

## ***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. Specific Comments

1. Page 1, Line 13: should be "most frequently"

Changed

2. Page 1, Line 19: dominate what? Inter-member spread in ECS?

Other feedbacks dominate the overall cloud feedback. We specified that.

C1

3. Page 2, Line 1: "uncertainty" should be plural

Changed

4. Equation 1:  $\Delta H$  should be deleted. It is not a separate term in the global TOA energy budget, and is roughly equivalent to  $\Delta R$

We would argue that  $\Delta H$  matters for the global TOA energy budget. Before equation (2), we acknowledge that it vanishes when a new equilibrium is reached. We prefer to keep it as is.

5. Page 2, Line 9: suggest replacing "at the time of" with "in response to"

Changed

6. Introduction section: there are many references to the IPCC reports rather than to the original literature.

We now refer to the original papers by Dufresne and Bony (2008) and to Yokohata et al. (2010).

7. Page 2, Line 12: TCR is specifically defined for runs in which CO<sub>2</sub> is increased 1

We added that.

8. Page 2, Lines 5-20: I find it odd that there is no mention of the standard method for computing ECS in fully coupled AOGCMs used in CMIP5: that of Gregory et al. (2004).

We now mention the Gregory method.

9. Page 2, Lines 22-23: this line about reversing the sign of feedbacks is confusing and unnecessary

We deleted this sentence.

10. Page 2, Line 23-24: the residual term (which does not appear in any equation, so it is unclear what it refers to), I believe, also includes errors in the kernel method. Also,

C2

the citation is missing.

We added that the residual term also includes errors in the kernel method and added references to Soden et al. (2008) and Shell et al. (2008).

11. Page 2, Line 27: "not" should be "nor"

Corrected

12. Page 2, Line 31: "contributor" should be plural; "are" should be "is", or rephrase sentence appropriately

Corrected

13. Page 2, Line 33: "positive" is misspelled; seems like there is a missing word(s) after "with"

Corrected

14. Page 3, Lines 12-13: please provide references for this statement about precipitation efficiency

We added references.

15. Page 3, Line 21: should be "viewed with caution"

Corrected

16. Page 3, Line 28: strictly speaking, ECS and TCR are completely independent of aerosol forcing. Perhaps you mean observational estimates of ECS and TCR?

We deleted everything related to aerosol forcing to make the paper more precise.

17. Figure 1: this has no (a), (b) labels, though the panels are referred to as such in the caption

Labels have been added to Figure 1.

C3

18. Page 12, Line 15: "to initially to"

Corrected

19. Page 12, Line 30: reference to Loeb should be updated to the latest EBAF product (Loeb et al. 2017)

We added reference to Loeb et al. (2018).

20. Page 13, Line 27: "noticeable" should be "noticeably"

Corrected

21. Page 13, Line 29: "too high" - suggest restating as "too large" so as to not confuse magnitude of the peak with vertical location in the atmosphere

Corrected

22. Page 14, Line 20 - Page 15, line 3: rephrase/combine to remove redundancy

The redundant sentence has been removed.

23. Page 16, Line 1: what hypothesis? Is there are reference to a paper, or to something stated earlier?

The hypothesis is explained in the abstract. We added a more thorough description here and referenced two new studies on this topic (Frey and Kay, 2017 and Bodas-Salcedo, 2018).

24. Page 16, Line 2: "absorbed in clouds" - do you really mean this? Or not enough SW reflected back to space by clouds?

You are right, we meant not sufficient SW is reflected back to space, we changed that.

25. Page 16, Line 6: "proof" should be "prove"

Corrected

C4

26. Page 16, Line 10: I think "single column" refers to each sub-column, as in those generated by COSP; probably need to be more clear here, and refer to COSP (Bodas-Salcedo et al. 2011)

We added the reference to COSP-ISCCP and Bodas-Salcedo et al. (2011) and refer to the sub-columns of the simulator.

27. Figure 5: These all look very different to me, and suggest that there are major differences in mean-state clouds as a function of CTP and tau that may be as important or more important in driving feedback differences than can be gleaned from looking at Figs 1-3 and Table 2. I suggest showing the mean-state histograms too.

We added mean-state histograms.

28. Page 17, Lines 5: question mark before citation; also the original citations for this technique are Zelinka et al. (2012a, b), unless you are performing the decomposition separately for low clouds vs non-low clouds, in which case this citation is appropriate

We do not see a question mark....We changed the citation to Zelinka et al. 2012a,b because we don't look at the differences in low vs. non-low clouds.

29. Page 17, Line 21: suggest inserting "liquid" before "cloud droplets"

Liquid cloud droplets would be redundant because cloud droplets are always liquid. Therefore, we kept cloud droplets.

30. Page 20, Lines 3-6: it is not obvious to me that stronger entrainment drying should lead to decreased low cloud optical depth rather than low cloud coverage; also "thin-ner" should be "thin".

A decrease in cloud coverage will also lead to a decrease in cloud optical depth. We kept that but corrected "thin-ner" to "become thinner".

31. Page 21, Line 3: "optical" should be "optically" (twice)

C5

Corrected

32. Page 21, Lines 4-8: is the altitude feedback computed just for free tropospheric clouds as advocated in Zelinka et al (2016), or is it done for all clouds?

We used both methods (feedbacks calculated for all clouds and for low and non-low clouds separately) and found qualitatively similar results. For simplicity we use the feedbacks from all clouds but mention new that the decomposition by low and non-low clouds gives qualitatively similar results.

33. Page 21, Line 12: should "affected" be "compensated"?

We think that both adjectives would do, but compensated sounds more precise. We changed that.

34. Page 21, Lines 9-21: It is not clear whether this paragraph is mostly speculation, or if the authors have performed analysis to convince themselves but are not showing it to the reader. The notion that the latent heat of fusion provides additional buoyancy allowing clouds to penetrate higher has been shown to be incorrect, since the atmosphere adjusts its temperature profile to closely match the temperature profile of convection (Seeley Romps 2016). Moreover, when I look at Figure 5, I see huge CTP anomalies for thick clouds in ALL\_LIQ, which are not seen for the other three simulations shown. Is this because the other simulations have few clouds at these large tau values in the mean state, so there is no way of getting a strong altitude feedback, or is the upward shift truly different in the ALL\_LIQ case? The change in cloud fraction profile (Figure 7) looks roughly the same in all models, so my sense is that the larger altitude feedback in ALL\_LIQ comes from the fact that clouds are optically thicker (higher emissivity) in the mean-state, not something to do with how much the clouds rise in that simulation vs other simulations.

Thanks for your comments. You are right that ALL\_LIQ has more thicker clouds in the mean state (as also seen in the vertical profile of cloud liquid water in Figure 2). At

C6

the same time, the change in cloud liquid water profile in the warmer climate is very different in this simulation. This causes the altitude increase in optically thicker clouds as you suggested. We corrected our argumentation.

35. Page 21, line 35: "large increase in cloud top pressure" should be "large increase in cloud altitude feedback" I think.

You are right, we meant "cloud top pressure feedback". We added "feedback".

36. Page 22, first paragraph, also page 24, lines 17-18: I found this paragraph very hard to follow, and it seems like a lot of speculation to me (though not acknowledged as such). First, models run at GCM resolution typically cannot simulate convective aggregation, so it is doubtful that that is playing a role here. Second, Figure 9 (which also lacks a caption) does not really help elucidate the processes described. A zonal mean  $\Delta$ OLR figure would be a step in the right direction, so the different runs could be compared more easily. I am not aware of anything in Hartmann and Larson (2002) describing convective aggregation or a negative feedback from decreased high cloud coverage. I think the better citation is Bony et al. 2016, which relies on principles from Hartmann and Larson (2002).

On page 22, line 2 of the original manuscript we hypothesize why ECS is not changing, i.e. we acknowledged that we speculate, but we rewrote the paragraph to make that clearer.

In our version, OLR had a caption. Anyhow instead of showing maps of changes in OLR, we now show zonal mean changes of all radiative fluxes in order to provide a complete picture.

You are right that the clustering of convective clouds in the warmer climate is best described by Bony et al. 2016. We added that. We also now refer to an older study with ECHAM which showed a clustering of convection in a simulation in which cirrus scheme had an emissivity of one, i.e. where their infrared optical thickness was highest.

C7

37. Section 6: It is never described how ERFari+aci is computed, and with what type of experiments (fixed SSTs but modified aerosol loading, as in CMIP5?). Can the ari and aci components be separated to get better insights about direct and indirect effects?

As stated above, we removed the entire section on ERFari+aci to make the paper more concise.

38. Page 24, lines 1-9: In and of itself, a large present-day aerosol forcing does not guarantee large TCR or ECS. I think you need to insert words to clarify that "given the observed change in surface temperature and ocean heat uptake, a large present-day aerosol forcing. . ." Similarly, TCR as strictly defined does not depend on ERFari+aci; it is simply the temperature change at the time of doubling for a simulation with CO2 increasing at 1% per year. I think you mean "TCR inferred from observations".

As stated above, we removed the entire section on ERFari+aci to make the paper more concise.

39. Page 24, line 11: Suggest replacing "faster" with something clearer.

As stated above, we removed the entire section on ERFari+aci to make the paper more concise.

40. Page 24, line 12: it is not clear what "that" refers to, or if it is shown in the paper.

As stated above, we removed the entire section on ERFari+aci to make the paper more concise.

41. Page 24, line 32: "variable values" is a little awkward. Also "in contrary" should be "in contrast"

Changed to "calculated values". The spelling has been corrected.

42. Page 24, lines 6-7: Is this Southern Ocean bias really getting to the heart of the

C8

matter, or is it just another symptom of the fundamental issue? My understanding is that SW reflection by clouds in models whose clouds are too optically thin (e.g., due to having too low SLF) is overly sensitive to phase changes because phase changes in these models can actually have a non-negligible effect on cloud optical depth. In contrast, phase changes in models whose mean-state clouds are not too optically thin have a negligible effect on cloud optical depth. Is this correct, and if not, could this important point be stated more clearly in the paper? It would be nice if this could be shown more unambiguously in the paper. Is it demonstrated anywhere that the models with larger SLFs actually have LWPs that put them in the range of optical depths where albedo is saturated and hence insensitive to phase changes?

Yes, you are right that this problem is not limited to clouds over the Southern Ocean, but there it is most obvious. Albedo only saturates when optical depth exceeds 500 (Kokhanovsky et al., ACP, 2007). Thus, it is not so much the actual saturation than it is the sensitivity of albedo to changes in optical depth that becomes much less at high optical depths.

43. Page 24, lines 8 and 10: "absorbed in clouds" – I don't think this is what you mean

Corrected

44. Page 24, line 32: as far as I can tell, Forster et al (2013) only plots ECS against the total radiative forcing, not ERFari+aci.

As stated above, we removed the entire section on ERFari+aci to make the paper more concise.

45. General comment: It seems that there should be some mention of the various studies finding observational support for a model overestimate of the cloud optical depth feedback at middle latitudes (Gordon Klein 2014, Ceppi et al. 2016, Terai et al. 2016). Currently the Tan et al study is cited alone but it is really one among several studies that are suggesting a bias, one that likely implicates the too-strong phase feedback.

C9

Agreed. We added more references.

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

C10