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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Influence of Giant CCN on warm rain processes in the ECHAM5 GCM" *by* R. Posselt and U. Lohmann

**Anonymous Referee #1**

Received and published: 17 January 2008

General comments:

This study focuses on the incorporation of giant CCN (presumably sea salts) into the parameterization of large scale cloud microphysics in ECHAM5-GCM. The method to include GCCN is simple but efficient. SCM sensitivity tests show that influences of GCCN on cloud water and precipitation are reasonably consistent with previous studies.

As far as the reviewer is aware, this study is the first attempt to include G CCN into a GCM. It is very encouraging to see the model run successfully. The GCCN effect on cloud properties and precipitation are presented. Influences of GCCN on global radiative forcing between pre-industrial period and present day are also investigated.



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Interactive Discussion

Discussion Paper

The title of this manuscript is "Influence of giant CCN on warm rain processes in the ECHAM5-GCM", and authors claim in the text that this work focuses on the difference between GCCN and RAIN cases. However, lots of efforts are put into the explanation on differences between OBS/HAM and RAIN case (e.g., sections 3.2.1 and 3.2.4). As the reviewer understands, the validation of RAIN case should be done in the work of "Introduction of prognostic rain in ECHAM5..."; where the prognostic rain scheme is introduced. The reviewer expects to see in this manuscript an analysis focusing on the coupling of GCCN microphysics with rain processes, that is, the difference between model results of GCCN and RAIN cases. The validated RAIN case is supposed to be a reference case used in this study instead of HAM or OBS. Only if the prognostic rain scheme is recognized and results of RAIN case are validated with OBS and/or HAM, this study is able to go on.

Additionally, this study lacks of mechanism analysis and quantitative evidences when the coupling of GCCN with cloud physics is discussed. For example, authors frequently mention about the feedback effect of GCCN on convective precipitation when GCM results are shown, but the mechanism of this feedback is unclear, no direct evidences show the feedback on convective precipitation at all. More similar problems are indicated in the detailed comments.

This work is of significant importance and worthy of a publication on ACP. The manuscript succeeds in the description of method but the presentation of results is poorly organized. Some irrelevant materials are included while the necessary analysis lacks of details. GCM validation on the prognostic rain scheme in "Introduction of prognostic rain in ECHAM5..."; is a prerequisite for this study. Important revisions are suggested for this manuscript before a consideration of acceptance. Detailed comments are listed below:

Specific comments:

1) P. 14768, Line 9-13, Abstract: Authors might not have to mention too many details

in terms of the sea salt properties. They are not directly relevant to this study thus can be shortened in the abstract.

2) P. 14768, Line 21-27, Abstract: This paragraph should be elaborated. To what extent the included GCCN modifies the partition between cloud water and rain water masses? How many percents of the cloud water path and cloud droplet number are increased due to the addition of GCCN? Give quantitative results instead of vague statements. For example, in Line 25, "very high GCCN concentration" refers to what magnitude of the number? "Exaggerated transfer" refers to how much water converted from cloud water to rain?

3) P. 14769, Line 5, "0.1-0.25 w/m2": Are these numbers derived from RAIN minus GCCN10 and RAIN minus GCCN5 in the last row of Table 2? If so, it should be 0.09-0.27 w/m2. Be more precise and consistent with the numbers shown in text. Moreover, clearly indicate which case is the reference case (RAIN case here), or remove results from irrelevant cases (HAM and OBS).

4) P. 14769, 1st paragraph of introduction: Authors may want to provide some literature references when they mention about the first and second indirect effects.

5) P. 14769, Line 18-20: GCCN also act as CCN in previous models, i.e., authors mentioned in the manuscript that GCCN are accounted into the total soluble aerosols to be activated. The main reason that GCCN has effect on precipitation is the coupling of GCCN with the rain initialization process in model.

6) P.14769-14771, section 1.1: This section can be compressed. Relations between sea salt concentration and wind speed obtained from literatures can be concluded in a table. In the last paragraph do authors mean that Schulz et al (2004) is the sea salt scheme used in ECHAM5-GCM? If so this paragraph should be moved to section 2 because section 1 is only an introduction.

7) P. 14774, line 20-23: SCM including prognostic rain scheme results in a better agree-



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ment with observations, but GCM results are less consistent with observations (as shown in Fig. 5). The GCM validation of RAIN case should be done in the work of "Introduction of prognostic rain in ECHAM5..."; in stead of in this manuscript.

8) P.14777-14778, section 3.1: Is precip in Figs 3 and 4 large-scale precipitation or total precipitation? May authors show the partition of large-scale and convective precipitation in the SCM results? The GCCN feedback on convective precipitation is mentioned frequently when GCM results are analyzed in section 3.2 but no direct evidences are shown. If the SCM results can provide some quantitative proof, it will be helpful.

9) P. 14778-14781, section 3.2.1: This section can be largely compressed. Comparison between HAM and OBS has done in Lohmann et al (2007). Difference between RAIN and HAM/OBS should be analyzed in the previously mentioned work "Introduction of prognostic rain in ECHAM5..." Since authors claim that "this study focus on the relative changes due to the incorporation of the GCCN", RAIN case should be taken as the reference case for comparison with GCCN case.

10) P. 14781, Line 21-P. 14782, Line 18: Are the simulated distributions in Fig 7 global mean, annual mean or 10-year mean? If so, how could it be compared with the measurements in certain locations within several days? Actually, the comparison shown in Fig. 7 is not very meaningful to this study. Only number concentration of sea salt with radius $>$ 5/10 um is relevant to this work. Fig 7 shows that within all wind speed ranges the GCCN number is underestimated by model. But point-to-point data in Fig 8 shows same trend with a more precise comparison (data points are taken from same location as observation). Thus the comparison in Fig. 7 is inappropriate and unnecessary. Authors may consider removing this part. Fig. 8 is enough to prove the simulated GCCN number for this study.

11) P. 14783, section 3.2.3: This section should be more elaborated and more quantitative results are expected. For example, P. 14783, Line 20-22: "The changes in the stratiform precipitation are rather small. The precipitation changes for the 5 and 10



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um cufoff follow the same pattern and are of same magnitude". However, Both Figs 3 and 4 show apparent increase of precipitation with an order of magnitude increase in GCCN number. Why does not the similar increase appear in the GCM results? Show the partition of large-scale and convective precipitation and discuss the GCCN effect on them separately.

12) P.14784, Line 4-10: More explanation on the triggered convection is expected. How is the reduction of large-scale precipitation related to the convective source terms in model? How significant this influence is? Authors claim that GCCN directly influences the large-scale rain and feedbacks on convective rain. How could the feedback be even stronger than the direct GCCN effect, i.e., changes in convective rain are larger than in large-scale rain (Fig 9)?

13) P. 14784, Line 11-28: It will be very helpful if some quantities are listed to make up for the analysis. For example, show the simulated RWP, LWP, autoconversion rate, accretion rate of RAIN, GCCN5 and GCCN10 cases respectively. If the numbers showing a lower TWP, and smaller conversion rate in GCCN case than the RAIN case, it would be more convincing. BTW, this paragraph presents the reduction of large-scale rain caused by GCCN, the previous paragraph describes the feedback on convective rain. Authors may want to rearrange the contents in a more logical sequence.

14) P. 14784, Line 29-P. 14785, Line 2: As shown in Table 1, reduced TWP and TCC result in a reduced NI from RAIN to GCCN case, which leads to a decrease in SCF. This should be stated in some details here as a coherent result from the above paragraphs.

15) P. 14786, Line 10-12: Use quantitative values instead of the vague statement, e.g., how much are the autoconversion and accretion rates lowered? How much is the "exaggerated transfer of condensed water"? How many is the "too many" GCCN?

16) P.14786, Line 13-22: Actually, in Fig 11, only the winter case in ATL shows consistent decreases of rain from RAIN to GCCN10 and further to GCCN5. In the other 5 panels, either one GCCN case or both GCCN cases produce more rain than RAIN

case. The reviewer does not agree to explain everything with the feedback on convective rain. For example, the GCCN effect might be suppressed in winter because moisture is less and CCN number is low. It is very probable that no GCCN effect is exerted on the precipitation in a certain case. Showing quantitative numbers of relevant variables and analyze each case by combined with the local dynamics. That will help to draw a more reasonable conclusion.

17) P. 14787, Line 11-P14788, Line 1: Again, keep the RAIN case as the reference case; focus on the analysis of difference between RAIN and GCCN cases in this study. Otherwise it is very easy to confuse the reader.

18) P. 14788, Line 10-17: The reviewer thinks that the increased CCN/aerosol number in Northern hemisphere (centered panel of Fig 12) favours the stronger GCCN effect. However in Southern hemisphere the increased concentration of GCCN (shown in Fig 13) might be responsible for the increased TWP while the background CCN/aerosol number is not changed obviously.

19) P. 14788, Line 29-P.14789, Line 1: Why does the 10 m wind speed increase from PI to PD?

20) P. 14790, Line 17-19: If authors insist to compare RAIN and GCCN cases with OBS in this study, retuning has to be done.

21) P.14790, Line 25-29: Are there any direct evidences for this statement? Authors may consider showing the convective precipitation and large-scale precipitation respectively in the SCM model results so that the feedback on convective clouds can be clearly seen.

22) P. 14791, Line 26 and Line 29: The comparison of forcing is between GCCN and RAIN or between GCCN and HAM? Indicate the reference case clearly otherwise the numbers are meaningless.

Technical corrections:



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1) P. 14769, Line 17, "however, the 'size' of both of these effects...": Do you mean the "magnitude" of effect here?

2) P. 14778, Line 8, (see Fig. 4,): It should be Fig. 3.

3) P. 14784, Line 27: "...in turn effects..." should be "affects".

4) P. 14785, Line 8-10: Give the short name such as INA, CHINA, ATL here.

5) Fig 3 and 4: precip refers to large-scale precip, convective precip or total precipitation?

6) Fig 4 above the panel: Sizes of assumed rain drop are marked wrongly with different line styles.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 14767, 2007.