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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climate responses to anthropogenic emissions of short-lived climate pollutants" *by* L. H. Baker et al.

Anonymous Referee #2

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The manuscript by Baker et al. examines the possible effects on climate from idealised global anthropogenic aerosol emission removals. The authors highlight the strong model dependence of some of the effects examined, and the role that natural climate variability and vertical aerosol distribution play. It concludes that the major climate-relevant aerosol "player", when accounting for the above factors, is expected to be sulphate, with much smaller effects from carbonaceous aerosols. The paper is a useful addition to the discussion on aerosol-climate interactions, and has the extra advantage that it uses multiple models to extract more robust conclusions. Also, the manuscript is well written, and certainly within the scope of ACP.

There are several minor amendments that I suggest should be made before publication (see below), but otherwise I do not have any major concerns.

SPECIFIC COMMENTS:

Page 3824, Line 17: There are four models in the study, but here the authors refer to “all three models”.

Page 3824, Lines 17-18: Perhaps rephrase to “northern hemisphere mid and (especially) high latitudes”.

Page 3825, Line 7: Typo: SCLPs -> SLCPs.

Page 3825, Lines 9-10: According to the UNEP definition, methane is also included in SLCPs, so the authors could include all species up to methane here in their definition, but mention that they restrict their focus to the constituents with lifetimes of days to months, which therefore have a particularly inhomogeneous distribution.

Page 3826, Lines 2-3: Need to also mention explicitly the cloud lifetime effect here.

Page 3826, Lines 6-7: Please mention why BC warms the surface when near it (reemission in thermal wavelengths).

Page 3826, Line 25: Please add “global” before “temperature”.

Page 3828, Line 12: Circulation changes are not really assessed in the paper, so I would remove this word from here.

Page 3828, Lines 23-25: Please clarify whether photolysis is affected by the aerosol tracers in the models.

Page 3829, Lines 11-13: Please mention whether stratospheric chemistry is simulated too.

Page 3829, Lines 21-26: I would suggest mentioning how aerosol effects on clouds are simulated.

Page 3830, Line 8: It is mentioned earlier that the gas-phase chemistry is not modelled online in ECHAM6-HAM2. Where do the oxidants fields used for aerosol production

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come from? Worth mentioning.

Page 3831, Lines 5-6: Is the BC/dust deposition effect on surface albedo accounted for in the two models mentioned earlier in the text? Please clarify.

Page 3831, Line 7: Please clarify again here that the NCAR CESM model is only used for the BC analysis.

Page 3831, Line 27: Please add “globally” after “removed”.

Page 3832, Line 2: Was the spin-up performed for the control and the perturbation runs of equal length?

Page 3832, Line 26: Please mention explicitly what you mean by “other natural emissions”, as this currently sounds a bit vague.

Page 3833, Lines 1-2: What about methane in CESM-CAM4 – how is it treated?

Page 3833, Line 12: It is a bit counter-intuitive that Africa has such strong anthropogenic emissions. Does this include agricultural biomass burning? If so, the distinction should be made clearer (i.e. whether there is absolutely no BB component in the anthropogenic emissions removed or whether there are exceptions).

Table 1: Sulphate is not mentioned in the caption. Also, the caption says “three models”, whereas burdens for four models are shown.

Page 3833, Lines 20-25: It would be useful to briefly mention here which model may be closer to reality when it comes to vertical BC distribution. Any ideas?

Figure 3: Please mention in the caption that these means are for the surface.

Page 3834, Lines 8-11: Are similar drifts also present in the perturbation simulations (so that they cancel out and do not affect the differences between perturbation and control runs)?

Page 3834, Line 15: Please add “in” after “interested”.

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Figures 4 & 6: Please mention in the caption whether the SW TOA fluxes are calculated for clear-sky cases only.

Figures 5-8: Making the fonts of some of the labels (e.g. on the colour bar, or above the panels) somewhat larger would help with the readability of the figures.

Page 3835, Line 25: Please amend typo (“smilar”).

Page 3835, Lines 25-28: Please clarify why it is more likely to be the aerosol indirect effect rather than direct (from pollution outflow and associated radiative effects).

Page 3836, Lines 1-2: Please support this with an example reference (there are plenty).

Page 3836, Lines 16-19: The reduction in precipitation is seen south of the equator, whereas the reduction in SW TOA flux is maximum just north of the Equator. How do the authors explain this inconsistency?

Page 3836, Lines 20-23: Worth highlighting the Sahel wettening as well. And mentioning a few key references, as done for the South Asian case.

Page 3836, Lines 23-25: Any ideas why much of Europe and parts of the US become drier? Possibly circulation adjustments? Or a northern expansion of the NH subtropical regions?

Page 3837, Lines 9-10: Not in temperature, it seems, as in Fig. 4a HadGEM seems a bit higher than ECHAM.

Page 3837, Line 13: I would say, “qualitatively agree”, as the agreement on the magnitude is not apparent, with one model showing half the response. You could then add that two of the models show very good quantitative agreement too.

Page 3837, Lines 24-25: It would be appropriate to refer to Table 2 here. And generally to mention Table 2 a bit more often in the text, as it can help the reader make linkages.

Page 3838, Lines 4-5: However, it is worth mentioning here the findings of the (very)

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recent paper by Myhre and Samset (2015), which claims that current models tend to underestimate BC forcing.

Page 3838, Line 18: I think the authors meant to write “high-latitudes” instead of “high-altitudes”.

Page 3838, Lines 15-18: Was the methodology for generating the different ensemble members in CESM-CAM4 different to that in NorESM? Were the initial conditions in any way more drastically perturbed in the extra member of CESM-CAM4? Or do the authors believe that this disagreement/agreement between members is a totally random non-linear feature?

Page 3839, Lines 5-7: Yes, but what about the widespread warming over Eurasia? This does not seem related to sea ice.

Page 3839, Lines 7-10: It is not as clear as what the authors claim. Sea-ice decreases in some parts of the Arctic, but the changes are fairly localised, and there are even areas of increased sea ice.

Page 3840, Line 2: Preferably rephrase “model-mean” to “multi-model mean”.

Page 3840, Lines 17-19: It is a bit counterintuitive that precipitation decreases over land, where most of the BC exists, and where most of the de-stabilisation of the atmosphere is expected due to the BC removal. It would be useful here to briefly discuss possible explanations.

Page 3841, Line 2: I would add “qualitatively” before “agree”, given the very much smaller positive changes in ECHAM, as seen in Fig. 5f.

Page 3841, Line 15: Please mention that comparisons of the short-term instantaneous or effective forcing are “(not shown)”, as otherwise the reader may be misled to think that you are referring to the SW flux comparisons pursued in the manuscript, which I presume is not the case (as the latter are the effect and not the cause of what is discussed here).

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Page 3842, Line 4: Not all is caused by the ITCZ shift. The higher latitude changes probably have to do with the thermodynamic effect of temperature increases.

Page 3843, Lines 14-16: I presume the authors mean that there are fluctuations that happen at frequencies lower than 50 years (otherwise the average effect of natural variability would be negligible). This needs to be made clearer here.

Page 3843, Line 17: I would suggest changing “some” to “large”.

Page 3843, Line 28: It would perhaps make sense to add a sentence at the end of this paragraph speculating that possibly the expected effect is somewhere in the middle.

Page 3844, Lines 14-15: I suggest rephrasing to: “where natural variability is a relatively large contributor” (as internal variability is what it is and does not have special features in this study).

Page 3844, Line 20: The scenarios are idealised, but are they really “extreme”, given the drastic decreases expected for the aerosols examined here in the future? It might be worth comparing these reductions with changes between present-day and e.g. 2100 in a typical future emissions scenario, to put things in perspective. Also, it is worth discussing here or in the Discussion section the possible future role of nitrate aerosols, and whether their inclusion in the models could have led to different conclusions or not (both regarding the future role of aerosols in general and regarding the effects of the aerosol types examined here, e.g. sulphate).

Page 3844, Line 22: Perhaps add “mainly” before “using”.

Page 3844, Lines 23-24: Suggested rephrasing: “. . . to capture the fast and slow responses due to these emissions perturbations, as well as the uncertainties in these responses.”

Page 3845, Line 10: Suggested rephrasing: “. . .AOGCMs due to responses in ocean temperature and circulation, sea-ice, and atmospheric circulation and cloud responses that are realised on long timescales.”

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REFERENCES:

Myhre, G. and Samset, B. H.: Standard climate models radiation codes underestimate black carbon radiative forcing, Atmos. Chem. Phys., 15, 2883-2888, doi:10.5194/acp-15-2883-2015, 2015.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 3823, 2015.