

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

“Using simulations from an atmosphere-only global climate model, the authors highlight two main results:

1. If emissions of black carbon (BC) aerosol were reduced independently from co-emitted species, then the climate system would be cooled.

2. However, if emissions of co-emitted species (sulphur and organic carbon, OC) were also simultaneously reduced with BC, then the climate system would be warmed.

The authors argue that, since “there are no effective ways to remove the BC exclusively without influencing the other co-emitted components” (p33118.19), a reduction in BC would lead to the second situation above.

The paper contributes to the literature. However, I think that the contribution would be significantly improved if the authors act on my recommendations below.”

Specific comments (major)

“1. Justification of the co-emission assumption. The authors state that “there are no effective ways to remove the BC exclusively without influencing the other co-emitted components” (p33118.19), “the emissions of sulphate and OC will be reduced accordingly if the emission of BC is [reduced]” (33120.9), and “the

emissions of some coemitted scattering aerosols and their precursor gases will be inevitably reduced when BC emission is reduced due to their homology” (p33128.6). Indeed, this assumption (phrased in three alternative ways) is key to the ultimate conclusion of the paper. However, the validity of this assumption is not discussed and references are not provided (beyond a cursory reference to Lamarque et al., 2010, at p33120.8). Due to the important role it plays, the authors should discuss the assumption in much more detail, and references should be provided.”

Reply: Because actual operational reduction in BC emission in most of severe polluted countries, like China, is often to cut the usage of coal and other fossil fuels, as well as forbid open burning to reduce biomass burning emissions, all these major measures will result in the emission reductions in BC and its co-emitted components at the same time. We have added these statements and more discussions and references to justify the validity of the co-emission assumption in section 1. The details can be found in line 96 - 112, page 5 - 6 in the revised manuscript.

“2. Decomposition of radiative flux perturbations (RFPs). The key results in the paper concern RFPs. Ghan (2013, www.atmos-chem-phys.net/13/9971/2013/) recommends that “clean-sky” CRF is used when decomposing RFPs. Do the authors have sufficient diagnostics to diagnose “clean-sky” CRF and also the surface albedo forcing? If not, then discussion of why $\Delta DRF + \Delta CRF \neq \Delta FNT$ (Table 3) is warranted. Also, decomposing the change in CRF into SW and LW components may provide further insight.”

Reply: In this study, we don't perform additional simulations in which aerosol scattering and absorption are neglected to diagnose “clean-sky” CRF and surface albedo forcing according to the method by Ghan (2013). We have supplemented these statements and the formulas of calculating the different aerosol effects in Section 2.2 to explain why $\Delta DRF + \Delta CRF \neq \Delta FNT$ in our results. The details can be found in

line 218 - 233, page 10 - 11 in the revised manuscript. In addition, we have separately presented the changes in SW and LW CRF in Table 3 in the revised manuscript.

“3. Nature of the indirect effect. Does the model contain a representation of only the albedo (first) indirect effect, or does it also contain a representation of the lifetime (second) indirect effect on stratiform clouds? This should be clearly stated, and the implications considered. If the lifetime effect is included, do the authors know which of the two indirect effects dominates in the model? Does looking at column CDNC (Fig. 4) really offer much insight for the interpretation of Fig. 5? Would it be sensible to also look at changes in cloud fraction, or CCN concentration at the surface, or maps of the SW and LW CRF RFP?”

Reply: The model contains representations of both albedo and lifetime indirect effects on stratiform clouds. This further statement has been added in Section 2.1. We don't know which of the two indirect effects dominates in the model. Therefore, we added the figures of changes in CCN concentration at the surface and shortwave and longwave cloud forcings (Fig. 4 and 6 in the revised manuscript) and the corresponding analyses in Section 3.2.2. The details can be found in line 325 - 336, page 15 - 16 and line 345 - 356, page 16 - 17 in the revised manuscript.

Specific comments (minor)

“4. Title. The title is somewhat misleading. I would recommend pointing out the co-emission assumption, by using a title along the lines of the following: “Simultaneous reduction in emission of black carbon and co-emitted species will weaken the aerosol net cooling effect”, or, if you want to keep “black carbon” at the start, “Black carbon reduction: simultaneous reduction with co-emitted species will weaken the aerosol net cooling effect” (although it is grammatically awkward).”

Reply: Accepted. We have used the title “Simultaneous reductions in emissions of black carbon and co-emitted species will weaken the aerosol net cooling effect” instead of the original.

“5. Use of “coupled”. The use of “coupled” is potentially misleading, as many readers may understand this to mean that the model contains a dynamical ocean component. To avoid confusion, I would recommend using “aerosol-climate atmosphere-only model” at p33118.9, p33120.23, p33121.3, and p33127.10. The term “prescribed-SST simulations” could also be used early in the manuscript (e.g. in the abstract and/or the final paragraph of the introduction) and in the Conclusions – of course, the acronym “SST” would need to be expanded the first time it is used.”

Reply: Accepted.

“6. Table 1. The differences between the simulations, and the interpretation of those differences, seems rather complicated on an initial read. Readability could be improved by creating separate column columns for the BC emissions and for the SO₂ and OC emissions in Table 1, and by incorporating aspects of the interpretation of the scenarios (p33123) into the table – see a modified version of Table 1 below.”

Simulation	BC emission	OC & SO ₂ emissions	Interpretation (compared to SIM1)
SIM1	year-2000	year-2000	Present-day reference scenario.
SIM2	RCP2.6 year-2100	year-2000	Maximal reduction in BC; no reductions in OC & SO ₂ .
SIM3	RCP2.6 year-2100	RCP8.5 year-2100	Maximal reduction in BC; minimal reductions in OC & SO ₂ .
SIM4	RCP2.6 year-2100	RCP2.6 year-2100	Simultaneous maximal reductions in BC, OC & SO ₂ .

etc.

Reply: Accepted. We have modified the representation of Table 1.

“7. Calculation of ratios (SIM5). Were the ratios calculated and applied for each individual grid box and month? Or were global annual means used when calculating the ratios? Please clarify this in the manuscript.”

Reply: Yes, the ratios were calculated and applied for each individual grid box and month. We have added this in Section 2.2. The details can be found in line 192 - 193, page 9 in the revised manuscript.

“8. Sensitivity to BC inventory. If present-day BC emissions have been substantially underestimated (Cohen and Wang, 2014, doi:10.1002/2013JD019912), how may this affect the conclusions? This would be worthy of discussion.”

Reply: Accepted. We have added this discussion in Section 4. The details can be found in line 395 - 402, page 18 - 19 in the revised manuscript.

“9. Limitation of not including SST feedbacks. The limitation of using prescribed-SST simulations when looking at temperature (and, for that matter, other important climate variables such as rainfall) is noted at p33125.29–p33126.5. This should also be highlighted elsewhere in the manuscript (e.g. in the Conclusions, and maybe also Section 2.2). Alternatively, the temperature results in Table 3 could be removed from the paper (or relegated to Supplementary Material) to avoid confusion.”

Reply: Accepted. We have removed the temperature results from the paper.

Technical corrections/suggestions

“- p33118.5 - ‘in a short term’ to ‘in the short term’.”

Reply: Done.

“- p33118.14 - The sentence starting “However” is long, devoid of punctuation, and not very fluid. I would suggest re-writing it, thinking carefully about the order of ideas, seeking to improve fluidity. The more accessible your abstract is, the more likely it will be that readers will continue reading the rest of your paper.”

Reply: Accepted. We have re-written this sentence. Please see line 37 - 42, page 2 in the revised manuscript.

“- p33119.3 - ‘Since the industrial era, an increase in atmospheric aerosols leads to’ to ‘Since the start of the industrial era, an increase in atmospheric aerosol emissions has likely led to’.”

Reply: Done.

“- p33119.17 - ‘greenhouse gases’ to ‘greenhouse gas’.”

Reply: Done.

“- p33119.21 - I would question whether the sentence starting ‘BC can therefore...’ is true, due to the generality of the claim. I would advise that the authors make the sentence more specific, clarifying that the claim about the importance of carbon dioxide and BC is in relation to global warming (as opposed to e.g. health, stratospheric chemistry, or atmospheric circulation). For

example, the authors could make the claim that BC is the second most important anthropogenic positive radiative forcing agent instead.”

Reply: Accepted. We have revised this sentence. Please see line 76, page 4 in the revised manuscript.

“- p33119.24 - ‘in a short term’ to ‘in the short term’.”

Reply: Done.

“- p33119.26 - ‘sulfide’ to ‘sulfate’ (or ‘sulphate’, the British English spelling, as ACP is a European journal - the copyeditors should be able to advise on this).”

Reply: Done.

“- p33119.27 - ‘K’ to ‘^oC’, for consistency with e.g. p33120.1.”

Reply: Done.

“- p33120.10 - ‘tried to remove from its sources’ to ‘reduced’.”

Reply: Done.

“- p33120.13 - ‘would the global warming be slowed down necessarily by’ to ‘would global warming necessarily be slowed down by’.”

Reply: Done.

“- p33121.25 - Have I understood correctly that the two-moment cloud microphysics scheme is a modified version of Morrison and Gettelman (2008,

doi:10.1175/2008JCLI2105.1)? If so, then it may be helpful to also include this reference, as MG08 will be familiar to users of another commonly-used atmosphere model (CAM5).”

Reply: Yes, it is the version of Morrison and Gettelman (2008). We have added this reference in line 161, page 8 in the revised manuscript.

“- p33122.28 - When I click on the URL, I land on a webpage that says “docs under construction”. Is the URL up-to-date, or have symbols been omitted from the URL when typesetting? Can a more reliable URL be provided? (The problem may be that equals signs have been dropped from the URL when typesetting. <http://tntcat.iiasa.ac.at:8787/RcpDb/>, a shorter version, may be effective.)”

Reply: Accepted. We have revised the URL.

“- p33123.16 - Start a new paragraph before ‘The aerosol direct effect...’.”

Reply: Done.

“- p33125.29 - Start a new paragraph before ‘It should be noted...’.”

Reply: This part has been removed.