Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-212-RC1, 2019

Geoscientific Model Development Discussions
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EGU

Interactive comment on "A new approach to simulate aerosol effects on cirrus clouds in EMAC v2.54" *by* Mattia Righi et al.

Anonymous Referee #1

Received and published: 14 September 2019

Righi et al. implemented a parametrization of deposition nucleation into the EMAC model, although there seems to be insufficient support for such a parametrization based on observations. The new model fails to reproduce the observed dependence of ice effective radii on temperature and it is not clear what effect the assumption that soot acts as INP has on the results. Additional runs without the assumption that soot is an efficient INP and major clarifications are needed before I can recommend this manuscript for publication.

Major comments:

1) Page 3, lines 4 to 9: What was the rationale behind implementing the Hendricks et al. (2011) parameterization in EMAC-MADE and applying it to soot? Evidence from observations suggests that the paramterized process is not effective. I would expect

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this model to compute an unrealistically high estimate of the impact of black carbon emissions from aviation on climate. Or am I missing something? Is there perhaps something that makes you think that the ice nucleation rates that one might expect from pore condensation are similar to the ones one might expect from condensation nucleation? Parameterizing pore condensation requires information on porosity and pore condensation is likely to be important for porous particles such as dust (David et al., 2019). Please explain this better.

2) The aerosol effective radiative forcing (ERF) computed with EMAC-MADE3 is -1.76 ± 0.04 Wm-2. How big is the contribution of assuming that soot acts as INP? Please also state the ERF of soot emissions from aviation with and without assuming that soot acts as INP. The impact of assuming soot to be an efficient INP in EMAC-MADE must become clear somehow, and I don't think that asking for these very specific results from these sets of additional sensitivity runs (which are easily set up) is asking too much. In my opinion it is very important here to quantify the effects of these specific model development choices on the key model results.

3) In the light of my first major comment, I strongly suggest to use a model version without the assumption of soot being an efficient INP as the base version, and to present the simulation with soot INP as a sensitivity study.

4) Fig 2: the very pronounced north-south asymmetry of LWP is inconsistent with observations and with the bulk of CMIP5 models. This indicates that the cloud lifetime effect is probably too strong in EMAC-MADE3. Please discuss.

5) On the one hand, I appreciate that you chose a lower CDNCmin than ECHAM-HAM (Neubauer et al., 2019, doi:10.5194/gmd-12-3609-2019, see also https://doi.org/10.5194/gmd-2018-307-RC1 on this point). On the other hand, I wonder how you can justify a radiative imbalance as large as 5 W/mˆ2. Please explain.

6) Fig 5: Observations suggest a clear dependence of R_ice on temperature (e.g. lower panel in Fig. 5c), which I have also seen reproduced in a global model with a

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two-moment ice microphysics (Salzmann et al., 2010, doi:10.5194/acp-10-8037-2010). EMAC-MADE3, on the other hand, does not reproduce this dependence. Instead, EMAC-MADE3 slightly overestimates R_ice at very low temperatures and strongly underestimates R_ice at higher temperatures. Both seems consistent with various heterogeneous nucleation processes being too efficient. (Overly efficient heterogeneous nucleation at very low temperatures could in principle suppress homogeneous nucleation, which can lead to an underestimate of the ice numbers.). Too small ice crystals in the mixed phase regime may also affect LW-ERF estimates. Please discuss.

Other comments: Perhaps move p. 3, l 26 ff to p3, l7? On the other hand, it would be better to re-write this entire paragraph taking into account the major comments above.

p. 19, line 9: -1.76 ± 0.04 Wm-2: please discuss this result in the light of major points 2,3, and 4.

Fig 2: it would be good to also show zonal mean LWP plots.

p. 1, lines 9 to 11 ("The performance ..."): please refer to my major comment #4 regarding the hemispheric asymmetry of the simulated LWP above.

p. 1, line 10f: please be specific

p. 1, lines 12ff: please refer to my major comments #4 and #5 above and to my comment regarding p. 20, line 13.

p. 7, lines 14ff: "Since the EMAC-MADE3 ....": this seems pure speculation. A least one would have to show that these biases do not occur in models that use different emission data sets in a comparable AMIP-style setup.

Fig 3: since the model values are mean values, I suggest to include vertical bars which give some measure of the spread between the model values (e.g. standard deviations or quantiles).

p. 10, l. 6: as an aside: rather than only matching observations it would perhaps be better to include a process-oriented approach in the future. For example, if the warm rain formation rate is overestimated compared to observations and too little rain forms via the ice phase, this indicates that the cloud lifetime effect may be overestimated. On the other hand, tuning strategies taking such process-oriented metrics into account are only starting to emerge.

p. 17, l. 2: would it be feasible to regrid the observations to model resolution in order to avoid this problem?

p. 17, l. 12: the simulated RH is also controlled by the large-scale condensation scheme, the assumption regarding cloud phase in the saturation adjustment, and other microphysical terms. In the original ECHAM5, the assumption regarding cloud phase in the saturation adjustment is based on a threshold for the mass mixing ratio of cloud ice, but this is different in EMAC. Line 18 on page 15 says that EMAC allows supersaturation over ice based on Murphy and Koop (2005) and supersaturation over ice is also seen i Fig. 6c. The simulated supersaturation depends on this parameterization.

p. 18: line 15: are you comparing a model with prescribed SSTs to the CMIP5 coupled models or is precipitation reproduced remarkably well also when compared to AMIP-style simulations with other models?

p. 18, l. 21: see my comment regarding p. 7, lines 14ff

p. 19, l. 6: I tend to think of the aerosol radiative forcing as the direct forcing. I think what you computed is better called effective radiative forcing (ERF), which by definition includes fast adjustments.

p. 19, l. 5: I assume you are using the standard double calls to estimate cloud radiative effects. Or does Dietmüller et al. (2016) do something special? If yes, please explain. If no, please mention that this is a standard method, so that people who know this method won't have to check Dietmüller et al. for details.

p. 20, line 13: "the total aerosol RF reported by the IPCC ranges between -2.05 and

0.05 Wm−2". Please be specific. How does this fit with AR5 Table 7.4? Also note that in the model with the largest ERFari+aci in AR5 Table 7.4, the ocean heats the atmosphere in the 20th century (Fig. 4, Golaz et al., 2013, doi.org/10.1002/grl.50232) in a setup that uses basically the same aerosol activation scheme, but a different microphysics scheme.

p. 21, l. 3: tuned for what? Usually, I would think about TOA balance. But the model is very much out of balance. I think you need to make clear that by tuned you mean tuned mainly to match LWP, IWP, ICNC, SWCRE, and LWCRE to values that were derived from satellite (remote sensing) observations. The large TOA imbalance of 5.23 W/mˆ2 precludes a successful coupling to an ocean component without further (fairly drastic) tuning measures.

p. 21, l. 7: satellite measurements: space-borne instruments measure radiation. Not even clear-sky radiation in a cloudy scene (as used to compute CREs in the model) is among the measured quantities.

p. 21, list item 1: please see my major comment #4 above.

p. 21, list item 3: please see my major comment #6 above. (I realize that a part of this point is implicitly included in the last sentence. But I still think the unrealistically weak dependence of effective radius on temperature is notable.)

p. 21, list item 4: here you are explicitly referring to "coupled" models from CMIP5. In the context of CMIP5, "coupled" usually refers to the fact that the models are run with an ocean component. Because EMAC-MADE3 is run in atmosphere-only (AMIP) mode (with fixed SSTs), cloud biases (e.g. the double ITCZ) should definitely be smaller than in coupled models. Note, however that in addition to results from coupled model experiments, the CMIP5 archive also includes results from dedicated atmosphere-only (AMIP) model experiments. In order to justify the statement that you are making here, you should compare EMAC-MADE either to uncoupled CMIP5 models (AMIP runs) or else to uncoupled (i.e. without ocean component) AeroCom experiments.

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p. 21, list item 5: please be specific

p. 21, item 6: where is this shown?

p. 21, line 4 from bottom: please see my comment regarding p. 20, line 13.

p. 21, line 2 from bottom: please see my major comment #1.

p 23ff: it would be nice to analyze how much each of the processes contributes to the ice crystal number at a given temperature. One could for example plot time averaged process rates on the x-axis and temperature on the y-axis.

The conclusion section should include a discussion of uncertainties which are specific to this new model development effort. Furthermore, there are some general caveats to this type of study that could be mentioned in the introduction section. In particular, numerous studies suggest that in cases in which the cloud lifetime is limited by turbulent mixing and not by precipitation, coarse scale models may overestimate the cloud lifetime effect (see e.g. references in Salzmann et al., 2010, doi:10.5194/acp-10-8037-2010). Other studies have shown that the aci component of ERFari+aci in one model depends on the CDNCmin tuning parameter (Neubauer et al., 2019, doi:10.5194/gmd-12-3609-2019, Hoose et al., 2009, doi:10.1029/2009gl038568) and in another model on the autoconversion threshold (Golaz et al., 2010, doi: 10.1175/2010JCLI3945.1).

Technical: p. 8, l. 19: calculate -> calculated Correct line numbers would have helped with the review.

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