

Interactive comment on “GO5.0: The joint NERC-Met Office NEMO global ocean model for use in coupled and forced applications” by A. Megann et al.

Anonymous Referee #1

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General Comments

The paper provides an overview of a recent version of the NERC-Met Office ocean model. It meets the GMD criteria of a model description paper, but does not advance the science of ocean modeling. Overall, I felt it is lacking even as a simple description. There is no motivation for what improvements were sought in the changes implemented in the model, just an unprioritized list. The main conclusions are that the main improvements from the prior version of the model GO1 are due to a.) a bug fix, and b.) some changes in some parameters in the vertical mixing code. The variable names from the code for these parameters are provided, but they are not defined in terms of the model equations or what physics they control. The justification for these later changes

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is a citation of a grey-literature report (Calvert and Siddorn, 2013). In short, there is little of interest here to anyone except those using this particular version of the model, and then only as a supplement to the user guide. I find little reason to recommend publication.

Specific Comments

pg 5752, line 11-14: What if any smoothing or interpolation is used to go from the 1/60 deg. topography data to 1/4 degree model grid?

pg 5752, line 18 “reducing polewards”: at what rate?

pg 5752, line 20 “no parameterization of eddy mixing is used” This is false. Isoneutral diffusion is a parameterization of eddy mixing.

pg 5753, line 23 “180 days” This should be expressed as a piston velocity (independent of vertical resolution) to be compared with other published values.

pg 5754, line 24: The results of the Calvert and Siddorn study need to be summarized here, or a standard citation provided. These are shown to be among the key changes in the model.

pg 5754, line 27 “a number of additional parameterizations”: These need to be enumerated if this manuscript is meant to be a documentation of the model design.

pg 5755, line 15 “use of in situ salinity”: This will cause a global non-conservation of salt. What is the magnitude of this drift in the solutions presented?

pg 5756, line 21-22: What are these parameters? What physics do they control?

pg 5756, line 24: What is the “lake parameterization” How does it affect the open ocean?

pg 5757, line 25 “cool bias over most of Northern Hemisphere” This is not at all clear from the figure. There is a strong positive bias over the western boundary currents and the California Current system for example. What is the hemispheric mean bias?

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pg 5758, line 20-25: These statements do not agree with what is shown in the figures, nor do the figures make sense. For example, the minimum MLD in the tropical Pacific shown in Fig 2a appears to exceed the maximum MLD shown in Fig 2c. The figure also lacks latitude and longitude axis labels.

pg 5758, line 27 “deep mixing extending from the Weddell Sea”: The observations are emitting in this region, how can you compare with the solution?

pg 5759, line 10 “Note the . . .” Why should we note these? There is no discussion of the density field illustrated in Fig. 3.

pg 5759, line 26 “higher end”: No, they are beyond the range of the observations.

pg 5761, line 20 “Gibraltar Straits” The net flow through Gibraltar should be exactly zero when using a virtual salt flux b.c.

pg 5766, line 2: “ probably a signature of ENSO” No need to speculate. Test this.

Pg 5766, line 27-28: Provide values for the spatial RMS.

pg 5768: “larger effect .. where the surface salinity is saltier ..”: This is false. The constant reference salinity is probably around 35 psi. The error where the salinity is very high, say 38, is about 10%. The error where the salinity is 0 psi is 100% (the salt flux goes to zero when using the locally reference salinity).

pg 5770, line 1-2: No idea what a lake parameterization is or why it should produce changes similar to that of a BBL scheme?

Technical Corrections

pg 5758 line 15-16 realistically . . . realistically

pg 5764, line 23: There is no Fig 6i

pg 5765, line 16: What do you mean by a “drift” in the climatology? A long-term trend?

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Interactive comment on Geosci. Model Dev. Discuss., 6, 5747, 2013.

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