Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-639-RC2, 2019
© Author(s) 2019. This work is distributed under
the Creative Commons Attribution 4.0 License.

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global response of parameterised convective cloud fields to anthropogenic aerosol forcing" *by* Zak Kipling et al.

Anonymous Referee #2

Received and published: 23 October 2019

Kipling et al. use a model with parameterized deep convection to study aerosol effects on deep convection. They do so in an atmosphere-only model setup that does not allow sea surface temperatures (SSTs) to respond to the aerosol radiative forcing. The main advantage of this study is that they take into account feedbacks from the large scale circulation (or at least those effects which are not mediated by SST changes). Unfortunately, however, in spite of many nice and useful diagnostics, the discussion of aerosol effects in the strongly forced regions ultimately still seems fairly superficial. I think that in order to better understand the overall effect of taking into account aerosol-deep-convection interactions, potential reasons for the qualitative difference of the CCFM effects between the India region and the Amazon region should be explored

in this study. A specific suggestion can be found below, but I hope that perhaps the authors can add to this. To me, the explanation that "[i]n a strongly-forced deep convective environment, there may be sufficient energy input that any aerosol modulation of the latent heat release will have little effect" seems overly simplistic regarding the qualitative difference between the CCFM effects in the strongly forced regions in Figure 5. I think that after some additional analysis to address the differences between aerosol effects in strongly forced regions, this study will ultimately be a useful addition to the literature on the interesting and important topic of aerosol effects on deep-convection. I recommend major revisions. It should be noted that convective invigoration could in principle affect precipitation intensity while the time mean precipitation amount remains constant.

Major comments:

1) India region: I suggest to analyze ocean and land in the India region separately. As far as I can see, the change regarding LS ACI + ARI in the India region in the Figure S5 is at odds with many studies of aerosol effects over India. Figure 2 suggests that this could in part be due to changes in the Arabian Sea and the Bay of Bengal.

2) Amazon region: As far as I can see, Figure S2 suggests some invigoration in CCFM-microphys in the Amazon region which is partially compensated by CCFM-anvil. Please discuss.

3) I think that the reasons for the apparent convective suppression due to including aerosol effects in CCFM in the India region should be analyzed and discussed in more detail.

4) Because the CCFM effects are defined as CCFMall_ari-CCFMfix_noari, I don't understand why the CCFM effects over India still seem to compensate the large scale effects in such a rather systematic manner (see Fig. 2 and Fig. 6, discussion in line 294 and elsewhere). Please explain.

Minor comments:

l. 6: what does "explicitly simulate" mean here?

l. 12: "rather small" compared to large-scale effects? Fig.6 suggests similar magnitudes for India.

l 25: Yes, "total precipitation is constrained to balance evaporation and by energetic constraints". The problem here is that sea surface temperatures are fixed. In coupled climate models aerosol does affect sea surface temperature and consequently also evaporation. When the sea surface temperatures cannot respond to the aerosol forcing, a large part of the precipitation response that one would see in a coupled climate model is lost. In other words, it is not possible to fully simulate the effects of aerosol on global mean precipitation in the setup used here. I think this point should already be stressed in the introduction and later also be taken into account in the results section (especially in Sect. 4.1) as well as in the conclusion section.

l 310: it seems odd to me that the effect of aerosol on global mean precipitation is emphasized in the conclusion section. When SSTs are allowed to respond to forcing, global mean precipitation changes. When it is not, global mean precipitation stays more or less constant. An exception to this are model runs in which the forcing is from greenhouse gases which by definition absorb longwave radiation. Greenhouse gases affect precipitation more directly than scattering aerosol via their influence on the radiation budget. I think that instead of discussing changes of global mean precipitation in quite some detail, the consequences of keeping SSTs fixed should be discussed. It should also be noted somewhere that convective invigoration could affect precipitation intensity while time mean precipitation remains constant.

l. 256: quasi-equilibrium: I am not convinced that this is actually a problem here since the deep convective heating is applied to the large scale circulation and since large scale water vapor is also modified. More active deep convection may result in less large-scale condensation, but there would still be a shift in precipitation production

from "stratiform" cloud microphysics to the deep convection parameterization, which would then constitute the signature of deep convective invigoration. In my opinion, fixed SSTs are potentially more problematic than using the quasi-equilibrium assumption in the deep convection parameterization.

l. 1: references?

l. 33: "we are probably decades away from having the computer power to run convection-resolving global climate models for more than short time periods": Could you please cite a reference or provide some backing for this statement in the reply to this comment. This does not have to be included in the manuscript.

l. 67: "still lacking non-local interactions": I reviewed Wang et al. (2011), but I don’t understand what the authors are referring to. Please explain.

l. 109: what does "explicit" mean here? Maybe explain briefly how this differs from A+S74.

l. 142: if I understand this right, the anvil effects includes the effect from detrainment into the large scale. Does this have consequences for the separation between CCFM and large scale effects? Please clarify.

l. 142: please also explain briefly how the effect of detrainment on CDNC (and ICNC?) is treated.

l. 156: a fixed CDNC is assumed in all CCFM convective clouds <- how many CDNC?

l. 214f: based on my understanding, having a more inactive deep convection scheme leads to more stratiform precipitation and vice versa. Sometimes, this behavior can be influenced by tuning parameters in the convection scheme. The potential explanation given here is, however, fine with me.

l. 263f: I don’t understand how this explanation fits with the CCFM results for India in Figure 5 and 7.

l. 301: Please repeat which idealized studies this refers to.

l. 301: I can see a number of potential problems with limited area studies, and I think that it is important to perform the type of study presented here. For example, applying a large scale forcing in a limited-area model means that in a strongly forced case, precipitation will more or less simply equal the large scale vapor large scale forcing. At the same time, it is not directly clear to me why phase changes due to precipitation formation delays should be less important in a strongly forced case, unless CCN are depleted by precipitation.

l. 309: what does "via energetic and water-budget constraints" mean? Please be more specific.

l. 310: Although invigoration might not change the time average amount of precipitation, it could still have an impact on precipitation intensity. Please discuss.

Fig 1: does this depend strongly on the model run?

Fig. 4: I think it would be good to explain the x-axis better. Perhaps you could mention the number of ensemble members in CCFM somewhere. I would like to obtain a rough idea about how the values on the x-axis come about without having to consult additional literature.

Technical:

l. 98: uses -> is diagnosed (and remove diagnostic in line 99)

l. 177: months' -> should it just be months without the apostrophe?

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-639, 2019.