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Geoscientific
Model Development
Discussions

Interactive comment on "Evaluation of a near-global eddy-resolving ocean model" *by* P. R. Oke et al.

Anonymous Referee #2

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The discussion paper describes the settings of a large-scale ocean general circulation model coupled to a biochemistry model. Physical parameters and chlorophyll are verified against global hydrography and satellite fields.

I am surprised that the model configuration is not state-of-the-art in ocean modeling, despite the fact that it is based on a modern model architecture. Without a sea-ice model (hence exclusion of the Arctic and important parts of the Southern Ocean) and the application of atmospheric fluxes the model only seems to represent a global, free run - but it isn't. Even though the authors claim these deficits in the discussion, they show a series of comparisons that cannot be applied here. Before supporting the publication of the manuscript I would demand a more critical verification and discussion of the forecast capabilities. My detailed comments below may give some guidance.

(1) Most important for the verification is certainly the absence of a sea-ice model in combination with the application of the atmospheric forcing. Provided as heat and freshwater fluxes the model certainly tends to drift enormously in temperature and salinity, hence the needed strong restoring towards observed SST and SSS values. It remains vague and somewhat misleading between sections 2 and A3 what exactly is applied as restoring data. I would read that interannual varying Reynolds SSTs are used, but climatologically averaged CARS SSS. If that is correct, the comparisons with SST (Fig. 3) and NINO indices (Fig. 6) are strongly misleading since these quantities are mainly determined by the restoring term itself (10 days timescale!). In particular, it does not make sense to compare model SSTs with those fields that have been used for restoring. It is also of no surprise that SST is strongly damped, seen in RMS variations (Fig. 9) and seasonal cycle (Fig. 3).

(2) The evaluation of the circulation is too sloppy, mainly due to the use of global maps. A closer look at Figs. 1 and 8 shows that the separations of Gulf Stream and Kuroshio are incorrect, a Northwest Corner in the North Atlantic is not visible, and that the path of Agulhas rings is too regular. Model resolution alone is not guarantee for a proper circulation, and an "excellent agreement" (p. 4316, l. 19) is incorrect. It would help to focus into some crucial areas, rather than to argue with global maps alone. The same applies to the MLD (Fig. 2): The excessive deep convections in the Labrador Sea and Weddell Sea are barely mentioned. Some more information (E.g., if the convection reaches unrealistically down to the bottom) and discussion on causes (missing ice model) and (most important) impacts (over/under-representation of Labrador Sea Water and Antarctic Bottom Water) is needed. Owing to the missing sea-ice model there is certainly a special treatment of the surface fluxes for cold temperatures (usually the fluxes are cut off below a certain threshold) - that should be mentioned in the appendix. In addition to a more careful and critical description of the overturning stream function (How strong is the drift of the Atlantic MOC over the course of the model run? What is the cause for the locally enhanced AABW cells at low latitudes?) the meridional heat transport is needed (given the strong deviations in zonally mean temperatures seen in

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Fig. 4).

(3) Volume transports provide a crucial verification. However, section 3.6 remains too vague; figures showing time series would help. Examples: - For the ACC transport a range between 144 and 176 Sv is given. Is that an interannual to decadal variability or rather (as often the case) a downward trend over the course of the integration due to a bad representation of the AABW formation? - There is a lengthy discussion on the INSTANT comparison, with separation into the individual straits. However, a total number for the ITF transport is not given.

(4) Appendix A3 states that "to avoid any significant drift in the deep ocean fields, the temperature and salinity are restored to CARS climatology below 2000 m. . ." (p. 4325, l. 25). Does that apply to the closed northern/southern boundaries only? Or is that true for the global ocean? If so, the applicability of the T/S comparison in the deep ocean (Fig. 4) would even be more questionable.

Minor points:

p. 4306, l. 1: "18-yr run" is misleading since the run was longer.

p. 4318, l. 1: The reference is wrong, Biastoch et al. (2009) used a 1/10° model.

p. 4319, l. 1: I do not agree with the statement that the measurement errors are dominating because of the low signal-to-noise ratio. Here, the application of the SST restoring seems to be the cause.

Table 1: The DiMarco reference is outdated, for the transport through the Mozambique Channel Van der Werf et al. (JGR 2010, doi:10.1029/2009JC005633 is a better reference and actually closer to the model values.

Fig. 11 is wrong, shows the observed (= Fig. 12) instead of modeled chlorophyll

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Interactive comment on Geosci. Model Dev. Discuss., 5, 4305, 2012.