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Geoscientific Model Development Discussions

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 1:

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

This manuscript evaluates the impact of various configurations of the embedded cloud-resolving model in the ECHAM climate model on a relatively short climate simulation when compared to various climate-relevant observations and reanalysis. Overall, this is a worthy effort and it should be published. However, I feel there are many issues (both minor technical and more significant general) that need to be addressed before the paper is accepted.

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.

General comment: U.S. National Science Foundation supported the Science and Technology Center called Center for Modeling of Atmospheric Processes (CMMAP) between 2006 and 2016, see http://saddleback.atmos.colostate.edu/cmmap/. There is an extensive list of publications produced by CMMAP at http://saddleback.atmos.colostate.edu/cmmap/research/pubs-ref.html that the authors of the paper under review may find useful for the motivation of their investigation. I vaguely remember that some of the superparameterization (SP) tests reported in the current paper were also tried by the people involved in CMMAP (e.g., M. Khairoutdinov, M. Pritchard). Perhaps such efforts should be mentioned in the current manuscript and some of the outcomes can be compared.

Thank you for mentioning CMMAP which we have been aware of. We included some references of this project within the introduction and mentioned findings of particular publications where comparing their outcomes with ours.

Specific comments.

1. I found the title of the paper awkward. First, 15-month simulations cannot be considered long from the climate perspective. Second, the two parts of the title are poorly linked. Please revise.

We have revised the title to avoid confusion and thereby specified the main focus of this paper. The new title is: 'Superparameterised cloud effects in the GCM EMAC (v2.50) - influences of model configuration'

2. P2L23 (page 2, line 23) and in couple other places in the manuscript: it is not clear to me what is meant by "embedding an ensemble of interacting CRMs". Only a single CRM is embedded in each climate simulation, correct? And the configuration is changed in different simulations, correct? If so, referring to an ensemble of simulations is confusing. Please revise.

The term "ensemble" is misleading because it is usually applied to combine different models groups or model setups. In this instance ensemble is referring to the individual grid boxes of a single CRM which are interacting within a GCM column. To avoid any possibility of confusion the term "ensemble" in connection with the CRM has been avoided and revised throughout the manuscript.

3. P2L30: "drastically reduced". First, the cost depends on the configuration. According to M. Khaiouritdinov, the initial implementation of SAM in CAM as reported in 2001 GRL paper slowed down CAM about 200 times. For "larger" CRM (i.e., more columns or higher resolution that increase CRM effort) this number should increase. The dependence on the number of CRM columns should also be valid for the 3D CRM. That said, there are also obvious benefits of separating small-scale and large-scale dynamics, such as parallelization, what model equations to use, etc. Grabowski (JMSJ 2016,p. 327, "Towards global large eddy simulation: super-parameterization revisited") discusses some of these issues.

The cost to run a model including a superparameterisation is strongly dependent on the number of columns of the embedded CRM. Even the grid size of a single CRM grid box has an effect on the CPU time, i.e. keeping the number of CRM cells within each GCM grid box constant and lowering the resolution increase the CPU time. This is due to small instabilities, which occur more often for smaller CRM resolutions for a fixed CRM time step, within SAM which (to some degree) are automatically avoided by subsequently decreasing the CRM time step. In comparison to CAM (Khairoutdinov, 2001) EMAC has slowed down by a factor between 40 (2D orientation, 16 CRM cells, 4 km) to 120 (3D CRM, 64 cells, 1 km; depending on the CRM configuration). All in all, as stated in Grabowski (JMSJ, 2016), the conventional usage of a superparameterisation reduces the amount of computational time by approximately three orders of magnitude in comparison to global CRMs like NICAM.

4. P3L1: A reference to CMMAP would be appropriate here. A selection of papers from the CMMAP website can be used in this paragraph.

References of CMMAP have been added.

5. Fig. 1. First, it shows two timesteps, not three as stated in P5L7, correct? Second,

the coupling terms shown in the figure should be shown in text with appropriate reference (e.g., Grabowski JAS 2006). Is momentum coupled as well as thermodynamic fields?
Figure 1 shows two timesteps (three points in time - corrected in the manuscript) and the coupling terms have been referenced to Grabowski, 2006 JAS. As mentioned briefly in the text (P5L15) momentum (CRM forcing for u and v) is coupled as well but CRM feedback (convective momentum transport) is only applied for 3D CRM cases (simulation number 15 to 20 in table 1). 2D CRM configurations neglect zonal/meridional convective momentum on the large-scale flow. This has been rephrased to avoid ambiguities.

6. P5L17: Why is radiation singled out here? What about surface fluxes and boundary layer transports? What about the land-surface model? Ocean SST? Please explain clearly which processes are treated by the GCM and which by CRMs.
In order to consider subgrid-scale clouds and their radiative feedback on the subgrid-scale the radiation code as well as the cloud optical properties have to be modified to run on the CRM grid. This was done after the implementation of the superparameterisation was succesful. In order to account for model differences due to the usage of a superparameterisation instead of using cloud and convection parameterisations we chose to only switch these parts of the model. A further modification concerning cloud optical properties and radiative transfer would complicate the analysis to differentiate model discrepancies between SP-EMAC and CTRL. Differences could be either due to a different cloud development within the superparameterisation or cloud radiative effects considering subgrid-scale cloud fractions.
SST is prescribed and is not changed by the superparameterisaion. Surface fluxes as well as boundary layer transport is done on the GCM grid based on formulations of Roeckner (2003; https://www.mpimet.mpg.de/fileadmin/publikationen/Reports/max_scirep_349.pd).
The section of the model description has been expanded to account for these pro-

cesses within the description.

7. Table 1. I vaguely remember that Dr. Khairoutdinov conducted similar tests to some of those included in the table. Perhaps he can point the authors to results of some of those tests.
We have modified table 1 marking all SP-EMAC configurations which have been used in previous literature. Most of these simulations are difficult to compare with ours because different vertical grid spacings or further modifications of microphysics, turbulence or radiation have been added. In addition to that, many simulations cover even shorter time periods (couple of months). Nevertheless, results are compared in the context of these simulations.

8. P6L5: "ensemble: - see 2 above.
See answer 2 above.

9. P6L9. Sending reader to the supplement is not appropriate. At least some basic features of the simulation setup should be mentioned here.
The most important characteristics for all simulations are listed within this paragraph: resolution, time period, initial conditions and parametrisations used. The supplement provides only additional information to accurately repeat the simulations. Some extra information has been added in terms of: surface fluxes, boundary layer transport, surface scheme, cloud optical properties and radiation. See answer 6 and 14.

10. P7L2. 15 months is pretty short for climate simulations. How robust are results reported in the paper?
In order to evaluate robustness of our results two SP-EMAC simulations have been elongated to provide interannual variability which is be presented in table 2 in the new

manuscript. All in all, the results for the 15 month period are robust with respect to the two multi-year long simulations.

11. P7L26. I am sure Khairoutdinov ran and reported results from small 3D CRM setup in SP CAM. Again, referring to his experience with this extremely small domain would be needed here.
These edge case scenarios covering very small CRM domain sizes within a GCM grid cell have not been reported by Dr. Khairoutdinov. An exception is the work of Parishani (JAMES, 2017), who embedded a 3D CRM but with a much higher horizontal (250m) and vertical (down to 20m) resolution. This research focused only on shallow cumulus boundary layer clouds where small domain sizes have successfully been employed (Ackermann, GRL, 2003). Domain sizes are far more restrictive for deep convection and associated high clouds, where mesoscale organization and cold pools play a crucial role for its development.
The paragraph concerning very small domain sizes has been revised.

12. P7L30. Please explain how momenta are coupled. See 5 above.
See answer 5 above.

13. Table 2. It would be great to have some error bars for all entries in the table. For the observations, annual variability can provide that, correct? The same could be done for multiyear simulations, except that the simulations are short. This is an important aspect and it requires some comment and maybe additional simulations.
Error bars for observations have been included as uncertainty in table 2. This uncertainty includes interannual variability over the observational time period as well as measurement uncertainty given within the observational descriptions. Providing interannual variability for all simulations is not possible because of too much computational time. To provide an estimate of interannual variability, two simulations are further

integrated to achieve a multiyear simulation (10 years). The annual variability will be given in table 2 as well providing exact model configuration which has been chosen for the multiyear simulation.

14. P10L8. For a fair comparison between EMAC and SP-EMAC, one needs to ensure that cloud radiative properties are prescribed as closely as possible. Please explain this element of the model setup. Is this included in the supplement? See 9 above.
Cloud radiative properties are treated in a similar manner for all simulations using the submodel CLOUDOPT (Dietmueller, GMD, 2016, section 2.4; doi:10.5194/gmd-9-2209-2016). In order to calculate cloud optical properties the following input variables are necessary: cloud cover, cloud water content, ice water content, cloud nuclei concentration whereas CNC has a fixed exponentially decreasing profile for land and ocean separately. Based upon LWC and IWC effective radii are calculated (Johnson, 1993; Moss, 1996). In order to account for cloud overlap the assumption of maximum-random overlap has been chosen.
The technical aspects and theory is described in Dietmueller, 2016. For our purpose the important aspects are:

- model resolution-dependent parameters (asymmetry factor and cloud inhomogeneity) kept fixed for all simulations based on the T42 resolution of the host GCM
- channel objects are different for CTRL and SP-EMAC:
  - for CTRL cloud water content, cloud ice content, cloud cover are large-scale variables calculated within the CLOUD submodule (Roeckner, 2006)
  - for SP-EMAC cloud cover, cloud water and ice are calculated as horizontal means over all CRM grid boxes within a GCM column. Thereby no subgrid-scale calculation of cloud optical properties (as well as radiative tendencies)

is done. This method has been applied intentionally to use subgrid-scale information of CRM but condense it onto the coarse GCM grid and use the same submodels.

Our SP-EMAC simulations are equivalent to Cole, 2005 (doi: 10.1029/2004GL020945) experiment number 3. This type of experiment ensures that the only difference between CTRL and SP-EMAC is the substition of the Tiedtke convection and large-scale cloud parameterisation with the embedded CRM as a superparameterisaion.
An additional paragraph has been added to section 2.3 explaining the coupling. See answer to question 6.

15. P10L29. It si not clear to me how a 15-month simulation can be compared to the reanalysis. Perhaps it can if the setup is designed appropriately. Please explain.
The simulations are designed to represent a simulated climatological reference to prescribed SST/SIC and greenhouse gas distributions. NCEP Reanalysis is used as quasi-observations to compare atmospheric quantities at different heights (here: specific humidity) to evaluate the new model ability to capture the distribution and variability. A short sentence has been added to clarify this.

16. Figure 2 (and maybe other figures). I suggest not to use a color for CTRL, but a symbol (e.g., a star). This would allow CTRL to better stand out.
Using a different symbol for CTRL in all Taylor diagrams would be preferable. The problem is that for some Taylor diagrams multiple quantities are shown separated via different symbols (e.g. Figure 2 - radiation, specific humidity and Figure 9) therefore it would confuse the reader to see the same symbol for CTRL indicating different quantities. Using a dark purple color is the best choice in our opinion.

17. P12L4. "Thereby almost no water vapor. . .". I do not see the link between this

sentence and the previous one. Either way, is this really correct?
The link between the unresolved stratospheric circulation and the transport of water vapour is the Brewer-Dobson circulation. Because the vertical resolution within the lower stratosphere and upper troposphere (as well as the uppermost model layer which is located at 10 hPa) is too coarse to explicitly resolve the large-scale Brewer-Dobson circulation no water vapor is transported from tropics to the poles. The origin of higher water vapour concentrations at the tropical UTLS is mostly convectively produced via vertical transport (updrafts) of moist air from the lower troposphere.
The sentence has been rephrased.

18. Figure 5. Are the differences statistically significant? See 13 above. Also, maps in the right panel show very little variability. Are they needed?
The differences in figure 5 are statistically significant. In conjunction with figure 4, which shows only regions which are different on a significance level of 90 %, the Warm Pool region and southern ocean mid-latitudes have been chosen because they reveal the most distinct differences in precipitation rates. Even if multiyear averages (for EMAC CTRL) are taken into account these regions show significant differences in comparison with GPCP (Tost et al, 2006, ACP).
Concerning the maps in the right panel showing very little variability: This is primarily due to the range of the color bar used to produce these maps. This will be updated to a smaller range in order to better visualize the variability.

19. P14L15: "The most distinct. . .". Looking at the figure, I am not sure what the authors have in mind here.
Looking at the PDF of monthly precipitation for the maritime continent in figure 5 it is evident that some individual SP-EMAC simulations (not shown) are very close to the GPCP data. This can be stated as a most distinct feature because the variability (grey shaded area displaying the range of all superparameterised simulations) of all

SP-EMAC simulations covers almost the entire range of observed precipitation rates (purple line).

20. P15L23: A reference to Guichard et al. (QJ 2004) would be also appropriate here. Added this reference.

21. I feel one should also mention vertical resolution (both in a GCM and in CRM) as a potential factor affecting model results. This should be brought somewhere in the paper.
A paragraph concerning vertical resolution has been added within chapter 3.3 as well as in the discussion. See question 24.

22. P17 and Fig. 7. Problems with radiative fluxes over the Southern Ocean are well appreciated by the climate community. This region was targeted in recent field campaigns (e.g., SOCRATES, see https://www.eol.ucar.edu/field_projects/socrates). I think the scientific consensus is that the representation of cloud microphysical processes such as partitioning between water and ice is an important factor. Can SAM's rather poor microphysics cope with this issue? Should this aspect be mentioned in the discussion?
This issue should be mentioned in the discussion. The simple microphysics within SAM in conjunction with the relatively low vertical resolution is an important factor controlling the CRE in these regions. Reducing this problem can be achieved by using a higher vertical resolution of the embedded CRM (Parishani, 2017, JAMES) as well as using a two-moment microphysical scheme and a higher-order turbulence scheme (Wang, 2017; https://doi.org/10.1002/2014MS000375).
A short paragraph has been added in the discussion.

23. P18L12: The sentence "Therefore it not appropriate. . ." comes out of nowhere! Is part of this sentence missing? Please revise or explain what specifically is meant here.
The sentence has been rephrased. —"Future studies with SP-EMAC should always look at the different cloud radiative effects to avoid misinterpretations of model results. This is necessary because not all SP-EMAC configurations are appropriate to use and addresses the need for a tuning activity for SP-EMAC in the near future."

24. P19L14. Allowing CRMs to rotate was first applied in Grabowski (JAS 2004, p.1940). This reference should be added here. Is the model vertical resolution relevant to the problem discussed in this section? I would think so.
Reference has been added. The vertical resolution plays an important role considering boundary layer clouds especially for the stratocumulus region (Parishani, JAMES, 2017). Another point is that model layer thickness at the tropopause and within the stratosphere is very coarse to reproduce the transport of water vapor through penetrating deep convective clouds realistically. Related to that is the explicit representation of cirrus clouds which is hardly possible using a vertical resolution on the order of $\mathcal{O}(3)$ metres.

25. Figure 8. What is the reason for the noise evident in CRE_SW SP-EMAC (and leading to noise for NetCRE)? This noise is also noticeable in CTRL simulations.
The reason for the wave-like structure ("noise") most prominent in the Pacific is due to the spectral core of the host model EMAC.