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Interactive Comment

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.

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General comments:

"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. .... "...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."

The authors are surprised by the reviewer's stance on this point. The paper describes a configuration of NEMO that will be used widely in applications that are high profile, and there are few ocean modelling configurations that will have as much user interest. This model will be used as part of the GMES Marine Core Services (delivering daily forecasts to European taxpayers), it is being developed as part of a coupled Numerical Weather Predictions system; it has been incorporated into the UK's seasonal forecasting system that is widely used within government advice and policy; and it will be a substantial stepping stone on the road to producing the UKs contribution to IPCC AR6. Additionally it is made available as the global ocean model for UK research institutes. It is essential that the users of this configuration have a baseline description of the configuration and its performance.

So, even if this paper is only of interest to users of this particular configuration, that is a significant (and in our view unusually significant) group which alone makes this paper worthy of publication.

Additionally, NEMO is a widely used model and the assessment of the changes from NEMO3.2 to NEMO3.4, and the significant impacts that will have upon model performance, are not published to our knowledge. This result will, therefore, be of significant interest to those beyond the users of this configuration.

"There is no motivation for what improvements were sought in the changes implemented in the model, just an unprioritized list."

We have improved the description of the sensitivity studies. Section 5.3 is now divided into subsections and paragraphs which deal with the individual changes to processes and parameters. In each case we have provided a rationale for the change, its expected effect (even if it is expected to be negligible). Some changes are included simply because they are more physically correct (such as the improved bathymetry and the salinity-dependent freezing point) irrespective of whether or not they improve the simulation. We have also provided more rationale for the changes made in Sections 3 and 4.

"The main conclusions are that the main improvements from the prior version of the

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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 is a citation of a grey-literature report (Calvert and Siddorn, 2013)."

We accept that the physical significance of the key mixing parameters was not made clear, and we have made changes to the text to improve this. In particular the main results of Calvert and Siddorn (2013) are now presented in Section 3 along with a justification for the changes made and what the expected improvement were (mainly in Sections 3 and 5.3). The function of each of the parameters is also described in this section. Our use of the term "bug-fix" was perhaps unfortunate – it is better to say that the model does not now feed the mixing due to convection scheme back into the turbulence scheme, which we consider to be more physically realistic. We have clarified the text describing the changes between versions, as well as giving descriptions of the physical role of each of the parameters.

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?"

The derivation of the Drakkar bathymetry is described by Barnier et al (2006). Initially, each model grid cell was assigned the median of all observations falling within the boundaries of that grid cell. The initial estimate was then modified by application of two passes of a uniform Shapiro filter and finally hand editing was performed in a few key areas. We have clarified this in the text,

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

The bilaplacian horizontal viscosity is proportional to the cube of the maximum horizontal grid dimension at each grid cell; thus at 60°N the horizontal viscosity is approx-

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imately 1/8 of its value at the equator. We have now added this clarification to the text.

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

We have replaced this with the more accurate statement: "The isopycnal mixing scheme of Gent and McWilliams (1990) is not used in this configuration."

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.

This is "rn_deds" in the namelist, equal to -33.33 mm/day/psu. We have added this clarification to the text.

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.

We have added some sentences summarising the results of Calvert and Siddorn in the first paragraph of Section 3.

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.

We have added the following text to the first paragraph of Section 3: "Additionally, the NEMO implementation of the scheme includes a number of parameterisations to represent additional unresolved turbulent processes, including surface wave breaking (Craig and Banner, 1994) and Langmuir turbulence (Axell, 2002). A further parameterization represents the enhanced mixing due to breaking of near-inertial waves as an additional source of TKE exponentially decaying from the surface."

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?

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This was an erroneous statement in the first version of the paper: in fact NEMO v3.2 and v3.4 both use the in-situ surface salinity to calculate salt fluxes. With this in mind, given that this drift will be almost the same in all the model configurations that we discuss here (and given that the SSS relaxation and river runoff also contribute to salinity non-conservation) we feel that analysis of this is outside the scope of the paper.

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

This refers to the TKE scheme parameters rn_ebb, rn_mxl0 and nn_htau. Text has now been added to the relevant part of Section 3 to clarify the purpose of these parameters and their expected effects.

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

The lake modification adds inland seas and lakes ready for a coupled configuration and has no effect on the global ocean basins in an ocean-only simulation. In our simulations the lake modification was not enabled (we apologise for our previous lack of clarity on this). This means that the sensitivity experiment originally described as "bottom boundary layer and lakes", in fact only added the bottom boundary layer, and we may therefore ascribe the changes described entirely to the BBL. We have accordingly removed references to the lake scheme.

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?

We have revised and corrected the description of the surface biases as follows: "There is overall a warm bias over most of the global ocean, with a global mean bias of +0.72°C, and with the largest biases (of over 1°C) in the tropics, the Southern Ocean, the subpolar North Atlantic and over the separated western boundary currents in the North Atlantic and North Pacific. There are cool biases of 0.25-0.50°C extending over much of the subtropical North Atlantic and North Pacific."

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.

The legend on Figures 2(a) and 2(b), showing the minimum MLD, was incorrect: they are plotted with a different scale (with a range of 0-100m) from that for the maximum MLD, but the same legend was erroneously used for all panels. This has now been corrected, removing the inconsistency noted by the reviewer, and we have also added lat/long axis labels.

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

The large Weddell Sea Polynya that emerges after year 20 of GO5.0 has some similarities with the feature observed in 1976-78, although its persistent recurrence in the latter part of the run is probably unrealistic. We have cited observations of winter mixing in excess of 500m by Arthun et al using Weddell seals, but have acknowledged the paucity of winter observations.

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

The isopycnals were added to illustrate the position of the biases with respect to the main pycnocline. We have revised the text, including an explanation of the contour lines on this figure, and have rephrased this sentence.

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

We have replaced the phrase ...at the higher end..." with "...significantly stronger than...".

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

No figure for Gibraltar throughflow was included in Table 3, and we have removed reference to it from the relevant text, which now only refers to Bering Strait throughflow.

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

We have revised this sentence, noting that the subsurface variability is not well correlated with the surface variability, so is unlikely to be directly related to ENSO.

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

We have added global mean RMS SST errors for JJA (reduction from 0.93°C in GO1 to 0.65°C in GO5.0), DJF (reduction from 0.79°C in GO1 to 0.67°C in GO5.0), which confirm the improvements in the fidelity of the annual cycle in GO5.0.

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 they 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 references salinity).

This is indeed true, but we have now removed the relevant text because (as mentioned earlier) the surface salinity forcing formulation is in fact identical in the two model versions.

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?

We have confirmed that the lake modifications are disabled in the experiments described here, so any changes between these two experiments may unambiguously ascribed to the BBL. We have accordingly removed all mention of the lakes scheme.

Technical Corrections pg 5758 line 15-16 realistically... realistically. Remove first instance.

Done.

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

This has been corrected to refer to Figure 6h.

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

We have replaced "drift" with "trend".

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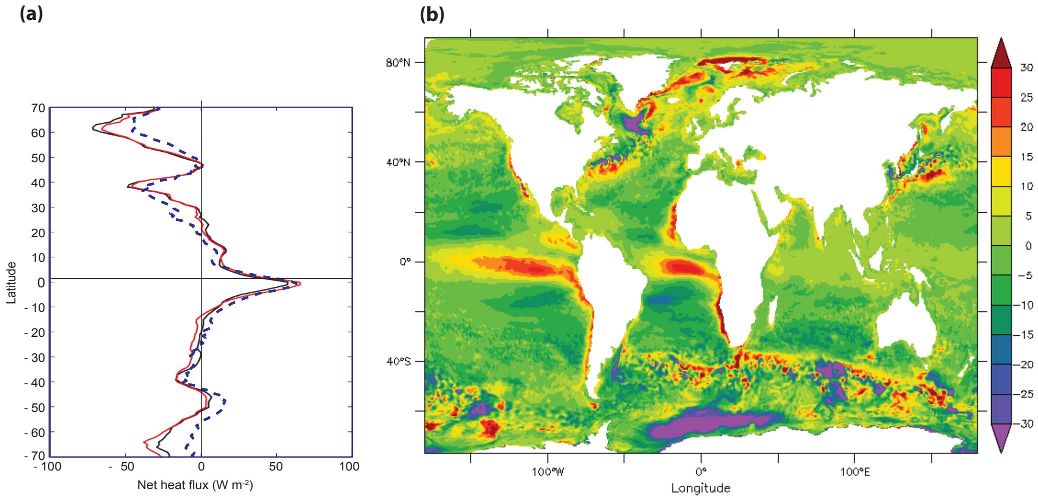

Figure 10. (a) Zonal mean net air–sea heat flux in GO1 (black); GO5.0 (red) and CORE2 data (dashed blue line) in years 1996-2005; and (b) surface net downward heat flux difference GO5.0 minus GO1.

Fig. 1. Figure 10 (heat fluxes)

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