

Interactive comment on “Comparing transient, accelerated, and equilibrium simulations of the last 30 000 years with the GENIE-1 model” by D. J. Lunt et al.

D. J. Lunt et al.

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We hope that you will be able to consider our revised manuscript for publication in Climate of the Past.

We have made several changes to the original manuscript in this revised version, in response to the reviewers' comments. Here is a point-by-point list of the changes, following the specific comments of the two reviewers.

Reviewer 1

1. We have added discussion and figures showing sea ice evolution and comparison

- with HadSM3, and the evolution and mean state of the MOC.
2. We have made the point in the text that the d18O record is affected by both ice volume and temperature. We explain that we are using the d18O record not to reconstruct ice volume (this comes from the Peltier reconstruction), but to introduce sub-millennial variability. By using the d18O we are probably therefore overestimating the variability. In the context of this paper, that is OK because we find that the system is remarkably close to equilibrium - less variability in the boundary conditions would in fact reinforce this result.
 3. We have included a plot and discussion of the pre-industrial climate (temp+seaiice), along with that of HadSM3. We find that the comparison is reasonable, including seaiice (which is, however, under-simulated in terms of fractional coverage). We have also made it clear which parameter set we are using.
 4. We have described ENTS, and explained that a preliminary 'ad-hoc' tuning was carried out to arrive at the 'Default' parameter set. We have also expanded the description of the vegetation model somewhat, and clarified what is meant by 'prescribed' vegetation, and added a table with many of the model parameters.
 5. We have explained why we haven't changed the land-sea mask - this would introduce spurious rapid changes in the boundary conditions when gridboxes flipped between land and ocean (due to the fact that GENIE-1 has a binary land-sea mask). We make the comment that any model with a binary land-sea mask would suffer from this problem.
 6. We have shown the seaiice for the LGM, and compared it to HadSM3 - for the Holocene it was not very instructive as it's so similar to modern.
 7. There are in fact 2 RMS values (they are differences of differences). We have specified them both.

8. Changed 'good job' to 'reasonable'. The 'local' sentence was a typo which has been cleared up.
9. We have taken out the discussion of spin-up time, in response to points made by reviewer 2.
10. We have commented on the linearity of the system, and also shown in addition that fact that other parts of the system (e.g. the MOC) is not so linear. We have not included snapshots of temperature profiles as this didn't reveal anything of real interest, and would increase the length of the paper too much, in our opinion.
11. We have shown the modelled sea ice at the LGM and pre-industrial and they are in reasonable agreement with HadSM3. We haven't included any more plots of temperature at depth, again for space reasons.
12. We show a plot of the evolution of the MOC with different accelerations. This reveals that there are no fundamental shifts in the state of the THC over the course of the 30,000 years. However, the response to the boundary conditions is very non-linear.
13. We have explained the provenance of the 'Default' parameter set, and in addition re-run all the simulations with another tuned parameter set.
14. we have improved labeling of figures.
15. we have changed the colour scales in most of our regional plots, to have less intervals, and also have overlaid isolines where appropriate. We have increased the size of the plots slightly; as far as i can tell they are now as big as possible given two plots per page width (quadrupling the area of all the regional plots would result in a MUCH longer paper!).
16. Minor comments - all addressed.

Reviewer 2

1. We have left in the detailed description of how we made the boundary conditions - we think this is important if others want to carry out similar simulations with a different model.
2. see reply to reviewer 1, comment no. 2.
3. We have added a reference for the mixing in GENIE-1, as well as a table showing the mixing parameters for the different parameter sets.
4. We comment that although we found no evidence of bistability, it is likely that bistability does exist in certain regions of parameter space, and for certain initial conditions. By initialising the equilibrium simulations with an isothermal ocean, it is possible that we have preconditioned the ocean into a certain state. We suggest that possible future work could include initialising the transient simulations with 'THC-off' ocean states, to see how this influences the results.
5. We have re-run a sensitivity test with the Hargreaves et al parameter set.
6. We have taken out this discussion of spin-up times. We agree that it is highly dependent on the arbitrary nature of the spin-up criterion.
7. We have added a comparison with HadSM3 of the LGM mean-state sea ice distribution - this gives some confidence that the sea ice is being treated reasonably, as well as testing the sensitivity to resolution. However, as we show, Antarctic bottom water is not well represented in GENIE-1.
8. Made ice volumes into sensible units
9. corrected the labeling.

In addition to the above changes, we have made a number of minor stylistic changes, and slightly modified the abstract and acknowledgments. We have added Robert Marsh as a co-author - he helped set up the new sensitivity studies and helped with the discussion relating the results to other EMICS and GCMs.

Many thanks,
Daniel Lunt

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