. M., Muhs, D. R., Levis, S., Rasch, P. J., Yoshioka, M., Zender, C. S., and Luo, C.: Change in atmospheric mineral aerosols in response to climate: Last glacial period, preindustrial, modern, and doubled carbon dioxide climates, J. Geophys. Res., 111, D10202, doi:10.1029/2005JD006653, 2006.

Zender, C., Bian, H., and Newman, D.: Mineral Dust Entrainment and Deposition (DEAD) Model: Description and 1990s Dust Climatology, J. Geophys. Res., 108(D14), 4416, doi:10.1029/ 2002JD002775, 2003.

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 multimember 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 temperature, are not statistically significant.

As described before, regions passing the statistical test at the 90% significance level have been highlighted in the figures 7, 9, 12-15, and the discussion related to these figures are correspondingly modified. We have also added more descriptions on how long the simulations are integrated and how the statistical tests are done in the revised version.

[Referee Comment # 2]

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.

Anonymous Referee #2
Received and published: 4 November 2010

General Comments The manuscript is a valuable contribution that uses a series of numerical experiments to investigate various scenarios of black carbon, organic matter and dust in the atmosphere and deposited in snow vs. CO_2 and the associated radiative impacts. While revisions are necessary to more fully address the treatment of dust in the experiments, the ability of the model to produce actual snow cover distribution, and the seasonality of snow cover fraction and snow water equivalent, the study is valuable towards assessing the role of absorbing impurities on the snowpack and hydrological cycle. Detailed comments/ suggestions are provided below.

Section 2.2 model evaluation: The authors overstate the ability of the model to capture the snow cover fraction (specifically p. 22863 ln 8). The model overestimates SCF for much of the plateau, which needs to be more clearly addressed in the text. Later results in the paper are based on seasonal variations for SCF, and also for regions that extend beyond that presented in Fig. 2. To be able to more fully assess the results presented in the paper, seasonal SCF variability (ideally modeled and from MODIS) must be presented, and needs to be expanded to encompass the full region presented in most figures (e.g., Figures 5, 7, 9 etc). Currently Fig. 2 only presents SCF for MAM, but SCF for other seasons are addressed in the manuscript without information on seasonally simulated SCF.

We have added a new figure (new figure 3a) showing the seasonal variability of both snow cover fraction (SCF) and snow water equivalent (SWE) from the model and SCF from MODIS, which provide not only the absolute values of SCF and SWE in the control experiment by also more quantitative information on how much the SCF is overestimated in the simulations. To understand the SCF bias, we have added a comparison of simulated and observed precipitation over the TP. The results show that the overpredicted precipitation during winter and early spring is a leading cause for the excessive snowpack in the model (Figure 3b). These comparisons are helpful for future model improvement in predicting precipitation and snowpack in GCMs.

We recognize the significant overestimation of snow cover, especially over central TP. We have correspondingly modified the texts to emphasize the uncertainty of this study related to the excessive snowpack and probably overestimated BC content in snow as well over the TP, and make clearer the caveats of this study. We have emphasized in the Title, Abstract and Conclusions that this is just a model sensitivity study and the results could be model dependent. The current results likely only represent some upper limits of snow impurity effect due to the uncertainties thus they should not be directly extrapolated to the real world or beyond CAM 3.1.

As summarized in IPCC (2007), uncertainty and inter-model inconsistency in precipitation simulation are still very large among IPCC AR4 GCMs. The confidence in predicting the snowfall is even lower than, especially over mountainous regions. Roesch (2006) evaluated the snowpack simulation in a dozen of IPCC AG4 GCMs and found that most GCMs predict excessive snow mass (by 20-100%) in spring due to excessive snowfall during winter and spring.

Although the model is not perfect and the simulation results have bias, the discussions and conclusions of this paper are valuable for improving model performance and insightful for future studies. The fundamental problems in GCM indicate that it remains a challenging task to improve the performance of precipitation and snowpack simulations in climate models, especially at a regional scale.

While it is valuable to provide more information on snow seasonality, it should be noted that the focus of this paper is spring snowpack over the TP and its impact. Spring is more susceptible to snowmelt due to the presence of BC than winter; snowpack is too small in the summer and fall (based on both model and observation) for snow pollution to have any appreciable effects. We discussed the runoff changes in the summer, but this is a consequence of spring snowpack change. Although we discussed the changes of summer East Asian monsoon (July), this is a lagged response of spring snowpack change over TP, as revealed in many previous studies.

Roesch, A. (2006), Evaluation of surface albedo and snow cover in AR4 coupled climate models, *J. Geophys. Res.*, 111, D15111, doi:10.1029/2005JD006473.

p. 22863 ln 12. BC content in snow is reported in ug/kg, but more information needs to be provided regarding what the authors mean by BC. Based on the concentrations provided I assume the authors are referring to BC as including refractory BC plus a portion of colored organics rather than refractory BC alone (which would have lower BC concentrations in this region). The ice core BC concentrations presented by Xu and Ming (which are used for comparison with the simulated BC) are based on methods that assign BC as refractory BC plus a component of colored organics (not just refractory BC).

The BC content in snow follows from the simulated lifecycle of BC, starting with emissions. Hence, our definition of BC is identical to that used in Bond et al (2004) in developing the emission inventories, namely: "the mass of combustion-generated, sp2-bonded carbon that absorbs the same amount of light as the emitted particles." Thus, the simulated BC is indeed refractory-like, but is meant to account for an identical amount of light absorption as the emitted refractory BC and colored organics. We have added above paragraph in 2.1.

p. 22863 ln 17. The assertion that the Indian summer monsoon transports BC to the S. slope of the Himalayas is incorrect (also on p. 22865 ln. 3). Observational studies (e.g., Marinoni et al., 2010) clearly show very low BC concentrations during the monsoon season (due to wet removal of aerosols during higher precipitation periods). The westerlies do transport absorbing aerosols during the winter-spring to this region.

We have modified the manuscript. We agree the pollutants transport and deposition are more important in pre-monsoon season (April-early June.), which is consistent with the maximum forcing in April and May as shown in our paper. Our analysis also focuses on the spring snowpack.

Meanwhile, we noticed that BC concentration in the atmosphere is not linearly related to BC deposition on the ground. Low BC concentration in the atmosphere does not necessarily mean low BC deposition on the ground. The BC concentration over the southern slope of TP is higher during the drier pre-monsoon season and lower during the monsoon season due to efficient wet removal, as observations suggested (Marinoni et al., 2010, Decesari et al., 2010; and Bonasoni et al., 2010). The wet removal of BC, which accounts for more than 80% of total deposition globally partly because of the fine sizes and longer lifetime of BC (Textor et al., 2006), is mainly controlled by precipitation. During the monsoon rainy season, lower BC content measured in the atmosphere is due to the efficient washout process by cloud and precipitation. In other words, the removal rate is higher and the lifetime of aerosol is shorter in rainy season.

BC deposition in the summer is not important because very little snowpack is left on the TP and our study focuses on snow pollution effects. To evaluate model results, surface measurements of aerosol deposition rather than concentration in the atmosphere are urgently needed in a variety of regions over the TP.

We have modified the text correspondingly by adding more discussion about this issue. We have also cited Marinoni et al., 2010, Decesari et al., 2010; and Bonasoni et al., 2010.

Marinoni, A., Cristofanelli, P., Laj, P., Duchi, R., Calzolari, F., Decesari, S., Sellegri, K., Vuillermoz, E., Verza, G. P., Villani, P., and Bonasoni, P.: Aerosol mass and black carbon concentrations, two year-round observations at NCO-P (5079 m, Southern Himalayas), Atmos. Chem. Phys. Discuss., 10, 8379-8413, doi:10.5194/acpd-10-8379-2010, 2010.

Decesari, S., Facchini, M. C., Carbone, C., Giulianelli, L., Rinaldi, M., Finessi, E., Fuzzi, S., Marinoni, A., Cristofanelli, P., Duchi, R., Bonasoni, P., Vuillermoz, E., Cozic, J., Jaffrezo, J. L., and Laj, P.: Chemical composition of PM_{10} and PM_1 at the high-altitude Himalayan station Nepal Climate Observatory-Pyramid (NCO-P) (5079 m a.s.l.), Atmos. Chem. Phys., 10, 4583-4596, doi:10.5194/acp-10-4583-2010, 2010.

Bonasoni, P., Laj, P., Marinoni, A., Sprenger, M., Angelini, F., Arduini, J., Bonafè, U., Calzolari, F., Colombo, T., Decesari, S., Di Biagio, C., di Sarra, A. G., Evangelisti, F., Duchi, R., Facchini, MC., Fuzzi, S., Gobbi, G. P., Maione, M., Panday, A., Roccato, F., Sellegri, K., Venzac, H., Verza, GP., Villani, P., Vuillermoz, E., and Cristofanelli, P.: Atmospheric Brown Clouds in the Himalayas: first two years of continuous observations at the Nepal Climate Observatory-Pyramid (5079 m), Atmos. Chem. Phys., 10, 7515-7531, doi:10.5194/acp-10-7515-2010, 2010.

Textor, C., M. Schulz, S. Guibert, S. Kinne, Y. Balkanski, S. Bauer, T. Berntsen, T. Berglen, O. Boucher, M. Chin, F. Dentener, T. Diehl, R. Easter, H. Feichter, D. Fillmore, S. Ghan, P. Ginoux, S. Gong, A. Grini, J. Hendricks, L. Horowitz, P. Huang, I. Isaksen, T. Iversen, S. Kloster, D. Koch, A. Kirkevåg, J.E. Kristjansson, M. Krol, A. Lauer, J.F. Lamarque, X. Liu, V. Montanaro, G. Myhre, J. Penner, G. Pitari, S. Reddy, Ø. Seland, P. Stier, T. Takemura, and X. Tie: Analysis and quantification of the diversities of aerosol life cycles within AeroCom**,** Atmos. Chem. Phys., 6, 1777-1813, 2006.

Section 3.1. -How is dust defined and simulated? There is scarce information on dust in the manuscript- more details regarding dust are needed.

The focus of our study is on black carbon. Dust was not the focus of this study, but we included dust in experiment PD1 to achieve a more realistic simulation. Prognostic dust emissions are based on the Dust Entrainment and Deposition Model (Zender et al, 2003), and advected in 4 bulk size bins. Dust processes in this version of CAM are summarized in Mahowald et al, (2006). The visible single-scatter albedo values assumed for the four size bins of dust in snow range from 0.88 (largest size) to 0.99 (smallest size), as mentioned in Flanner et al (2009).

We did not design an experiment to isolate the role of dust (i.e., by removing dust). In experiment PD1 dust is included together with Biomass Burning (BB) BC and OM, so we cannot look at the dust effect alone in the current experiment design. As the reviewer notes, dust may in fact be the dominant absorber in this region, but the uncertainties associated with modeling dust are probably even greater than those associated with BC, largely because the emissions of dust are prognostically simulated, whereas BC emissions are prescribed based on bottom-up emission inventories (Bond et al, 2004). That's another reason why we are not attempting to interpret the real world based on the results of this study.

In the revised version, we have added two paragraphs (Section 2.1) introducing the dust treatment in the model and the uncertainty related to dust simulation (Section 5). We have also added a new plot showing the seasonal deposition of dust, but we are not able to present more details on the impact of dust alone based on the current experiment design. We would investigate the impact of dust on snowpack in future study.

References:

Mahowald, N. M., Muhs, D. R., Levis, S., Rasch, P. J., Yoshioka, M., Zender, C. S., and Luo, C.: Change in atmospheric mineral aerosols in response to climate: Last glacial period, preindustrial, modern, and doubled carbon dioxide climates, J. Geophys. Res., 111, D10202, doi:10.1029/2005JD006653, 2006.

Zender, C., Bian, H., and Newman, D.: Mineral Dust Entrainment and Deposition (DEAD) Model: Description and 1990s Dust Climatology, J. Geophys. Res., 108(D14), 4416, doi:10.1029/ 2002JD002775, 2003.

More attention in general needs to be paid to the seasonality of aerosols (BC and dust) rather than just SCF.
-BC and dust deposition in the Himalaya and TP peak during the winter and spring and have very low concentrations during the summer monsoon season. I'd like to see the manuscript more fully address that the seasonal importance of impurities in the snowpack is driven both by seasonality of snow on the ground and the timing of aerosol deposition. Summer monsoon precipitation has a very low aerosol loading. This is briefly addressed at the end of section 3.1 related to dust, but not to BC.

This comment brought up a potentially interesting issue, so we have taken the suggestion seriously by adding more analysis (e.g. adding a new figure 3, which includes 4 panels) and a few paragraphs discussing the seasonality of snowpack and BC/Dust deposition. See below:

Figure 3a shows the monthly mean snow cover fraction (SCF) and snow water equivalent (SWE) from the model and SCF from MODIS. The observation shows around 25-35% of areas are covered by snow during winter and spring over the TP. The model overpredicts SCF by 20-100% from November to April, but underpredicts SCF in the warm season probably because the model with coarse horizontal resolution and smoothed terrain fails to capture the snowpack at very high elevation during summer. Although SCF reaches a maximum in winter, snow continues to accumulate so SWE peaks in March until snow melt.

To investigate the causes of snowpack overestimation over the TP, we compared the simulated monthly precipitation over the TP against three different observational datasets (Figure 3b). The model substantially overpredicts precipitation during the cold season, which probably is the primary factor causing the excessive snowpack over the TP in winter and spring.

Figure 3c shows the seasonality of BC deposition. Model results show that the total deposition of BC is mainly (>80%) contributed by wet deposition partly because of the finer size and longer lifetime of BC, which is consistent with other model studies (Textor et al., 2006). Overall the seasonal variation of BC deposition is not very large. It should be noted that aerosol deposition is not linearly correlated with aerosol concentration in the atmosphere, i.e., a higher (lower) BC concentration in the atmosphere does not necessarily mean a higher (lower) BC deposition on the ground because precipitation and cloud control wet removal, and boundary layer structure and land surface properties affect dry deposition.

Measurements studies show that maximum concentrations of BC occur during the pre-monsoon season, while minima appear during the monsoon season and post-monsoon period for the coarse mass (Marinoni et al., 2010; Decesari et al., 2010; Bonasoni et al., 2010). The lower BC content in the atmosphere measured over the TP in the monsoon season is probably due to the frequent and rapid washout process (Marinoni et al., 2010), which implies a possible higher wet deposition in the summer. In other words, removal rate is probably higher and lifetime of aerosol is probably shorter in the rainy season. It should be noted that the above measurements are made over the southern slope of Himalayas, but the model results shown in Figure 3c&d are an average over the entire TP. Besides India, the emissions from other regions also contribute to the BC over the TP (Ming et al., 2008, 2009). To evaluate model results, surface measurements of aerosol deposition rather than concentration in the atmosphere are urgently needed in a variety of regions over the TP (Kaspari et al., 2011).

BC deposition in the summer is not critical in this study since very little snowpack is left over the TP in the summer (Figure 3a). The seasonal dependence of snowpack impurities is driven both by seasonality of snow on the ground and timing of aerosol deposition. In Figure 3c we show the monthly mass of BC in the top snow layer averaged over the areas with snow in the TP. The highest BC mass in snow can be found in late spring because deposition continues but less new snow is accumulated. In addition, snowmelt in spring may decrease snow thickness and increase BC mass accumulating over snow since a fraction of BC is washed away by the melt water.

In contrast to BC, dust deposition peaks at April, as shown in Figure 3d, because BC and dust have different sources and lifetime. The magnitude of dry deposition for dust is comparable with that of wet removal due to the larger size of particles.

Kaspari, S.D., M. Schwikowski, M. Gysel, M. G. Flanner, S. Kang, S. Hou, and P. A. Mayewski (2011), Recent Increase in Black Carbon Concentrations from a Mt. Everest Ice Core Spanning 1860-2000 AD, Geophys. Res. Lett., doi:10.1029/2010GL046096, in press.

Section 3.3. The manuscript reports seasonal changes in SCF and SWE under the various scenarios, but never reports what the assumed initial SCF and SWE are. Authors need to provide information on inputs into the model of SWE and SCF for the full region interpreted in the figures, including seasonality.

We are not so clear what the reviewer is asking for here. Anyway we have added a new plot (Figure 3a) showing the observed and simulated SCF and SWE (absolute values rather than changes), if the reviewer requests to show absolute (initial?) model snow cover. There is a specific definition in climate model for the word "initial field", which means the state when you start to run the model. The GCM simulations reported in this study were generated using a coupled model that includes atmosphere, land, and ocean. The same forcings are applied each year for 50 years. Snow is initialized at the beginning of each simulation, but we analyze only results of the last 15 years of each 50-year long simulation, so the initial SCF and SWE values are irrelevant. The seasonal changes reported in the study are calculated by comparing SCF and SWE averaged over the last 15 years from different simulations that include BC, CO2, etc, with the control simulation (i.e., the changes are not calculated with respect to initial conditions). If the reviewer wants the model parameterization of snow fraction, model SWE depends on model precipitation and snowpack processes, and snow fraction depends on snow thickness and snow density according to Niu and Yang (2007).

Niu, G.-Y. and Yang, Z.-L.: An observation-based formulation of snow cover fraction and its evaluation over large North Amer- ican river basins, J. Geophys. Res., 112, D21101, doi:10.1029/ 2007JD008674, 2007.

p. 22869 ln 3 related to greater precipitation coming in the form of rain rather than snow. Relative changes are presented in the manuscript of changes in temp and SCF and SWE- is there substantial information to determine that more precipitation would occur as rain rather than snow? Many regions of the study area are at very high elevations where rain occurs infrequently (or never).

We have modified the text to weaken the statement here since we didn't calculate the relative contribution to the decreased snowpack induced by more rain instead of snow versus more melting. However, our discussion here is based on the average of the TP. Although many regions in the study area are at very high elevation, there are also many regions at lower elevation where surface air temperature is close to the threshold of freezing so precipitation could occur either as rain or snow even during winter time.

Tables and Figures: more detailed figure and table captions are needed. For example Fig 13 caption refers to Fig 12, which refers to Fig 7. This makes discerning what the figures are showing difficult. While each figure doesn't need to explain the model scenarios, the parameters shown in the figure should be clear from the caption, including seasons. So many captions referring to 'same as in Fig X' made it difficult to determine what was shown in the figures, and for which season (or months).

We have revised the figure captions to avoid confusion (for examples, new figure 13, 15).

Specific Comments:
p. 22857 ln 24- reference needed for soot reduction of albedo.

Added.

p.22858 ln 1- local RF from soot in snow can be much higher ln. 10- sentence not clear restructure portion related to TP above 2000m

Modified.

p. 22859 ln 9- observational or modeling studies? Section 2.1 ln 14- what is meant by annually repeating emissions? Is the seasonality of aerosols in the atmosphere included in the model? What is used for dust inputs?

Both modeling and observational data analysis studies.

Yes, there is seasonality in both BC and dust emissions, atmospheric concentrations, and deposition to snow. We prescribe BC emissions that vary by month (Bond et al, 2004), but are identical each year of the simulation. Dust emissions (Zender et al, 2003, Mahowald et al, 2006) are prognostic, and depend on local soil moisture, surface wind speed, soil erodibility, and vegetation. Hence model dust emissions vary seasonally and inter-annually.

We have modified the text correspondingly.

p. 22861 ln 17- include justification for multiplying organic carbon emissions by 1.4.

This OM:OC ratio is appropriate for fossil OC emissions (Russell, 2003). Biofuel and biomass burning emissions can contain higher ratios. The prescribed biomass burning OM emissions (and hence OM:OC ratios) vary with vegetation type (van der Wer et al, 2006).

Russell, L. M.: Aerosol organic-mass-to-organic-carbon ratios, Environ. Sci. Technol., 37, 2982–2987, 2003.

van der Werf, G. R., Randerson, J. T., Giglio, L., Collatz, G. J., Kasibhatla, P. S., and Arellano Jr., A. F.: Interannual variability in global biomass burning emissions from 1997 to 2004, Atmos. Chem. Phys., 6, 3423–3441, 2006, http://www.atmos-chem-phys.net/6/3423/2006/.

p. 22863 ln 29 include references for measured BC in snow (and as stated prior it should be clear what is meant by BC). Section 3.2 and Figure 7. The greatest amount of warming occurs in NE China (as mentioned in text). Is this what is expected in reality? Without including information on SCF for this region this is harder to assess, but surprising that the warming in this region would be greater than that on the Plateau. Is this an artifact of the model, or is greater warming expected here?

References are added. The definition of BC in this study is also added. The large warming in NE China is consistent with the region being one of the most industrialized areas in China (e.g. Liaoning, Jilin) and BC emission is very large in those provinces. Since our study focuses on the effects of snow pollution in the TP, discussion of the BC distribution and its impact on snow over NE China is beyond the scope of this paper. More details on BC content in snow and its impact in this area can be found in Huang et al. (2010).

Huang, J., Q. Fu, W. Zhang, X. Wang, R. Zhang, H. Ye and S. G. Warren, Dust and black carbon in seasonal snow across Northern China, *Bull. Amer. Met. Soc.*, Bulletin of the American Meteorological Society 2010 , doi: 10.1175/2010BAMS3064.1

p.22868 ln 13. Again, if NE China is included in discussion of SCF, the manuscript needs to provide information on SCF for this region in Fig. 2. On numerous occasions in the manuscript the authors use the wording "We can find". Rewrite these sentences to avoid this wording- writing will be more succinct.

The focus of this paper is on TP so we have removed/changed some discussions related to NE China. The MODIS SCF over other parts of China can be found in Hall and Riggs (2007) so it is not necessary for this paper to present similar analysis or discussion again (Ye and Bao, 2005). We have also removed "we can find' in some places.

Hall, D. K., and G. A. Riggs (2007), Accuracy assessment of the MODIS snow products, *Hydrol. Processes*, *21*(12), 1534–1547, doi:10.1002/hyp.6715.

Ye H and Bao Z 2005 Eurasian snow conditions and summer monsoon rainfall over South and Southeast Asia: assessment and comparison *Adv. Atmos. Sci.* **22** 877–88.

p. 22873 ln 13- mean fig 12 (as opposed to fig 13)?

We refer to the resulting feedback of clouds, so should be old Fig 13b.

p. 22873 ln 17- True during spring, but again, aerosol loading associated with the summer monsoon season is very low- well established in the observational record.

We have addressed this in the revised manuscript. Also here we state "from April to Mid-June".

p. 22875 ln 5- SH and LH need to be defined (sensible and latent)

Done.

p. 22879- paragraph beginning line 8- how well constrained do the authors consider the seasonality of aerosol deposition, SCF and SWE? While improved models and higher grid resolution are key to improving this type of study, improved observational data is also needed to assess the validity of the model results. Observational data are currently scarce.

We have already addressed the seasonality issue of aerosol deposition. Yes we agree that more observational data is critical to constrain the impact of snow impurities over this region. We have added this comment in the text.

Table 1- define PI and PD. Fig 2- needs to be expanded to encompass full study area, and show seasonality of SCF and SWE (not just MAM since other periods discussed elsewhere in manuscript).

Explanations are added in Table1. The reason for focusing on the springtime snowpack over the TP has addressed and new figures are added.

Fig 3- what is meant by in cycle? Is BC in snow data based on Xu and Ming, and if so, where does the data reflect (average concentration from cores; from which period?)?

Circle, corrected. Ming and Xu's data over various sites are collected from different periods. More details for each dataset are referred to their papers. BC in-ice core can only be used as an order of magnitude estimate for BC content in snow. We have modified the text and emphasized the uncertainty in this kind of comparison.

Fig 4 and p. 22864 ln 20- figure caption says annual forcing, text refers to MAM. Revise to make consistent.

Good catch. We have corrected the text.

Fig 9- units needed for color bar. Is this %?

Not %. 0-1.

Fig 11. Why are Sep-Dec excluded?

The temperature changes in those months are very small or even negative (see figure 9) so it doesn't make sense to calculate the snowmelt efficacy for those months.

[Referee Comment # 3]

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.

Anonymous Referee #3
Received and published: 22 November 2010

General The climate effect of black carbon (BC) deposition on snow has been highlighted since the release of IPCC AR4 (2007), and dust deposition issue is also being hot due to its absorbing nature in snow. The melting of the glaciers on the Tibetan Plateau (TP) is concerned by societies for it is a very important water supply for billions of people. Depositions of BC and dust were blamed for being responsible for part of their melting. This work primarily used modeling methods to interpret the impacts of BC and dust on the snow and ice melting on the TP and then the variation of general circulations for monsoons. And it is a first comprehensive report on influence of BC and dust. From this point of view, it should be published at last.

However, I have some specific comments that should be addressed:

1) Line 5 in P. 22856. The statement "the TP glaciers have : : : in the world", is not true. Some of the glaciers on the TP are advancing. This sentence could be change to "the TP is one of the regions that glacier are suffering the fastest melting".

This sentence has been moved, as suggested by Reviewer 1.

2) Line 24 in P.22857. A reference literature should be after ": : : by weight (ppmw) of soot".

Added.

3) Line 21 in P.22859. I doubt the reference "Qin et al., 2006" stated that, as I have pointed out in 1). Please change another reference.

This sentence has been modified.

4) Line 5-7 in P.22860. The statement is not correct. Both of Ming et al. and Xu et al. meant the increase of BC concentrations in the ice cores were on the northern slope of Himalayas. And I could not find Xu et al. 2009 in the reference list. Please check that throughout the paper.

This sentence has been changed and Xu et al. (2009) has been added in Reference.

5) Line 13 in P.22863. Should add a reference (Ming et al., 2009) after "Figure 3 : : : in snow". Also in the caption of Figure 3.

Added in both places.

6) Line 9 in P. 22864. Ming et al., 2008 should be changed to Ming et al., 2008 and 2009. And Ming et al., 2008 is BC in the ice core, and Ming et al., 2009 refers to BC in the snow (Ming, J., C. Xiao, H. Cachier, D. Qin, X. Qin, Z. Li, and J. Pu (2009), Black Carbon (BC) in the snow of glaciers in west China and its potential effects on albedos, Atmospheric Research, 92(1), 114-123).

Changed. Ming et al. (2009) has also been added.

7) Line 26-28 in P. 22885. I did not find the authors cited Xu et al., 2006.

The citation added.

8) Fig. 3 in P. 22890. Has been pointed out in 5).

Done.

9) Fig. 4 in P. 22891. The plot just showed the result in the Northern Hemisphere. So just the upper half of the figure is enough.

It looks better to show the globe. The white color in southern hemisphere indicates the forcing is very minor in Antarctic.

10) Fig. 8 in P.22895. The legend name of the experiment should be in consistent with Table 1 using capital letters. The same issue also existed in Fig. 16 and the text.

We have modified Table 1.

[Short comment # 1]

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.

T. J. Yasunari
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Received and published: 9 November 2010

The authors attempted to estimate how black carbon and dust impact on TP (Tibetan Plateau) glacier melting, water cycle, atmospheric circulation, and radiative forcing by a modeling study. This approach is right way and the outcomes are very important for the next step in the TP climate study. However, as Dr. Lau mentioned, I also have the same question on the outcomes because the authors did not show any statistical significance levels. I do not know how robust this study is. Currently, the topic on Himalayan glacier retreats is very sensitive to general public because of the misleading of the glacier disappearance mentioned in IPCC (2007). Hence, we should be more careful for this kind of topic and do not exceed proper interpretation. Please do not forget that this study started from the overestimate of snow cover over TP like TP ice sheet. It means that the author only can discuss in terms of sensitivity and currently can not connect to the real TP condition and climate. Before the acceptance of this paper, the following things should be sure to satisfy. After these revisions, this paper is worth publishing in ACP.

1. As Dr. Lau mentioned, if the authors carried out ensemble simulations, the statistical significance levels should be shown. If the authors carried out single simulation for each case, they should carry out ensemble simulations with confidence limits. Only single simulation for each case loses the robustness of this paper.

This issue has been addressed in the response to Reviewer 1. (Specific Comments 1 and 3)

2. Please re-check the whole draft carefully and remove misleading and overinterpretation parts so that general public will not believe that this study is consistent with real world over TP. Especially for snow-related statement such as P. 22863 Lines 8-9, etc. The authors should not use "well captured or well simulated" for the snow-related statements including BC concentrations in snow because the snow cover over TP was largely overestimated and not real world.

We have revised that part by adding more analysis and new figure 3. We have also clarified more clearly the caveats in this study in the Abstract and conclusions.

3. The comparison of BC concentrations in snow between the sporadic observations and simulations (annual mean) in Fig. 3 is not good because annual and seasonal BC concentrations are quite different (e.g., Fig. 3 in Xu et al., PNAS, 2009). I recommend the authors to show spring or summer mean of BC concentrations over TP. What the authors can do currently is to only compare order of magnitude in BC concentrations and emphasize this point again.

We agree with the comment on the comparison between modeled BC content in snow and ice-core measurements. In fact, BC in-ice core can only be used as an order of magnitude estimate. We have added more sentences here to clarify the uncertainty in this kind of comparison in this section.

Other parts
P.22856 Line 12: The information on vertical resolution should be added such as 0-2 cm snow.

We have removed that number, as suggested by Reviewer 1.

P.22858 Line 28: The EHP effect was first mentioned by Lau et al. (2006, Climate Dynamics) and the authors should cite the first paper here.

Cited.

P. 22860 Line 19: Mt. Everest ice => Mt. Everest ice core

Done.

P. 22863 Line 27: References lacked. Maybe, Xu et al. (2006), Ming et al. (2008, 2009).

Added.

P. 22869 Lines 2-3: The authors did not show how much snowfall decreases and rainfall increases. Please show the data and discuss this statement.

We have modified this sentence.

P. 22870 Line 1: Sect. 3.4 => Sect. 3.3?

Changed.

P. 22877 Line 7: A reference lacked. Maybe Ming et al. (2009).

May not need a reference since a related reference was already given in the Introduction and here is just a summary.

P. 22877 Line 14: 100 ug/kg in 0-2 cm snow?

We have removed that number, as suggested by Reviewer 1.

Fig. 7: So much red color is difficult to compare. Please change the colors in the color bar.

It would look better if you read the printed one. In the revised figures we have highlighted the areas passing the 90% significance level. We also use contour lines to highlight the areas with warming larger than 1.0^{o}C.

Figs. 6 and 8: For pd1-pi1 case, the forcing has the peak in March but the temperature difference has the peak in April. Please explain this one month lag.

The air temperature change is a response to the surface energy budget perturbation, which first changes the snowmelt and skin temperature, so it is not surprising that the air temperature change occurs later than the surface energy budget change.

Fig. 10: Why does the difference in runoff have the peak in March (1 month earlier than snow fraction and SWE)? Please explain this.

The increased runoff comes from not only the earlier snowmelt but also possibly from a greater percentage of precipitation coming in the form of rain rather than snow in a warmer climate. So it is not surprising that the changes of runoff and snowpack occur at different months.

Fig. 16: pd1-pi1 in surface forcing has maxima in March-April, but the SH flux has the maxima in April-May. Please explain these relationships and why 1 month lag exist here.
In addition, please explain in detail in the main text how soil moisture retain and timelag of runoff after precipitation is taken into account in the calculation of land surface model.

Already briefly explained in the previous responses. Please see Yasunari et al. (1991) and Chow et al. (2008) for more details on time-lag response to the snowpack and the role of soil moisture.

[Short comment # 2

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.

X. Chen
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Received and published: 24 October 2010

In the abstract, 'Contributed by the significant increase of both sensible heat flux associated with the warm skin temperature and latent heat flux associated with increased soil moisture with long memory', i don't agree with the 'increasing of sensible heat flux associated with the warm skin temperature'. A simple equation of sensible heat flux as following: H= rhoa * cp * Cd * U *(Ts-Tair) , rhoa is air density, cp is the specific heat capacity of air, U is wind speed, Ts is skin temperature, Tair is air temperature. The variation of sensible heat flux is determined by several variables, not only by skin temperature. The wind speed is weakening over the Tibetan Plateau,demonstrated in Qinglong You et al.(2010). Actually, according to two recent published papers (listed below), the sensible heat is decreasing.

Guo, X., Yang, K., and Chen, Y.: Weakening sensible heat source over the Tibetan plateau revisited: Effects of the land–atmosphere thermal coupling, Theoretical and Applied Climatology, 1-12, 10.1007/s00704-010-0328-1, 2010.

Yang, K., Guo, X., and Wu, B.: Recent trends in surface sensible heat flux on the tibetan plateau, SCIENCE CHINA Earth Sciences, 1-10, 10.1007/s11430-010-4036-6, 2010.

Qinglong You, Shichang Kang, Wolfgang-Albert Flügel, Nick Pepin, Yuping Yan, Jie Huang. Decreasing wind speed and weakening latitudinal surface pressure gradients in the Tibetan Plateau. Climate research, 2010, 42:57-64.Doi:10.3354/cr00864

What's your considerations of the variations of Ts-Tair in the equation?

It is true that the SH change is determined by several factors including Ts-Tair. Our statement is based on the simulation results as shown in Figure 16 and 12. It should be noted that our conclusion is based on the results of sensitivity experiments with or without including snow impurities effect. Given the many uncertainties in this study, we are not attempting to compare the simulated results with observations in the real world, which are affected by many other factors.