Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-97-RC2, 2018

Atmospheric Chemistry and Physics Discussions
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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

Anonymous Referee #2

Received and published: 7 March 2018

Review: The importance of mixed-phase clouds for climate sensitivity in the global aerosol-climate model ECHAM6-HAM2 by Ulrike Lohmann and David Neubauer
Manuscript Number acp-2018-97

In this study, the authors investigate the impact of mixed phase clouds on equilibrium climate sensitivity (ECS) and effective radiative forcing due to aerosol-radiation interactions plus aerosol-cloud interactions (ERFari+aci). Motivated by Tan et al (2016), who found that modified versions of CAM5 with higher supercooled liquid fraction (SLF) exhibited higher ECS values owing to increasingly weaker negative / stronger positive cloud optical depth feedbacks, the authors assess the robustness of this result in

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Discussion paper

similar experiments with the ECHAM6-HAM2 model. The impact of cloud phase on ECS is apparent only in comparing the reference experiment to an 'extreme' simulation with ice at all temperatures below freezing (in which case the optical depth feedback is large and negative, and the ECS is small). Experiments with progressively higher SLF show small differences in ECS, and what little change there is, is driven primarily by differences in cloud altitude feedbacks and non-cloud feedbacks. ERFari+aci values are largely insensitive to the various model formulations, but are larger in magnitude in simulations with smaller minimum cloud droplet numbers and with smaller biomass burning emissions, mimicking the effect that clouds are more susceptible to aerosol perturbations in more pristine environments.

Overall, I found the paper to be an interesting and worthwhile contribution to the literature. I have scientific concerns in a few places, and there are issues with English, so I recommend publication pending revisions.

Specific Comments

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

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

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

4. Equation 1: ΔH should be deleted. It is not a separate term in the global TOA energy budget, and is roughly equivalent to ΔR

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

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

7. Page 2, Line 12: TCR is specifically defined for runs in which CO2 is increased 1% per year.

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).

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

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, the citation is missing.

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

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

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

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

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

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?

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

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

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

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

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

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

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

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

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

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)

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.

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

29. Page 17, Line 21: suggest inserting "liquid" before "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 "thinnen" should be "thin".

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

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?

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

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. It 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.

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

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 ΔOLR 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).

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?

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".

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

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

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

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

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

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.

45. General comment: It seems that there should be some mention of the various stud-

ies 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.

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