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Geoscientific Model Development Discussions
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Interactive comment on "A 1/16° eddying simulation of the global NEMOv3.4 sea ice-ocean system" *by* Doroteaciro Iovino et al.

Anonymous Referee #1

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General comments

This paper presents a summary of the basic performance for a ∼ten year run of a new 1/16th global model, which is to be employed for operational ocean forecasts. Some performance comparisons are made with a similar 1/4 degree configuration of the model.

Overall I thought it was a well written paper which presented some interesting results. Most of the analyses are fairly standard, but to me this is as one would expect for a system definition paper.

My key comments are the paper could make better use of the $\frac{1}{4}$ degree parallel experiments they use and describe in places. For example:

âĂć It is key to emphasize that resolution is not a panacea – there are many atmospheric forcing set errors, and errors due to limited physics, i.e. parameterisation, e.g. mixing in particular. There may also be error cancellation, e.g. a bias might get worse due to removing a cancelling error, e.g. with ocean forcing set errors, by increasing resolution.

âĂć I would ideally prefer to see many more comparison sub-plots for existing figures to identify differences of the 1/12th compared to the $\frac{1}{4}$ twin run so we can see how many of the features you describe are common to both $\frac{1}{4}$ and 1/12 and how many are really down to

âĂć The reader ideally needs to know exactly which parameters and config settings are different for the $\frac{1}{4}$ degree run compared to the 1/12th run. For example, is it the same version of NEMO, you mention it has level vertical levels so some differences could be due to vertical rather than spatial resolution. Also what coefficient for isopycnal mixing on tracers do you use at $\frac{1}{4}$ (as the $\frac{1}{4}$ simulation and biases are quite sensitive to this)?

Minor comments:

Line 42-43 I think it may depend on the definition but is there not a factor of pi between the Rossby radius (based on wave number) and eddy scale? This makes a difference as one only needs two grid cells per Rossby radius to get 6 cells per eddy. This would be worth clarifying?

Line 53 – should you mention 'often (wrongly) termed eddy resolving' in view of the fact you go on to state they are not eddy resolving at high latitudes as shown by Hallberg (2013)?

Line 71-74 – This is a long sentence. Also should you qualify this statement in view of your paragraph above, i.e. state we are now able to at least resolve eddies mostly equatorward of 50-60N/S BUT we don't resolve high latitude eddies or sub mesoscale or associated energy cascade anywhere. Furthermore, results are sensitive to grid scale closure, particularly viscosity, as you state in your eddy kinetic energy section?

Line 88 – As stated above should it be described as a step forward, particularly for mid latitudes where this resolution resolves eddies but $\frac{1}{4}$ doesn't?

Line 123 – what about connection from Marmara sea to Aegean - Dardanelles Strait is very important for seasonal freshwater input to Northern Aegean.

Line 127 – Comment only – I believe some 1/12th NEMO configurations use partial slip in Labrador Sea to generate more eddies to help re-stratification after convection?

Line 141 – Comment only – important to note that uncorrected ERAI interim fields, e.g. radiation fields due to cloud errors, will have large errors which would be expected to impact on or even dominate near surface biases.

Line 158 – Why do you need to use SST restoration? This will mask model errors in near surface fields and there is already inherent relaxation back to air temperature in the forcing set?

Line 194 – I wonder why you didn't remove the seasonal cycle of MKE from EKE as otherwise the seasonal cycle of flows will be included in EKE estimates?

Line 215 – Can one really say much about SST biases from a ten year run with SST relaxation?

Line 215 – Should you show equivalent plots for figs 2 for $\frac{1}{4}$ degree or state that they are indistinguishable from the 1/12th if it is? The difference with the $\frac{1}{4}$ degree model is surely a key result?

Line 236 – Will the deep (>1000m) ocean really have equilibrated in a ten year run? I am guessing you are probably looking mainly as biases due to isopycnal heave which occurs reasonably fast?

Line 253/296 – More discussion or plots for GLOB4 on figs 3 and 4 would be useful?

Line 263 – Surely one can not say much about AABW in a ten year run?

Line 317 – Implied heat transports assume equilibrium? How large are you heat content tendencies with a short ten year run that may well still be drifting?

Line 430 – I did not think the location of mixed layer maxima agreed so well with the observations in Southern Ocean?

Line 430 – How does GLOB4 look in spatial plots? If it is very similar is it worth stating this?

Line 439 – I wonder if a different temperature based criterion might make the model mixed depths appear better? For example, if there is salinity compensation then density criterion can be rather sensitive to salinity errors.

Line 550 – I would emphasize the point about viscosity sensitivity more and include it in introduction. There is often an optimum viscosity level for EKE (and associated MKE) as too little enhances grid scale noise which damps eddies and too much obviously damps eddies.

Line 617 – Southern Ocean MLD maxima appear also not too good in Southern Ocean?

Line 633 – As it appears that you change both vertical and spatial resolution it is hard to conclusively attribute GLOB16 versus GLOB4 differences to spatial resolution. Could you state all configuration differences between GLOB4 and GLOB12 configurations as bullet points. In an ideal world one would mimimise the differences, e.g. use same number of vertical levels and vary only viscosity, isopyncal diffusion and perhaps slip between the two runs?

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