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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observed high-altitude warming and snow cover retreat over Tibet and the Himalayas enhanced by black carbon aerosols" *by* Y. Xu et al.

Anonymous Referee #1

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The authors present a study on the impact of different climate forcers on the regional climate of the Tibetan plateau. They used the CESM1 global model coupled to an aerosol scheme representing different types of aerosols like BC, dust, sea salt, sulfate and coupled to the CLM4 land surface scheme for the representation of snow and snow processes. The authors looked at the specific roles of CO2, BC in the atmosphere and in the snow, and sulfate in the atmosphere on the warming over the Tibetan plateau in the recent decades. They concluded that the simulations represent well the observed decrease in snow cover. They also state that BC plays a more important role in the observed warming over the Tibetan plateau compared to the global mean of the warming

effect of BC. This stronger impact of BC, thus, contributes to the stronger-than-average increase of temperatures in the studied region. While the presented results and conclusions are possibly justified based on the results of the simulations, I find that a major and indispensable step in the model validation is missing: It remains unclear how well the snow cover itself concerning parameters like extent, duration, or melting date is represented in the global model. It further remains unclear how well the BC in snow concentrations are simulated. For validation purposes the authors only show observed and simulated trends of the snow cover. Since these patterns are similar the authors assume that the model well represents the impact of a changing snow cover. In my opinion this conclusion is not justified based only on the presented data and further validation of the model is needed. Therefore, I recommend major revisions before a potential publication of the manuscript in ACP.

Comments:

The authors need to define the meaning of the parameter "snow fraction". For a full description of the snow cover multiple parameters are needed like snow height, SWE, snow cover extent, snow cover duration, snow melt-out dates, and so on. It remains unclear what parameter is used. Since the observations are based on remote sensing data, I assume that the snow fraction is related to snow cover extent? But what trend is shown in Figure 1? The trend in the maximum snow covered area or the period with snow cover? This needs to be specified.

It is well known that global models tend to overestimate the snow cover of the Tibetan plateau. One potential reason is that the blocking effect for the moisture transport crossing the Himalayas is too small due to the coarse resolution of the global models. As a result the precipitation over the Tibetan Plateau is overestimated. This limitation can partly be overcome with models using higher spatial resolutions (e.g. Ménégoz et al., 2013). By the way, how well are the high altitude regions represented in the used global model? The authors explicitly state that the observed warming has been important in high altitude regions. A spatial and temporal overestimation of the snow

cover over the Tibetan Plateau in general will certainly lead to an overestimation of the snow-related effects. Therefore, it is crucial to validate the simulated snow cover using observations. Validating the model with simulated and observed trends can only be a second step.

The impact of a changing snow cover and the involved feedback mechanisms are very complex and depend on many parameters: timing of the melt-out dates, incoming solar radiation, latitude, altitude, and possibly others. These parameters all influence the derived radiative forcing. For example, Jacobi et al. (2015) showed monthly averages of the radiative forcing related to the presence of BC in snow in the Himalayas. It can be assumed that if the melt-out dates are wrongly simulated the same shift in the melting of the snowpack can lead to an incorrect radiative forcing because it will not be similar for different months. Again, a correct model response regarding the impact of a changing snow cover can only be expected if the snow cover is correctly represented. I am also surprised to note that the simulated radiative forcing is larger for the reduction of the snow albedo due to the presence of BC compared to the radiative forcing caused by the earlier melting of the snowpack. This is opposite to results of many previous studies concerning light-absorbing impurities in snow (e.g. Flanner et al., 2007; Painter et al., 2007; Jacobi et al., 2015). Is this difference related to an overall limited representation of the snow cover in the model?

What are the simulated BC in snow concentrations? Do they correspond to observations? I admit that the available data are scarce, but still the few observations give an order of magnitude for the BC in snow in the Himalaya/TP region. If in the simulations the BC in snow concentrations are incorrect, but the simulated trends in the snow cover as well as in the albedo are correct, this would in my opinion suggest that the model sensitivity is incorrect.

SO4 should be substituted by either “sulfate” or SO_4^{2-}.

There are no SO4 emissions. The authors probably refer to emissions of SO_2?

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