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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Microphysical process rates and global aerosol-cloud interactions" *by* A. Gettelman et al.

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Replies to Anonymous Referee #3

» We thank the reviewer for their time and attention to this manuscript, and for their detailed and extensive comments. These comments have been helpful in improving the manuscript. We have clarified the text in many places as suggested by reviewer #3 and the other reviewers, and redone a few of the figures. We have worked to clarify the discussion and to shorten and focus the manuscript as suggested by the reviewers' detailed comments.

Results have been changed by being more consistent with application of pre-microphysics liquid water path, which was not used in the earlier draft for the GCM

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susceptibility figures 7 and 10. The revised figures are more consistent with previous work. We have eliminated figure 8 from the previous draft, which will help simplify and clarify section 5 as requested by the reviewer: this section will be totally rewritten. We have also added error bars to these susceptibility figures, which will clarify the significance of the results and differences discussed. We have also investigated further the use of the probability of precipitation (POP) as a metric as suggested by revewer #1.

We think in the revision, with the comments detailed below, we will have satisfied all the major reviewers' concerns. In particular, we have rewritten section 5 and made all the changes and clarifications suggested by Reviewer #3, which we think will address the broad concerns of this reviewer about readability. We have eliminated a figure (figure 8) from the original draft as part of trying to shorten and clarify the manuscript as suggested.

Detailed replies to be implemented in the text are contained below. These are incorporated in a revised draft that per ACP policy will be uploaded separately.

Manuscript overview The manuscript investigates the rate of cloud microphysical processes (autoconversion and accretion) in a climate model (National Center for Atmospheric Research (NCAR) Community Atmosphere Model version 5.2, CAM5). A simplified cloud model is used to analyze the role and effect of parameterizations and simplifying assumptions that are used in the description of the microphysical processes in the climate model. The dependence of the microphysical process rates as on liquid water path in the climate model is compared with observed values, and the precipitation susceptibility of the climate model is discussed relative to large eddy simulations and observations. The sensitivity in the climate model of the response of global radiative and cloud properties to anthropogenic aerosol emissions (pre-industrial to present-day) to changes in the rate of microphysical processes is investigated.

Main findings

In the climate model used in the study (CAM5), âĂć ... autoconversion and accretion

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are the most important microphysical processes determining the liquid water budget and its partitioning into its the various microphysical states in most regions.

âĂć ... autoconversion and accretion do not behave in the climate model as expected based on large eddy simulations and observations; in particular, autoconversion appears to increase too rapidly with increasing liquid water path.

âĂć As a consequence, autoconversion contributes too much to the formation of rain and makes the clouds in model too sensitive to aerosol concentrations.

âĂć ... the parameterizations of autoconversion and accretion appear not to be responsible for the incorrect behavior. It is shown with a simplified cloud model that the issue seems to arise from diagnosing rain in the climate model at every time step, rather than treating rain water as a prognostic, advected quantity.

Review summary

The manuscript nicely investigates and analyzes the microphysical process rates and their behavior in CAM5. This analysis of the microphysical process rates, their comparison with observations, the use of a simplified cloud model for a more detailed analysis, and the the investigation of the sensitivity of the effect of anthropogenic aerosol on clouds to changes in the microphysical process rates are valuable informations for the ongoing development of CAM5 and of other climate models.

On the downside, the manuscript does not present fundamentally new insights beyond the analysis of the behavior of CAM5. In particular, the importance of describing rain water prognostically has been known from other models, and has been implemented therein (ECHAM5-HAM, Posselt and Lohmann, ACP, 2008; Met Office Unified Model, Walters et al., GMD, 2011). This raises the question why the authors have put time and effort into conducting the present work, rather than focussing on implementing prognostic rain in CAM5.

» This study is aimed at a fundamental understanding of numerical treatments of the

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warm rain process and how that translates into large-scale models. This goes beyond previous work in trying to better understand the issues involved, rather than jumping right in and adding complexity for the sake of complexity in global models. It reaches similar conclusions to previous work that diagnostic precipitation might be important. But other work does not perform such comparisons with observations, for example.

Furthermore, the manuscript is quite hard to read as of Section 4; the text and figures are confusing in a number of places, and some text appears extraneous and irrelevant within the big picture of the manuscript. My recommendation would be to streamline the text, make it easier to read, improve and linearize the flow of information to the reader, and focus on the most relevant findings.

» In response to this and other reviewers' comments we have tried to do that. Sections 4 and 5 are actually pretty tight in the manuscript. Out of 19 pages, they only account for 8 pages for the major GCM results. We think the detailed comments of the reviewers have helped in this regard.

Detailed comments

Title: "Microphysical process rates and global aerosol-cloud interactions" – Please change this to something like "Microphysical process rates and global aerosol-cloud interactions in CAM5". Without the qualification, the title implies general validity beyond the framework of a climate model, something the manuscript does clearly not deliver.

» Actually, this paper is not just about CAM5 in detail, it is about microphysical process rate formulations and how they are applied. We have added some text to the introduction and model description to emphasize that CAM is a typical state of the art climate model. Note that in the introduction, the discussion is entirely generic: it would not matter which GCM we used. The end of the conclusions also brings things back to the larger picture.

Table 3: The change in top of atmosphere radiative flux (R) from PI to PD seems to be quite insensitive to the changes in the microphysical processes, with the exception of the simulation QrScl. Could you please comment on this insensitivity, and whether it means that uncertainties in the microphysical processes are unimportant for the model?

» We note this in the global results, and now added some speculation that the processes are perhaps buffered against different changes. The point of the paper is that there are large effects with the QrScl change.

Table 3: "QrScl" - Is it possible that this should read "QrScl0.75"?

» Yes, changed.

11793/22: "so that increases in drop number decrease rain rate (qr)" - qr would seem to be the rain water content, not the rain rate.

» Yes, changed

11793/24: "The rain mixing ratio qr in CAM5 is diagnostic: only from rain formed at the current time step." - This sentence does not make sense as written. Maybe the following would work better: "The rain mixing ratio qr in CAM5 is diagnostic: it is determined by partitioning the total amount of liquid water at a given time step into cloud water and rain water amounts."

» Changed, using this wording and clarification that rain water is removed as a surface flux in the time step.

11794/19: "Figure 1 shows that regardless of the cloud regime or region, accretion and autoconversion largely determine the sink of cloud liquid water." - That seems not to be the case in the VOCALS region (Figure 1b) - it would help to add an explanation why in that region, liquid water sedimentation is the dominant sink of cloud water, and what that means/why that is so.

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» In response to this query and another reviewer, this has been clarified.

Section 3 would benefit from re-organization and linearization in order to improve the information flow to the reader: In the status quo of the text, the Figures with the results of the steady state model simulations are referred to before the reader knows enough about the simulations. The simulations should be explained in detail before the figures are discussed. The best location to do this would be the between the first and second paragraphs in Section 3.

» We have modified this as suggested, and added sub-sections describing the cases to improve the flow of this section.

Also, in Table 1, which lists the simulations, more information should be given: The information there is insufficient to properly describe the characteristics of the simulation - e.g. the entry "Different accretion: with auto converted liquid" is too unspecific.

» Changed (to specifically refer to Equation 3)

Equation 3: Please explain in the text (and maybe in Table 1) that this equation is the key feature of the simulation DiagQr.

» Done (in response to comment above)

11796/8: "This formulation (in blue in Figs. 2 and 3)" - Is it possible that DiagQr is in fact the red data in Figure 2 and 3, and not the blue data?

> Whoops. Yes, changed (colors were swapped before submission, and this reference was not changed).

Last paragraph in Section 3: What is the take-home message of the simulation DiagQr0.5"? I understand that it partially recovers the original behavior of the steady state model while using the diagnostic rain rate, but what is the insight the reader should obtain from this?

» Added description at the end and transition to the next section.

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11797/12: "Note the similarity of Fig. 3a (inverse) and b with d." - Please explain this; the meaning of this sentence is not obvious.

» Clarified.

11798/2: "As expected, autoconversion (Fig. 4d) and accretion (Fig. 4b) rates are both larger in midlatitudes than in the tropics where stratiform liquid water paths are higher." - There is potential for an ambiguous interpretation of this sentence: "... where stratiform liquid water paths are higher" could refer to either midlatitudes and tropics.

» Clarified.

11798/6: "The ratio between accretion and autoconversion (Fig. 4e) is large in the tropical troposphere below the freezing level." - The meaning of this is not immediately obvious; do you mean "... is large in the tropical troposphere at altitudes below the level at which freezing occurs" or "... is large in the tropical troposphere at altitudes where freezing occurs"? Either way, a plot showing the zonal mean temperature might help.

» Clarified. It really refers to the melting level (the 0C isotherm).

11798/15 (and 11799/1): "In LES simulations (Jiang et al., 2010), the ratio of accretion to autoconversion increases with LWP." - Jiang et al. (2010) address warm trade wind cumuli - however, the GCM results and observations shown in Figure 5 cover very different regions with very different cloud regimes. This should be mentioned so that readers do not inadvertently draw overly general conclusions.

» Noted.

11799/6: "... which is not what would be expected from the formulation in Khairoutdinov and Kogan (2000)." - It would help to add a reference here to eq. 1, which gives the formulation in Khairoutdinov and Kogan (2000).

» Reference added.

11799/11: "Accretion decreases with AOD (Fig. 5f) in the S. Ocean and S. E. Pacific,

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but is nearly constant with LWP globally." - This needs clarifying explanations: After all, the decrease of accretion with AOD in the S. Ocean and S. E. Pacific is broken by the data point of highest AOD (light green and cyan curves in Figure 5f), while accretion is in fact not constant at all; it seems to have a decreasing trend with AOD (orange curve in figure 5f) and an increasing trend with LWP (orange curve in Figure 5c).

» Clarified as noted. The language was not correct.

11799/15: "However, in the steady state model with modified accretion ..." - It would improve the information flow to the reader to mention that the "modified accretion" means "modified to reproduce the behavior of accretion in the GCM".

» Done

11799/22: "... to the rain rate (R) ..." - Please specify which rain rate is used (surface rain rate? vertically integrated rain rate?)

» Done (surface).

11799/22: "Previous studies (e.g., Wang et al., 2012) note that that the autoconversion/ rain ratio is important in determining LWP response to CCN." - Please add a sentence that explains why that ratio is important.

» Noted (autoconversion is dependent on drop number).

11800/15: "The GCM Au/R ratio is more consistent with the increase in Au/R ratio with LWP in steady state model simulations using modified accretion (Fig. 3b, blue)." – But it seems that the Au/R ratio in Figure 3b (blue curve) in fact decreases with LWP – this would contradict the statment of this sentence, would it not?

» Yes, this should say Fig 3b red. Corrected (to also note that the scaled version also decreases)

11800/15: "The relationship between accretion and rain rate is also very different in the steady state model (Fig. 3c): where accretion increases relative to rain rate for

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increasing LWP, but decreases in the GCM (Fig. 6c)." - In fact, in the GCM, accreation does not show a monotonic change with LWP (shown in Figure 6b, rather than 6c): It increases at lower LWP and decreases at higher LWP. The comparison seems inconclusive.

» Clarified to note the GCM has decreases in Accretion for high LWP.

11801/4: "... but not for the case with altered accretion formulation (DiagQr), where there is higher susceptibility at high LWP (Fig. 3d, red)." - Should this read "... but not for the two cases with altered accretion formulation (DiagQr and DiagQr0.5), which produce a constant susceptibility at high LWP (Fig. 3d)." ?

» Yes, that clarifies the sentence.

11801/4: "This relationship is different from Sp values reported by previous studies (Jiang et al., 2010 ..." - Rather than being different, three of susceptibility curves shown in Figure 7 (TWP, N. Atl., Arctic) are qualitatively in agreement with the results of Jiang et al. (2010). This not only contradicts the statement of the sentence, but is the more surprising because Jiang et al. simulated warm trade wind cumuli, while the GCM results are sampled over all kind of cloud regimes. Please explain.

» Clarified. The quantitative values are different (what was intended). The relationship globally does continue to increase. The decreases are seen in the N. Atl, Arctic and TWP in the last bin only.

Figure 8: While this figure gives the spatial distribution of precipitation susceptibility in the GCM, it is not immediately clear how this information serves to deliver the main points of the paper. Could you please better integrate this figure into the takeaway messages of the paper? Otherwise, it may not be necessary to have this figure in the paper.

» This is linked better with the previous paragraph to show that over land in the model there are smaller increases or decreases in susceptibility at high LWP, while over the

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oceanic storm tracks there are increases. This now explains the differences with previous work better (see above comment). We think this now adds more value to the paper.

11802/2: "5.1 Experiments" - I think we modelers should resist the fashion of calling simulations experiments. The significance of the difference between the two is that an experiment can, at least in principle, conclusively verify or falsify a hypothesis, while a simulation cannot, even in principle.

» This is a philosophical statement that deserves a much fuller discussion. The word experiment is used because it is a convenient way to refer to a pair of simulations. We have changed the phrasing to 'Sensitivity tests' or 'tests' in this section.

11802/11: "Aerosol indirect effects (ACI) are estimated by looking at the Radiative Flux Perturbation (RFP) ..." - Please move the explanation of RFP from 11803/12 here, so the reader learns the meaning of the term when it is first used.

» Done.

11802/18: "... in a third we scale the rain mixing ratio for accretion by an exponent of 0.75 (QrScl0.75). The QrScl0.75 simulation is similar to the DiagQrl0.5 steady state model experiment." - Please explain the physical meaning and effect of scaling the rain mixing ratio for accretion in the GCM, so the reader can more easily understand the significance of this test.

» Done.

11805/5: "There are significant changes in the precipitation susceptibility in the different simulations with altered process rates." - This comparison (and the acompanying Figure 9) raises a number of questions that may or may not be significant for the interpretation of the results; could you please comment on these and, if appropriate, add corresponding explanations in the text?

»We have rewritten this part of the section and redrafted the susceptibility figures,

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including error bars to appropriately respond to these comments.

- How should the negative values of precipitation susceptibility in the GCM be interpreted? Negative values mean that an increase in CCN causes an increase in precipitation

» The redrafted figures have no negative values that are significant. The Sp values are lower in the GCM than other analyses due to scatter. Terai et al 2012 have shown in the appendix that adding noise to the linear regression in log-log space reduces the slope of an Sp calculation. We now cite this in the text.

- what is the mechanism behind that?

» See above, there is not a physical mechanism, but there is a mathematical explanation.

- Why does a reduction in autoconversion by a factor of 0.1 increase the precipitation susceptibility? Shouldn't it in fact reduce precipitation susceptibility? After all, it is autoconversion which is sensitive to CCN number, not accretion.

» Autoconversion drops (Figure 9B), but so does accretion, so that the Ac/Au ratio increases only slightly (Figure 9A). Accretion rates are still larger, and the slope of autoconversion with LWP is still larger. This probably what plays into the susceptibility calculation. The entire balance of processes is altered, as is the LWP. So the LWP is not constant in the different sensitivity simulations. This has been noted in the text, and also answers a query from Reviewer #1.

11807/6-27: These two paragraphs contain a summary of the GCM simulations, but it is not obvious what take-home message they convey, and they are hard to read. It seems they could be removed from the text without any loss to the manuscript.

» The first paragraph has been integrated into the end of section 5 (which should help with the explanation of susceptibility noted above). The second paragraph has been shortened and moved to the discussion of Table 3 in section 5.

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11808/10: "Possible sensitivities to LWP confound this interpretation" - "confound" means "confuse", is that the intended meaning here? Or would "complicate" be a better word?

» Changed to “complicate”

11808/15-23: This last paragraph could be removed without detrimental effects on the significance of the manuscript; it seems quite irrelevant for the take-home messages.

» We feel the numerical issues are actually quite important and should be raised. We have shortened this by eliminating a discussion of future work.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 11789, 2013.