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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 #2

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General comments

The authors present an evaluation of clouds and precipitation simulated in three models ECHAM6.3-HAM2.3, ECHAM6.1-HAM2.2 and ECHAM5.5- HAM2.0 in comparison to global observational datasets. They discuss the performance of each model and the reasons for improvements.

The purpose of the paper is to provide a model documentation in a nutshell and to characterize the quality of clouds and precipitation simulated in the lates model E63H23 as well as in earlier versions. This is valuable information for all users and developers of these models and thus fits well to the scope of GMD. Overall the paper is clearly

structured and well written. Figures are of good quality.

My main recommendation is to add discussion to the obvious deviation between observed and modeled IWP near the equator and also in the NH storm tracks. Otherwise only minor modifications and corrections are necessary, so that a minor revision seems sufficient before publication.

Specific comments

Abstract, L14: low cloud amount → amount of low clouds (?)

P2 L1: "... Also, the spatial structure of multiple clouds shows a large variability on different scales as it depends not only on large scale motions of the air but also on convective and turbulent motions at different scales. ... " The important point is that the strong diabatic cooling/heating occurring with phase changes of water vapor causes a tight coupling between clouds and circulation, which is much less the case for other constituents.

P4 L12: orographic cirrus cloud → orographic cirrus clouds (2x)

P5-7 Section "2.1.5 Changes and improvements in E63H23" The preceding section lists already some process models and indicates cases where these are optional but not used in this study. In section 2.1.5 it is not pointed out that SALSA is available but – to my understanding – not used in this study. Is is also the case for other process models, for which improvements are listed here?

P7 L20: use the year → used the year

P7 L30: ... use a climatology for monthly values of sea surface temperature (SST) and sea ice cover (SIC) ... This setup excludes the influences of El Niño/La Niña on the variability of the atmospheric circulation. Thus the simulated "internal climate variability" is reduced compared to simulations which included El Niño/La Niña. This should be made clear to the reader. This reduction of variability is relevant for the later evaluation, see your comments on P8L18: "... to increase the signal of ERFari+aci compared to variations in TOA net radiative flux due to internal variability of the climate system. . . . "

P8 L1: . . . the default configuration of these model versions . . . → . . . the default configuration of these ECHAM-HAM model versions . . . ECHAM6 on its own uses 47 levels (Stevens et al., 2013). P9 L17: . . . are described in Mauritsen et al. (2012) . . . The recent publication Mauritsen et al. (2019) provides more information for the tuning of ECHAM6.3: Mauritsen, T., Bader, J., Becker, T., Behrens, J., Bittner, M., Brokopf, R., et al. (2019). Developments in the MPI-M Earth System Model version 1.2 (MPI-ESM1.2) and its response to increasing CO2. Journal of Advances in Modeling Earth Systems, 11. https:// doi.org/10.1029/2018MS001400

P10 L26: . . . Table 3 . . . compared to observations (OBS) or multi-model mean values (MMM) when observations are not available . . . It would be helpful to mark each entry in the OBS/MMM column whether it is OBS or MMM.

P11 L32: cloud over → cloud cover

P12 L12: . . . The underestimation is particularly large in the tropics. . . . Here the authors should add explanations. What is the reason for this major deviation? Microphysics of deep convection? Cirrus cloud processes? As the authors have expertise in this field they should discuss this obvious modeling problem.

P13 L12: . . . Over land in the Northern Hemisphere the models overestimate cloud cover . . . This seems not correct for the dry continental regions: Sahara, Australia, . . ., even when accounting for less observational certainty in the Sahara region. Here the models underestimate the cloud cover.

P14 L12: . . . and in all model version shallow convection is triggered frequently . . . What is the role of the markedly increased shallow convection entrainment rate used in E63H23?

P14 L28: . . . The regional distribution of IWP of all three model versions agrees in

general quite well with the observations. . . . I do not agree with this general statement. The general disagreement in the equatorial region and also in the northern storm tracks is clear. Here, or earlier in the presentation of the zonal mean results, there is really the need to address this problem. I do not expect that a full explanations can be given – otherwise the modeling problem could be solved – but the authors should comment on this challenge and provide their insight in the possible reasons. This would make the discussion of the IWP much more interesting.

P15 L28: . . . the areas and magnitude of precipitation differs . . . → . . . the areas and magnitude of precipitation differ . . .

P17 L4: . . . the root-mean-square (RMS) error . . . → . . . the root-mean-square error (RMSE) . . . RMSE seems more useful for the following usage than RMS.

P20 L9: . . . The equilibrium climate sensitivity (ECS) is strongest in E55H20 (3.5 K), weaker in E61H22 (2.8 K) and weakest in E63H23 (2.5 K) (Fig. 13). . . . Here it would be valuable to have explained also the ECS values estimated for the base atmospheric models (ECHAM5, ECHAM6.1 and ECHAM6.3), as discussed in the literature. This would provide a better background for the discussion of the ECS estimates from the ECHAM-HAM models presented here. The recent Mauritsen et al (2019) paper also provides more information to the sensitivity of the ECS to certain model modifications.

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