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Geoscientific Model Development Discussions

Interactive comment on "GENIE-M: a new and improved GENIE-1 developed in Minnesota" *by* K. Matsumoto et al.

Anonymous Referee #1

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The manuscript by Matsumoto et al. describes a procedure for and results of a new phase of model development on an intermediate complexity Earth System Model, “GENIE”. The authors make a number of structural and parameter changes to an earlier version of the model in order to improve its performance with regard to physical and biological tracers. The study makes a significant contribution to expanding the applicability of such models (EMICS) to a wide range of biogeochemical problems, and the paper is generally well written. There are two things that were not wholly satisfying about the study however, and I focus my comments on these with the idea that some revisions might improve the authors’ valuable contribution.

First, a large part of the paper is devoted to tuning the model’s physical parameterizations to maximize the consistency with observations. The data metrics chosen for comparison are ocean temperature and salinity and atmospheric temperature and humidity. A fairly sophisticated procedure is used to sample the model’s parameter space and to choose the best parameter values. This phase of objective tuning is found to give only small improvements to the above metrics, but it reduces consistency with some transient tracer data (CFCs, anthropogenic CO2). So it is followed by a “targeted tuning” of certain parameters to increase model fidelity to these latter measures.

My primary concern with this part of the paper is that it is unclear why 1) the authors did not make structural changes (e.g. variable Kv) before undertaking objective tuning, and 2) the authors did not include transient tracer metrics in the objective phase of tuning, rather than use them to guide adjustments afterwards. As it stands, the objective tuning procedure appears to be undermined by those subsequent alterations (of Kv and FWF). These changes move the model into a distinctly different region of its error space (Table 2), where the previous parameter values will no longer be locally optimal. This is probably not as important as it sounds: the initial objective tuning did little to improve the model/data fit to begin with (Table 2; GENIE-1 versus Control), so I suspect that the tuned parameters are still quite good. My comment is therefore not meant to cast doubt on the end result, but rather on the manuscript’s portrayal of how the end result was achieved. It seems to me that the significant improvements in model skill are due primarily to the expert judgment exercised by the authors, and not to the brute-force objective tuning procedure. Unfortunately the objective tuning sections of the paper make it much more difficult to read, and yet (if my argument above is correct) do not contribute anything to the final result. In my opinion, the paper would be better if it did not place so much emphasis on the objective tuning, and instead discussed the more ad hoc changes described in Table 2. For instance, can anything be learned from the model’s lack of sensitivity to certain variables?

Second, the paper uses the best physical configuration to examine biogeochemical parameterizations. Formulations of organic matter production and remineralization mod-

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ified from OCMIP and previous work by Matsumoto contain several free parameters, and two of these were tuned to match oxygen distributions and export flux. In comparison to the physical tuning, this section of the paper strikes me as quite incomplete. The authors examine two data metrics and two model parameters, but other possibilities are not discussed. For instance, they choose O2 as a target field for calibration, which is quite reasonable. But the choice of POC export seems odd, since this is not a directly observed quantity and has a considerable uncertainty itself. It would seem that NO3 should provide a powerful constraint, but the manuscript states that it is “not as sensitive to change”, without elaboration. (As an aside, Table 2 suggests that the same could be said for Temperature!) It is also unclear why certain model parameters are not investigated. For instance, the fraction of production that is allocated to dissolved OM is usually found to have a significant influence on O2 and export, but this is not examined (nor is the value used stated). In my opinion, this section of the paper is too incomplete to usefully document model performance and sensitivity to biogeochemical tracers.

In conclusion, the new version of GENIE developed by Matsumoto and colleagues makes significant advances in its ability to capture known metrics of model ventilation and physical. The paper emphasizes the use of an objective tuning procedure, but the primary improvements are achieved through judicious changes introduced by the authors. The testing of the model’s biogeochemical is far less thorough, and is unlikely to be so satisfying for readers trying to evaluate the reliability of future scientific results from this model.

Specific Comments

4,10: On latitudinal grid spacing, a more concrete statement would be useful, perhaps “…uniform in sine of latitude, varying from x degrees at the equator to y degrees at the pole”.

4,12: “frictional geostrophic equations” is jargon that is not widely used, some clarification would be useful (simply stating which terms are retained/modified in the momentum equations).

5,8: partitioning of uptake into sinking/dissolved OM should be spelled out here for those who don’t know OCMIP. Also, are the parameter values the same as OCMIP (kappa=0.5yr, sigma=0.67)? Please provide.

6,6: Model estimate of anthCO2 would be more appropriately compared to the Sabine estimate before they added marginal seas (I think the number was 106), since these are not represented in coarse res. Models like Genie.

6,16: “since” and “subsequent” are redundant in this context.

pgs 13-15: The description of the methodology is quite dense and filled with jargon, some of which seemed unnecessary (e.g. is “Pareto optimal” different than simple minimization of the sum of RMS error across multiple data fields? If not, why introduce economics jargon for a concept familiar to earth science? If so, why not describe the difference?

8,16: As written, first sentence sounds like Atl-Pac FW flux is also being influenced by ssmax. Perhaps you could delay mentioning the MOC issue until the next paragraph where it is addressed, to avoid confusion.

9,17: What is B? The text mentions “biomass turnover”, but it’s unclear what that means? (The units of B would seem to be biomass itself [mol/vol], rather than a turnover rate).

10,22: Without more details on N cycle, not worth including.

13,15: theat → that

Fig 6,7: Quantities being plotted are not clear. Is O2 averaged over the top 1000m (fig 7)? What is the “O2 error”, is it a global RMS value?

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