Geoscientific
Model Development
Discussions

Interactive comment on "UK Global Ocean GO6 and GO7: a traceable hierarchy of model resolutions" *by* David Storkey et al.

Anonymous Referee #2

Received and published: 13 May 2018

The paper describes several ocean configurations based on NEMO code and justifies the choice of the particular ones (GO6 and GO7) which will be used in the coupled version GC3.1 and in UKESM. The manuscript is well organized and consistently describes effects from the variety of options available in NEMO version 3.6 and below, providing sensitivity tests.

After reading the manuscript I first asked myself what I have learned. Besides the technical knowledge about all the details of the ocean component in GC3.1, some (already known) key aspects of improving the realism of the simulated ocean are provided. From the latter ones the main messages are:

1. increase in the near-surface mixing deepens the summer MLD and in the SH, through reduction in SST, suppresses the unrealistically large polynyas in the Wed-

dell Sea.

2. inclusion of multi-layer thermodynamics increases the summertime sea ice in the NH by ∼20% (although the authors attribute it to a slightly increase in p.9 l.22) as compared to zero-layer thermodynamics. Simultaneously, the effect in the SH is negligible.

3. Hollingsworth instability in the vector-invariant form of momentum advection needs a special treatment, otherwise it introduces large spurious tracer diffusivities.

I see this paper as a reference to GO6/GO7 configurations and the GMD is a proper journal for this. At the current stage some minor issues need to be solved before the manuscript can be published. Below I give my comments.

1. I could not find any information on performance and throughput. This section is definitely missing.

2. When giving sensitivity tests starting from figure 5 in section 4.4 I wonder about robustness of these tests. The coupled (15 years ?) as well as the forced (10 years) runs appear to be too short for making any conclusions. In the both runs I expect the differences to grow with time providing more insight into what is going on. If possible, I would recommend to integrate these tests up to at least 30 years long (even longer for coupled) and analyze the last decade like it was made in section 4.3.

3. Same is true for section 5.1. The CORE-II forced integration with 1/4° configuration shall be extended to at least one cycle of 60 years. Authors aim at using GO6 in CMIP6 and will probably do this run anyway.

4. Is Gent and McWilliams eddy parameterization used in GO6? If so it needs to be described in section 4.4 or in a separate section.

5. In section 6, besides what has been nicely discussed by authors, in fig. 18 I see that all Hovmöller diagrams for salinity show positive drift at and below 1000m which does not change with resolution. Does it come from Mediterranean? Having a longer run with 1/4° it would be nice to track the origin and the amplitude of this drift. Overall,

how do findings in this chapter compare to other CORE-II participants?

6. Section 5.2 Attributes changes in results to model changes only for SH. It would be great to track the changes in NH as well and, if applicable, track the key aspects I mentioned at the beginning.

7. Same is true for the summary. To my opinion it also requires more structure in summarising the main findings from chapters 4.x.

8. In the summary (P.15 L.8) as well as in abstract (P1 L.9) the authors attribute the improvements to the isopycnal mixing which I could not see in right panels of figures 5 and 6 (at least for the forced runs).

9. At the same time left panel of figure 5 where the difference from SST climatology is shown looks biased towards winter. Is it just a coincidence or something went wrong with averaging the model data?

10. P.3 L25 authors probably mean "Total Variation Diminishing"