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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Significant climate impacts of aerosol changes driven by growth in energy use and advances in emissions control technology" *by* Alcide Zhao et al.

Anonymous Referee #4

Received and published: 17 August 2019

This paper assesses the influence on radiative forcing, temperature and precipitation of changes in anthropogenic aerosol emissions between 1970 and 2010 in the CESM1-CAM5 model. The paper decomposes the effects into the contribution from aerosol emissions increases due to increased energy use and the contribution form aerosol emissions decreases due to technological advances and air quality abatement initiatives over the same time period. The authors find that the two effects have partially cancelled each other over the analyzed period and do so nonlinearly and non-uniformly.

The paper is well written overall. There are several useful and interesting points made. The experiment design is clean and well thought out. However, I find that the motivation and certain aspects of the analysis require additional depth, in order to make the novelty of the paper clearer. I am also concerned that certain aspects of the results seem counterintuitive, suggesting problems with the model set-up (see Other Major Comments 2 and 3 below). At present it is not entirely clear what new the authors are adding to the conversation, though I do believe that this analysis has the potential to be a useful contribution with some additional depth. I believe that major revisions to provide further depth would be needed for the paper to merit publication in ACP. These are discussed further below:

- The best estimate results shown in this manuscript are essentially an assessment of impact of aerosols on climate over the last 40 years, which has been conducted extensively in the existing literature. The main novelty in the paper, therefore, lies in the decomposition into the two factors described above. However, I believe the authors must do more to center and frame this decomposition.
- At present, the authors do not make a strong case for why decomposing the total effect into these two components is a valuable exercise. This should be explicitly laid out in the introduction.
- The authors need to be clearer that the assessment of last 20th century effects of aerosols has been widely done elsewhere and provide this more as background, acknowledging where their work is replicative of and consistent with that done elsewhere, and focus more thoroughly on unique insight gained through the decomposition.
- At the same time, the analysis of the decomposition should be further deepened. Although the authors report many signals, they do not provide any depth of analysis on why these signals emerge. Given how many other studies have analyzed the role of aerosol increases and decreases in the second half of the 20th century and made many of the same points the authors make here, simply reporting

these signals is not sufficient. Where do the two effects counteract or reinforce each other in ERF, temperature, or precipitation? Why does this occur? What are the implications for future anticipated shifts in aerosol emissions due to the two effects?

- A main result that the authors emphasize is the result that the individual ERFs of the aerosol species do not add up linearly, giving rise to the authors' repeated statement that ERF calculations should be considered to be dependent on experiment set-up. While this is in some ways a fairly obvious statement, it would be useful to have this explored in further detail. However, the authors do not provide any explanation for this result. I believe the authors must assess the mechanism behind this more thoroughly for it to be a valuable contribution. Is the nonlinearity of the ERF when the aerosols are emitted separately versus together a result of changes in the total atmospheric burden or of changes in the spatial distribution and resulting radiative interactions? The authors allude briefly to the total burdens of each species being "identical" in the separate and combined simulations (P8, L20), but do not show it. This should be explicitly shown. The remaining explanations that are currently only suggested in Section 4.1 should be explicitly assessed.

Other Major Comments:

1. Confidence intervals should be provided for all global-mean values.

2. I agree with Anonymous Referee 3 that the negative global mean temperature sensitivity value in the best estimate case seems to violate basic energy conservation. How can a positive global-mean ERF result in a negative global-mean temperature change or vice versa? This suggest a major issue with the model formulation. If it is somehow robust, it needs to be explained.

3. Figure 1: Why does the large increase in SO4 burden result in a positive ERF? This needs to be explained, particularly when compared to the larger negative ERF of the smaller (and less reflective) OC burden.
4. Section 4.3 is largely a literature review on the formulation of aerosol emissions scenarios and does not explore what implications the results of this analysis have for future projections. How may their findings "help better assess and interpret such uncertainties in future climate projections"?

Minor Comments:

- The authors use a somewhat confusing formulation throughout the paper in which they refer to both the aerosol increases from increased energy use and the aerosol decreases from technological advances as "aerosol emissions" (e.g. P8, L23; P10, L26). I recommend rewording these to something like "aerosol emissions changes" throughout, to make clearer that it is in some case the absence rather than the presence of aerosol emissions that is being analyzed.
- The model and experiment design section would benefit from a clearer description in the text of what the different model simulations are and what scenarios they are intended to test. This is provided at the moment entirely in the caption to Table 1, but should be at least summarized in Section 2.2.
- It is not entirely clear what the residual emissions are that are occurring in the B10 simulation but that are not captured in the SEN and STC simulations. It would help for this to be more thoroughly described and for its magnitude relative to the emissions changes to be provided, especially given the authors repeated references to it.
- P3, L3-4: recommend replacing "will" with "may", as certain of the Shared Socioeconomic Pathways do simulate regional increases in anthropogenic aerosol

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- P5, L5: "where the climate system equilibrates to imposed permutations but the deep ocean" ← this phrase is unclear
- P7, L30: The drying over Europe as well as Asia seems counterintuitive, since all the other analysis suggested opposite trends (in burden, ERF, and temperature) over Europe and Asia. This bears further explanation.
- P8, L7-9: I'm fairly certain the estimate from this paper and from PDRMIP have almost entirely overlapping error bounds, making this difference hardly worth noting. I would rather read this as suggesting that the precipitation sensitivity is very well aligned with that found by PDRMIP. This is one reason why the authors need to provide confidence intervals on their global-mean values.
- P8, L10-12: "This may suggest that regional..." This statement seems deeply obvious and well-established in the community. Recommend cutting.
- Figure 1: It is not clear that the presence of hatching means that it is statistically significant rather than no statistically significant until one reads the text. Recommend rewording L5-6 to be clearer. It is also quick difficult to see the statistical significance hatching over the ERF values – recommend a different color (e.g. light grey) for the hatch lines.
- Figure 5: It is incredibly difficult to visually parse the symbols. The Global and Global Ocean symbols are essentially indistinguishable in the crowded areas of the graph. Very few of these are actually discussed in the manuscript, and I recommend removing some of the symbols (perhaps the latitude band symbols) to make this figure legible.

Typographic comments:

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- There are several instances of in-line citations still being enclosed by parentheses that should be corrected (P4: L27, L28, L31)
- P7, L13: “domain” → “domains”
- P9, L12-13: “is difficult to be quantified” → “are difficult to quantify”
- The paper contains a couple stub sentences without verbs: p4, L14-15 and p8, L27-28

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