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Interactive comment on "The importance of mixed-phase clouds for climate sensitivity in the global aerosol-climate model ECHAM6-HAM2" *by* Ulrike Lohmann and David Neubauer

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We thank the referee for his/her valuable comments and suggestions. We marked the responses to the comments in red.

General Comments:

It is nice to see that more modeling groups are now looking into the issue of cloud phase and its importance for the overall cloud feedback and climate sensitivity. As such, this paper is a timely and important contribution, but it needs additional work before publication because of the following main reasons:

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1) The comparison of supercooled liquid fraction (SLF) with CALIOP, which in many ways forms the foundation for the study, is not done correctly (see comment below).

Sorry, this was written incorrectly. We did do it correctly and now explained it properly.

2) The claim in the abstract that the findings are contrary to those of Tan et al. are not supported by the results. Tan et al. found a systematic change in equilibrium climate sensitivity (ECS) with changing cloud phase. The same relationship between cloud phase and ECS is found here, but ECHAM6 appears to lie elsewhere on the SLF scale (has higher SLF) to begin with than the model used in Tan et al. (CESM). Many of the model experiments presented are not designed to differ from REF predominantly in their cloud phase, so they shed very little light on this relationship. In my opinion, the experiments that are relevant in this respect are ALL_ICE, REF and ALL_LIQ, but for ALL_LIQ and ALL_ICE there is at present not enough information provided on how these simulations were designed.

We added a more thorough description of the design of the ALL_ICE and ALL_LIQ experiments in section 3. You are right that experiments GFAS3.4 and 10/cc were not designed to address ECS, therefore we removed these two experiments. Cloud phase does change profoundly in simulation NOCONV so we kept this simulation. Likewise, we kept simulation HET because a change in ice cloud optical thickness could impact ECS.

In addition to the above comments, I have the following additional ones (chronologically, so minor and major comments are interspersed):

Page 1, line 13: frequent should be frequently

Corrected

Page 1, line 16: Remove one ’is’

Corrected

Page 2, line 2: uncertainty should be uncertainties

Corrected

Page 2, line 15: Emphasize that TCR is most relevant to present-day/modern climate change.

Done

Page 2, lines 9-17: ECS is really the surface temperature change in response to a doubling of CO2 once the fully coupled atmosphere ocean have reached a new equilibrium state. Therefore, simulations with an atmosphere and a MLO are only an approximation of that equilibrium state, and that should be acknowledged here. There are papers that compare the differences in equilibrium climate state depending on whether simulations are run with MLO or full ocean. It is important to point out here that the experiments in this paper differ from those of Tan et al. in this respect.

Point well taken, we now acknowledge both aspects.

Page 2, line 29-30: It wasn't the spread that varied between 1.4 and 4.1 K, but the estimates themselves.

Thanks for noting this. It's now corrected.

Page 2, line 30: Do you mean "somewhat higher" here? Page 2, line 31: positiv should be positive

Both error are corrected.

Page 3, line 3: Maybe clarify that it is the mid-latitude storms that shift poleward?

Added

Page 3, lines 16-24: Tan et al. used the fully coupled CESM model, not just the atmospheric CAM5. Please clarify here what aspect of the climate of the CESM simulations in Tan et al. was not realistic. This was not specified in Sherwood and Gettelman

(2017), so instead of repeating their unsubstantiated claim it would be better to here explain what specifically was unrealistic in those simulations. Tan et al. also did not claim that ECS in CESM was too low (it is actually quite high), but that the cloud phase bias in isolation would bias ECS low. Other biases in CESM could potentially bias ECS high.

Agreed. We now state explicitly that we are referring to the cold bias of 2-3 K in those simulations in Tan et al., that best matched the CALIOP results. This would lead to more ice in the present-day climate and bias the cloud phase feedback high.

Page 5, line 13: What is "cloud blinking"?

Cloud blinking occurred in ECHAM6.1 because of a bug in the cloud cover scheme. The cloud cover was either 0 or 1 in a grid box but almost never any fractional cloud cover was produced. This on and off in cloud cover is called cloud blinking. In later model versions (including ECHAM6.3) this bug was removed and fractional cloud cover in grid boxes is simulated.

Page 7, lines 25-27: How were the ALL_ICE and ALL_LIQ simulations designed? Did you switch off heterogeneous ice nucleation in ALL_LIQ? And in ALL_ICE, did you increase ice nucleation, increase ice crystal growth (through e.g. the WBF process), or both? What about detrained ice/liquid from convection, did you modify that?

We now describe our simulations in more detail.

Page 8, line 3: I'm guessing this should be "horizontal resolution"?

Yes, corrected.

Page 11, line 35 - Page 12, line 1 2: This is a widespread problem in GCMs in general, not just in CAM5/CESM.

You are right. We changed that and added more references.

Page 13, line 35 -Page 14, line 1: This is not the appropriate way to sample the sim-

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ulated SLF to compare with CALIOP. CALIOP measures SLF for ALL cloud tops, irrespective of their optical depth, but if the cloud top layer has an optical depth < 3 it will ALSO be able to retrieve cloud phase from the cloud layer below cloud top. That is very different from only sampling SLF from optically thin clouds (tau < 3) in the simulations, so the comparison to CALIOP in Fig. 3 is not meaningful at this point.

We actually did it correctly but wrote it wrongly. We corrected that.

Page 14, line 8: Do you mean "detrainment" here? It would actually be very helpful to know exactly how ECHAM handles the cloud phase of detrained clouds.

Yes, we meant detrainment. We now explain how we handle cloud phase from detrainment in section 3.

Fig. 4 and related discussion: The simulations presented here are not comparable to the experiments in Tan et al, for the following reasons: With the exception of ALL_LIQ and ALL_ICE, the differences between REF and the other experiments go far beyond just cloud phase. Whatever difference (or lack of difference) in ECS relative to REF can therefore not be attributed to cloud phase changes alone. In addition, as pointed out above, SLF is not extracted from the simulations in an appropriate manner, so it is hard to say how SLF would compare if they had been, and since it is unclear how ALL_LIQ and ALL_ICE were constructed it is hard to make an informed comparison with the corresponding simulations in Tan et al.

As mentioned above, we did extract SLF correctly, thus the comparison to Tan et al. is justified.

Page 16, line 6: proof should be prove.

Corrected

Table 3: Give two decimals consistently in this table.

We actually prefer to keep the number of decimals according to the value of the quantity. We have 2 decimals for precipitation, where they are needed because the different global mean values are rather similar. For all other quantities 1 decimal is sufficient. To respond to your request, we added one decimal for the oceanic cloud top cloud droplet number concentration.

Fig. 5 shows that there are a lot of other things going on in these simulations other than the cloud phase feedback. Clearly, the ALL_LIQ cloud changes are very different from those in REF, so the fact that they have a similar ECS is the result of multiple compensating differences between them, cloud phase only being one of them.

We agree. We are now discussing this in more detail.

Fig. 7 and related discussion: This figure, if anything, confirms the findings of Tan et al. if you focus on the simulations here that differ ONLY in their cloud phase. The cloud optical depth feedback changes quite dramatically (becomes less negative) from ALL_ICE to REF, and a small additional change is seen from REF to ALL_LIQ. The total change of 0.5K/Wm-2 is very strong. The main difference is that the default ECHAM6 is closer ALL_LIQ in its behavior, while CESM was more similar to the equivalent of ALL_ICE. The other experiments have lots of other changes in them beyond cloud phase and are not relevant for the discussion of the impact of cloud phase on climate sensitivity.

We actually mention that at the very end of section 4. We now discuss that again in the conclusions.

Page 20, line 5: thinnen should be thin

Changed

Page 21, line 3: optical should be optically

Corrected

Page 22, lines 1-2: The only way that a marked increase in the overall cloud feedback can not correspond to a marked change in ECS is if either: i) The simulations had not equilibrated in the analyzed 25 year time periods, or ii) Other climate feedbacks compensate for the change in the cloud feedback. I'm guessing it's the latter, but both aspects should be addressed in the paper. In other words, what was the radiative balance for the respective experiments for the 25 yr time periods analyzed, and how did the other (non-cloud) climate feedbacks change between the experiments?

Unfortunately, we did not diagnose other feedbacks. What we can say is that our simulations are in equilibrium after 25 years, so that is not the problem. We now discuss the hypothesized compensating feedback in more detail.

Page 24, line 31 -33: These findings are not contrary to Tan et al. - the SLF is higher in ECHAM6 than in CESM/CAM, so there is a smaller ECS increase associated with increasing SLF. It is completely consistent with Tan et al., as far as I can tell. A caveat here is obviously that in the ECHAM6 simulations SLF is not calculated they way it has to be in order to be comparable to CALIOP, so once that's corrected the SLF comparison may look different.

See answers to both points above.

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