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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 #2

Received and published: 24 April 2020

Thornhill et al. analyse a set of Earth System Model simulations with atmospheric chemistry and aerosol parameterisations to quantify climate feedbacks associated with aerosol and chemistry processes. The methodology allows to attribute the climate feedback to different chemical and aerosol processes and thereby provides in some cases important insights. The paper is highly relevant and fits well to the scope of ACP. The paper is generally well written, but the quality of the individual sections varies considerably.

The abstract should list the feedbacks assessed here and should be much more explicit about the major findings of this study (I would assume that this would be a summary of Figure 5). It is unclear to me why the methane effects are highlighted here, while this is not mentioned at all in the Conclusion section. In general, the authors should try to

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clarify the main messages from this paper in abstract, introduction and conclusions.

The introduction is somewhat simplistic in that it only lists studies that have attempted to assess non CO2 climate feedbacks. For the general audience and the orientation of the readers it would be helpful to start with a somewhat more detailed description of the major processes and feedbacks considered here and why they matter to the climate system.

The choice of the authors to rely on 4xCO2 experiments to diagnose climate feedback implies that some of the feedbacks considered are less climate change related, but mediated by the effect of CO2 on vegetation productivity and cover. This is an important caveat that should be explained in the Methods section for those processes that do respond to CO2 as well as climate. Also, this needs to be reflected critically in the Conclusions section/Abstract.

The methods section should be expanded by a description on how the authors have dealt with uncertainty in this study. What do the reported $\pm$ ranges represent for individual estimates, how are errors of the multi-model mean derived from these (error propagation of the IAV?), how is the error range of the total forcing estimate determined, how have varying estimates from emission/burden based methods been dealt with in the total feedback assessment.

I rarely recommend merging results and discussion, but I agree with reviewer #1 that in this case, where a lot of different processes are at play, it would be advisable to merge section 4 and 5 in the sense to have results and discussion for each of the different forcing agents together. The quality of the results presentation and their discussion varies substanially, and the authors should strive to be more explicit in terms of describing and explaining the important differences between models, and where possible provide an appropriate comparison to previous studies. There should then still be an overall discussion section 5/6 in the end where the overall contribution of the non-CO2 chemistry and aerosol feedbacks are discussed in the light of other climate feedbacks

(physical, carbon, ...).

Minor comments:

L35: define warmer temperatures

L36: positive methane feedbacks?

L44: consider adding Arneth et al. 2010, Nat. Geo (Doi: 10.1038/ngeo905) to this list

L72: Briefly explain why you not just use one of the options. I also think that this question deserves more attention in the results section where you for some forcings can compare the magnitude of the alternative estimates more systematically to derive at a joint assessment of the individual feedback factor.

Section 2.2: It would be helpful to know which of the feedbacks is calculated which way here. Also, given the need for standardisation here or in the discussion section, there should be a discussion about the assumption of linearity of the radiative forcing response to emissions/burden across a large range of emissions/burden.

Which ensemble members were selected for this study, or does the study use an ensemble mean?

L86: It is unclear whether this is based on simulations presented in Collins et al. 2017, or based on new AerChemMIP experiments, please clarify

L102: Provide an explanation for this scaling factor rather than referencing a full IPCC-chapter

L105: For completeness, give value assumed for M_atm as well as the molecular masses of CH4 and air

Section 3.1 should reference table 2 but does not.

Section 3.2: natural emissions of what?

L119: this sentence needs to be clarified. There are multiple climate-relevant land-

based emissions beyond dust and BVOCs. What do the authors want to state here?

Table 3: define LAI, PAR. Given an indication what LAI varies and interactive vegetation imply. The table captions says BVOC, the header VOC, which is correct?

Given that Section 4.2.3. discusses wetland emissions, the models used should be described here briefly as well.

L134: same as L119

Table 4: what is the difference between wind dependent and wind speed?

L147: Does this sentence imply all models use the same paramterisation?

L151: refer back to Section 2.1 or remove as this is partly redundant.

L155: For the non-expert reader, explain how long the development of a new equilibrium takes and how large the difference on average would be.

Table 5: No SD?

L163: Figure S1 does not separate shortwave and longwave effects to make this claim.

L165: the positive shortwave forcing OF DUST AEROSOLS? Is it possible to provide an explanation for this CNRM response?

Figure 1 (and subsequent following figures): use stippling or alike to show areas of model dis-/agreement. Also revise figures to ensure the legend is readable without magnifying glasses

Table 6 (and similar subsequent tables): Why are certain cells blank?

L201: Why does this discrepancy occur, and how can the AOD be still similar? This paragraph should also have a discussion on why MIROC6 deviates in terms of the ERF response

L222: Figure 3 does not show this.

Table 8: Please check values, at least the alpha emission multi model mean cannot be correct.

L253: Figure S4 does not exist.

L279: BVOC-related aerosols, or aerosols in general?

L281: refer back to Section 2.2.

L297: Methane Burden/Emissions? does not change

L304: The 0.015 Wm-2 %-1 are not described in Section 2.2. but should be

Section 4.2 general: I think it would be easier to follow if the indirect effects of NOx and BVOC on methane were discussed jointly and possible even in one table, as they rely on the same methodology and type of experiments

Section 4.2.3 I find this section troublesome given the lack of explanation of the simulated methane emissions, particular because this presentation confounds the direct effects of CO2 on methane emissions (via CO2 fertilisation of wetlands) with the direct effects of temperature on methane-emissions, but exclusively attributes this to temperature. The result of which is an inflated methane-emission climate feedback compared to Ciais et al. 2013. I wonder whether there are simulations with interactive methane but no biogeochemical coupling to CO2 available from the C4MIP project that would allow to tackle this separation? As a minimum, this confounding effect needs to be explained and discussed.

Table 14: What is the justification to assume at 14% uncertainty on methane radiative efficiency?

Section 4.2.4 should be labelled atmospheric temperature and water vapour?

L356: the residual is then ASSUMED TO BE the direct effect. This statement could be backed up by a brief explanation that BVOC and NOx are the only agents affecting ozone and methane lifetimes next to climate in these models. Otherwise, it should be

explained why other factors may be small and negligible.

L367: Consistently use CESM-WACCM

Section 4.3: Figure 5 is not referenced. The text needs to be explicit that the feedbacks are the multi-model mean, and that not all feedbacks could be calculated for all processes considered. A discussion that I have been missing here is whether these terms are really additive and linear as assumed. It is possible that there is a compensation of feedbacks between models, so I wonder whether it would be possible / interesting to compare the sum of feedbacks across processes for those models that have calculated similar feedbacks

Figure 5: use consistent labelling of models. use consistent labelling of forcing factors (e.g. total non-CH4, wetland CH4 etc.) Use a clearer abbreviation for lightning NOx than lNOx. The figure caption should also explain, how and why feedbacks from table 16 were aggregated in the figure.

Section 5.2 is not helpful is no guidance is given as to the origin of the large range in the estimates and the plausiblity of the different model projections. The comparison to the literature numbers is insufficient in that the numbers aren't directly comparable. This section needs substanial revision.

Section 5.6 response to my previous comment, but then implies that this shouldn't really be listed here as a climate feedback, but a biogeochemical carbon-methane feedback.

Section 6: I would have liked to see a somewhat more broader discussion of the feedbacks derived here in the context of physical and other biogeochemical feedbacks, as for instance summaried in Ciais et al. 2013.

L500: This is an important caveat that should not be left as a foot note in the conclusion section, as it is a fundamental problem of the approach. I strongly recommend to be more explicit about this in the Methods section, where relevant in Section 4 as well as

specifically in the presentation of Figure 5 and Table 16.

L503: This is a point worth discussing more. Are the feedbacks non-linear and therefore we expect them to be larger/smaller when looking at the difference between present-day and 4xCO2?

L505 and 507: The uncertainties given are the SD of sum of the multi-model mean feedback components, but there are larger uncertainties in the derivation of these feedback that should be discussed and acknowleged.

Data availability: It would be helpful if the authors would list the exact names of the experiments used, including an indication of the ensemble members selected

Please carefully edits and update Table S1

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