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Interactive comment on "The CO5 configuration of the 7 km Atlantic Margin Model: Large scale biases and sensitivity to forcing, physics options and vertical resolution" *by* Enda O'Dea et al.

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1) The term AMM is used in the title and in the 1st para of the Introduction, and nowhere else. There should be an explicit connection to CO4 and CO5. Better yet, reduce the terminology to one term for the bulk of the paper

Reply: CO5 is in effect a version of the AMM. AMM is the modelling system for the of the NWS run at the Met office and dates back to POLCOMS models of the same region. CO5 is a particular version of AMM, and CO4 was it's immediate predecessor with which we compare against. The previous version of a long hindcast was only done with POLCOMS which is an even earlier version of AMM.

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Thee revised text will make this link so that the reader can understand what AMM is and how CO4, CO5, POLCOMS are all linked to AMM .

NWS and AMM are similar but not quite the same, the AMM is the modelling system and NWS is the geographical region.

2) Make clear the connection between AMM and CO5

(as above)

3) p2 l6-8 p2 l17-18 are the same:

L6-8: In this paper the subject of interest is what we label the standard Coastal Ocean configuration version 5 (CO5).

L17-18 Here we describe the non-assimilating CO5 control hindcast that provides a reference to understand underlying biases and drifts attributable to changes in the physics updates alone.

Reply

The reviewer suggests replacing L6-8 with that of L17-18 to avoid repetition.

That is clearly sensible, and contains the information required about this paper being about non assimilating part.

4) P2 l14-17 List of changes should include atmospheric fluxes Reply: Will change to:

"Changes include new riverine forcing, updated Baltic boundary conditions, increased vertical resolution, different surface forcing, as well as updating the base NEMO version from 3.2 to 3.4."

5) Section 3: Add sub-section numbers 3.1 – 3.4 at the appropriate places, e.g., p4.l17, p4.l28

Reply: Can amend as suggested below:

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“shorter runs detailed for the forecast implementation of CO4 in O’Dea et al. (2012). Here we describe in detail each of the changes and in Section 4 a set of sensitivity experiments explores the impacts of these changes. 3.1 Relative to CO4, which uses the stretching function in Song and Haidvogel (1994), CO5 features both more model levels (increased from 33 to 51) and uses the stretching function as detailed in Siddorn and Furner (2013) for the terrain following coordinate system. We refer to the stretching function in CO4 as SH and that in CO5 as SF. The new stretching function”

“coupling where again consistent air-sea exchange will be important. 3.2 The second significant change between CO4 and CO5 is the data source for riverine input. In CO4 an annual climatology of some 320 European rivers mapped to 165 outflow points on the CO4 grid constitutes the riverine input regardless of the 30 model year (Young and Holt, 2007). “

Other changes The Third . . . 3.3

And The Surface 3.4

6). p5.l1: Add url for E-HYPE data

Reply: Will add reference to http://hypeweb.smhi.se/europehype/long-term-means/ http://hypeweb.smhi.se/europehype/about/

7) p6.l13: Add a referral to fig. 11

Reply:

We will insert Figure reference here to show the masking as suggested by the reviewer.

“No attempt is made to model the Danish straits and they are removed from the domain as seen in the hashed out region of Fig. 11"

8). p7.l5-. . .: Give a reason for why the surface bdry condition was changed.

“The surface boundary condition in CO5 has also changed from CO4”

Reply:

The purpose was to develop long hindcasts for which the NWP data used in CO4 does not exist.

Whilst this is explained later in the text, the reviewer suggestion here is to bring forth that explanation here where the surface bdy is first discussed. This shall be done in the revised text ti aid the reader.

The later description is in 5.4 year sensitivity runs: "The constraint on the start date is the availability of Met Office NWP flux data and the open (double open a typo) boundary conditions used in CO4 which start from November 2006"

9) . p8.l14: "...amphidrome in southern Norway ... better represented" needs to be substantiated.

We will add the reference: "M.J. Howarth, D.T. Pugh, Chapter 4 Observations of Tides Over the Continental Shelf of North-West Europe, In: B. Johns, Editor(s), Elsevier Oceanography Series, Elsevier, 1983, Volume 35, Pages 135-142, 143, 145, 147-188, ISSN 0422-9894, ISBN 9780444421531, http://dx.doi.org/10.1016/S0422-9894(08)70502-6. (http://www.sciencedirect.com/science/article/pii/S0422989408705026) " However, the issue here is the observations are sparse here. The text needs to reflect this. The text should be updated to say that whilst the amphidrome coincides better with the above observationally derived charts, the observations are sparse and there is large uncertainty here. It cannot be said that the change is an improvement. and references to improvement in the amphidrome position will be removed.

10.) p8.l15-16: "It is found that..." without further explanation means "unsubstantiated claim" in my book. Was a sensitivity test done? Are the CO4 and CO5 bathymetries otherwise identical?

"represented in CO5. It is found that the change in the bathymetry and land sea mask due to the new Baltic boundary condition is the main driver behind the shift in the amphidrome.”

Reply:

The reviewer is correct, the text should have pointed out that the bathymetry for CO4 and CO5 is the same except for the new masked out region and the text will be amended to include this detail. Two CO5 like runs where used to check it effect also.

This should have been reported in the paper and will be included in the revised paper.

11) p12.l15-18: Is this meant as an explanation for why the bottom profiles in fig. 7 look different? If so, say so.

“The location of the transects are chosen to intersect 15 regions of particularly large bias. Note that these comparisons use the CMEMS POLCOMS dataset, which was interpolated onto standard depth levels from the native POLCOMS grid which uses 40 s-levels in the vertical (Holt et al., 2012). The interpolated POLCOMS data is particularly coarse at depth which is reflected in the step like nature of the POLCOMS bias plots at depth”.

Reply:

I had thought that was covered by the follow-on sentence but this should be made clearer, being more explicit e.g. “.. step like nature of the POLCOMS bias plots at depth, which accounts for the differences in the bottom profiles in Fig 7”

12) P18.Figure 11: Add “The figure shows CNTL minus S30_1. Grey area in Kattegat indicates interface to Baltic NSBS model data.”

“Figure 11. Comparison of mean salinity at 5 m between CO5 with 51 vertical levels using the Siddorn and Furner stretching function (CNTL) and 33 vertical levels using the Song and Haidvogel stretching function (S30_1).”

Reply: This will be added and is relevant to the reviewer’s suggestion in 7) so that the reader can clearly see the update to the model domain with respect to the changed Baltic bdy.

13) p19.l5: should be "slightly less saline" if the Figure 12 caption is correct.

"The IoW boundary results in a slightly more saline SSS over in the Norwegian trench."

Reply:

The caption actually has part C incorrect and needs to be amended,

Figure 12. Comparing SSS using climatological river and Baltic inputs against E-HYPE rivers and IoW Baltic. Panel (a) 33 SH levels with E-HYPE rivers and IoW Baltic (S30_1) Vs EN4. Panel (b) 33 SH levels with climatological rivers and climatological Baltic (S30_2) Vs EN4. Panel (c) E-HYPE rivers minus climatological rivers (S30_4-S30_2). Panel (d) IoW Baltic - climatological Baltic (S30_4-S30_1).

That should have been S30_3-S30_1 as in the plot title

With regards to the caption for Fig 12 (d) I have double checked the data and scripts and the caption is incorrect also. It should have read Climatological rivers (S30_4) - EHYPE rivers (S30_1) . The S30_x labels were correct but not the figure text. However, the text in the body remains intact. That is the IoW Baltic data results in a slightly more saline SSS.

14) p13.l13: to me "near bed" means right at the bottom, but Figure 13a shows that the PDWL scheme reduces bias over more than half the water column. Reformulate.

P19.. "Figure 13(a) compares each run on shelf in regions of seasonal stratification. Using the PDWL light scheme has three effects; it increases the warm surface temperature bias, it increases the mid depth cold bias and it reduces the near bed bias. "

Reply:

The term “near Bed” was incorrectly used here as it comes from information that can’t be ascertained from Fig13 alone. The bathymetry is quite variable over the area considered. Ranging over the 40-100m depth range shown in the aggregated profile plot of Fig 13. Thus the bed is often at 40m. However, this is irrelevant in terms of light schemes and the reviewer is correct to request a change to the text.

e.g. “ From 40m to the sea bed” would be a more accurate description”

15) p25.l11: same as previous as above.

16) p22.l21-23: Give a reference for the cold/fresh bias in ORCA025.

Reply:

There Reanalysis inter comparison papers are global in nature and it is difficult to see from the plots the small patch of the North Atlantic.

Instead we can compare the GLOSEA data against WOA data as was done with CO5 to show the biases in this region and reword to reference the additional figure attached.

17). p22.l25-30: There appear to be two differences at play here: different sources (Met Office NWP and ERAI) and how they are applied (as direct fluxes or via CORE bulk formulae). “ERAI fluxes” is a bit misleading; use “ERAI-derived fluxes” or similar in stead. Is data availability the reason for doing this sensitivity test?

Reply:

Yes, data availability is the issue here. The NWP fluxes do not extend backwards in time as far as the ERAI fluxes. The paper shall be reworded to ERAI-derived fluxes as suggested by the reviewer.

18). p22-24 Section 5.42: Why not show the SST/SSS biases in the two runs in Figure 15? I would imagine the discussion would be easier.

Reply:

Bias plots for the short sensitivity runs were avoided in the paper as it was felt that it may be misleading with respect to the longer 30 year sensitivity experiments. It was felt that only the relative impact of the shorter experiments to each other should be used, as such a short period could be anomalously cold/warm and lead to incorrect conclusions. It does make the discussion more difficult but avoids the issue of short term anomolous bias plots.

However, if the review feels strongly about this issue, these can be added and the text modified accordingly.

19). p23.l1-p24.l2 and p26.l2-7: This is interesting and could be substantiated more. What is the actual impact on the circulation? "Significant impacts" is not very useful.

P23 L1"However, it should also be noted that because direct fluxes use the Haney correction the resulting model SST is indirectly relaxed to the prescribed SST in a hindcast simulation."...

Relevant lines for clarity:

P24 l2-7: "The differing resolution of the surface forcing and the use of absolute instead of relative wind stress is thus likely to play a role in the different sensitivity experiments. In Fig 15(b) it is shown that the ERAI forced experiment is slightly more saline on shelf but significantly fresher in the Skagerrak and the Norwegian Trench mirroring the SST differences here. The difference in the shear stress modifies both the transport of the surface fresh layer 5 out of the Skagerrak and the transport of relatively saline water from the North Sea into the Skagerrak. The difference in relative and absolute winds are significant also along the shelf break from the Shetlands northwards."

Discussion: P26 l6-7: "The combined difference of absolute versus relative winds and differing details in the fluxes combine to have significant impacts in local regions such as the Skagerrak."

Reply:

The reviewer is correct, the discussion here is too blunt and should restate how the salinity is altered (and better still by how much) We will also include a plot or a panel to show the difference in mean surface currents (or transport) to further illustrate the point.

20). P24.l22-24: Needs some explaining, as mentioned above

“Overall the CO5 tides are of comparable quality to CO4. The reference density of 1035 kg m−3 used in the control run slightly degraded the tidal predictions. The position of the degenerate amphidrome in southern Norway is slightly improved in CO5 mainly due to a slight change in the land sea mask originating from a change in the Baltic boundary condition.”

Reply:

Here we cannot state the amphidrome has improved only that it has changed and that it coincided with poorly constrained observational based estimates of the amphidrome.

21). P24.l29-31: CO5’s SSS bias in the German Bight is also affected by E-HYPE.

P24 l29-31 “As in the SST, CO5 has a similar pattern of fresh bias in the near surface salinity from Iceland to the FSC as well as a large fresh bias in the German Bight and a dipole of surface salinity bias along the Norwegian Trench that suggests insufficient lateral mixing. POLCOMS in contrast is slightly too saline in the German Bight and uniformly too saline at the surface along the Norwegian Trench.”

Reply: Here we will insert as the reviewer suggests the issue with respect to EHYPE data and the German Bight.

General comments: "I also suggest adding a table with the relevant features of the 3 models: POLCOMS, CO4, CO5; it would make the similarities and differences clearer in the discussions in Section 5."

This is very worthwhile suggestion and will be added to the revised text to aid the reader.

The author is also grateful for the technical corrections 1-7 and the revised text will address these issues.

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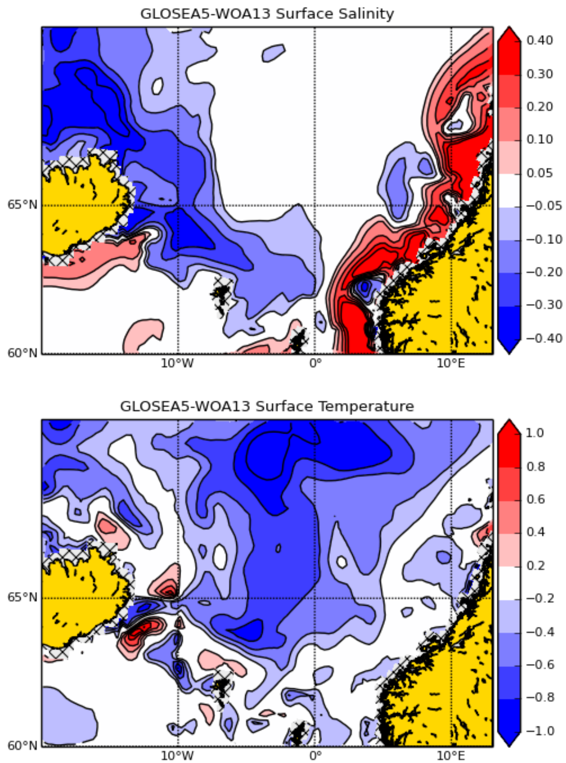
GLOSEA5-WOA13 Surface Salinity
0.40
0.30
0.20
0.10
0.05
−0.05
−0.10
−0.20
−0.30
−0.40
65°N
60°N
10°W
0°
10°E
GLOSEA5-WOA13 Surface Temperature
1.0
0.8
0.6
0.4
0.2
−0.2
−0.4
−0.6
−0.8
−1.0
65°N
60°N
10°W
0°
10°E

Fig. 1.