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Interactive comment on "Effects of model configuration for superparametrised long-term simulations – Implementation of a cloud resolving model in EMAC (v2.50)" *by* Harald Rybka and Holger Tost

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Authors response to review number 2:

Review comments pasted in black.
Author response in blue.
IMPORTANT NOTIFICATION (in red)!

General comments:
This paper systematically presents climatological simulation results of EMAC with super-parameterization with 20 various configurations, including CRMs orientation, cell size and number of cells. It is useful and impressive to see these results as in Table 2. This paper is publishable if clearer messages are provided.

We would like to thank the reviewer for their helpful report. The manuscript has been revised according to the referees comments.
One important thing to mention is that we have found a bug (precipitation fluxes have been set to zero after CRM has been called) in almost two third of the SP-EMAC simulations and re-simulated these. Almost all results are affected, except the precipitation PDFs and diurnal cycles of rainfall in chapter 3.2 (Fig. 5 and 6). Nevertheless, most of the important features did not change. Taylor diagrams (Fig. 2) changed slightly.
Precipitation biases (Fig. 3 and 4) did not change significantly as well as cloud radiative effects (Fig. 7 and 8).
Chapter 3.3 has been rewritten, because the separation in a Sub-Ensemble A and B was caused by this error in the code. Straightaway, all (latent and sensible) heatfluxes are in agreement with NCEP reanalysis. An additional analysis has been performed in section 3.3 to evaluate the issue of CRM configuration onto the simulated cloud amount in SP-EMA.
Most of the reviewers comments concerning chapter 3.3 are referred to this bug-fix.

For the current version, the readers only know from the abstract that only some aspects of tropical precipitation are better represented with the super-parameterized EMAC compared with the CTRL with a convectional convection parameterization. The other aspects depend on the choice of the CRM setup, and the super-parameterized simulations deteriorate in some cases. What the readers want to know are whether and when the super-parameterized EMAC becomes better than CTRL, and what kind of suitable setup should be chosen for CRMs.

The abstract has been modified to include the best suited setup for SP-EMAC as well as more detailed description of advantages and disadvantages using the superparameterisation.

One interesting result is that results of the super-parameterized simulations are divided into two groups (sub-ensemble A/B). More analysis is suggested to explain why this separation exists. It seems that the results do not clearly depend on the CRM setup.

Due to a bug this division in two sub-ensembles had happened (see important notification at the beginning of our response). Section 3.3. has been completely modified to emphasize other difference of the superparameterisation due to specific configurations.

It is suggested that effects of momentum transport should be summarized in the abstract. In order to analyse the disitinct effect of momentum transport further simulations are needed to allowing/preventing the momentum transport for each configuration. This issue is out of our scope but could be evaluated in a future study.

The authors also should argue about the similarities and the differences of the general behaviors of the effect of the super-parameterization with previous studies of the other groups (Khairoutdinov, W-K Tao, etc.).

The manuscript has been revised to include more comparisons with previous studies (this point has been mentioned by reviewer number 1 as well).

Specific comments:
p. 1, L11, "cloud cover": This is ambiguous. Is this high cloud fraction or total cloud fraction? Needs a clear definition.

"cloud cover" changed to "total cloud cover".

p. 1, L12, “hydrological overturning is too efficient”: This is not clear and may cause confusion. What is the meaning of “efficient circulation”? The authors might want to mention precipitation efficiency. However, in this case, it is not straightforward to related cloud fraction and precipitation efficiency. Even precipitation efficiency is unchanged, cloud cover may decrease if cloud thickness decreases.
We have modified this terminology to avoid confusion. The systematic underestimation is related to the "untuned" version of SP-EMAC. Instead of linking total cloud cover to precipitation efficiency, section 3.3 reveals in-atmosphere cloud amount to certain CRM configurations. Thereby the sensitivity of SP-EMAC configuration on total cloud cover is estimated and recommendations are provided for future studies.

p. 1, L15, “diurnal cycle of precipitation”: In general, diurnal cycle of precipitation is reasonably captured by CRMs. One may think that if “diurnal cycle of precipitation” is not properly simulated, something wrong in parameter settings of CRMs.
This is in general correct but for all superparameterised simulations the diurnal cycle (over land) shows a similar onset in precipitation during the morning hours. This feature is totally misrepresented by using a convection parameterisation. The amplitude of diurnal precipitation exposes a dependency on the number of CRM cells. This is not related to wrong parameter settings but to a prohibited cloud development because the CRM domain becomes too small when decreasing the number of CRM cells.

p. 3, L8-9, “To our knowledge this is the first attempt summarizing the effects of different configurations of the super-parameterization onto the model mean climate state.” I think that there exist similar studies on the effects of different cloud microphysics schemes on the model climate.
Of course there exists multiple publications concerning new setups of superparam-

eterisation especially including different microphysical scheme. A few studies show different setups for superparameterisations (Khairoutdinov, 2005, Marchand and Ackermann, 2010, Pritchard and Sommerville, 2009). We have included within table 1 which configuration has been used in previous literature. Nevertheless no study has evaluated the total impact of CRM configuration in such a manner for a simulated climatological year.

p. 9, Table 2: precipitable water should be added and discussed.
A few sentences have been added within the paragraph to integrate total precipitable water in the context of global mean values.

p. 10, L6, “observed value”: Please add a reference to this value.
The reference to the CERES data has been added as well as the reference to table 1 including the exact observational data set used for comparison.

p. 10, L19, “All simulations show shortwave and net radiative fluxes at TOA that are in close agreement to observed fields”: This is not clear whether the SP-EMAC runs are better than CTRL.
That is correct. When comparing SP-EMAC with CTRL no significant improvement of the shortwave or net radiative flux at TOA can be deduced. But globally speaking a very similar skill is achieved compared to CERES data. This paragraph has sligthly been modified to specify this.

p. 10, L22-23: Any difference between “cloud cover” and “cloud amount”? Is “cloud cover” of SP-EMAC generally better than CTRL? Why?
When speaking of "cloud cover" the total cloud cover within a GCM grid column is meant, i.e. it is the two-dimensional representation of cloud coverage including cloud

overlap assumptions. In-atmosphere "cloud amount" refers to the cloud cover within a GCM grid box, i.e. three-dimensional cloud cover variable. This has been changed in the manuscript.
As mentioned within the paper the total cloud cover in comparison with CTRL is in improved in terms of spatial and temporal variability. In particular the northern hemisphere as well as tropical land shows an improvement for total cloud cover. Oceanic regions reflect an underestimation of total cloud cover. This is predominantly related to the too early onset on precipitation for maritime (almost pure liquid) clouds thereby removing most of the condensate from the atmosphere. In order to improve shallow boundary layer clouds a higher order microphysical scheme is needed and ultimately a finer (vertical) resolution. The improvement for continental clouds is mostly due to a better partitioning of liquid and ice clouds. This is related to the dynamical aspect of subgrid-scale cloud development within the CRM representing a better cloud evolution in terms of updrafts and downdrafts. This allows water vapour to be transported into the correct heights for condensation.
These aspects have been included within the specific sections in a new version of the paper.

p. 10, L35, “The overestimated variability of specific humidity is mainly a cause of too much water vapor transport over tropical continents and too less over tropical oceans”: Is this general behavior of SP-EMAC? Why?
This effect is not only visible in SP-EMAC. The control simulation shows a similar behavior with parametrised convection. Especially one configuration (OR2 4km 64) shows a almost perfect representation of water vapour distribution at 250 and 500 hPa (see Fig. 2, Taylor diagram for specific humidity green triangle and square). Therefore not all SP-EMAC simulations show this behaviour. In order to evaluate this aspect in more detail further sensitivity simulations are needed which is out of scope for our research.

p. 12, L11-15: From this summary paragraph, it is not clear the real advantage of the super-parameterizaion. Is this the correct message of this paper?

This paragraph should not emphasize the advantages of using SP-EMAC. Regarding global statistics of the evaluated variables it performs as good as CTRL with some improvements as well as a slight deterioration for precipitation. This efforts are achieved without the focus on tuning this model setup. Therefore it can be assumed that with additional tuning efforts SP-EMAC would outperform CTRL.

p. 13, L5-11: Related to the discussion around this paragraph, the authors should refer to Luo and Stephens (2006), "An enhanced convection-wind-evaporation feedback in a superparameterization GCM (SP-GCM) depiction of the Asian summer monsoon" (Geophys. Res. Lett., 33, L06707, doi:10.1029/2005GL025060).

The reference to Luo and Stephens 2006 has been added mentioning the importance of feedback mechanisms.

p. 14 L3-6: The logic of this paragraph is not clear. Why the super-parameterization affects land-ocean contrast?

It is the other way around. Land-ocean contrasts affect precipitation rates of the superparameterisation depending on the chosen CRM setup. Changing the orientation, size or number (all parameters for the CRM) affects precipitation rates especially above oceans and coastal regions within the ITCZ and northern and southern mid-latitudes inducing a high sensitivity for these regions. We suppose that regarding oceanic rainfall the setup should be carefully chosen because this could degrade the simulation results. For coastal regions we speculate that the orientation of the CRM has an effect because summarized effects of changes in roughness length, higher wind speeds (sea breeze) and evaporation is influencing the precipitation within the superparameterization depending on the chosen orientation.

p. 15, L15-17: It is suggested that Figure 3 should be compared with the zonal mean precipitation of global cloud-resolving models by Stevens et al. (2019, Fig. 5, https://doi.org/10.1186/s40645-019-0304-z).
Comparing figure 3 and GCRM zonal mean precipitation of Stevens et al. 2019 can not be easily done because the simulation period is quite different. This paper illustrate zonal means of annual averaged precipitation whereas Stevens et al. uses 30-days (of August 2016). A possibility could be to compare only August data but this would raise the question to what extent the August of 2016 represents a more or less climatological averaged month of August. We refrain comparing our results with Stevens but include the new possibilities to compare state-of-the-art GCRMs with superparameterisations within the discussion!

p. 16, L4: Which product of TRMM is used. Refer to Sato et al. (2009), "Diurnal cycle of precipitation over the tropics simulated by a global cloud resolving model." (J. Clim., 22, 4809-4826, doi:10.1175/2009JCLI2890.1).
The reference has been added. The TRMM data reflects a 12-year long period for the month of July (1998-2010). The reference for the data is available at figure 6 (see caption). This has been specified in the text.

p. 18, Section 3.3: Please discuss robustness of the difference between the sub-ensemble A and B. It seems that the difference is not systematically depend on the CRM configuration. Can the authors say in which cases the category of the sub-ensemble is determined.
This results is obsolete due to the bug-fix mentioned in the beginning. Section 3.3 has been re-written.

p. 19, L28-29: Why the results are very different from MODIS?
The results are different from MODIS observations because of neglecting sub-grid

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cloud variability and the missing of COSP as a satellite simulator. As emphasized within the description of the superparameterisation setup we have adopted the modification to account for subgrid-scale cloud variability within calculation of cloud optical properties and radiation after performing all simulations. A totally fair comparison with MODIS would additionally imply the implementation of the COSP package to adapt the satellite simulator which was out of our scope. A few references and sentences to COSP and the importance of subgrid-scale cloud variability has been added to the paper (Bodas-Salcedo, 2011; Song, 2018, Swales, 2018).

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