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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Climate-driven chemistry and aerosol feedbacks in CMIP6 Earth system models" *by* Gillian Thornhill et al.

Anonymous Referee #1

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The paper is an interesting summary of the magnitude of chemistry and aerosol feedbacks in available CMIP6 climate models. The paper is generally well-written, however in its current form the manuscript is somewhat fragmented and some important discussion about is missing. Some aspects of the methodology are described concisely, yet some important details are missing entirely, or are described only briefly. The chemical and aerosol forcing agents are considered independently which helps compartmentalise the results and some of these section include important insights. However, other sections have not been crafted with the same care.

The paper would benefit from merging sections 4 and 5. Currently results from several CMIP6 climate models are somewhat mechanistically portrayed in section 4. Section 5 contains some context for interpreting the differences between models, but uses identi-

cal subsection headings and much of the content is more suited to the introduction of a paper on one or more of the forcing agents. The chosen format makes the manuscript unnecessarily disjointed and does not help contextualise the main results. Once sections 4 and 5 are merged, they should be revised to include discussion of the physical processes that cause differences between models. Currently, this is only achieved for one or two of the forcing agents.

The article has two main themes. Firstly, the differences in aerosol and chemistry forcing efficiency and burden sensitivity are considered. Secondly, the magnitude of feedbacks from forcing agents are contrasted. It is not clear what the authors intended the main message of the paper to be. The abstract provides very few conclusions about either of these aspects and is overly focussed on methane-specific results. If the paper is intended to focus on the second aspect, then the majority of the feedback summary tables could be moved to the SI without reducing the impact of the paper. However, I think it would be better to retain these tables and include a process-based discussion of the causes of model differences as suggested above.

The use of standard deviations to represent uncertainty in a handful of models is not appropriate. It is possible that this is not what the authors have done, but their method is currently unclear. The authors need to clarify their multi-model uncertainty calculations in the text and if they are currently using standard deviations to represent uncertainty in only 3-6 values, need to seek more appropriate ways to communicate this information. Currently multi-model uncertainties are communicated through table captions, but should be described fully in the main text.

Some other points that require attention include:

All figures require subfigure labels as per ACP guidelines, to match references in the captions and main text.

Line 34: "with warmer temperatures" needs a fuller description. 4Xco2 induced warming

Line 37: VOC needs to be defined.

Line 40: GCMs do these things already. ESMs include the interactions between these systems, by coupling them and hence can expect a greater degree of consistency of information across model components. This needs to be clarified in the text.

Line 57: Here and in the conclusions, it is important to mention that some of the forcing agents considered make important climate contributions at the regions scale that are neglected when global mean temperatures are used to represent climate change.

Line 102: The scale factor is not well justified. The cited document is a substantial IPCC chapter. Presumably, authors are referring to section 8.2.3.3? Including the page number would help reader. However, the derivation of the scale factor used here is unclear and some explanatory text is required.

Line 105: The use of the value 9.25 also needs justification and a description of how it corresponds to values supplied in the referenced document.

Line 110: “four have ... and three have ...” is ambiguous. “Three of these four also have...” is clearer. Table 2 makes this clear, but is not currently referenced.

Line 119: Table 3 is currently referenced in a way that suggests it will compare emissions from all natural sources, whereas it actually shows differences between models for dust and BVOCs only. The text needs to be revised. This error is repeated on the first line of section 3.2.2.

Table 3: “PAR” needs to be defined. The phrase “Not dependent on vegetation” is redundant.

Table 4: There are inconsistencies in the table. For example, sometimes “wind” is used and at other times “wind speed dependent”. Descriptions here are too brief. What is the difference between DMS emission and oceanic organic aerosol complexity for NorESM2-LM and UKESM1 for example?

Section 4, Line 150: Section 2.1 should be referenced in the first paragraph, so that the normalization of temperatures can be put in the context of γi as defined in that section.

Line 155: For non-specialist readers an indication of the number of years required to reach equilibrium on average is needed.

Line 163: Figure S1 does not obviously support this claim. Global mean ERF values should be provided for each model. Also, the authors should explicitly state they are discussing "global mean" effective radiative forcing here.

Line 165: The strong regional forcing over Africa should be mentioned as the primary cause of positive SW forcing. Some speculation of the process parameterisations that cause this model behavior should be given.

Line 182: Refer to table 6 again. Also, some speculation on the physical processes causing the increased lifetime should be given. This is a good example of the need for additional discussion and how merging, then adapting content from section 5 will improve the interpretation of results. Line 189: It is not clear what the 2nd use of "for instance" here is referring to. This sentence needs to be rewritten to improve clarity.

Table 6: The reason for missing values in this and other tables needs to be explained more clearly within the text.

Line 200: These forcing values are far larger than for dust. Are the forcing-emission-feedback relationships expected to be linear? If not, there will be discrepancies in the gamma terms across emission types, even if normalised. This assumption on linearity and its implications need to be discussed here and/or in section 2.2.

Line 201: Why 20x? Is this caused by the choice of size bins? This warrants some discussion. Why is the AOD of a similar magnitude? What model processes have been adjusted/tuned to make the AOD similar? The reasons models have similar values for very different reasons need to be better understood. This is important for understanding the causes of model diversity in climate projections.

Line 205 - 209: All positive except MIROC needs to be explained/considered. What regions show a decrease in emissions that causes the global mean response to be negative? Maps for each model in the SI are needed.

Line 220: It should be explained here that all models could have run the 2xdms experiment. Interactive ocean biogeochemistry is not a prerequisite, since emissions could have been scaled within the flux parameterisation as with the 2xdust experiment.

Line 222: Fig 3 does not show the forcing values for each model as implied. Table 8 should be referenced to here.

Line 222-224: Maps of sulphur concentrations and changes in concentrations need to be included as a figure in the SI for each model, so the reader has a clear understanding of the magnitude of regional compensation across models.

Line 225: GFDL-ESM4 values only contributes to the multi-model sensitivity to emissions/concentrations, but not to the multi-model radiative efficiency. The assumption made here is that all models have similar radiative efficiencies. This is an important assumption, given the diversity of model responses highlighted up to this point in the manuscript. Is it appropriate to assume GFDL-ESM4 has the same radiative efficiency as the two models used in the sensitivity calculation? Some justification is required if the authors want to maintain this approach. An alternative approach would be to only use the 2 models with sufficient information to calculate both the multi-model sensitivity and multi-model radiative efficiency. This subjective choice to include partial information from one model needs to be justified more clearly and the implications of extrapolating the multi-model radiative efficiency to other models needs to be considered and openly discussed.

Line 229: The magnitude of the increase should be quantified in the text. Line 249: Here and elsewhere in the text, the word "significant" is used without mention of associated statistical tests. The values should be state with “significant” removed, or the methodology more accurately described.

Line 253: Incorrect label. Figure S3 only shows the multi-model mean. Given the diversity in aerosol forcing from this source, maps of CDNC should be provided for each model. Also, interpretation of the differences between models needs to be included here.

Line 253-257: Examples of regions where these behaviors are likely, with an explanation of why is needed.

Figure 4: Fig S3 could be a subfigure of Fig 4.

Table 9: Uncertainty values are missing for UKESM1 and multi-model mean values are missing for Scaled Mass

Line 278: Is there an hypothesis the authors could provide to explain the causes of model diversity in BVOC partitioning into ozone and aerosol forcing? This sort of discussion is essential to develop a better understanding of the importance model differences and will affect interpretation of climate feedbacks across models.

Table 10: There is no explanation of why 14% is used. This should be in the methods section, not hidden in a caption.

Line 300-302: This sentence needs to be rewritten to improve readability.

Line 302: It is not clear from the text as written, how BVOC burden sensitivities are used in the methane sensitivity calculation.

Section 4.2.4: The title of this section is misleading. Several non-emission drivers are considered, not just these two.

Line 265: “1” missing from UKESM1.

Section 4.3: This section needs some comment about the importance of climate forcing agents that have climatic importance at the regional scale, to prevent the results of this manuscript being interpreted incorrectly.

Line 380: The authors need to specify that these are multi-model feedbacks, here and in the table caption. Figure 5 needs to be referenced. In addition, the cancellation between models with opposite signs again needs to be mentioned within this section, as does the fact that a different number of models were used to calculate the multi-model means because of data availability.

Line 402: Can the feedbacks be interpreted in the context of the magnitude of forcing from these forcing agents over some specified period? Uncertainty in these magnitudes should be included in the discussion with appropriate references.

Line 423: There is no use citing these values if not directly comparable. This text should be removed to avoid confusion. Further discussion of the causes of model differences is required here.

Line 433: Please clarify the difference between primary production and DMS production in the text.

Line 505: This value needs context to aid interpretation. e.g. What is this as a proportion of the GHG forcing required to increase temperatures by 1 degree?

Line 507-508: The uncertainties in these values are substantial and need to be included in this discussion and interpretation of results.

SI: S1, some descriptions are missing entirely and need to be included.

SI: All figures require subfigure labels.

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