We are grateful to the reviewer for their interest and comments on the paper. These comments are very valuable and have helped improve the manuscript. Here we outline how we have addressed these comments in the revised manuscript. The newly added discussions and rephrased sentences have been highlighted in green in our replies below.

The study uses the CESM global climate model to quantify the impact on effective radiative forcing, surface temperature, and precipitation rates of increases in aerosol emissions due to energy generation and decreases in emissions due to technological advances over the period 1970-2010.

The authors find that technological advances only partly offset impacts of energy generation. They also highlight the non-linearity of effective radiative forcing and diversity in temperature sensitivities. The paper is well written, and the figures support the discussion well. Figure 5 is perhaps the most novel aspect of the study and acts as a very efficient summary of the main findings. There are however results I do not understand, and the discussion could be improved in places. For these reasons, I recommend minor revisions.

While revising the manuscript, we realised that the isolation of the effects of aerosol changes in the "best estimate" experiment was probably biased due somehow to the experimental set-up. To address this issue, we have carried out a new experiment, which is now used throughout the revised manuscript (including Figures and texts). Note that, while this has resulted in different estimates of the impacts of aerosol changes in the best estimate case, it does not have any bearing on the major findings of this study on the comparison of the two different retrospective emission scenarios.

Main comment

1. I understand why temperature sensitivities, as defined in the study, could be negative (i.e. located in the top left corner of Figure 5) locally because (i) albedo and circulation feedbacks mean that temperature responses are not collocated with ERF and (ii) both ERF and surface temperature distributions are inherently noisy. But I cannot understand how temperature sensitivities can be negative globally, as stated for the best estimate (Page 7 line 16). Is that really the case? According to Fig 3 and 4, ERF is +0.1 W m^{-2} and DeltaT is +0.09, so the sensitivity should be positive. If there are negative global sensitivities, I would be concerned that the model does not conserve energy.

We are grateful to the reviewer for pointing this issue out. As mentioned above, we have accrued out a new model simulation to isolate the effects of aerosol changes in the "best estimates" experiment. As a result, we have repeated the related analysis and edited the manuscript accordingly (including Figures and texts).

The sign of global mean temperature response to aerosol changes isolated using the new experiment is now consistent with that of the ERF. There are still pronounced inconsistencies at regional scales, and a discussion of the underlying mechanisms has now been included in the revised manuscript.

Other comments

2. Abstract, page 1, line 19: "must be interpreted in the context of experiment designs" is a cryptic statement that is in some way true of all experiments. I would suggest rewriting, perhaps to something like "must be interpreted in the context of the reference baseline".

By "the context of experiment designs" we refer not only to the reference baseline (i.e., background warming, concentrations of GHG and aerosol levels, and etc.), but also to those experiments where emissions/concentrations are scaled rather arbitrarily to maximise aerosol signals (e.g., the PDRMIP experiment), given the existence of large nonlinearities in the aerosol impact on climate (Feichter et al., 2004; Ming and Ramaswamy, 2009; Dobricic et al., 2019). As such, we decide to keep this statement as it is.

3. Page 3, lines 3–4: That statement could be toned down – improving air quality is getting harder, and climate feedbacks (e.g. this year's record Arctic wildfires) may negate parts of the decreases.

We thank the reviewer for the suggestion, and have toned down the statement into "Anthropogenic aerosol emissions are expected to be reduced worldwide during the 21st century (Markandya et al., 2018)."

4. Page 3, line 10: The reason why the impact of future changes in aerosols have not been looked in isolation before is that (i) uncertainties in emission pathways also affect other forcings, so aerosols are not alone in that respect, and (ii) considering all forcings together makes sense because sectors emit a cocktail of gaseous and particulate species. So the authors should make a better case for what we really gain at considering aerosols in isolation.

We agree with the reviewer regarding the links between aerosol emission pathways and those of other forcing agents, as well as the existence of large uncertainties in their future evolution. We also acknowledge that the nonlinear interactions between aerosols and other forcings are difficult to quantify. Nevertheless, we argue that it is still very useful to quantify the uncertainty range of future climate projections related to uncertainties in aerosol emission pathways, while all the other things being the same.

In response to the comment, we expand P3 L10 into "Yet, possible differences in the climate response to varying aerosol emissions trajectories, all the other forcings being the same, have been mostly overlooked so far (e.g., Sillmann et al. (2013); Pendergrass et al. (2015); Bartlett et al. (2016)). This, nevertheless, is still useful for partially assessing the uncertainty range of future climate projections related to uncertainties in aerosol-related emission pathways alone, despite the fact that emissions of GHGs also differ between those emission pathways."

5. Page 3, lines 19–21: What did the Turnock study find?

We thank the reviewer for the suggestion. We have rephrased the statement into "Using a chemistry-climate model, Turnock et al. (2016) reported that the avoided aerosol/precursor emissions due to legislation and technology measures have improved air quality and human health over Europe, but have also led to a regional warming of up to 0.45 ±0.11 °C"

6. Section 2.2: Which aerosol radiative forcing mechanisms does the model represent?

The CESM (CAM5) model we employed in this study uses the Modal Aerosol Module 3 (MAM3) which includes both aerosol direct (aerosol-radiation interactions) and indirect (aerosol-cloud interactions) effects.

7. Page 5, line 5: permutations -> perturbations

Corrected

8. Page 5, line 29: Technically speaking, a decrease of−4 is an increase!

We thank the reviewer for pointing this out; we have corrected the text accordingly.

Captions of Fig 2 and 3: "raised up" is unnecessarily ambiguous. Simply say "multiplied"

Done

9. Page 6, line 15 Why is there an increase over Southern Africa? It looks statistically significant. Are there technological advances that increase emissions?

The reviewer is right in pointing out the statistically significant increases in AOD over Southern Africa in the technology advances experiment. We examined changes in each individual aerosol species, and found this is predominantly associated with sea-salt and organic aerosol emission changes in the technology advances experiment.

We added comments on this at the end of Section 3.1 as "It worth noting the AOD increases over the Southern Africa due to increases in sea-salt and OC which may be related to the additional warming induced changes in meteorology in the technology advances experiment."

10. Page 6, lines 22–24: That statement is not obvious to me. The area of strongest cooling, in Siberia, looks to be to the north of where emissions changes are probably located, in China. (It would really help to see maps of emission changes.)

We thank the reviewer for the comment, and note again we used a new experiment to isolate the impacts of aerosol changes in the "best estimates" experiment. All related analysis has been updated, with the underlying mechanisms now included.

11. Page 6, line 31: Is the enhancement again due to sea-ice albedo?

We tend to agree with the reviewer that the enhancement is mainly through the local sea-ice feedbacks. We have included analysis on the underlying mechanism in the revised Results section.

12. Caption of Fig 5: "error bars represent model uncertainty" – how is that calculated?

The uncertainty is calculated as the 25th -75th percentile spread of the differences between two sets of the 30 annual mean values. That is, the uncertainty is reported as the interquartile (25th -75th percentile) range of annual mean differences.

13. Section 4.1: An alternative conclusion of that section is that trying to decompose ERFs and response into component-based estimates is misleading in an internal mixture context. Doing so is just not as informative as it first appears. Source-based decomposition would be more useful and, perhaps, less subject to nonlinearities.

We are grateful to the reviewer for providing a new angle to interpret our results. We assume that by "source-based" the reviewer means decomposing emissions into each individual emission sectors. We agree that such decomposition may be less sensitive to nonlinearities, given that emission sectors emit a cocktail of gaseous and particulate species. However, note this is a different research problem from the one we are trying to address here. We designed the sensitivity experiments to investigate the differences between absorbing (mainly BC) and scattering (SO4) aerosols, and to assess the relative contribution of changes in each species to the total. We feel that the nonlinearities found here are very worth stressing given that massive efforts have been made to examine the climate impacts of different aerosol species separately (e.g., in the PDRMIP project (Myhre et al., 2017)).

14. Page 11, line 11: "Despite the global mean negative forcing from 1970-2010 aerosol changes" – but according to Fig 3, your best estimate ERF is positive.

We apologize, there was an error in the figure. This has been corrected in the revised version.

15. Page 11, line 21–24: Beyond model dependence, being critical of one's model is also useful. CAM is not the best-behaved model in Malavelle et al. 2017 doi:10.1038/nature22974, strongly overestimating rapid adjustments to aerosol-cloud interactions. But then even the best-behaved model gets the wrong rapid adjustments compared to observations Toll et al. 2017

doi:10.1002/2017GL075280. That would imply that ERFs, and the subsequent temperature and precipitation responses, are too strong in that model.

We thank the reviewer for the comment. We have expanded P11Ls21-24 into "We acknowledge the caveat/limitation of this study that all our findings may be model dependent, which is particularly the case for aerosols, given the high degree of parameterisation and divergence in aerosol schemes across present generation climate models. Also note carefully again that CAM5 has relatively larger aerosol forcing compared to other Coupled Model Intercomparison Project Phase 5 (CMIP5) models (Allen and Ajoku, 2016; Malavelle et al., 2017; Toll et al., 2017; Zhou and Penner, 2017). "

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