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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Role of climate model dynamics in estimated climate responses to anthropogenic aerosols" *by* Kalle Nordling et al.

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We would like to thank the referee for carefully reading our paper and for the helpful comments and suggestions. We have modified the manuscript according to these suggestions, and detailed answers to each comment are listed below. The reviewer comments are in italic and our answers are in normal font. In the modified manuscript the changes are shown in red font. The modified manuscript can be found from the supplement material of this post

Specific comments:

Comment 1.

The discrepancies in projected climate features among current climate models have recently been related to the differences in representing the processes of aerosol and aerosol-cloud interaction in these models. This study addresses this issue by investigating whether arbitrarily eliminating the differences in models' aerosol forcing strength and distribution could limit the above-mentioned discrepancies. For such a purpose, the authors have designed two sets of equilibrium-climate simulations: firstly to use two climate models (NorESM and ECHAM6) driven by their own slab-ocean modules while masked with the same prescribed direct and first-indirect radiative effects of aerosols (MACv2-SP), and secondly to force one of these two models, NorESM to adopt derived aerosol forcing field from the other model, ECHAM6. Certainly, the results are not entirely a surprise that the two models even with largely the same aerosol foring distribution and strength would still produce different climate responses particularly over regional scale, for example as reflected from modeled precipitation. The comparison involving the two sets of runs lead the authors to a conclusion that the discrepancy between the two models appears to be largely resulted from the differences in model components beyond that of aerosol. The model simulations were designed straightforwardly and supportive to addressing the science issue of the study. The paper is well organized, and the result is clearly presented. The content of the paper is perfect for the readers of ACP. Nevertheless, there are a few issues the authors should adequately address before the paper could be accepted for publication.

The authors have drawn one of their major conclusions that "further improvements in the model aerosol descriptions can be expected to have limited value in improving our understanding. . .". Such a statement does not make any logical sense based on the results of the paper. Firstly, simply making any two models to have a nearly exact radiative effects of aerosols does not necessarily mean that they both had already been equipped with an improved representation of aerosol and aerosol-cloud interaction. Furthermore, we perhaps all agree that such representations in our current climate models are far from being ideal and in fact, even unable to correctly simulate some of the key physical processes. Therefore, no one could predict the outcome in *terms of modeled climate features should an ideal aerosol module be eventually produced and included. Secondly, per the current modeling efforts in this study, the applied constraint of aerosol forcing does not even include that on cloud response to aerosol perturbation through precipitation and other critical cloud features – as indicated by the authors, not mentioning that on aerosol resuspension through activation-dissolution-evaporation. Even putting aside these comments relating to rather specific processes, giving the well-known status of our current climate models, logically and realistically, the same conclusion made by the authors to the improvement of aerosols could be applied to any other major model components or aspects. Therefore, the above-commented statement, especially presented as a major one for the paper, is not logically meaningful and adequate, in addition, it does not accurately reflect the nature and science meaning of this very study.*

Author response:
We agree with the reviewer that our conclusion was perhaps overstated, and we fully agree with the reviewer that aerosol descriptions in current models are far from being ideal. The root of our original argument stemmed from the fact that models with same simplified aerosol (or forcing) descriptions (ECHAM6.1 and NorESM1) show no less regional variability in their climate responses that models with more complex (albeit far from complete) intrinsic aerosols descriptions (Samset models). However, it is true that it does not make sense for us to argue that aerosol descriptions would not matter. Nevertheless, even if we would have perfect aerosol descriptions inside the global climate models, uncertainty arising from the differences in circulation responses between the models would likely still result in a significant uncertainty in regional climate responses. For this reason, we have changed our conclusion in the abstract accordingly.

Deleted:

“Hence, further improvements in the model aerosol descriptions can be expected to have a limited value in improving our understanding of regional aerosol climate impacts, unless the dynamical cores of the climate models are improved as well."

Added:

"Hence, even if we would have perfect aerosol descriptions inside current global climate models, uncertainty arising from the differences in circulation responses between the models would likely still result in a significant uncertainty in regional climate responses"

Comment 2.

In order to make a statement as strong as "differences in aerosol descriptions among different models are not the main cause of variation in the regional distributions of climate response among different models", one needs to compare the results produced by the versions of the two adopted models with their own intrinsic aerosol module without arbitrary constraints on forcing. Such a comparison would serve as a good reference to evaluate the real effect by eliminating aerosol forcing discrepancies.

Author response:

We agree that this was a too strong statement. In this study, we have not explored regional differences among the same model with different aerosol descriptions, as pointed out by the referee.

Change in the manuscript:

Deleted:

"This implies that differences in aerosol descriptions among different models are not the main cause of variation in the regional distributions of climate responses among different models. Rather, differences in model intrinsic dynamic responses appear to dominate the differences in regional climate responses."

Added:

"The lack of improvement in the correlation coefficients suggests that differences in aerosol descriptions are not the only cause of regional differences in climate signals between the models. Rather, the differences in model circulation responses appear to dominate the differences in regional climate responses."

Comment 3.

The use of the term "aerosol-cloud interaction" seems quite casual in certain places. Giving the nature of this study that dealing primarily with direct radiative effect alongside the so-called first indirect effect of aerosol, when discussing the context of the study itself, the authors should stay closely within the proper scope of their topic.

Author Response:

We agree that the use of the term "aerosol-cloud interaction" was too vague particularly in the abstract. First, we have modified the abstract to be more accurate and state specifically only the first indirect cloud effect. Later in the text this term is only used to summarize previous research.

Change in the manuscript:

Deleted:

"We carry out experiments of equilibrium climate response to modern day anthropogenic aerosols using an identical representation of anthropogenic aerosol optical

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properties and aerosol-cloud interactions, MACv2-SP, in two independent climate models (NorESM and ECHAM6)"

Added:

"We carry out experiments of equilibrium climate response to modern day anthropogenic aerosols using an identical representation of anthropogenic aerosol optical properties and the first indirect effect of aerosols, MACv2-SP, in two independent climate models (NorESM and ECHAM6)."

Comment 4.

The authors borrowed the results presented in Samset et al. (2018) in their discussions. It does tell us that Samset et al. indeed derived a much larger discrepancy among models with intrinsic aerosol scheme. On the other hand, one needs to realize that Samset et al. did not include the same models that are adopted in this study, and the simulation design in that work (with fully-coupled ocean models and most importantly, based on preindustrial era only with perturbations adopted from current climate) are quite different. The performance of climate models with fully coupled ocean component would be different than that of the models with slab ocean module, e.g., likely occurring over high latitudes as discussed in many previous works. The authors should discuss the limitations of such a comparison.

Author response:

We agree that our comparison with Samset (2017) dataset has its limitations. However, we would like to point out the Samset et al. study for aerosol reduction is not based on pre-industrial era, but carried out at climate that has warmed by 1.5 K from pre-industrial due to elevated CO2. Samset et.al have also included the same NorESM1 model although with a different ocean description. They have used fully-coupled ocean

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models whereas we have slab ocean, and it is known that ocean can play a key role in model discrepancies. It is noteworthy, however, that the spatial correlation coefficients do not differ much between any set of models (Samset et al. models and our models). The role of oceans is now discussed in more detail in the revised MS:

In page 12 we now write:

"The fully coupled ocean models in the Samset et al. (2018) dataset also feature long-term internal variability in the ocean states that adds to the level of natural variation with respect to our models with simpler slab ocean representations used in this paper. Therefore, we would expect the Samset et al. data to include more noise than our results with slab ocean configurations. Furthermore, it is important to note that differences in the ocean descriptions are known to have a large impact in the regional climate responses between different models (Deser et al.; Kay et al. (2016)). Overall, we would expect that due to these differences the climate signals obtained from fully coupled models would intrinsically correlate less well with each other than those from models with slab ocean configurations. Somewhat surprisingly, this turns out not to be the case."

Also we note that we discussed the role of ocean in page 13: "However, it should be noted that the ITCZ shift is also sensitive to the type of ocean model used, and slab ocean models tend to exaggerate the change in ITCZ (Kay et al., 2016). "

Comment 5.

Pg. 4, Ln 2: remove "of" before "are based on".

Author response:

Done

Comment 6.

Pg. 4, Ln 5" "between aerosol optical depth and CDNC: : :" this seems implying that the modeled AOD rather than aerosol concentration is the primary input for applying MAC-SP constraint? Or, in fact CDNC itself has been prescribed based on MODIS AOD independent to the model predicted aerosol properties?

Author response:

It is true that this sentence is ambiguous. To be more precise, we have modified the text accordingly:

Change in manuscript:

Deleted:

"The relation between aerosol optical depth and CDNC is derived from MODIS data."

Added:

"The cloud albedo effect in MACv2-SP is parameterized by modifying the model-intrinsic natural cloud droplet number concentration (CDNC) via a relation based on the total change in AOD. This parametrization is derived using MODIS data."

Comment 7.

Section 2.3: The description of NorESM-EF is not very clear. When masking the aerosol forcings of NorESM using ECHAM6 derived values, how did the cloud fields produced in NorESM be considered, for instance, what to do with non-zero first-indirect effect from ECHAM6 in a no-cloud grid in NorESM, or, how to mask direct forcing into cloud fields in NorESM? Could these details be the reason behind the discussed difference between NorESM-EF and ECHAM6-MACSP?

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Author response:

We have now added a section to the appendix of this study to explain more detailed how NorESM-EF run was made, and refer to this section in the main text.

Change in manuscript:

Added into appendix :

The NorESM1-EF run employed radiative forcing extracted from the ECHAM6-MACSP run. First, multi-year monthly means of MACv2-SP aerosol radiative forcing (for TOA and surface radiative fluxes and atmospheric heating rates) were computed for ECHAM6-MACSP. Second, these values were interpolated to the NorESM horizontal and vertical grid and normalized by the monthly-mean incoming solar radiation at model top. Third, during the NorESM1-EF run, these normalized forcings were multiplied by the TOA incoming solar radiation at each radiation time step, and they were added to the radiative fluxes and heating rates computed without MACv2-SP aerosols. This treatment ensures that the diurnal cycle of the aerosol forcing is approximately correct; in particular there is no aerosol forcing during the night. However, the computed forcing is independent of the clouds simulated by NorESM1. Thus, while the aerosol radiative forcing is computed correctly in a monthly-mean sense, its sub-monthly correlation with clouds is ignored. In principle, this could impact the differences between NorESM1-EF and ECHAM6-MACSP. The impact is, however, most likely small. If neglecting the sub-monthly correlation between clouds and aerosol forcing were to have a substantial impact on the climate response to MACv2-SP aerosols, this should also show up in the differences between NorESM1-EF and NorESM1-MACSP. Yet the differences between NorESM1-EF and NorESM1-MACSP are very small (Tables 2 and A1), in fact much smaller than the corresponding differences between ECHAM6-MACSP and either NorESM1-EF or NorESM1-MACSP. This strongly suggests that the differences between NorESM1-EF and ECHAM6-MACSP are primarily caused by the use of a different climate model rather than by the subtle differences in radiative forcing.

Comment 8.

Pg. 12, Line 28: "identical anthropogenic aerosol representations in the models" is inaccurate

Author response:

This is true as the applied aerosol description results in a different total radiative forcing. We have modified the sentence mentioned in this comment so that it is unambiguous.

The sentence is now changed in the manuscript:

"We have here provided the first results on the equilibrium climate response of modern day anthropogenic aerosols using two different climate models, ECHAM6 and NorESM1, with the MACv2-SP (Stevens et al., 2017) anthropogenic aerosol representations."

Comment 9.

Pg. 14, Ln 1: please correct "essentially equally".

Author response:

The corrected text now says "nearly as".

Change in manuscript:

However, the correlation coefficients for regional distributions of climate responses, averaged over equal run length, were nearly as good among our experiments with prescribed aerosols and slab ocean representation (0.78 for temperature and 0.41 for precipitation) and among Samset et al. experiments with model-intrinsic aerosols and the fully coupled ocean representation (0.79 for temperature and 0.34 for precipitation).

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Comment 10.

Figure 1, 2, 4: the results of NorESM-EF run should be presented.

Author response:

This is done as suggested. We have included NorESM-EF results for radiative forcing, temperature and precipitation responses, and comparisons with NorESM and ECHAM6 responses.

Change in manuscript:

ECHAM-MACSP and NorESM1-EF difference is added to figure 1.

Figures 2 and 4 NorESM1-MACSP and NorESM1-EF difference are added to show the temperature and precipitation responses for NorESM-EF compared to NorESM1-MACSP.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-1335/acp-2018-1335-AC1-supplement.pdf

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