Geoscientific

Model Development

Discussions

[GMDD]

Interactive

comment

Interactive comment on "A near-global eddy-resolving OGCM for climate studies" *by* X. Zhang et al.

Anonymous Referee #1

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Summary of key results

The latest version of the Ocean Forecasting Australia Model (OFAM3), a near-global high-resolution (0.1°) ocean model is described, based on GFDL MOM v4p1d. Its grid extends from 75°S to 75°N. There is no sea ice, but the model includes the WOMBAT biogeochemical model. The model is forced by the JRA55 climatological surface fields. It is first run for twenty years with a repeat of the 1979 surface climatology to generate a three-dimensional restoration flux, based on that needed to maintain the model fields close to climatology in the first two decades. This flux is then applied to the ocean model to reduce drift during the subsequent integration with interannual forcing up to 2015, but allows the climate signal implied in the forcing to be reproduced.

General style

The paper is overall quite well written and structured, but it is occasionally evident that the text is for the most part written by someone whose first language is not English, so the manuscript would be benefit from further proofreading to check grammar and usage.

Specific corrections

P2, L30: Any list of recent eddy-resolving ocean configurations should include a reference to the latest UK 1/12° NEMO: Marzocchi et al, 2015: Journal of Marine Systems, 142. P4, L19: This is a common, but incorrect, use of the term "Boussinesq approximation". The latter, as (for example!) defined in Gill, 1982, refers to the neglect of density differences except where these imply pressure difference. This is not equivalent to ensuring constant volume.

General comments

The reasons for some of the choices made in creating the OFAM model configuration are puzzling to me, and need to be explained clearly in the paper. The development of a global ocean-only model whose grid reaches 75°S (most of the Antarctic coastline) and has biology but no ice model, and which excludes most of the Arctic, leads to potentially severe limitations on the general applicability of the model to climate studies, and needs to be clearly justified. Such a model may well be useful for understanding recent climatic variability in the midlatitudes and tropics, providing that enough confidence exists in the forcing dataset used (which is not the case before the satellite era), but cannot inform about the interactions between the high and mid latitudes, and certainly can say nothing about the climatically highly important polar regions. For this reason I believe it will have limited interest for the climate science community.

Would it not have been more logically consistent to develop the model from the start as a coupled system, including a fully global ocean? A non-global ocean model cannot directly be reframed as a free-running coupled climate model. The Discussion and Summary Section mentions the intention of re-running the model with the merged out-

put of an ensemble of CMIP5 coupled models, instead of the JRA55 forcing dataset. This is, of course, perfectly feasible, but would have the severe disadvantage of omitting the interannual and decadal climate variability, which would be averaged out in the ensemble mean.

The method of applying the fluxes equivalent to the initial drift of the model is evidently effective in this case in reducing drift on decadal timescales, and is an interesting way to address the thorny question of spinup in climate models. This has of course possible drawbacks, however: specifically, an implicit assumption that the drift is due to model deficiencies, rather than to errors in the applied surface fields or in the way the lateral boundary conditions are applied. In particular, the need for strong restoration of temperature and salinity at the northern boundary has serious implications for the performance of an eddy resolving ocean model, since there is an abrupt mismatch between the resolutions of the model grid and of the forcing data: the consequences for the representation of boundary currents, which are vital for the exchange between the Arctic and the rest of the oceans, are not clear, and should be discussed.

I have difficulty with the recommendation at the end of the paper, namely that the climate modelling community "can consider adopting the approach described in this study as an efficient short-term solution, at the same time also develop more sophisticated methods to address this important problem of model drift". This method was indeed often used in the coupled climate models used at the end of the last century, and was called "flux correction", though this was mainly (but not exclusively) restricted to surface fluxes. By around 2000, though, climate models had improved to a state where they had realistic enough surface fields that they were able to be integrated without flux correction (for example HadCM3, Gordon et al, 2000). Admittedly, interior drifts remain an issue in coupled models, but work is ongoing to reduce this; for instance the use of isopycnal-type ocean models (e.g. Megann et al, J.Clim 2010, Dunne et al J.Clim 2011), that reduce spurious numerical mixing. Do the authors consider that interior drift is a serious enough problem to merit such an invasive engineering fix? This is not dis-

cussed in the paper. In my opinion, the proposed use of flux correction in standalone climate models would be a serious step backwards. In any case, the question needs to be posed of the robustness of the tuning of the correction fluxes: would they still be appropriate for models used for future climate projections?

For publication in GMD, the intended application of the model described should be clear, and I am not convinced that is the case here: the introduction section of the paper does not make the case strongly enough for the utility of the OFAM model in climate studies. In particular, I do not really understand the inclusion of the word "forecasting" in the name of the model, since it is incapable of advancing beyond the limit of current forcing datasets. I would certainly like to see a clearer exposition of what the model is actually useful for.

Recommendation

In summary, although the paper is overall well structured and well written, the lack of clear justification for the design of the model, nor of any clear, plausible statement of its intended application, means I cannot recommend publication in GMD of the manuscript in its current form.

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