We first thank the very constructive comments of the reviewer. We have taken all of reviewer's comments into consideration and revised the manuscript accordingly. All the changes have been highlighted in the revised manuscript. Our detailed responses, including a point-by-point response to the reviews and a list of all relevant changes, are as follows.

General comments

"This is an interesting paper which shows single-model results of the net climate effect of reducing black carbon (BC) aerosol emissions, both with and without reducing co-emitted compounds. I recommend publication of the paper, but I do have some concerns which need to be addressed first. In particular, no quantification or discussion of the model uncertainty is given, the co-emission assumption should be further justified, and analysis of surface temperature change is confusing since the model has been run with prescribed sea-surface temperatures. Please see below for details."

Reply: We have addressed all the comments in the revised manuscript. Please see below for the detailed responses.

Specific comments

"Introduction. Semi-direct aerosol effect is not mentioned in the introduction. It is important for BC, and should be explained briefly. You could refer to e.g., Koch and Del Genio (2010)."

Reply: Accepted. This has been added in line 53 - 55, page 3 in the revised manuscript.

"Page 33119, line 20-21. It is good that the uncertainty limits from Bond et al. are

mentioned, but I think that this huge uncertainty in climate forcing of BC, and the associated ongoing debate should be emphasized more in the introduction. Other studies, such as Myhre et al. (2013, ACP), have much lower estimate of the direct aerosol effect of BC, which is reflected in the best estimate in the latest IPCC report (Boucher et al., 2013; Myhre et al., 2013). Recent literature also suggests that the climate effect of BC may be overestimated due to overestimation of its lifetime, and this might be worth mentioning (see e.g., Hodnebrog et al., 2014; Samset et al., 2014; Wang et al., 2014).”

Reply: Accepted. These have been supplemented. The details can be found in line 84 - 90, page 4 - 5 in the revised manuscript.

“Page 33120, line 17-21. An overall reference to the model used, and also to the RCP scenarios (van Vuuren et al., 2011) would be appropriate here.”

Reply: Accepted. These references have been added. Please see line 126 - 128, page 6 in the revised manuscript.

“Page 33122, line 12. Please specify which year GHG concentrations are from.”

Reply: Done. Please see line 180, page 9 in the revised manuscript.

“Page 33122, line 15. Year 2000 is already 15 years ago, so I would not call this “present-day conditions”. Alternatively you could call it “recent past”.”

Reply: Done.

“Page 33122, line 16-28. No references are given to the RCP scenarios – this is needed. Only a web address is given, and this does not even work.”

Reply: We have added the reference and revised the URL. The details can be found in line 196 - 198, page 9 in the revised manuscript.

"Page 33122, line 26-27. What about biomass burning emissions? Were they kept constant at year 2000 levels or are they also changed when using RCP scenarios?"

Reply: The biomass burning emissions are also changed when using RCP scenarios. We have added the explanation in Section 2.2. Please see line 198 - 199, page 9 - 10 in the revised manuscript.

"Page 33123, line 1-4. Which year(s) are these data representing? Do you have any reference to the data?"

Reply: We have added the year and reference on SST data. The initial fields have no specific time and are only a climatological data, which can be downloaded from the website http://www.cesm.ucar.edu/models/atm-cam/download/. We have added this. The details can be found in line 201 - 205, page 10 in the revised manuscript.

"Page 33123, line 4-5. I am not convinced that 10 years are enough for the analysis. The paper does not give any information about uncertainties in the results and the year-to-year variability. Usually when running climate models, even with prescribed SSTs, natural variability can be very large and long simulations are needed (at least 30 years, but this depends on the size of the forcing). This is particularly important for the semi-direct and indirect aerosol effects, which depends on the cloud cover, while the quantification of the direct aerosol effect varies less from year to year. Please justify that 10 years are enough to derive radiative fluxes that are within reasonable accuracy."

Reply: We have added the standard deviations of different aerosol effects in Table 3 to show the uncertainties in the results. Please see the revised manuscript.

“Page 33123, line 15. What is the argument for RCP4.5 representing the most likely future situation?”

Reply: RCP4.5 represents a medium-low emission pathway. We have changed this.

“Page 33123, line 17-20. In principle, when running with prescribed SSTs, the sum of the semi-direct and indirect aerosol effects should not deviate too far from the difference between the change in net radiation flux at TOA and the direct aerosol effect. However, results in Table 3 show that this difference is rather large. Is estimation of change in cloud radiative forcing (CRF) an appropriate way of quantifying the semi-direct and indirect aerosol effects? In Ghan et al. this is done differently for the shortwave. As indicated above, I am also curious how large the inter-annual variation is, especially for CRF.”

Reply: In this study, we don’t perform additional simulations in which aerosol scattering and absorption are neglected to exclusively diagnose indirect aerosol effect according to the method by Ghan et al., because we finally focus on change in net radiation flux (NRF). The change in cloud radiative forcing (CRF) is used as an approximate way of quantifying the semi-direct and indirect aerosol effects. The change in CRF could be affected by the aerosol direct effect (ADE) in our results (Ghan et al., ACP, 2013). Thus, the sum of ΔADE and ΔCRF is not equal to the change in net radiation flux. These statements have been supplemented in Section 2.2. In addition, we have decomposed the change in CRF into shortwave and longwave components in Table 3. The details can be found in line 218 - 233, page 10 - 11 in the revised manuscript.

“Page 33124, line 8-9. Any reasons for the underestimation? Is this a known

problem.”

Reply: The underestimation could be caused by a variety of factors such as uncertainty in the aerosol sources, coarse model resolution, and the uncertainties of physical processes in the model, and the absence of nitrate and ammonium aerosols and secondary organic aerosol in the model (Zhang et al., Clim. Dyman., 2012). We have added these. The details can be found in line 250 - 253, page 12 in the revised manuscript. This problem also exists in most of models.

“Page 33124, line 20-21. Specify that it is a net cooling effect that is enhanced.”

Reply: Done. Please see line 266 - 267, page 13 in the revised manuscript.

“Page 33124, line 18-21. Given the strong emission reduction for BC, the change in direct aerosol effect of 0.07 W m^{-2} is quite small. As far as I can see from the multi-model comparison in Myhre et al. (2013, ACP), the BCC model has much lower normalized radiative forcing for BC than most of the other models. I think this is worth mentioning.”

Reply: Accepted. This has been added in Section 3.2.1. The details can be found in line 267 - 270, page 13 in the revised manuscript.

“Page 33124, line 22. This implies that the semi-direct aerosol effect for BC in this model is positive and larger than the direct aerosol effect of BC. The IPCC AR5 indicates that the BC semi-direct effect is negative, although this is uncertain, with a best estimate of -0.1 W m^{-2} and a range from -0.3 to +0.1 W m^{-2} (Boucher et al., 2013). Some justification of this strong positive semi-direct effect would be useful, e.g., a plot of the change in cloud cover between SIM1 and SIM2?”

Reply: Here, the increase of 0.11 W m^{-2} in CRF is not due to only BC semi-direct effect but due to a combined effect of decrease in cloud evaporation and increase in cloud cover caused by BC reduction, changes in other aerosol concentrations due to quick adjustment of the atmosphere to BC reduction, and the resulting changes in cloud properties. We have added these explanations in Section 3.2.1. The details can be found in line 274 - 277, page 13 in the revised manuscript.

"Page 33124, line 23. BC also changes the stability of the atmosphere, and this could also lead to a change in cloud cover, in addition to the changes in cloud evaporation (which is caused by changes in relative humidity) (see e.g., Hansen et al., 1997; Cook and Highwood, 2004; Johnson et al., 2004)."

Reply: Accepted. This has been supplemented in line 275, page 13 in the revised manuscript.

"Page 33124, line 24. What is the cause of the decrease in sulphate mass concentration? Emissions of SO2 are the same in the two simulations."

Reply: Due to changes in meteorological fields caused by declining BC. This has been supplemented in line 278 - 279, page 13 in the revised manuscript.

"Page 33125, line 2-4. This is probably mostly due to the fact that prescribed SSTs have been used. Therefore, the global mean surface air temperature would not change much. I do not understand the point of including the surface temperature analysis in Table 3 and the discussion, and suggest removing it from the paper."

Reply: Accepted. This has been removed from the paper.

"Page 33125, line 5-8. Since this is a very important point of the paper, this needs to be further justified and referenced, rather than just stating that "SO2 and OC emissions are likely to be reduced proportionally when BC emission is decreased...". Furthermore, co-emissions of other compounds, such as CO2, might be more important than SO2 and OC, and this should be mentioned/discussed (see e.g., Rogelj et al., 2014)."

Reply: Accepted. These are further justified, referenced, and discussed in Section 1 and Section 4. The details can be found in line 96 - 112, page 5 - 6 and line 402 - 408, page 19 in the revised manuscript.

"Page 33125, line 20. Is only the cloud albedo effect included or is the lifetime indirect effect also included? This is not clear from the method section and should be specified."

Reply: The model includes both cloud albedo and lifetime effects. This has been added in the method section. The details can be found in line 141 - 142, page 7 in the revised manuscript.

"Page 33126, line 8. In Fig. 2, labels a, b, c, etc. seem to be missing."

Reply: This has been added.

"Page 33126, line 16. See earlier comment on cloud evaporation and atmospheric stability changes."

Reply: Done.

"Page 33127, line 14. See above. Perhaps better to use semi-direct aerosol effect instead of cloud evaporation?"

Reply: Accepted.

"Table 2. I assume these emission numbers include biomass burning in addition to fossil fuel and biofuel emissions? It would be good to specify this."

Reply: Yes, these emission numbers include biomass burning emissions. This has been specified in Section 2.2. Please see line 198 - 199, page 9 -10 in the revised manuscript.

"Table 3. As mentioned before, it would be useful to show some uncertainty values. E.g., you could include standard deviations representing the inter-annual variation of the different radiative effects. Again, I suggest removing the T_{2m} results to avoid confusion."

Reply: Accepted. We have added the values of standard deviations and removed the T_{2m} results in Table 3.

Technical corrections

"Page 33119, line 8. I suggest inserting "absorbed" after "radiation"."

Reply: Accepted.

"Page 33122, line 3-4. This sentence is a bit strange. I think there are some commas missing. Please fix or rephrase."

Reply: We have revised this sentence. The details can be found in line 170, page 8 in the revised manuscript.

"Page 33126, line 24. Replace "in most of areas" with "in most areas"."

Reply: Done.

“Table 3. Please insert “(DRT)” after “direct”, “(CRF)” after “semi-direct and indirect”, and “(FNT)” after “net effect at the TOA”.”

Reply: Done.

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