



Interactive comment on “Evaluation and climate sensitivity of the PlaSim v.17 Earth System Model coupled with ocean model components of different complexity” by Michela Angeloni et al.

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Received and published: 4 April 2021

We would like to thank reviewer #1 for his/her constructive comments which will help us to improve the paper and we would like to make some clarifications. Actually, the aim of this paper is not to perform an atmospheric tuning of PlaSim (which, as the reviewer points out and as we report in the paper, has already been the subject of an earlier study) but to focus on oceanic parameters in the currently available coupled versions of the original model. The paper uses the best configurations to explore equilibrium climate sensitivity in PlaSim and to discuss the most relevant feedbacks. These are results which, to our knowledge, have not been reported for coupled configurations of

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PlaSim so far. For the mixed-layer ocean we focused indeed on the horizontal diffusion coefficient, as pointed out by the reviewer, since this is the main parameter controlling meridional heat transport. The range of different horizontal diffusion coefficients tested, from 10^3 to 10^6 m^2/s , was found to be sufficient to obtain a significantly improved model climatology. We discuss and explain our decision to implement a differential diffusion coefficient between the two hemispheres in the text. Actually, we did not test only three profiles of the vertical diffusion in the Large-Scale Geostrophic ocean as suggested by the reviewer: we show the most relevant profiles in the manuscript but we explored a wide range of profiles, with different diffusivities both at the surface and at the bottom of the ocean, as stated at page 8 line 11. The overall climatology of PlaSim has already been explored in the past. The purpose of the sections reporting model climatology and energy balances is to document the main properties of the climatology obtained in the new coupled model configurations after changing the oceanic parameters, but not to repeat an in-depth analysis of the model climatology and we decided to make a compromise in terms of complexity for these sections. We thank very much the reviewer for suggesting additional literature to be included in the discussion of the ECS experiments and for the useful detailed suggestions/typo corrections which we will implement. Following the reviewer's comments we have come to the conclusion that our paper may not fit well to the scope of GMD. For this reason, while we will use the suggestions by reviewer #1 to improve our manuscript (and we thank the reviewer for the time spent), we prefer to withdraw this submission from GMD.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-245>, 2020.

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