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Geoscientific Model Development Discussions

Interactive comment on "PLASIM-GENIE: a new intermediate complexity AOGCM" *by* P. B. Holden et al.

Anonymous Referee #2

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General comments

The manuscript describes a new coupled atmosphere-ocean model in a rather compact manner focusing on the parameter tuning. The model should be very useful for the community and would promise contribution to the scientific advancement in this field. This aspect is enhanced particularly by the release of the model. The description of such model with potentially broad application is useful and deserves to be published in the GMD. The current description is, however, unsatisfactory in its current form for the reasons listed below.

Major comments

1. Quantitative (and physical, in some cases) meaning of some variables discussed in

the manuscript is unclear for the non-GENIE users unless the reader consults with the multiple previous papers. I do not expect that all variables are explained in details, but the highlighted variables such as "acllwr", "albseamax", "qthresh" "ADRAG", and "SCF" need to be expressed with equations. Otherwise, the discussion on the presented values does not mean much for many potential readers. The term "sea ice diffusivity" is also unclear although I would imagine this represents the diffusive effect of unresolved ocean currents (and other dynamical effect) on the sea ice concentration.

2. If the APM is a flux-correction parameter, should this depend on the flow? If the model simulates the moisture flux correctly, the addition of this flux correction would lead to a wrong total moisture flux. This is no longer a correction. By design, the flow responds to this parameter "forcing". I am not sure about the physical meaning of this parameter which appears to be an important tuning exercise here.

3. I wonder why the performance of only thermohaline circulation is discussed. In the introduction, it is mentioned that "dynamic ocean feedbacks are restricted to the thermohaline circulation" in the previous EMBM coupled version of the model. Then, the simulated wind-driven circulation and its interaction with the thermohaline circulation must be one of the selling points of the new model. Why not discussing the wind driven circulation (and its bias)? Similarly, the sea ice plays an important role for the thermohaline circulation. Why not discussing the sea ice distribution (and its bias)?

4. Throughout the manuscript, it is stated that the new model is substantially improved from the GENIE-1 (e.g., p.10693, l.15). I think it is very useful to show with figures which part of the simulated climatology is improved.

5. In Figs. 2-4, the model bias, i.e., the difference from the NCEP/NCAR reanalysis should be presented. The model error bar is unknown, otherwise.

Minor comments

1. p.10680, l,22: "that that" should be "that"?

2. p.10681, l.15: “transport” should be “transfer”?

3. p.10681, l.26: Please explain “self-shading”.

4. p.10687, l.15: Would it be more helpful to plot the equation using the revised values of 0.5 and 0.7?

5. p.10692, l.15-16: The years of the NCEP/NCAR reanalysis data need to be stated.

6. p.10692, l.16: Which variables are selected? Are these in Table 1? Then, writing “selected variables (Table 1)” is more helpful.

7. Even if the tuned parameters are mostly ocean model parameters, the description of atmospheric field/circulation and its bias is useful as a description paper of the new coupled model.

Interactive comment on Geosci. Model Dev. Discuss., 8, 10677, 2015.