

# ***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***

## **Anonymous Referee #1**

Received and published: 24 September 2019

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.

General comment:

Printer-friendly version

Discussion paper



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.

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.
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.
3. P2L30: “drastically reduced”. First, the cost depends on the configuration. According to M. Khaouritdinov, 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”) dis-

Printer-friendly version

Discussion paper



cusses some of these issues.

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

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?

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.

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.

8. P6L5: “ensemble: - see 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.

10. P7L2. 15 months is pretty short for climate simulations. How robust are results reported in the paper?

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.

12. P7L30. Please explain how momenta are coupled. See 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.

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.

15. P10L29. It is 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.

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.

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?

18. Figure 5. Are the differences statistically significant? See 13 above. Also, maps in the right panel show very little variability. Are they needed?

19. P14L15: “The most distinct...”. Looking at the figure, I am not sure what the authors have in mind here.

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

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.

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](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?

[Printer-friendly version](#)[Discussion paper](#)

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.

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.

24. 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.

---

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2019-193>, 2019.

Printer-friendly version

Discussion paper

