

Interactive comment on “Climate and air quality impacts due to mitigation of non-methane near-term climate forcers” by Robert J. Allen et al.

Anonymous Referee #2

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Allen et al. use model output from the AerChemMIP intercomparison project to evaluate 2015-2055 changes in climate variables associated with two future air quality control scenarios. By comparing a “weak” policy scenario to a “strong” policy scenario, they show increasing trends in temperature and precipitation over the period that are driven by near-term climate forcers (ozone and aerosols), suggesting a climate penalty associated with air quality improvements.

The manuscript is generally well written and well structured and makes good use of the AerChemMIP simulations. It addresses an important question that is well suited to the scope of ACP. I do have a few concerns about the statistics and a few more minor comments and suggestions, discussed below.

GENERAL COMMENTS

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1. The trends have been calculated using least squares regression. There is very little information on exactly how that was implemented, so from my reading it does not appear that this is a weighted least squares regression, or that the uncertainties have been accounted for in any other way. This is concerning because, looking at Fig. 3 for example, there is a large amount of variability in the individual models that are used to construct the multi-model means. I am not convinced by the robustness of some of the reported trends in the multi-model mean, or that they are truly statistically significant as stated. The multi-model mean trend calculations should be performed using a method that accounts for variability/uncertainty in the mean (e.g., weighted least squares, but there are other options) before the paper is publishable in ACP. In addition, some discussion of the method used and the influence of the variability/uncertainty is warranted.

2. For the global trends in climate variables, it would help to contextualise the values associated with NTCFs by also providing the trends from the two individual scenarios (or at least from the one with weak NTCF control, as the other can be determined from the difference trends provided). Without this, it's hard to tell how important the NTCF climate penalty is. I note that this is done in the figures for the regional trends, but not for the global trends. I would strongly encourage the authors to add these in some form (for example, a figure in the SI equivalent to Fig. 3).

3. The manuscript is very well structured and quite well written, but the heavy use of acronyms and technical identifiers (e.g., SSP3-7.0-lowNTCF, lowAERO3, etc.) makes it harder to read & follow than it needs to be. I would encourage the authors to simplify this wherever possible and then use a consistent, easy to interpret nomenclature throughout. For example, frequently the two scenarios are referred to as strong and weak air quality control, and these are much easier to interpret than SSP3-7.0 and SSP3-7.0-lowNTCF. I would suggest strong and weak air quality control could replace SSP3-7.0 and SSP3-7.0-lowNTCF everywhere, in particular in figure legends and captions where the reader may not be referring back to the text. Similarly, NTCF mitigation is easier to interpret than SSP3-7.0-lowNTCFSSP3-7.0.

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4. The manuscript cites a lot of “in prep” and “submitted” papers. In most cases, these are cited as part of long lists of other references, so they aren’t really needed to make the points. If these are not at least in ACPD by the time of publication, they should not be included in the citation lists (except in cases where they are the only publications available to back-up the point).

SPECIFIC COMMENTS

L30: Does the net radiative effect here refer just to OC or to BC+OC? Please rephrase to clarify.

L59: Is this newer estimate of mortality for all air pollution or outdoor ambient only? Please rephrase to clarify.

L90-108: This information would benefit from being summarised in a table listing the scenarios and some of the relevant information (e.g. air quality controls weak/strong, ozone and aerosols high/low, CH₄ high/high, etc.) to make it easier for the reader to synthesise.

L120-122: I find this a bit confusing. What is the difference between CESM2 and CESM2-WACCM in this case? Is it the aerosol treatment? And if they are basically the same model, is it fair to include them as two separate data points in the multi-model means?

L141-144: So nitrate aerosol was not included in PM_{2.5} at all, even for the models that do include it? It would be nice to see how much uncertainty this adds, given nitrate can be an important component of aerosol loading in some regions. I’d suggest adding a version of the PM_{2.5} figures including nitrate to the supplement, and a brief discussion of the impacts of excluding nitrate either in the main text or in the supplement.

L156: Is there a reference for these ground-based observations? Or is this the same GASSP observations mentioned above? If the latter, please state explicitly in the text.

L172-L180: This is confusing when paired with the figure. It is completely legitimate to

not include the differences in CH₄ pathways for this work, but anyone skimming quickly and focusing on the figures will miss that point. In my opinion, Figure 1 should only show what was used in this work, not scenarios that are not used here. I strongly encourage the authors to remove the SSP3-7.0-lowNTCF (right?) and difference lines from Figure 1. The comparison between the scenarios can be moved to the supplement if the authors feel it is important to include.

L181-187: Similarly, I don't think this discussion belongs here. It is the first section of the results, yet it is mostly discussing what is not done in this work. I would suggest this could be removed entirely, or moved to the supplement or to the conclusions as part of a discussion of what future work should be done to build on what the authors have done here. Sect. 3.2 and Figs 3-4: Generally speaking, is the changes in atmospheric composition (aerosols and ozone) that are driving the changes in climate. Thus it seems a bit odd to show and discuss the changes in climate variables BEFORE the changes in composition (ozone, PM_{2.5}). I would suggest restructuring such that Fig. 3 comes before Fig. 4, with the text order changed to match. (I note this is already the order used in the abstract and conclusions).

L204-205: Unless you rename & define the scenarios in the methods as discussed above, please clarify how “under NTCF mitigation” is defined here (I understand that it is the difference between the two scenarios, but that wasn't clear to me on first read).

L211-218, L223-228 (and elsewhere): Much is made of the difference between the lowAER and lowAERO₃ outcomes. Given that one of these only includes 3 models and the manuscript states explicitly that the difference is not significant, it is not justifiable to be interpreting this as a result. This appears to me to be over-interpretation of noise, and I would suggest this discussion be removed before publication in ACP.

L233: This land-only result appears to be insignificant for 75% of the models (including those that show increases) and so this statement should be removed or qualified.

L238: CDD does not show a statistically significant increase in the overall MMM (or

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in the subset MMM or in any of the individual models bar one) – therefore should be removed from this sentence.

L255-256: Is the land-only warming pattern shown anywhere? Is the land-only warming weaker or stronger than the overall warming? If there is a difference, it would be useful to see an equivalent figure in the supplement. (And if there is not a difference, it's not clear why this is discussed separately.)

L264: "...forcing and response do not need to occur in the same regions." Can this be explained a bit more?

L269-271: Do I understand Fig 6 bottom panel correctly that models don't agree about this feature? If so that would be worth stating in this discussion

L307: For a discussion of seasonal patterns to make sense, consideration should be given to the different seasonalities of the two hemispheres. Figure 7 should either be separated or at least ordered/demarcated by hemisphere – I'd suggest NH extratropics, tropics, and SH tropics.

L370: Why is one model listed explicitly when all (including that model) are available from the same location? Also please spell out ESGF here and provide a link or doi.

Figs 2, 5, 7: regional legend labels on x-axis are impossible to read because they are so small. Perhaps give each region a number instead? Or include some other sort of key to make this clearer?

Fig 3 caption (and elsewhere): Does "hottest day" refer to "surface temperature on hottest day"? Similarly for wettest day? Please clarify somewhere.

Fig 3 caption: It seems the thin coloured lines show the trends for the individual models, but this has not been explicitly stated in the caption. Please update caption to clarify.

Figs 3, 4, 5: why are different units used for the trends in the precip variables (mm/day vs. %) in the global and regional trend figures? Same question for PM2.5 and O3. Can

these be standardised to more easily compare?

Fig 6d-f: these plots are not currently discussed in the text and therefore should perhaps move to the supplement (or be mentioned in the text)

Table 1: I found this table hard to understand while trying to refer to it while reading the text. A few suggestions to improve the clarity. (1) Add lowAER and lowAERO3 identifiers above the list of relevant models in each sub-section so it's easy to see which group is which. (2) If text and figures are re-ordered as suggested above, move PM2.5 and O3 columns to be left-most, followed by the climate variables. (3) Move the three "MMM total" lines either to a separate part of the table or (preferably) to a new table altogether as the numbers aren't comparable to the lines above/below which makes it difficult to interpret (and already a lot to interpret in the table!). (4) For the lowAER models' O3 response, replace 0.0 with n/a since these values are not included in the Overall MMM calculation (as is, looks like the overall will be an average of the lowAERO3 values and three zeros).

TECHNICAL COMMENTS

L139: "were are" → "we are"

L357: "complex" → "complexity"

Fig 2 caption: "astriks" → "asterisks"

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-1209>, 2020.