Response to review of **"***Anthropogenic aerosol forcing under the Shared Socioeconomic Pathways"* by Marianne T. Lund, Gunnar Myhre, and Bjørn H. Samset.

We thank the anonymous referee #2 for the careful and thorough review of our paper, and the useful suggestions. Responses to individual comments are given below.

Abstract this text "aerosols under three different levels of air pollution control: strong (SSP1), medium (SSP2) and weak (SSP3). " should be revised, given that far more than air pollution controls impact emission levels in these three scenarios. It would be more accurate to describe these as representing three contrasting projections for air pollutant emission levels.

A valid point, we have modified according the referee's suggestion.

More context should be given in the introduction when the scenarios are introduced. It's too simplistic to simply call the scenarios simply high/low air pollution. Air pollution controls plus the magnitude of the various drivers of emissions (e.g., population levels, economic growth, rural access to modern energy, GHG emissions policy, etc.) all play a role in determining the ultimate emissions level. For example, referring to Rao et al. Figure 2, for the Ref case scenarios (e.g. no GHG emissions reduction policy) emissions can differ significantly between SSP1 and SSP5, even though both of these scenarios represent storylines with strong air pollution controls. Similarly, emission levels are generally quite a bit higher in SSP3 as compared to SSP4, even though the emission control assumptions are similar. Note also that the SSPs are from different projection models, which means that one also has be cautious in such comparisons.

We thank the reviewer for these reflections. For detailed descriptions of the assumptions underlying the scenarios and how they drive the differences in emissions, we refer to the cited literature. However, we have rewritten the paragraphs describing the projections to clarify the connection between the air pollution storylines and SSP baseline marker and climate mitigation scenarios, and to emphasize the complexity of the interplaying factors. While the strong/medium/weak terminology is kept for consistency with Rao et al, recognizing that this is a generalization, we include the high/medium/low challenges to mitigation and adaptation that characterizes the given SSP later in the text as well.

The impact of inter-annual variability should be discussed given that meteorology for just one year is used. How much does the selection of that year influence results?

Good point. The effect of different meteorological data sets on aerosol abundances in the OsloCTM3 was investigated in a recent documentation paper by Lund et al. (2018, GMD). Here we add a brief summary and add the following text:

"All simulations are performed with meteorological data for 2010. Lund et al. (2018) investigated the impact of meteorology on the simulated aerosol abundances using data for two years with opposite El Niño–Southern Oscillation (ENSO) index. Differences in global burden of up to 10% for some aerosol species where found, with larger values in localized regions over the tropical Pacific and Atlantic Oceans."

Line 47 - "generally reflect the assumption that stringent air quality regulations will be successfully implemented globally (Rao et al., 2017" Suggest replacing stringent with a somewhat more neutral

word (perhaps "substantial""). The RCP's represented a somewhat middle of the road air pollutant emission control assumptions, but by no means were they at maximally feasible levels (which is what might be read by "stringent"). The more important point to be made here is that there was limited variation in air pollution control assumptions across the RCP scenarios.

Modified to substantial. Based on a comment from referee #1 we have also added a paragraph about the underlying environmental Kuznets curve assumption, which limits the variation across RCPs, hence more explicitly addressing this point.

Line 95-105 What was assumed for open burning? These are also supplied in the future scenarios, (but are from van Marle et al 2017, not Hoesly et al., 2018). Note that these are not "natural" emissions, as much of these emissions are due to human activity.

We use the biomass burning emissions supplied by in the future scenarios. The "vegetation" emissions referred to as "natural" is biogenic VOCs. This has been specified in the text now.

Line 123 While I understand why this "For simplicity we refer to SSP1-1.9 as SSP1, SSP2-4.5 as SSP2 and SSP3-7.0 as SSP3 throughout the text." is done, however, this is inaccurate and may lead to misunderstanding on the part of readers, as there can be systematic differences even between scenarios with the same storyline. SSP1-1.9, for example, is a very strong GHG mitigation scenario which means that fossil fuel use is drastically reduced (and what fossil fuel that is used tends to have lower air pollutant emissions). So emissions will tend to be on the low side of what is already a low reference scenario. Emissions can be much lower than the reference case SSP1, particularly in earlier years. Similarly for SSP2-4.5, emissions here can be significantly influenced by the fact that the 4.5 scenario contains policies to limit greenhouse gas emissions. I suggest first, as mentioned above, that a little additional context be given for these scenarios. It would help, on first introducing the scenarios, that the fuller version of the scenario name that also contains the model name is used. That will help enforce to the readers that these are from different projection models. The presence, or not, of a climate policy should be mentioned (e.g. present in SSP1-1.9 and SSP2-4.5) should be mentioned in this introduction, since this can have a strong influence on the emissions pathway. Some of the figures have the fuller scenario names and some do not. Suggest that all figures have the names that contain the forcing target. I suggest the fuller scenario names (e.g., SSP1-1.9) be returned to in the discussion and conclusion section. This will help remind the reader of these issues. This will also facilitate comparisons with other literature results (for example, the detailed data in Gidden et al. 2019 for each scenario.).

We see the point and have included full names with forcing target throughout the text. We have also expanded the introduction and methodology sections.

Line 133 - "the similar characteristics" -> "similar characteristics"

Corrected.

Line 141 - The assumptions behind the RCPs were not "homogeneous" (each RCP was produced by different models, and assumptions were not harmonized between models). The assumptions would be more accurately described as "relatively similar", or some such wording.

Modified.

Line 156 "increased global ammonia (NH_3) emissions (not shown)," (would be useful to reference Gidden et al. 2019 Figure E3 here as these are shown there.)

Included.

Line 187 "However, towards the end of the century North Africa and the Middle East reaches similar levels." Its not clear what this line means, since there is no one behavior for this region. For SO2 (Figure 2), NAF-MDE either stays at current levels, or declines (depending on scenario), while BC either increases somewhat, or declines.

Similar to South and East Asia. Text has been clarified and now reads:

"Towards the end of the century North Africa and the Middle East are projected to experience levels similar to those in South and East Asia."

Line273: "whereas the mainly residential, and therefore more challenging, BC sources remain largely unchecked, the aerosol forcing may follow a different path than estimated here" This is too oversimplified, since residential sources also emit copious amounts of OC, which means that the net forcing from residential sources depends on the balance between BC/OC emissions, and the relative per Tg forcing of each in any particular model. The result is that the net forcing from residential emissions is quite uncertain, likely even as to sign (particularly since rapid adjustments reduce the impact of BC), and trends even more so.

As noted, this is a highly illustrative case meant to initiate discussion around whether there's a possibility that emissions of different species may follow different pathways in the coming decades. The RF estimates provided also include both OA and nitrate, and the rapid adjustments of BC are noted. However, following this comment and input from the anonymous referee #1, we have expanded the discussion with more on the role of nitrate, as well as the uncertainties surrounding the forcing of OA, which could also have a non-negligible absorption (brown carbon).

Line 307 " find no evidence that aerosol emissions reductions drive a particularly rapid near-term warming in this scenario. " Perhaps point out here that this points to the significant inter-model differences in aerosol response.

Yes, good suggestion. Included.

Line 331 "impose" is an odd word here, perhaps "drive"?

Modified to "lead to"

general: It would make this work more helpful for readers if the time series of global and regional forcing could be provided in the supplement. Also, forcing by species (sulfate, nitrate, BC, OC, etc.) (+ aerosol cloud interactions) should also be provided. These are, in part, discussed in the manuscript, but a table with numerical values should be provided.

Full time series of total RFari and RFaci has been added. Forcing by species is only available for the direct aerosol effect and for the selected years presented in Table S1.