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Interactive Comment

Interactive comment on "Prognostic precipitation with three liquid water classes in the ECHAM5-HAM GCM" *by* V. Sant et al.

V. Sant et al.

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Reply to Anonymous Referee #1

Dear Reviewer
Thank you for your review of: "Prognostic precipitation with three liquid water classes in the ECHAM5-HAM GCM" by V. Sant et al. Please find responses to your comments below.
Best regards,
Vivek Sant

Specific responses to Reviewer comments:

However, I have a few general concerns, and think this should be published in ACP with some important clarifications and revisions. First of all, there is not enough discussion of sensitivity of the scheme: what is the drizzle mode doing? That is really the new part here, and there is no sensitivity test that just focuses on the drizzle mode: does it matter in going from a 2 class to 3 class prognostic precipitation scheme. In general, as noted below, a few more sensitivity tests focusing on the main science questions would be helpful.

Agreed, but we would like to note that such a sensitivity was done in the publication of Sant et al. (2013). Nevertheless, with respect to the model version at hand, we see that the issue has not been discussed enough and we add a paragraph focusing on going from 2 to 3 classes.

Also, the aerosol indirect effects (AIE, or Aerosol-Cloud Interactions, ACI) are sort of a null result: less sensitivity to aerosols is shown diagnostically, but this does not seem to be the case in the simulations performed. The uncertainty levels (25% or ACI) are higher than any signal (10%) and it is of the wrong sign. I think breaking out the ACI by regime may be illuminating: why is the new scheme less sensitive, but has larger ACI (FNet).

We suppose that the reduced sensitivity to the aerosols you mention refers to the change in cloud liquid water path (CLWP) to changes in CCN. Note that this is based on process level and does not necessarily allow direct conclusions on the ACI. First, it does not include all clouds and one cannot predict how this influences the climate system as a whole. Nevertheless, the lower sensitivity did result in lower CLWPs allowing more shortwave radiation to be absorbed at TOA.

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Unfortunately, we do not understand how you estimated an uncertainty of 25 %, but we see that the uncertainty in FNet is rather large. To this end, the simulations will be run for a longer period.
We agree that breaking out the ACI by regime, i.e. contributions from different cloud regimes, geographical areas, etc., would be the next step in identifying reasons for the larger ACI. This will have to be part of future work.

With some more sensitivity tests and a bit more analysis of the details of the results, I think this will make a nice publication.

We hope that we can fulfil the reviewers expectations and thank her/him for the comment.

P7784, L15: more realistic how? What metrics?

We believe that going from a diagnostic to a prognostic treatment of precipitation at time steps of around 10 min allows for a more 'realistic' representation, in what the word means, i.e. a more accurate representation of the truth. In this study (and in others) microphysical processes, such as the collection process, are better represented - e.g. the ratio of autoconversion to accretion. With a diagnostic scheme, this cannot be achieved.

P7786, L10: has led to a negative...

Done.

P7786:L15: GCMs have been shown to overestimate...these aerosol-cloud- precipitation

Done.

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P7789, L9: the tuning is unclear to me: you imply nothing was tuned. Does that mean that both models have the same radiative balance? Or did you retune the model? If they are not in the same balance, please state that, or if tuning was done state that.

The CTRL version of the model was tuned to be in radiative balance. With the introduction of prognostic precipitation and the three liquid water classes we did not alter the tuning parameters - as not to impede the effects of the prognostic scheme. However, we wanted to note, that by implementing a new liquid water class and consequently a new warm rain scheme (for the collection rates), we inevitably changed the parameter space for tuning the model (since the autoconversion rate includes a tuning parameter). This has (obviously) led to some confusion, so we reformulate the paragraph as follows:

> *Note that with the introduction of the new collection scheme, due to the three liquid water classes, and the prognostic precipitation scheme, the parameter space for the tuning of the climate model (Lohmann and Ferrachat, 2010), to achieve radiative equilibrium, is changed. This is because the autoconversion rate is typically one of the tuning parameters used to achieve radiative equilibrium. In PROG this parameter is not used and the new collection scheme is implemented as described in Sant et al. (2013). All other tuning parameters were not changed as not to lose the comparability to the changes induced by the introduction of prognostic precipitation. However, if the climate impacts for both liquid and ice phase microphysics between the two model versions CTRL and PROG are comparable in terms of heating rates (Mauritsen et al., 2012), a similar climate can be expected.*

P7794, L21: some further questions: does the time splitting vary or is it fixed in both the calculation and sedimentation loops? I'm guessing fixed from below. Is the run time total model run time increase, or micro physical parameterizations only? Also: it would be nice to show some plots of autoconversion and accretion rates if you do not do it later.

The time splitting is fixed and is set at the beginning of the simulation. However, the sedimentation itself can be made subject to an extra time splitting, which would also be fixed. This could be deduced from the schematic flowchart shown in Fig. 1, but we will add this point to the model description. Thank you for pointing it out.
The factors given of increased computational costs are given for the whole model, i.e. the atmosphere. This has been added.
We do show some plots of autoconversion and accretion rates later.

P7794,L6: so the high frequency data are from month 4 of the simulation?

Yes.

That seems perhaps minimal for spin up of the land surface. Does it matter?

As we are not using an interactive land surface model (in our case JSBACH), this should not matter. Furthermore, we are only looking at instantaneous variables on the process level, the statistics will not change. If one has a few more or less clouds this will not influence the way the processes behave. Nevertheless, we will check this.

If you only run 5 years and have large enough variability that you cannot distinguish a 15 % change in ACI, perhaps you should run longer. Another option is to remove interannual variability in SST and sea ice and fix one year or a climatology.

We do use climatological SSTs. As mentioned before, we have prolonged the simulations to see whether the variability can be improved.

P7796, L10: is the difference in cloud cover between CNTL and PROG significant?

We will check. The prolonged simulations should also help answer this.

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P7797, L2: I'm not sure your impression of the hydrologic cycle lifetime is correct. PROG has less LWP, so one would expect less cloud, and you have not constrained the in cloud LWP. I do not think you can argue that fewer clouds = faster hydrologic cycle.

As we have climatological SSTs, the amount of precipitation is rather well constrained - as can be seen in Table 2 and Figure 2. Consequently, if there are fewer clouds, the hydrological cycle needs to be faster, i.e. rain is produced at a faster rate. However, we will calculate the lifetimes from the burden/sink to make sure of this.

P7797,L24: this clarifies my earlier point about tuning.

Ok.

P7798,L23: I do not think the re analysis LWC and IWC should be used for comparisons: they are not that well constrained and are the product of model physics in a different model.

However, it is difficult to get reliable observations of such hight resolved variables. We agree that these are also based on model physics of a different model, but they do include observations and therefore act as a sort of guideline. We have observations of IWC and will include these. Concerning the re-analysis we will emphasise that it is not to be taken as the truth, but that it shows a suitable and plausible alternative with which a comparison is fair.

P7799,L10: could you test this theory about redistribution through precipitation by a sensitivity test to change the fall speed of precipitation?

The idea is good and such a test could be done. However, the redistribution of the

CIWC and the CLWC is not only coupled to the fall speed of the precipitation, but in particular to the shift in the collection rates. Especially, the collection rates for the ice phase will be revisited in future work (as we emphasise at the end of the manuscript) to better quantify the response in ACI.

Can you estimate the mass of precipation in the control model to compare precipitation?

Well, the amount of precipitation is nearly the same in both model versions, consequently we unfortunately do not understand the suggestion made.

P7800,L10: can you estimate the fraction of precipitaton formed that remains in the atmosphere? It would probably be the ratio of the PRECIP/tendency of formation. This would be a good way to estimate the importance of prognostic precipitation, and the difference with CNTL.

This is a nice suggestion and we will see if we can diagnose this or re-run the simulation to do so.

P7800, L20: for figure 7, what region is this?

It is global.

It would be nice to divide this into different regimes: stratus regions, stratocumulus, storm tracks, continental mid-latitudes. A single global bar does not do this justice.

We will clarify if we find any differences or alternatively show a map.

Also, what about comparison to observations (e.g. TRMM)?

This cannot be compared to observations as we would need instantaneous values, not daily means or even monthly means.

Also: have you shown the maximum? That is not really clear.

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The point concerning the maximum is not quite clear. What maximum do you refer to?

P7801,l28: but isn't this also a function of the width of the drop size distribution? Is that constant here? Having a few large particle may matter quite a bit?

Yes, the width of the drop size distribution, i.e. the presence of large drops, is very relevant for the autoconversion and accretion rates. However, in a bulk microphysics scheme, the width is often assumed constant or not part of the parameterization. The latter is the case for CTRL. In PROG the drop size distribution is better captured. Although the width is assumed constant for the cloud droplets, the scheme takes the tail into account, which leads to the threshold type behaviour mentioned. For effective radii below 10 μm, the tail of the cloud droplet size distribution is small, such that the high autoconversion rates seen are not physical. We hope that answers the question.

P7802, L5: are these aggregation and accretion rates for ice or liquid or both: it is not clear from the text or notation (It looks like accretion due to snow). What is ACi?

It is the accretion due to snow. The notation is introduced in the line above and relates to the processes introduced in section 2.2. Table 3 does also relate ACi again to the processes summarized in Table 1.

P7803,L28: what is the uncertainty on these regression lines? Also, does the regression vary by region or cloud regime?

We will include the correlation coefficient and the number of data points to give an idea of the statistical variability. Most of the CLWP is from clouds in the lower levels, consequently looking at different regimes would be somewhat artificial. We wanted to comparison our simulations to the work by Wang et al. (2011), hence the global view. We will check the regional dependence.

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P7894, L16: can you show a map of FNet and the differences?

We could, but it is rather noisy and hence does not provide more insights.

How much of this is direct effect: I.e. Only the cloud part is shown in Figure 13.

The direct effect is hard to isolate (see Lohmann et al. (2010)). Thus, we prefer to only estimate $ERF_{aci+ari}$.

P7804, L21: what is 'this' referring to?

It refers to the difference in FNet between the two models CTRL and PROG. We agree that the sentence is not clear enough and will rephrase it.

The explanations here are not that convincing. If the sensitivity goes down, and the accretion goes up relative to the total, why are the aerosol-cloud effects on radiation increasing? You might need to do some sensitivity tests related to precipitaton.

We are not sure we understand the question correctly, but related to the second 'main concern' given (see above), the sensitivity to the CLWP cannot be seen as proxy for the ACI. The changes in both liquid and ice clouds contribute to the net radiation balance, such that it is difficult to say, why FNet has become larger, although the sensitivity of the CLWP to CCN has gone down. Therefore, as stated above, this will be evaluated in future work.

P7804, L24: what happens to precipitation in the radiation code? Is it treated or does it not exist for purposes of radiation?

Precipitation is not seen by radiation. The sensitivity to this will also be addressed in future work.

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P7895,l15: a bit more on why this is valuable to have a drizzle class would be warranted: what really justifies it? Could you construct a sensitivity test without drizzle?

The justification of a drizzle class lies on the microphysical scale, the representation of the drop size distribution, the collection processes and the role drizzle plays for St and Sc clouds and the subcloud layer. We will add some justifications for including a drizzle class and hope that it will become clearer.
We tried running a simulation without the drizzle class, i.e. a prognostic rain class using the collection rates from CTRL, but this led to a climate state for which retuning would have been necessary. We will improve references to the previous work in Sant et al. (2013) to answer this.

P7805,L23: if you tried this, then I would probably show it. You speculate it is important, but then show it is not?

The global effect of giant CCN (GCCN) has been shown in a previous study (Posselt and Lohmann, 2008b) and although the collection processes are improved, the effect on FNet is the same. On smaller temporal and spatial scales previous work (using the GCM in the single column mode version) has shown that with a drizzle class the sensitivity to GCCN is better captured. We believe that including this is beyond the scope of the paper. But we will add the motivation.
Concerning the effect of the drizzle class on radiation (cf. Wood, 2000), first tests showed little effect, because the model (in general) tends to underestimate the amount of clouds in the stratocumulus regions. This might not have come out clearly enough and we will improve this. Therefore, we do not 'show' that it is not important, but merely state that under the circumstances no effect was found. However, we cannot conclude that the effect is not important, due to the reasons named. Consequently, improvements in the representation of low level clouds might yield a more conclusive result. Having drizzle as a separate class enables to represent these intermediate drops at the top of the cloud better, as they won't

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sediment as fast if included into a rain class, better representing the spectrum of drops.

P7806,L17: versatile is not the right word here. Variable?

Alright. Done.

References:

- Lohmann, U., Rotstayn, L., Storelvmo, T., Jones, A., Menon, S., Quaas, J., Ekman, A. M. L., Koch, D., and Ruedy, R.: Total aerosol effect: radiative forcing or radiative flux perturbation?, Atmos. Chem. Phys., 10, 3235-3246, doi:10.5194/acp-10-3235-2010, 2010.