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Interactive Comment

Interactive comment on "Application and evaluation of McICA scheme with new radiation code in BCC_AGCM2.0.1" *by* H. Zhang et al.

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1. Reply to GENERAL COMMENTS

comments: The authors report tests of a new radiation scheme in the climate model BCC_AGCM2.0.1, and evaluate its impacts in a series of atmosphere-only simulations (prescribed SSTs). The radiation scheme modifications include, in addition to McICA as mentioned in the title (i.e., treatment of subgrid-scale cloud structure), revised treatment of gaseous absorption and cloud optical properties. The paper is well written and well organized. The single major concern I have regarding this paper is its somewhat limited scientific novelty. The McICA approach has been tested in several GCMs, and the radiave effects of changing subgrid-scale cloud structure are, at least qualitatively, well known before. Moreover, the results presented in the paper suggest that the other

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radiation scheme modifications actually have a bigger effect than the introduction of McICA per se. Therefore, I suggest that more weight be put on these other modifications, and less on McICA. See specifically comment 4 below. I don't object reporting the subgrid-scale cloud effects as such, but this part of the paper should be shortened, and some comparison to previous results be added. See comment 13 below.

Reply: Anonymous referee #2 is correct, the physics of McICA has been explored by many previous works. However in most of other GCM simulations, the decorrelation length is set to be a constant of 2km, which does not represent the true cloud overlap behaviour in reality. The decorrelation length is highly dependent on cloud types. Generally the convective cloud should have larger decorrelation length in contrast to other types of cloud. In the revised version, we have discussed the cloud type dependent decorrelation length and the related radiative impact. We believe this is physically new results.

We have combined the sections 4.2 and 4.3 and deleted the tests NEW_GO1 and NEW_GO3, to make the discussing on the impact of sub-grid cloud structures more concise, as following the suggestions of anonymous referee #2.

2. Reply to SPECIFIC COMMENTS

1) comments: This sentence cannot be understood without reading the paper. It should be made clear that "consistent subgrid-scale structure" means here assumptions consistent with the original radiation scheme (maximum random, plane-parallel horizontally homogeneous), that the improvement in clear-sky radiative fluxes comes from the revised treatment of gaseous absorption, and that the improved cloud radiative forcing comes, presumably, from changes in cloud optics.

Reply: This sentence is revised.

2) comments: p. 4935, lines 17-18: To provide a balanced view, this sentence should be expanded, e.g.: Depending on the properties of the cloud field, the widely used

maximum-random overlap assumption can yield even larger radiative flux errors than does PPH (Barker et al., 1999).

Reply: This sentence is revised accordingly.

3) comments: p. 4941, lines 21-23: What was assumed about greenhouse gases and aerosols?

Reply: The greenhouse gases are set the same for all experiments by using the current values. Aerosols are produced by a coupled aerosol model (named CAM) reported in Zhang et al., 2012. More details are added in the revised version.

4) comments: p. 4942: As it seems that the largest effects on radiative fluxes and simulated climate actually do not come from the introduction of McICA, but rather from the new gas absorption scheme and the (ice?) cloud optics, the experimental setup should be extended so that it can also isolate the effect of these factors. The set of model runs (with either interactive or diagnostic radiation calculations) might include (e.g.) the following: 1. old radiation scheme 2. new gaseous absorption only 3. new gaseous absorption + water cloud optics 4. new gaseous absorption + water cloud + ice cloud optics 5. new gaseous absorption + water cloud + ice cloud optics + McICA (with MRO-PPH) 6. new gaseous absorption + water cloud + ice cloud optics + McICA (with generalized overlap and/or inhomogeneous clouds) Experiments 1, 5, 6 and six are already included in the paper, so only experiments 2-4 would need new simulations.

Reply: It is difficult to study the impact of each single element in the new radiation scheme, since the band structure is different from the old one. The functions to calculate cloud optics in the old scheme only have the coefficients that are specially fitted to the old band division. Thus it is not easy to replace the old cloud optical property in the new radiation scheme. There are many new elements in the new radiation scheme. Most of them have been studied in GCM simulations, including the cloud optical property. However, it is indicated that when the cloud macro properties (i.e. cloud overlap) are kept the same, the old and new water optical schemes are unlikely to produce

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notable differences and the changes in SW and LW CRE very likely stem from the changes in ice optical properties (p. 4946, lines 16-21, 28-29; p. 4947, lines 1-3).

5) comments: p. 4943, line 22: The cloud generator of Räisänen et al. (2004), which (if I interpreted it correctly) is used in the study, defines two decorrelation lengths: one for cloud fraction and another for condensate. Were both assumed the same in this study?

Reply: In this study, decorrelation length for cloud condensation is set to be 1 km in all tests. We didn't include the variation of decorrelation lengths for cloud condensation because the impact of this is smaller and less important than that of the overlap of cloud fractions (Barker and Räisänen, 2005).

6) comments: p. 4944, line 17: Loeb et al. (J. Climate 2009) should be cited here.

Reply: This paper is cited in the revised version.

7) comments: p. 4947, line 3: For a reminder to the reader, extend this sentence by "...when maximum-random overlap of plane-parallel horizontally homogeneous clouds is assumed".

Reply: This sentence is revised accordingly.

8) comments: p. 4947: What about SW heating rates? Presumably, the differences were smaller than in the LW, but they should be commented briefly (in text, figures not necessarily needed).

Reply: The differences in SW heating rate is much smaller than that of LW. Actually, we have addressed it at the last sentence in the same paragraph. To make it clearer, we have moved it to the beginning of the paragraph.

9) comments: p. 4948, lines 1-3: Here, ít should be clear that the changes are not only due to the use of McICA, but more importantly, due to the other changes associated with the new radiation scheme.

Reply: This sentence is rewritten to stress that the new radiation scheme plays the most important role in causing the differences.

10) comments: p. 4948: Consider performing comparison with ECMWF's latest and, presumably, most accurate reanalysis ERA-INTERIM, instead of ERA40.

Reply: The simulations are re-compared with the ERA-INTERIM in the revised version. The result is very similar to that compared with ERA-40.

11) comments: p. 4948, line 17: here, it would be useful to remind the reader of the fact that SSTs are presbribed, which limits substantially the climate response to changes in model parameterizations.

Reply: Correct, it is mentioned in the revised manuscript that the SSTs used here are prescribed, which limited the model response.

12) comments: p. 4949, lines 3-8: What is said in this paragrapgh is true, but perhaps out of place. Note that the McICA experiments discussed until this point have assumed maximum- random overlap and horizontally homogeneous clouds, as in the old radiation scheme.

Reply: We have misplaced this paragraph. It is moved to the right place (after section 4.2) in the revised version.

13) comments: Sections 4.2 and 4.3. As pointed out in the general comments, many of the results in these sections are qualitatively obvious and known from previous research. I therefore suggest (i) to shorten the discussion and (ii) add some comparison to previous results. For the latter, one could focus on the effects that assumptions about subgridscale cloud structure have on global mean radiative fluxes. Relevant studies include, at least, Barker and Räisänen (QJRMS 2005) and Oreopoulos et al. (ACP 2012), and to some extent, Räisänen et al. (J. Climate 2007), Morcrette et al. (MWR 2007), and Räisänen and Järvinen (QJRMS 2010).

Reply: We adopted the suggestions of above (i) and (ii). Additionally, we added a new

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test in this section as mentioned in the “reply to GENERAL COMMENTS”, to intensify the scientific significance. Please see the revised manuscript.

14) comments: p. 4951, line 6: Barker and Räisänen (2005) didn’t use CAM3. Instead, they used some data produced in a superparametrization experiment with CCM3 (i.e., a predecessor of CAM3).

Reply: This citation is corrected.

15) comments: p. 4951: In comparing with ISCCP data in Figs. 11 and 12, it should be made clear whether or not the model results were processed with the ISCCP simulator. If not, the comparison should only be regarded as qualitative. For example, the overestimated total cloud fraction in the tropics could be a result of some of the model ice clouds being too thin to be detected by satellites.

Reply: ISCCP simulator is not incorporated in the model presently. Therefore we mentioned in the revised version that the results should be considered as qualitative.

16) comments: p. 4951, lines 21-23: Please rephrase this. The use of larger L_cf in the tropics and smaller L_cf at midlatidues may well be warranted. However, the "correct" value of L_cf shouldn’t be inferred from a comparison of GCM total cloud fraction with satellite data, as GCM layer cloud fractions may contain large biases.

Reply: These sentences are rewritten to make it more rigorous.

17) comments: p. 4955-57: The conclusions are totally focused on McICA and subgrid-scale cloud structure. What about the (often bigger) effects due to changed gaseous absorption and cloud optics?

Reply: The conclusion is revised accordingly.

18) comments: In Table 2, it is curious that the simulated (SW CRF) / (LW CRF) ratio for both DJF and JJA is substantially more negative than the annual mean value (while there is little difference in the observations). Check that these values are computed

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correctly.

Reply: We have carefully re-calculated the (SW CRF)/(LW CRF) ratios and obtained exactly the same results as in Table 2. It is shown in the SW CRF vs. LW CRF scattering figure (see the attached Fig. 1) that the BCC_AGCM2.0.1 model gives much more spread distributions than observed for a single season (and for a single month, not shown in the attached Fig. 1). However, when averaged over the whole year, the simulated SW CRF vs. LW CRF distribution becomes more collective. That is why (SW CRF)/(LW CRF) ratios are much more negative than the annual mean. It indicates that the model needs to improve its simulation of interseasonal variability.

19) comments: In Fig. 3 and 4, you could add the average and RMS differences in the panel titles (you could make room for that by eliminating the variable name, which is the same for all panels). This would provide a quantitative measure of the model performance going beyond just the global-mean values.

Reply: We added averages and RMSs to Fig. 3 and 4.

20) comments: The spatial maps in Figs. 13, 14 and 17 are quite noisy. I would recommend to show instead the zonal mean values, and perhaps for annual mean only. Figure 16 should be removed altogether. It would suffice to say that the precipitation differences are small.

Reply: The sections 4.2 and 4.3 are combined and shortened. Only necessary figures, and only zonal means are kept.

3. Reply to TECHNICAL CORRECTIONS

1) comments: p. 4934, line 18, and elsewhere: "cloud condensation" should be "cloud condensate".

Reply: All "cloud condensation" are changed to "cloud condensate".

2) comments: p. 4934, line 21: "prove the reliability". I suggest change this to "demon-

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strates the feasibility / viability", or something like this.

Reply: Changed as suggested. Thanks a lot.

3) comments: p. 4937, line 3: this should be "preceeding"

Reply: The word “preceding” is changed to “before”.

4) comments: p. 4937, line 4: "document" better word than "archive"?

Reply: The word “archive” is changed to “document”.

5) comments: p. 4938, line 16. First Equation (2) should be Equation (3).

Reply: It is corrected.

6) comments: p. 4939, line 3: "Morcorette" should be "Morcrette", and "Jarvinen" should be "Järvinen".

Reply: These are corrected.

7) comments: p. 4946, lines 19-20: Replace the first part of this sentence e.g. with "As all these regions have abundant high-level ice clouds ..."

Reply: This sentence is changed accordingly.

8) comments: p. 4947, line 7 and elsewhere. I think that "ratio" would be a more appropriate term for (SW CRF) / (LW CRF) than "slope".

Reply: “slope” is replaced with “ratio” throughout the revised version.

9) comments: p. 4947, lines 21-22: "cooling trend", "heating trend". More rigorous wording would be "increased radiative cooling", "reduced radiative cooling".

Reply: These expressions are corrected as suggested.

10) comments: p. 4947, line 26: replace "final state" with "total difference", or "all-sky heating rate difference".

Reply: The expression “final state” is replaced with “all-sky heating rate difference”.

11) comments: p. 4949, line 16: "middle troposphere" would be a more conventional term.

Reply: “Central troposphere” is changed to “middle troposphere”.

12) comments: p. 4953, lines 10-13: This sentence is not clear. Rewrite or eliminate it.

Reply: This sentence is eliminated.

13) comments: p. 4953, line 7: this should be "NEW_GO1-NEW_MRO".

Reply: It is corrected.

14) comments: P. 4953, lines 19-20: more commonly "ocean-atmosphere interaction"

Reply: “Sea-atmosphere interaction” is changed to “ocean-atmosphere interaction”.

15) comments: p. 4956, line 5: this should be "superiority"

Reply: “Superior” is corrected to “superiority”.

16) comments: P. 4957, lines 7: replace "is usually changeable" with "varies usually"

Reply: It is changed as suggested.

17) comments: Fig. 2: consider using colours. The curves are close to each other, which makes it harder to distinguish them. The same also applies to Figs. 7, 8, 15 and 16.

Reply: Figs. 7, 8, and 15 are changed to colors, and fig. 16 is removed as suggested above.

18) comments: Fig. 8: I find this figure difficult to read. Consider putting the differences in separate panels.

Reply: The differences are put in separate panels.

We appreciate anonymous referee #2 very much for his/her constructive comments.

Interactive comment on Geosci. Model Dev. Discuss., 6, 4933, 2013.

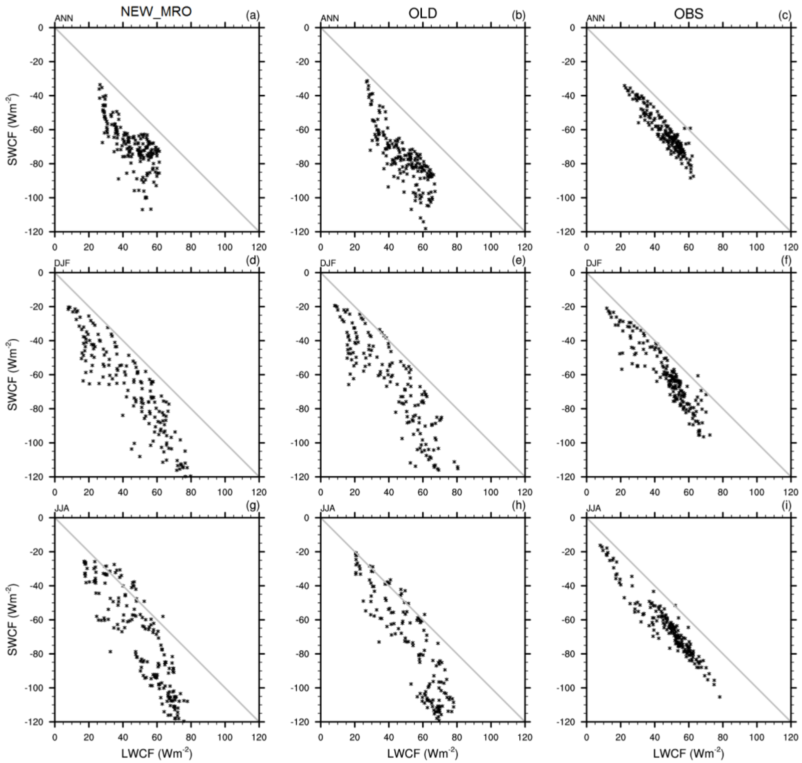

Fig. 1 Scatter diagram of SW CRF vs. LW CRF over the tropical warm pool region (10° S-20° N, 110° E-160° E) for NEW_MRO (left), OLD (central) and CERES_EBAF (right) in ANN (a-c), DJF (d-f) and JJA (g-i).

Fig. 1.

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