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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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Review of "The importance of mixed-phase clouds for climate sensitivity in the global aerosol-climate model ECHAM6-HAM2" by Lohmann and Neubauer.

Synopsis:

Lohmann and Neubauer investigate the impact of cloud phase partitioning, as quantified by supercooled liquid fraction (SLF), on equilibrium climate sensitivity (ECS) in the newest version of the ECHAM6-HAM2 GCM. Specifically, they modify the model several different ways to produce varying SLF and assess ECS for seven model configurations. They find that ECS is only strongly impacted when SLF is forced to be zero at temperatures below 0oC. This is in contrast to Tan et al. (2016) who showed

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a monotonic increase in ECS with increasing SLF in the CAM5 model. The authors hypothesize that instead of global SLF, it is SLF in mid-to-high latitude clouds that is important for ECS. They find that in ECHAM6-HAM2, ECS is only impacted in the model version where absorbed shortwave radiation over the Southern Ocean is not correctly simulated. They show that ECS and ERFari+aci are not correlated in their seven realizations of ECHAH6-HAM2, in contrast to previous versions of ECHAM and the CMIP5 ensemble.

The subject matter is appropriate to ACP and the paper represents a useful addition to an ongoing area of research into the impact of cloud feedbacks on ECS. However, the current version of the paper is unclear in places, most notably the description of the model configurations and the comparison to results in Tan et al. (2016). I have two general comments and several specific comments that I recommend be addressed prior to publication.

General Comments:

1. Model Configuration. Several aspects of the model set-up and experiments section are not sufficiently explained and as a result it is unclear how the way the models were set up and run might impact the results. Specifically:

Page 7 Line 25: How are the ALL_ICE and ALL_LIQ simulations set up?

- How do the methods used to modify the model here compare to those in Tan et al. (2016)?

- Are the changes in ALL_ICE and ALL_LIQ applied to all types of clouds globally?

It was unclear to me why these seven specific configurations (other than ALL_ICE and ALL_LIQ), which contain multiple differences other than SLF, were selected given the goal of the study is to determine the impact of SLF on ECS.

Page 8 Line 7-10. It is unclear how the various simulations were set up and run. You state that "To calculate ECS, ECHAM6-HAM2 has been coupled to a mixed-layer ocean

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(MLO). These simulations were spun up for 25 years and then run for another 25 years, over which the results were averaged."

- How was the mixed-layer ocean set up (i.e. how are mixed-layer depths and q-fluxes generated) for each of the seven different versions of the model?

- Are mixed-layer depth and q-fluxes different for the seven different versions of the model? If not, how might this impact results (e.g. Frey et al. (2017) showed that ocean heat uptake changes in response to differences in clouds similar to those, for example, between ALL_ICE and REF).

- How many and what types of simulations were run?

- Was a 1xCO2 (pre-industrial) simulation run for each model configuration?

- Was a 2xCO2 simulation run for each model configuration?

- When was CO2 doubled? Was it before or after the 25-year spin up period?

- You state that results were averaged over 25 years after model spin up. In the 2xCO2 case, were those 25 years all after the model had reached a new equilibrium after doubled CO2?

- Were fixed-SST runs accomplished as stated in Table 2? If so, which years? Were observed SSTs used?

- Were any fully-coupled model runs accomplished?

Table 1 could be expanded to include information to clarify many of the points above.

Page 8 Line 11-15: Model Tuning. Two parameters which impact precipitation efficiency were used to tune the various model configurations. Several places in the paper (e.g. Page 11 Lines 405, Page 12 Lines 6-7, Page 22 Lines 26-28) reference these tuning changes to explain differences between the model configurations, but a discussion of if and how tuning might impact ECS or cloud feedbacks is not included. Does model

tuning impact ECS or cloud feedbacks?

2. Comparison of results with Tan et al. (2016)

The results from this paper are contrasted with Tan et al. (2016) who showed a monotonic increase in ECS with increasing SLF in CAM5. In two places (Page 3 L20-21 and Page 25 L1-2) the authors state that the Tan et al. results should be treated with caution because the climate in their models was not "the most realistic", citing Gettelman and Sherwood (2016). This brings up two comments:

a) What about the Tan et al. (2016) model climate(s) is unrealistic that would impact ECS estimates? Are the model climates assessed in this paper more realistic? The authors compare their models to observations several ways in Table 2, but no direct comparison to Tan et al. is mentioned. The only direct comparison I could make is precipitation rate, which is similar in the Tan et al. models (their Table S2) and the model realizations presented here.

b) Gettelman and Sherwood (2016) also reference another version of CAM (Kay et al., 2016) which they state has a more realistic climate than that of Tan et al. (2016). Frey and Kay (2017) assessed the ECS of this "more realistic" version of CAM and showed an ECS increase similar to that in Tan et al. The Frey and Kay (2017) result may suggest that the Tan et al. result is not an artifact of an unrealistic climate, and should be discussed along with the Tan result.

Specific Comments:

1. Page 2 Lines 9-10: "Here we evaluate the increase in the global annual mean surface temperature (ΔTs) at the time of a doubling of carbon dioxide (CO2) with respect to pre-industrial concentrations"

- This is misleading. The only warming metric assessed in this paper is ECS, which is the equilibrium global mean surface warming resulting from a doubling of CO2. Not the warming at the time of doubling CO2.

2. Page 2 Line 10: The forcing due to a doubling of CO2 is model-dependent (e.g. Forster et al. 2013). Is the 3.7 W m-2 listed here an average value among IPCC AR4 (Solomon et al., 2007) or the value from ECHAM6-HAM2?

3. Page 2 Lines 11-14. This discussion of TCR and ECS is misleading as it appears to imply that they are both in part defined by the model runs used to estimate them.

- "ΔTs can be calculated in different ways". This is confusing. I think you mean that there are two metrics that describe the temperature response to a doubling of CO2. TCR (warming at the time of CO2 doubling after a 1%/yr increase in a fully-coupled model) and ECS (warming at equilibrium after a doubling of CO2).

- "or it can be obtained from coupled atmosphere - mixed layer ocean (MLO) simulations that are abruptly exposed to a CO2 doubling relative to pre-industrial concentrations and then run until a new equilibrium has been established (equilibrium climate sensitivity, ECS)"

- This definition of ECS is misleading. A MLO is not necessary to estimate ECS. ECS is the global, annual mean warming at equilibrium after a doubling of CO2. It is commonly estimated using MLO models (e.g. in IPCCAR4 Meehl et al., 2007) or fully coupled models (e.g. Gregory et al., 2004; and in IPCC AR5 Flato et al., 2013).

- When comparing results to Tan et al. (2016), it is important to note that they did not use a MLO to estimate ECS. This could impact the comparison because different methods can produce differing ECS estimates (e.g. Frey et al., 2017).

4. Page 3 Lines 6-15: This paragraph should cite some of the extensive literature on the negative optical depth feedback. Some relevant papers are Mitchell et al., 1989; McCoy et al., 2015; Ceppi et al., 2016; and the review paper by Storelvmo et al, 2015.

5. Page 3 Line 25: The transition between ECS and aerosol radiative forcing could be improved. It was unclear why aerosol forcing was being discussed until the last sentence of this paragraph.

6. Page 3 Lines 30-35: Are the simulations ALL_ICE and ALL_LIQ discussed here from this paper or from a previous paper? Please clarify.

7. Page 7 Line 19: Please define the acronym GFAS.

8. Section 4 (Pages 8-14): “Comparison of ECHAM6-HAM2 with observations” This section contains a lot of information and details on the observations used. I found it hard to pick out the comparisons of the model with observations among all of these details. It might be easier to read if the information on the observations used are presented first, and then the comparison with observations is done in a more compact way.

9. Page 9 Line 35: "Nl,oc,top reaches values ofÂă > 100 cm−3 35 between 30 N and 80 N in the observations (Figure 1).” I do not see observed data north of 60 degrees North in Figure 1d.

10. Page 12 Line 6: In Figure 1 it does not appear that ALL_ICE underestimates cloud ice in the extratropics, especially in the Southern Hemisphere.

11. Page 12 Line 31 and Page 13 Line 4: Please state which simulation is “the one with the extreme changes in SLF.”

12. Page 13 Lines 9-10: Does the fact that all seven model simulations overestimate the net negative radiative effect of clouds have an impact on the cloud feedbacks predicted by the models?

13. Page 14 Line 2: Here Figure 3, which shows how SLF varies between models, is introduced. Is it possible to include SLF comparisons in Table 1 and Figure 1 along with all of the other comparisons with observations?

14. Page 14 Line 19: ECS is the temperature at equilibrium after a CO2 doubling. Not “at the time of CO2 doubling”

15. Page 15 Line 3: First reference to table 3. In Table 3, how are the changes

defined? Are they the changes in response to doubled CO2? If so, which years from which simulations are used?

16. Page 16 Lines 1-5: The hypothesis put forward here is very similar to the hypothesis of Frey and Kay (2017). From Frey and Kay 2017: "Climate models overestimate the magnitude of the negative cloud phase feedback at extratropical southern latitudes because they overestimate the amount of cloud ice present in the mean state. Further, since negative feedbacks reduce warming, models with negative cloud phase feedbacks that are too large may underestimate the amount of warming resulting from greenhouse gas forcing, quantified by their equilibrium climate sensitivity."

17. Page 16 Line 9: First reference to Figure 5. In Figure 5, why is the cut off for the extratropics 60 degrees North and South? The negative cloud phase feedback acts poleward of these latitudes, especially in the Southern Hemisphere (e.g. Zelinka et al., 2012, Figure 4d).

18. Page 22 Lines 1-2: The fact that the cloud feedback increases between simulations without impacting ECS is an interesting finding. If the mixed-layer oceans are different between the different simulations, do differences in heat uptake impact this result? Are you able to assess other feedbacks (e.g. lapse rate, water vapor, surface albedo, etc.) to determine if there is compensation?

19. Page 25 Lines 12-15: This section is unclear. How is shortwave radiation at high latitudes related to the tops of deep convective clouds and low level tropical clouds? In the next sentence, what are "all of these other clouds"?

Technical Comments:

All Figures: Please specify which years from each model run (e.g. years x-y from 1xCO2 runs, years a-b from 2xCO2 runs) were used to create each figure? Which years were used for the observations presented?

Page 1 Line 13: Change "frequent" to "frequently" Page 2 Line 27: Change "not" to

"nor" Page 2 Line 30: Change "somehow" to "somewhat" Page 2 Line 31: Change "contributor" to "contributors" Page 2 Line 32: Change "positiv" to "positive" Page 8 Line 5: insert comma after ERFari+aci

Page 10 Figure 1: Label the panels a, b, c, etc. Are these model runs the MLO runs or fully-coupled runs?

Page 11 Table 2: What years are used for the observations presented here? The caption specifies some of the data sets but not all. Are the same years used in the model runs?

Page 14 Figure 2: What years are used for the observations presented here? Are the same years used in the model runs?

Page 17 Line 22: The sentence beginning on this line is very long and hard to follow. Page 20 Line 6: Is "this region" the subtropics?

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