Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-97-AC2, 2018

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

Ulrike Lohmann and David Neubauer

ulrike.lohmann@env.ethz.ch

Received and published: 4 May 2018

Response to reviewer 1:

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

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?

The simulation ALL_ICE is set-up such that ice crystals grow at the expense of cloud droplets, which evaporate at temperatures < 0 °C, that all cloud water that is advected to colder temperatures is forced to freeze instantaneously and all the detrained cloud condensate is in the form of ice at temperatures < 0 °C. The simulation ALL_LIQ is set-up such that heterogeneous freezing is turned off in the temperature range between 0 and -35 °C and detrainment of ice crystals from convective clouds is restricted to temperatures below -35 °C. Ice crystals sedimenting from colder into warmer cloud layers melt at -35 °C. The number of cloud droplets which freeze at temperatures below -35°C had to be reduced by a factor of 100 in order to keep the ice crystal number concentration realistic in simulation ALL_LIQ. We added this description.

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

The simulations ALL_ICE and ALL_LIQ are as similar as possible as in Tan et al., 2016, but we were more thorough in strictly enforcing this set-up. However, there is one difference such that Tan et al. used results from a fully coupled simulation, while we were using a mixed-layer ocean. We added this.

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

The changes are applied to all layer clouds. Convective clouds in ECHAM are not radiatively active, thus we didn't change the simple microphysics of them. However, we ensured that the detrained condensate, which is a source for large-scale clouds, is detrained in the phase corresponding to the ALL_LIQ and ALL_ICE set-ups. We added this as mentioned above.

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.

You are right. Our primary goal is the impact of SLF on ECS. As also the second reviewer commented on this, we have deleted the section of the aerosol radiative forcing and the two simulations that were exclusively done for this, namely GFAS3.4 and 10/cc. We decided to keep the simulations HET and NOCONV because they influence the distribution of clouds and shed light into 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. (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?

A mixed-layer depth of 50 m is used in all simulations. The deep ocean heat flux Q is computed from 25 year-simulations with fixed SSTs (AMIP climatology 2000-2015), which were run for each of the seven different set-ups separately. We added that.

- 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).

Mixed-layer depths are the same (50 m) but Q-fluxes are different (see our reply to the previous question). We agree that the deep ocean heat fluxes will adjust to the different set-ups, therefore we computed them for each set-up separately.

- How many and what types of simulations were run?

All different simulations are listed in table 1. All of them were run in three different set-ups: (i) atmosphere-only simulations with prescribed SST and sea ice cover using the AMIP 2000-2015 climatology with present-day aerosol emissions and greenhouse gas concentrations, (ii) atmosphere-MLO simulation at 1xCO_2 concentrations with pre-

industrial aerosol emissions and pre-industrial levels of the other greenhouse gas and (iii) as (ii) but with 2xCO_2 concentrations. We added that.

- Was a 1xCO_2 (pre-industrial) simulation run for each model configuration?

Yes.

- Was a 2xCO_2 simulation run for each model configuration?

Yes.

- When was CO_2 doubled? Was it before or after the 25-year spin up period?

The CO_2 concentration was doubled before the 25-year spin up period. We added that.

- You state that results were averaged over 25 years after model spin up. In the 2xCO_2 case, were those 25 years all after the model had reached a new equilibrium after doubled CO_2?

Yes.

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

Yes, the results in Table 2 are from 20-year atmosphere-only simulations with present-day CO_2 concentrations, present-day aerosol emissions and prescribed SSTs and sea ice cover. The present-day reference year is 2008. A climatology for the years 2000-2015 of AMIP SSTs and sea ice cover was used. Greenhouse gas emissions are for the year 2008. Aerosol emissions are for the years 2003 to 2012 and after that year 2008 values were used.

- Were any fully-coupled model runs accomplished?

No.

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

We added the information in the main text.

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?

According to the study by Klocke et al. (2011), only the entrainment rate for shallow convection and the cloud mass flux above the level of neutral buoyancy noticeably influence climate sensitivity. While they did not change the conversion rate from cloud water to rain in stratiform clouds, they did that for convective clouds and found a negligible influence. Judging from our results in Figure 6, we do not expect a large influence. Testing this, would be a study on its own and is thus beyond the scope of the present study.

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.

As far as we know their clouds over the Southern Ocean consist of too much ice, i.e.

their negative cloud phase feedback is too strong. The global mean temperature in their CALIOP-SLF1 and CALIOP-SLF2 is 2-3 degrees lower than in their reference simulation. As climate feedbacks are state dependent, especially those related to ice, a colder reference climate will have implications for their estimates of ECS.

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.

Thanks for pointing this out. We now refer to Frey and Kay (2017) in this context and revised this paragraph in question.

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.

We changed that.

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?

We used this value just to indicate the magnitude of the CO2 forcing. We know mention that it is an average value from the IPCC AR4 models.

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).

We changed that.

- "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).

We changed that.

- 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).

Thanks for pointing this out. We now refer to Frey et al., 2017 for this.

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.

Thanks for the references, we added some.

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.

As mentioned above, we deleted the entire section to make the paper more concise.

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.

They are from the Lohmann (2002) study. We made that clearer.

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

Not relevant any longer as we deleted this simulation.

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.

We prefer to keep the description of the observations where they are introduced. A summary of the different observations can be found in Table 2 and Figure 1.

9. Page 9 Line 35: "Nl,oc,top reaches values of > 100 cm-3 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.

Thanks for spotting this. We corrected that.

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.

You are right. We corrected that.

11. Page 12 Line 31 and Page 13 Line 4: Please state which simulation is "the one

with the extreme changes in SLF."

We now explicitly refer to simulation ALL_LIQ.

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?

That is a good question. We do not have any simulations in which the net CRE is smaller so we have no way to answer this question. We would speculate similarly as we did for the last question of your major comments, that we do not anticipate a large sensitivity because our model is generally not that sensitive to model changes as seen in Figure 6.

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?

This would be possible but we find this visual comparison more appealing than adding 6 rows to Table 1. We prefer to keep this figure separately.

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

We corrected that.

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?

Yes, these are the changes in response to CO2 doubling from the years 26-50. We added this in the table caption.

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."

Thanks for pointing us to this paper, we now cite them when discussing our hypothesis.

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).

For consistency, we now show Figure 5 polewards of 40°.

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?

We did not diagnose any other feedbacks and because of this need to speculate about the compensating processes. The heat uptake by the ocean is only relevant for TCR because it vanishes in equilibrium as we discuss in the introduction.

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"?

We want to make the point that only mixed-phase clouds with tops between 0 and -35°C matter for climate sensitivity but arguing why all other cloud types do not matter. We will make that clearer.

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?

We added the details in all figures.

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

These typos have been corrected.

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

The panels are now labeled with a,b,c,etc. The results are from the atmosphere-only simulations for the present-day climate. We will add that.

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?

We added the details about the observations in Figure 1 and refer to Figure 1 for this. The majority of the observations is averaged over 2003-2012. Therefore, we also use the average over the years 2003-2012 from the atmospheric-only present-day simulations for this comparison.

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

As mentioned above, we added that.

Page 17 Line 22: The sentence beginning on this line is very long and hard to follow.

We rewrote this sentence and split it in two.

Page 20 Line 6: Is “this region” the subtropics?

Yes, we now say that explicitly.

References:

Klocke, D., R. Pincus, and J. Quaas (2011), On constraining estimates of climate sensitivity with present-day observations through model weighting, J. Clim., 24(23), 6092–6099, doi:10.1175/2011JCLI4193.1.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-97, 2018.