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Geoscientific Model Development Discussions
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Interactive comment on "The global aerosol-climate model ECHAM6.3-HAM2.3 – Part 2: Cloud evaluation, aerosol radiative forcing and climate sensitivity" *by* David Neubauer et al.

Anonymous Referee #1

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Neubauer et al. evaluate important aspects of a new version of a three-dimensional global aerosol-atmosphere model. It is well understood that cloud condensation nuclei concentrations vary significantly between strongly and less strongly polluted conditions and that radiative forcing by cloud-aerosol interactions depends non-linearly on cloud condensation nuclei concentration. At a time when box-model studies of cloud-aerosol interactions inform energy budget box-model studies, I find this study by Neubauer et al. a truly laudable effort. My only major concern with this study is that even in the latest model version, the minimum CDNC number is still artificially set to 40 per cubic centimetre. The authors discuss this point in some detail. They show that ER-Fari+aci and ECS depend on this threshold, but they do not mention this result in the

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Discussion paper

abstract. Although one can certainly argue about this point, I find the reasons for applying this particular threshold unconvincing. Nevertheless, I consider this a very worthwhile study. Unlike some other studies, this study includes a sensitivity run (the E63H23-10CC run) that helps to assess a key uncertainty (partially re-iterating a point made in an earlier study). It also includes another useful sensitivity run (E63H23-LL) that helps to attribute differences between model versions to an individual change in a parameterization. In my opinion, after some revisions, this study clearly deserves to be published in GMD.

Specific comments and suggestions:

1. The fact that the CDNC threshold which leads to a lower ERFari+aci and a lower ECS is still applied should in my opinion definitely be mentioned in the abstract. As is shown in the manuscript, reducing this threshold results in a considerably larger ERFari+aci (first noted in Hoose et al., 2009) and interestingly also a larger ECS. It is not clear to me by how much an improved aerosol would reduce this larger ERFari+aci.

2. The weaker shortwave ERFari+aci in E63H23 is attributed to the new aerosol activation scheme and sea salt emission parameterization in E63H23 and a more realistic simulation of cloud water. Would it be possible to quantify individual contributions e.g. based on the E63H23-LL sensitivity study from Fig. S5 and perhaps also a run from Tegen et al. (2019)? Or would this require additional sensitivity runs?

3. It is concluded (p. 1, l. 27f) that "[t]he decrease in ECS in E63H23 (2.5 K) compared to E61H22 (2.8 K) is due to changes in the entrainment rate for shallow convection (affecting the cloud amount feedback) and a stronger cloud phase feedback". As far as I can see, especially the conclusion regarding a "stronger cloud phase feedback" (see also p. 24, l. 3) does not seem to be supported by sufficient evidence. Please either explain the existing evidence better, present additional evidence, or else please either preferably remove the statement or at least re-formulate the statement to reflect that this is not a finding but a speculation.

4. Cloud properties and cloud radiative effects are not only simulated by ECHAM-HAM but also by ECHAM. I wonder whether it would make sense to include ECHAM results in these comparisons. In my opinion, including ECHAM would in general help to better understand which biases in ECHAM-HAM are inherited and which biases are newly introduced by using e.g. different tunings and a different microphysics scheme. Including ECHAM results would also help to understand which differences between versions are due to changes in ECHAM and which differences between versions are due to changes in components that are specific to ECHAM-HAM. The discussion explaining the results frequently refers to changes in ECHAM, and some parts of it might be easier to follow if these changes were shown in tables 2 and 3 and especially also in the plots. On the other hand, the comparison of different ECHAM-HAM versions is useful without an additional focus on attributing the changes to changes in either standard ECHAM or in components that are specific to ECHAM-HAM, and including to many plots would also distract the reader from this comparison. Nevertheless, I think ECHAM plots would potentially be a nice-to-have. In case the authors decide against including ECHAM results, I would recommend to refer even more frequently to the literature documenting these results, especially when common biases are discussed. In some important cases (e.g. p. 20, l. 26), the references are already included.

Other specific comments and suggestions:

p. 1, l. 18ff "Biases that were identified in E63H23 (and in previous model versions) are a too low cloud amount in stratocumulus regions, deep convective clouds in the Atlantic and Pacific oceans form too close to the continents and there are indications that ICNCs are overestimated": I think that already here it would be good to clearly differentiate between biases that are inherited from ECHAM, biases that are specific to ECHAM-HAM in all HAM versions, and biases that change by the HAM modifications. Also, I think it would be good to clarify which biases were newly identified in this study and which biases are well-known and long-standing biases. Perhaps this can be achieved almost without lengthening the abstract. The resulting sentences could for example read: "Common biases in ECHAM and in ECHAM-HAM are ... . ICNCs are overestimated in ....". If the authors decide against including ECHAM plots (discussed in comment #4 above), as far as ECHAM biases are documented elsewhere, it would be sufficient to point to the corresponding literature somewhere in the text.

p. 1, l. 19f, p. 17, l. 17f, p. 23, l. 12f: Based on Figs., 3, 6, and 8, I am not completely sure what is meant by "deep convective clouds in the Atlantic and Pacific oceans form too close to the continents". Is this something that one sees when putting ECHAM results next to ECHAM-HAM results? For example, there seems to be little deep convection over Indonesia in ECHAM and in ECHAM-HAM. Focusing on Indonesia (I think), Mauritsen et al., 2012 (https://doi.org/10.1029/2012MS000154) note that "[a]n interesting and challenging issue in MPI-ESM is the Tropical precipitation distribution over land versus ocean. The model prefers precipitating in the ocean, whereas observations indicate a stronger preference to precipitate on land."

p 1, l. 19f: in the Atlantic and Pacific oceans -> over the Atlantic and Pacific oceans

p. 2, l. 4: resulting in -> substantially contributing

p. 2, l. 5: realistic representation -> increasingly realistic representation (Almost certainly some of the dynamic responses to increased aerosol take place on scales which are too small to resolve by present-day global climate models. This remains a major concern as several studies point out that this potentially leads to an overestimate of ERFaci in coarse-resolution models.)

p. 4, l. 5: how does evaporation and sublimation affect aerosol number concentration?

p. 4, l. 28: I don't understand how CDNC from convective clouds is determined. Please try to explain this better.

p. 6, l. 9: please indicate that SALSA2.0 is not used here to avoid confusion.

p. 6, l. 18: please refer to my comment regarding p. 4, l. 28.

p. 7, l. 8: could you please be slightly more specific?

p. 8, l. 19: climate system -> atmosphere (if the entire climate system including the ocean were considered, 20 years would be insufficient. Using identical fixed SSTs strongly reduces the influence of internal variability.)

p. 11, l. 31: over -> cover

p. 13, l. 7f: The COSP CALIPSO simulator was used here, right? Excluding areas where the cloud products differ by more than five percentage points looks like a good idea to me. The excluded regions are regions in which one might expect problems.

p. 14, l. 8f: please refer to my comment regarding p. 4, l. 28.

p. 15, l. 32ff: In my opinion this entire discussion would be more interesting if standard ECHAM was included in the comparison.

p. 16, l. 27: where -> which

p. 16, l. 11f: the authors could mention that such a bias is also found in other models (https://doi.org/10.1029/2012GL053421).

p. 17, l. 6: RMS -> RMS error

p. 18, l. 22: does the statement "due to a smaller gamma_r" on p. 18, l. 22 indicate that the E63H23-10CC has been retuned? If yes, is this retuning expected to affect ECS? Based on https://doi.org/10.1175/JCLI-D-16-0151.1, I would have perhaps thought brighter clouds in the base state to play a different role for ECS. How about ERFari+aci? I wonder whether the result of Lohmann and Ferrachat (2010) holds also in E63H23.

p. 19, l. 26: why "...(Schmidt et al., 2006; Hansen et al., 2005) or Fan et al. (2004)"?

p. 21, lines 4 to 6.: is there a reference for this?

p. 22, l. 22: did ECS also decrease in standard ECHAM?

p. 23, l. 12: please refer to my comment regarding p. 1, l. 19f above.

p. 23, l. 21ff: please refer to my comment #2 above.

p. 24, l. 4: please refer to my comment #3 above.

Table 1: what exactly is gamma_r?

Table 3: from E61H22 to E63H23 there is a large compensation of changes in SW and LW ERFari+aci. This has also been noted elsewhere (https://doi.org/10.1002/2016GL071975). Are you really sure that here it is mainly due to the removal of a bug?

Fig. 9 is too small. It can't be enlarged because the resolution is too low. Please increase the size and the resolution.

Fig. 10: I do not understand the rationale behind excluding areas with little precipitation. While relative errors tend to be large in these areas, absolute errors in these areas tend to be small. Excluding these areas in the bias calculation could in principle hide model deficiencies. Observations of small values may contain important information due to a small absolute error, even where the relative error is large. Using standard deviations already ensures that large relative errors in regions with small values and small absolute errors will not have an overly large influence on the comparison.

Fig. 11: please increase the font size of the variable names (right plot title) and the color bar labels.

Fig. 14: Amt is amount, right? Please state this somewhere.

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