

Interactive comment on “Performance of McRAS-AC in the GEOS-5 AGCM: aerosol-cloud-microphysics, precipitation, cloud radiative effects, and circulation” by Y. C. Sud et al.

Anonymous Referee #1

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Sud et al report on the implementation of the McRAS-AC in the GEOS-5 general circulation model. Implementing such an advanced cloud scheme into a GCM is a laudable effort, and the paper thus is pertinent to GMD. In general, I think the paper should be published in GMD.

However, I think a few important clarifications are lacking, and maybe the authors can already comment in the discussion so I could perhaps correct my review comments.

Main remarks

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(1) The authors compare cloud properties to satellite data. It is now widely recognized that for such an evaluation, it is necessary to diagnose cloud properties in the model via a “satellite simulator” (such as the Cloud Feedback Model Intercomparison Project Observational Simulator Package, COSP, <http://cfmip.metoffice.com/COSP.html> now operational in all general circulation models participating to the 5th Coupled Model Intercomparison Project, CMIP5). The authors should clearly specify whether they have used the COSP diagnostics for the comparison or not (and if not, how they assure comparability). (2) A clear explanation of the observational datasets used for evaluation is necessary. (3) A more specific explanation on which parts of the cloud parameterisation are replaced in the new implementation is necessary (see comment on p1387 l13). (4) Assessments as a “reasonably good” comparison between model and observations should be avoided. (5) It is necessary that the authors explain to which degree and with which objective the two simulations have been tuned.

I recommend the authors make use of the free copy-editing offered by the EGU. Thus, I do not report the orthographic errors or formulation mistakes here.

p1383 l23: This is an unnecessary discussion. Many of the points are very controversial: (i) “climate prediction” is an ill-defined term in this context, the importance of aerosols for weather prediction still has to be established; (iii) the weekly cycle discussed by Bell et al. and Rosenfeld and Bell is very controversial, (iv) the same is true for the Lau and Kim hypothesis. So I suggest the authors drop this paragraph.

p1384 l6: for constant cloud water path p16: “all GCMs” would be an equally valid statement: in any case cloud microphysical processes are parameterised. l26: Benedetti

p1387 l13: It would be necessary to clarify whether the McRAS covers all types of cloud (stratiform and convective, and for the latter, shallow and deep convective?). l26: LPNC and ICNC need to be defined.

p1389 l6: How are ocean, land surfaces and sea ice represented in the simulations? l23: How are both model versions tuned? Judging from Table 3, MAC is not so well

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tuned in terms of global-annual mean TOA net radiation, but both are ok in global-annual mean precip.

p1390 I25: Scope and title of Part 2 need to be explained somewhere earlier in the manuscript. Why is this paper not entitled “Part 1” if there is a part 2?

p1393 I20: Is the ISCCP simulator used for the definition of the cloud fractions? What is the overlap assumption used to generate the subgrid-scale variability? If no application of the ISCCP simulator: How are the various satellite issues taken care of in the preparation of the output?

p1396 I14: Is the MODIS simulator used? Are these grid-box average values, including clear skies?

p1397 I4: SSM/I data has specific deficiencies, e.g. Seethala and Horvath, JGR 2010.

p1398 I3: What does “interactive” mean here exactly? Is it really interactive in the simulation evaluated here? If this is possible, why is it not used by default? I24: The MODIS observations show particularly large effective radii compared to other satellite retrievals (e.g., Bréon and Doutriaux-Boucher, IEEE Trans Geosc. Rem. Sens., 2005). I27: r_{eff} depends on the aerosols at most with a cube root dependency.

p1399 I13/14: A formulation “reasonably well” should be avoided. Quantitative evaluations are necessary.

p1401 I11: (Discussion of definition of CRE): The two equations are equivalent only at instantaneous timesteps, it is not possible to get from one to the other for time-averaged quantities. In the observations, in fact, C_{tot} is always either 0 or 1 at pixel level, and a CRE can be defined only by comparing cloud-free to all-sky situations at one locations. This should be clarified.

p1421 Table 3: It is necessary to specify exactly also here the datasets used, i.e., versions of the datasets. What are resolutions and time periods used for the data?

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