Reviewer #2:

In this work, the authors estimate the role of BC increase in the tropospheric warming of the Tibetan Plateau region using a coupled AOGCM at high horizontal resolution, forced by observationally-based BC aerosol datasets.

Novelty: Use of High resolution coupled OAGCM with coupling between snow over land and BC deposition. I think this last improvement is crucial. This analysis is also based on a previous work where the BC forcing is re-estimated by using satellite + ground based optical depth, with a new methodology to separate the BC contribution to solar absorption from other aerosols and its direct rad forcing

I would recommend to publish the paper, after considering minor comments below:

Response: Thanks for reviewing our paper.

Specific comments

Lines 21-25. What do the CMIP5 models simulate as surface warming on that region in their historical simulation? Any reference about it?

Response: According to recent model evaluation papers, the strong warming trends in this region were not well captured by the CMIP5 historical simulations. For example, You et al., (2015) showed that "CMIP5 GCMs can reproduce the recent temperature evolution in the TP, but with cold biases... most CMIP5 GCMs underestimate the observed warming rates, especially the CNRM-CM5, GISS-E2-H and MRI-CGCM3 models."

We now include this reference in the 3rd paragraph of Introduction as follows: "To date global climate models forced by historical radiative forcing scenarios (such as those in Coupled Model Intercomparison Project Phase 5, CMIP5) have difficulty in simulating the observed record surface warming (You et al., 2015) or its anomalously strong altitude dependence in Tibet/Himalaya region."

You, Q., Min, J. and Kang, S.: Rapid warming in the Tibetan Plateau from observations and CMIP5 models in recent decades, Int. J. Climatol., n/a–n/a, doi:10.1002/joc.4520, 2015.

Line 15: I would suggest to add Lau et al. [2006] and Lau and Kim [2006] in the list of references

Response: Thanks for the suggestions. Now it is changed to "Many previous studies have linked Asian aerosols (including sulfates and BC) with monsoon systems and have demonstrated the aerosol impact on the summer rainfall (Ramanathan et al., 2005; Lau et al., 2006; Lau and Kim (2006); Meehl et al., 2008). "

Lau, K. M., Kim, M. K. and Kim, K. M.: Asian summer monsoon anomalies induced by aerosol direct forcing: The role of the Tibetan Plateau, Clim. Dyn., 26(7-8), 855–864, doi:10.1007/s00382-006-0114-z, 2006.

Lau, K. M. and Kim, K. M.: Observational relationships between aerosol and Asian monsoon rainfall, and circulation, Geophys. Res. Lett., 33(21), 1–5, doi:10.1029/2006GL027546, 2006.

Methodology BC treatment in the model: If you may briefly summarise here in which consists the correction you applied by the Xu et al papers, that would be very useful for the reader

Response: We now add more details on how the corrections were done in the model: "The present-day BC emission is adjusted from the standard model emission inventory (Lamarque et al., 2010) to account for the potential model underestimation of BC forcing. Emissions over East Asia regions are increased by a factor of two and South Asia regions by four. The emissions are adjusted by the same ratio in all economic sectors (energy, industrial, etc.) and all seasons by the same ratio. "

Model experiments Could you please specify here that you increase separately BC, SO4 CO2 in the perturbed equilibrium 5 ensemble members simulations?

Response: Yes, we now clarify that we used "(b) Four sets of perturbed simulations with instantaneously imposed present-day forcing: BC, SO4, CO2 and all three forcing combined. " and

"(c) Three sets of perturbed simulations but with fixed sea surface temperature. These are also forced by the instantaneous increase of BC, SO_4 and CO_2, separately,"

Which preindustrial and present day emissions have you used?

Response: We now added that "Except for the adjusted BC present-day emission as detailed in section 2.2(b), all other emission/concentration are from the standard inventory adopted by CMIP5 models, as descripted in Lamarque et al., (2010)."

Lamarque, J.-F., Bond, T. C., Eyring, V., Granier, C., Heil, A., Klimont, Z., Lee, D., Liousse, C., Mieville, A., Owen, B., Schultz, M. G., Shindell, D., Smith, S. J., Stehfest, E., Van Aardenne, J., Cooper, O. R., Kainuma, M., Mahowald, N., McConnell, J. R., Naik, V., Riahi, K. and van Vuuren, D. P.: Historical (1850–2000) gridded anthropogenic and biomass burning emissions of reactive gases and aerosols: methodology and application, Atmos. Chem. Phys., 10(15), 7017–7039, doi:10.5194/acp-10-7017-2010, 2010.

Not clear, lines 21 on: in the perturbed simulations you impose BC, SO4 and GHG as concentrations or you apply emissions for BC and sulphur (as specified in lines 8-12) and specify CO2 concentration?

Response: We now clarify that "The forcings were imposed by instantaneously increasing the emissions of BC or SO4, or by increasing CO_2 concentration to present-day level (400 ppm)."

Section 3. It seems to me by looking at figs 1, and S1 that there is an important decadal modulation of the snowcover, more than a trend. Would it be possible to look at the area averaged time series of snow cover from 1967 from dataset NSIDC? By averaging where there is a negative (blue) linear trend. Also applying a running mean could be useful in order to help in understanding the amplitude of such low-frequency variations. How different is this variability simulated by the model (at decadal timescale) w.r.t. observations in snow cover (for the 40 years)?

Response: Thanks. This is an important point regard to the snow cover variability/trend. Our argument as laid out in the submitted manuscripts was that at the multi-decadal time scale (1967-

2012 as in Fig 1), the changes along the Himalaya mountain range are long-term trends for which we aimed to attribute to various external forcings, while the changes at the multi-year time scale (positive changes in in 2001-2012, as in Fig S1(a,b) and negative changes in 1980-1991 as in Fig S1(c)) are more subject to the natural variability.

We have only showed the regional map of the snow fraction changes during the two 10-year periods in the submitted manuscript (Fig S1). Below is the time series of the regionally averaged snow cover for the last 40 years (smoothed to remove inter-annual variability) as suggested by the reviewer, and it supports our argument that the long-term trend is much weaker than short-term variation. Similar discussions were also found in many previous literatures, especially for seemingly increasing trend after year 2000 as more data become available.

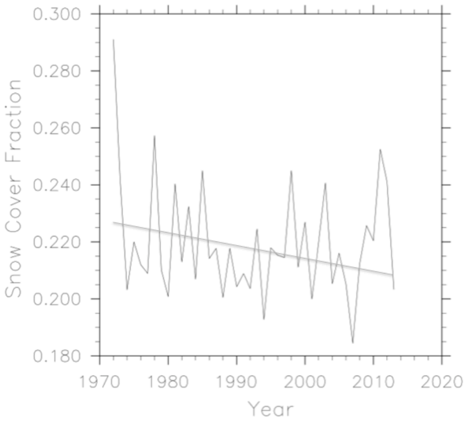


Figure. Annual mean snow cover fraction averaged over the Tibetan Plateau and its vanity region (20–50°N, 70–110°E), as the domain used in Fig 4.

However, we do acknowledge that the snow cover observational dataset over this region has issues with data availability. Therefore, we also cited a few other in-situ studies on mountain glaciers and permafrost to support the declining trend, in the beginning of section 3. " In-situ studies on regional glaciers and snow pack also reported strong declining trends. For example, Ma and Qin (2012) used 754 stations in China to document statistically significant declining trends of spring snow for the

Qinghai-Tibet Plateau for the 1951-2009 period. Consistently, permafrost degradation has been reported on the Tibet Plateau (Cheng and Wu, 2007; Li et al., 2008)."

You ascribe a better simulated snow cover to a high-resolution model (that may be ok for the better simulated orography), is this really the only factor?

Response: A number of new model features may have contributed to the improvement of snow cover simulation. We discussed in the method section that "The land model (CLM4) also includes major updates, making it more versatile in simulating snow packs (Lawrence et al., 2011). .....other parameterizations include snow compaction (Lawrence and Slater, 2010) and the albedo calculations for snow on or around vegetation (Wang and Zeng, 2009). Compared to the previous model versions, the albedo contrast between snow-covered and non-snow-covered area is more consistent with observations..." The model evaluation against observations and its improvement over older model versions were extensively documented by Lawrence et al., (2011).

However, we do think that the higher resolution and better resolved orography is a big contributing factor for this region, especially compared to a few cited previous studies on the same issue (Flanner et al., 2009, Qian et al., 2011, and Menon et al., 2010).

Is Figure 3 only over 80-100E or 0-360 longitude global average?

Response: It is the global averaged zonal mean. We now clarified in the figure caption " Globally zonal averaged radiative heating rate ....Fig. S2 shows the normalized temperature profile averaged just over the Tibetan Plateau (30 to 40°N and 80 to 100°E)."

Section 4 Which is the role of water vapour feedback in the T change increase versus altitude? And which is the role of changes in clouds?

Response: It is definitely an important point worth discussing. We now include the following sentences in the end of the discussion section. "Beyond the three main factors as we have discussed above, the changes of water vapour and clouds are also possible mechanisms contributing to the

elevation-dependent warming in the mountain regions. As shown in the schematic of a recent review paper (Mountain Research Initiative EDW Working Group, 2015), in a warmer and moister atmosphere, the latent heat release at the cloud condensation level may induce larger warming in high altitudes (cloud feedback) and the downward longwave radiation increase particularly fast in higher and drier atmosphere (water vapour feedback). It is difficult to identify or separate the contribution of these individual feedbacks from our current experiment setup. However, we note that these feedback mechanisms are operating regardless of forcing agents and therefore cannot explain the particularly large elevated warming in response to BC."

Mountain Research Initiative EDW Working Group.: Elevation-dependent warming in mountain regions of the world, Nat. Clim. Chang., 5(5), 424–430 [online] Available from: http://dx.doi.org/10.1038/nclimate2563, 2015.

How realistic is the simulated mean state and variability of the model temperature in this region?

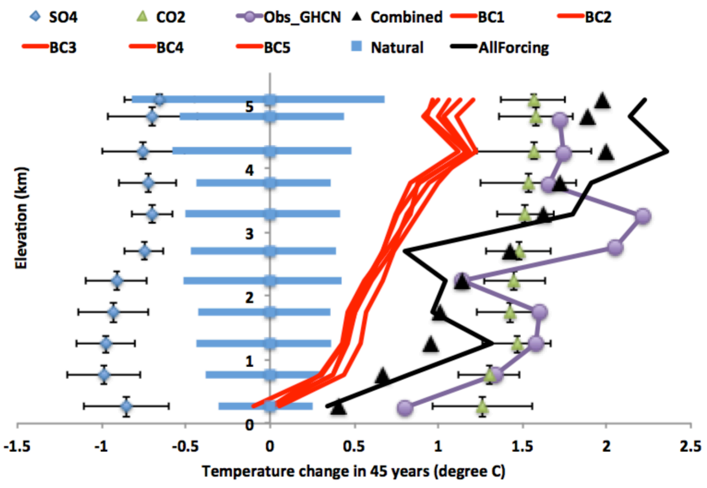
Response: The evaluation of model performance in simulating regional temperature, precipitation and snow cover are now included in Fig S2. This is also in response to comments from reviewer #1. Overall, the magnitude and spatial pattern of model simulation are in general agreement with observation. However, we now noted that the precipitation biases might lead to an overestimated snow cover, which affects the interpretation of the results. According to Reviewer #2, this is common in global climate models with coarse resolutions, and our 1-degree model has outperformed previous studies.

Related discussions are copied here for reviewer's reference. "Menon et al. (2010) attempted to simulate the snow reduction trends during 1990s but the spatial distribution of the observed trend was not well captured mainly due to the coarse resolution of the model. Qian et al. (2011) also acknowledged their model's limitation in representing the snow cover climatology and therefore may have biases in estimating BC impact on snow. It is well known that global models tend to overestimate the snow cover of the Tibetan Plateau, and 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 and too much snowfall is simulated (Ménégoz et al., 2013). This limitation can partly be overcome with models using higher spatial resolutions. The modelling work presented here is a major step forward in terms of spatial resolution (about 1º by 1º), as opposed to earlier studies [2.8º by 2.8º in Flanner et al. (2009) and Qian et al. (2011); and 4º by 5º in Menon et al. (2010)], which helps better resolving the complex topography in this region. As a result of increased spatial resolution and also the improved land scheme, the biases in snow cover simulation is significantly reduced from its earlier model versions [Lawrence et al., (2011), also contrast Fig. S2c with Fig. 2 of Qian et al., (2011)]. However, we note that the precipitation over the Tibet Plateau is still overestimated (Fig. S2b), and future studies, especially using regional climate models with even higher resolutions, are needed to improve the fidelity of model simulations of snow pack and glaciers over this topography-complicated region."

Role of natural variability: why the 80%, may you also show for consistency 90 and 95% ?

Response: 80% corresponds to the 10th to 90th percentile range of the 350 member of the 45-year trend. We show below the same figure as Fig 5, but showing 90%. The observed trends and modeled trend under all forcing is still far beyond the natural variability range (blue shading, now larger than Fig 5 in submitted manuscript).



However, we still retain The 80% (10th to 90th percentile) probability range as in Fig 5. This approach is consisent with several recent papers. For example, Fig 1 of Dai et al., (2015) shows "...the blue vertical bar indicates the 10th to 90th percentile range of the internal variability of T estimated using the CESM1 30-member ensemble simulations"

Dai, A., Fyfe, J. C., Xie, S.-P. and Dai, X.: Decadal modulation of global surface temperature by internal climate variability, Nat. Clim. Chang., 5(6), 555–559 [online] Available from: http://dx.doi.org/10.1038/nclimate2605, 2015.

If we use the ctrl simulation to estimate the natural variability, how different would be the estimate with a different model, i.e. for example another ctrl simulation coming from the CMIP5 or maybe all the CMIP5 simulations. Would also in this case the observed trends be significantly “outside” the natural variability?

Response: The pre-industrial controls from other CMIP5 are generally available. Presumably, the internal variability certainly is model dependent, but there is no evidence that CESM1 is an outlier in terms of unforced natural variability. In fact, in a recent BAMS article (Kay et al., 2015), 30-member ensemble of CESM1 (1-degree and the same configuration as in our control simulations) were examined with respect to CMIP5 models. It is found that "in Fig 6, the trend spread generated by internal climate variability alone—estimated using the CESM-LE—is often statistically indistinguishable from the spread in trends within CMIP5. At least for DJF surface air temperature trends."

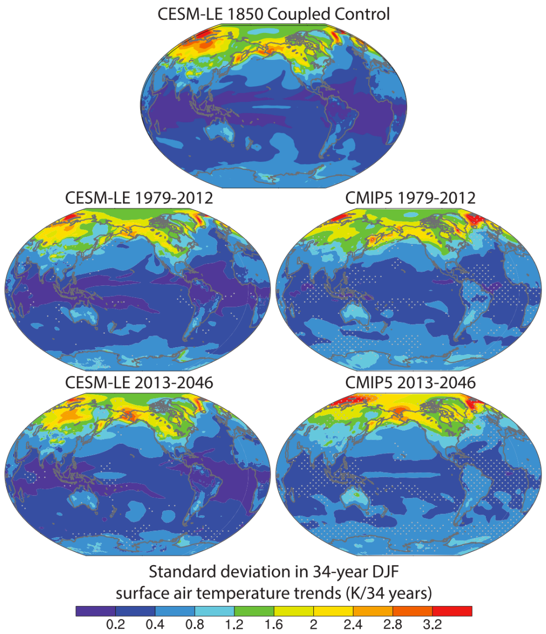


Fig. 6 of Kay et al., (2015). Global maps of standard deviation in 34-yr DJF surface air temperature trends for the (top) preindustrial (1850), (middle) historical (1979–2012), and (bottom) near-future (2013–46) periods. For the historical and near-future periods, trends are shown for both the 30-member CESM-LE ensemble and the 38-member CMIP5 ensemble (Taylor et al. 2012). Stippling on the historical and near-future CESM-LE trend maps indicates standard deviations that are statistically different than the CESM-LE preindustrial period. Stippling on the historical and near-future CMIP5 maps indicates standard deviations that are statistically different than the CESM-LE for the corresponding period. Stippling is based on an f test and a 95% confidence interval. For CMIP5, we used a single (the first) ensemble member of the following models.

Kay, J. E., Deser, C., Phillips, A., Mai, A., Hannay, C., Strand, G., Arblaster, J. M., Bates, S. C., Danabasoglu, G., Edwards, J., Holland, M., Kushner, P., Lamarque, J.-F., Lawrence, D., Lindsay, K., Middleton, A., Munoz, E., Neale, R., Oleson, K., Polvani, L. and Vertenstein, M.: The Community Earth System Model (CESM) Large Ensemble Project: A Community Resource for Studying Climate Change in the Presence of Internal Climate Variability, Bull. Am. Meteorol. Soc., 96(8), 1333–1349, doi:10.1175/BAMS-D-13-00255.1, 2015.

Therefore, we now include the following discussions in the manuscript. "One further concern is that the internal variability deduced from the long-term pre-industrial control simulation can be model dependent. However, comparing a 30-member ensemble of CESM1 simulation (1-degree resolution and the same configuration as in our control simulations) with 38-member CMIP5, Kay et al., (2015) found that the ensemble spread in the 30-year trend of surface temperature in CESM1 ensemble is statistically the same with the spread in trends within CMIP5."

Change in the UT temperature (for example as in Fig 2), do imply any significant change in convection and precipitation in the model?

Response: This is true. We now added that "The temperature response in the troposphere is associated with strong meridional circulation change. The mechanisms behind the free atmosphere circulation change, especially for the SO4 case which does not have strong atmospheric forcing, is discussed in details in Xu and Xie (2015)." In addition, we are actively studying the precipitation pattern at global scale utilizing similar model experiments.

Xu, Y., and S.-P. Xie (2015), Ocean mediation of tropospheric response to reflecting and absorbing aerosols, Atmospheric Chemistry and Physics, 15(10), 5827–5833, doi:10.5194/acp-15-5827-2015.

How important is the indirect effect in the model? Is it a minor contributor to the simulated and discussed changes?

Response: The aerosol indirect effect due to cloud changes are simulated by the model as we stated in the method section "The new cloud microphysics scheme (Morrison and Gettelman, 2008) allows the number concentration of cloud drops and ice crystals to be affected by aerosol concentrations and therefore accounts for the "indirect radiative forcing" of aerosols." The indirect forcing constitutes a large faction of SO4 forcing. A smaller fraction of BC forcing is due to indirect effect as BC is assumed less water soluble in the model (Liu et al., 2012).

Morrison, H. and Gettelman, A.: A new two-moment bulk stratiform cloud microphysics scheme in the community atmosphere model, version 3 (CAM3). Part I: Description and numerical tests, J. Clim., 21, 3642–3659, doi:10.1175/2008JCLI2105.1, 2008.

Liu, X., Easter, R. C., Ghan, S. J., Zaveri, R., Rasch, P., Shi, X., Lamarque, J. F., Gettelman, a., Morrison, H., Vitt, F., Conley, a., Park, S., Neale, R., Hannay, C., Ekman, a. M. L., Hess, P., Mahowald, N., Collins, W., Iacono, M. J., Bretherton, C. S., Flanner, M. G. and Mitchell, D.: Toward a minimal representation of aerosols in climate models: Description and evaluation in the Community Atmosphere Model CAM5, Geosci. Model Dev., 5, 709–739, doi:10.5194/gmd-5-709-2012, 2012.

You never discuss if there is any role of the dust. Is there any change in the transported dust? For example is that possible that in the simulation with increased CO2 / BC the pathways of transport of dust are changed because for example there is a different El Nino (Kim et al., Climate Dynamics 2015)?

Response: Thanks for raising this question. Firstly, we now include a paragraph in the end of section 4: "Lastly, it is also worth commenting the role of other snow impurities. In this study we used BC, a strong solar radiation absorber, to understand the climate response and the mechanisms due to absorbing aerosols that also include dust (Di Mauro et al., 2015; Gabbi et al., 2015) and organic aerosols (Qian et al., 2015). Similarly, we used SO4 to characterize all other reflecting aerosols. Any changes of dust and organics may induced changes to the snow cover, as their atmospheric heating and surface deposition are readily captured by this model, although the magnitude of response might be smaller since they are partially reflecting as well."

Gabbi, J., Huss, M., Bauder, A., Cao, F., and Schwikowski, M.: The impact of Saharan dust and black carbon on albedo and long-term mass balance of an Alpine glacier, The Cryosphere, 9, 1385-1400, doi:10.5194/tc-9-1385-2015, 2015.

Yun Qian, Teppei J. Yasunari, Sarah J. Doherty, Mark G. Flanner, William K. M. Lau, Jing Ming, Hailong Wang, Mo Wang, Stephen G. Warren, Rudong Zhang. (2015) Light-absorbing particles in

snow and ice: Measurement and modeling of climatic and hydrological impact. Advances in Atmospheric Sciences 32, 64-91.

Di Mauro, B., F. Fava, L. Ferrero, R. Garzonio, G. Baccolo, B. Delmonte, and R. Colombo (2015), Mineral dust impact on snow radiative properties in the European Alps combining ground, UAV, and satellite observations. J. Geophys. Res. Atmos., 120, 6080–6097. doi: 10.1002/2015JD023287.

Secondly, This is an interesting point that dust emission is changing as a response to climate change. We looked at the dust AOD difference between BC driven warming and pre-industrial simulation, and there is no robust changes in this region (see the figures below). The dust changes under CO2 warming are relatively larger. But the changes are still not statistically significant, and radiative forcing due to dust change is certainly smaller than CO2 radiative forcing.

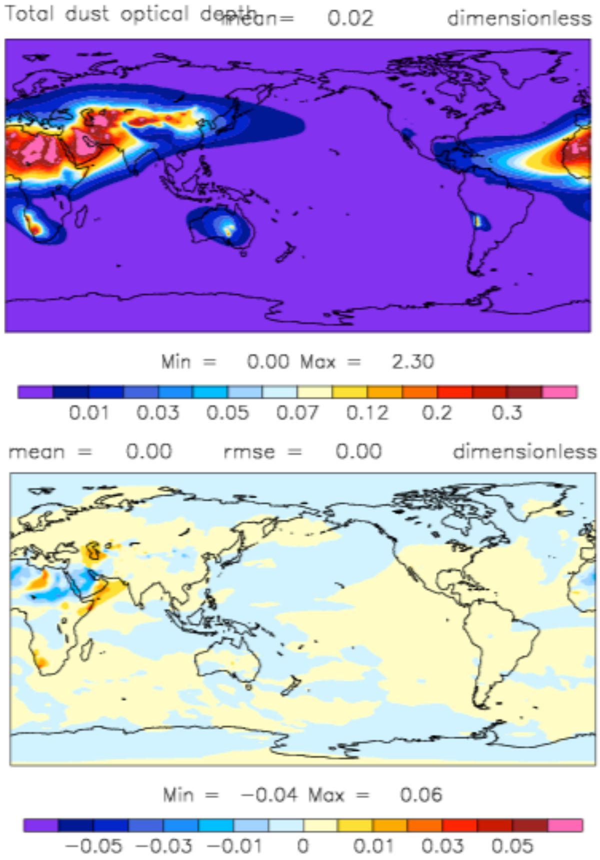


Fig. (Upper) Dust aerosol optical depth in pre-industrial run. (Lower) Change of Dust AOD in response to BC warming.

However, this "aerosol-feedback" is worth further investigation. Actually, we are studying the aerosol changes solely due to global warming. (Xu, Y., et al., Global warming impact on future PM2.5 pollutions, in preparation). The dust emission and transport under different ENSO conditions as in Kim et al., (2015) is a good reference point for our future work.

Fig S6 is missing!

Response: Sorry for the oversight. That figure is now removed from the manuscript. We now only briefly mentioned in the discussions that "A look at the seasonality of snow depth change suggests the early spring melting is important for this feedback."