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Geoscientific Model Development Discussions

GMDD

Interactive comment

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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Response to GMD-2017-15-RC1

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

1) "A part that could be really nice would be to have also comparison of CO5 with itself but for example with different Baltic outflow parameterizations or different runoff sources etc. . . to provide a better understanding of how this affect processes"

Reply: The purpose of the article was to assess the impacts of specific changes made from the older CO4 configuration to the CO5 configuration. It first compares the configurations against climatologies and observations to present the large scale biases. It then goes on to examine the impacts of the changes bearing in mind the already presented background bias of the entire system.

With regards to Baltic outflow, there are two boundary conditions considered, that of CO4 which specifies the Baltic exchange using to River points, and in CO5 which takes temperature and salinity data from the IoW GETM model.

The impacts upon salinity to these differing Baltic boundaries are shown in Figure 12, panel (d). And the article details the impact as “The IoWboundary results in a slightly more saline SSS over in the Norwegian trench. The effect of the Baltic boundary condition is much smaller than the freshening due to the E-HYPE rivers resulting in an overall freshening compared to the climatologies.”

This may be cross referenced against Figure 6 panels (e) and (g) which show the CO5 is too fresh at the surface in the Norwegian trench.

2.1)

“there is a mention of CO4 as a reference, which obliges the reader to read O’Dea et al 2012, so for example there is not explicit mention of the vertical grid of CO5. I guess it is z*-sigma coordinates but it would make the article a lot easier to read if this was mentioned explicitly, this part is described in many details in O’Dea et al 2012 so it would be nice to have a little summary, especially after what comes just after”

Reply: This is correct, the coordinate system has not changed between CO4 and CO5. The main difference is the stretching function and the number of levels used. The details on the stretching function or within the reference “Siddorn, J. and Furner, R.: An analytical stretching function that combines the best attributes of geopotential and terrain-following vertical coordinates, Ocean Modelling, 66, 1–13, 2013.”

However, we will explicitly state this as suggested by the reviewer to aid the flow of the

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Discussion paper

manuscript.

2.2) Which means, if I understand rightly is that the Laplacian viscosity is applied using a rotation to fit geopotentials, and Bi-Laplacian is applied using model levels, and both at the same time ? Is it possible to activate both Laplacian and Bi-Laplacian in Nemo natively or some code has been developed ?

Reply: That is correct both forms as in CO5 are used simultaneously. The NEMO code at 3.6 does not support this natively. The relevant code changes may be found here:

ldfslp.F90

http://preview.tinyurl.com/nx7x4hc

ldfdyn_oce.F90:

http://preview.tinyurl.com/khkea3b

dynldf.F90:

http://preview.tinyurl.com/kva8bkr

dynldf_bilapg.F90:

http://preview.tinyurl.com/n7cojz

dynldf_iso:

http://preview.tinyurl.com/lllr49o

The purpose of the bilaplacian is to stabilize grid noise in the numerical solution, the purpose of the laplacian is to parameterise unresolved processes at the coarse 7km resolution.

2.3) And by the way, how is tracer diffusion handled ?

Reply: Tracer diffusion as in CO4 is simply laplacian diffusion on geopotential levels.

Details may be found at:

https://www.researchgate.net/publication/311107924_NEMO_namelist_for_CO5_AMM7

2.4) Figure 3: It would be really nice to have mean values of all runoff datasets. And perhaps mean values per basins.

Reply:

Attached is a figure for inclusion into the updated article with the difference in the mean flow along coastal sections between the EHYPE data set used in CO5 and the climatological rivers used in CO4. It highlights the larger input of fresh water with the EHYPE dataset. This should be compared with Figure 12 Panel C that compares the model SSS using the different river sources, and again referenced back to the fresh bias of CO5 in Figure 6 panels (e) and (g). There is a strong correlation between the increased freshwater input using EHYPE and the SS fresh water bias in CO5.

Additionally the units are corrected on Fig 3

2.5) Additionally, in places like the North Sea, SSH variability does not only create transport but also mixing and tidal straining which greatly affect freshwater dynamics. So the salinity bias explanation that comes after could also be related with a bias in barotropic dynamics, not only the runoff. A deeper analysis could include a computation of the North Sea gyre circulation, and that of the Strait of Dover.

Reply:

We have conducted a computation of the transports around the North Sea using the standard NOOS transects. A plot which will be included in the amended article is attached which includes the mean EHYPE river inputs within the area bounded by the transects. The transects 1, 2, 3, and 13 are in the same ball park as the observational references : NOOS 1 : Otto et al 1990: 0.6 Sv NOOS 2: Otto et al Inflow (0.7-1.11Sv) Outflow 1.8Sv NOOS 3: Otto et al 0.3 Sv NOOS 13: Prandle et al 1996 0.094 Sv

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The Net flow at NOOS Transects 9 and 2 will be incorrect (too low) as the barotropic boundaries are not specified in CO5 for the Baltic.

The transports of the North Sea circulation look reasonable which adds weight to the riverine inputs being the leading order term in the salinity bias for CO5. The revised paper will include this first order analysis of the transports, but a more detailed analysis the transport should be the subject of a separate paper.

3) About the Baltic boundary approach using GETM, the approach is interesting but a bit heavy to carry. Basically one needs always to run GETM, and there is little estimation of the impact of having a realistic Baltic Sea outflow. It would really nice to see a real sensitivity experiment of the impact on salinity structure along the Swedish/Norwegian coasts. The Baltic Sea outflow is mostly a barotropic process driven by wind forcing over the Baltic:

Reply:

It would definitely be of interest to compare the effect of the various Baltic model data in the Skagerrak and Kattegat.

However, the barotropic processes in CO5 will not be correctly represented in the Skagerrak/Kattegat due to the simplistic boundary conditions used. The model is likely to do poorly here and doesn't warrant further investigation. The GETM model is not used for operational forecasts but at the model data was readily available from hindcasts it was used for the reanalysis. Baltic CMEMS data is used operationally. Future higher resolution configurations (1.5km CO7) will move the boundary south of the Danish Straits and used full boundaries. It will certainly be of value perform a detailed analysis of this system with a variety of source boundary data.

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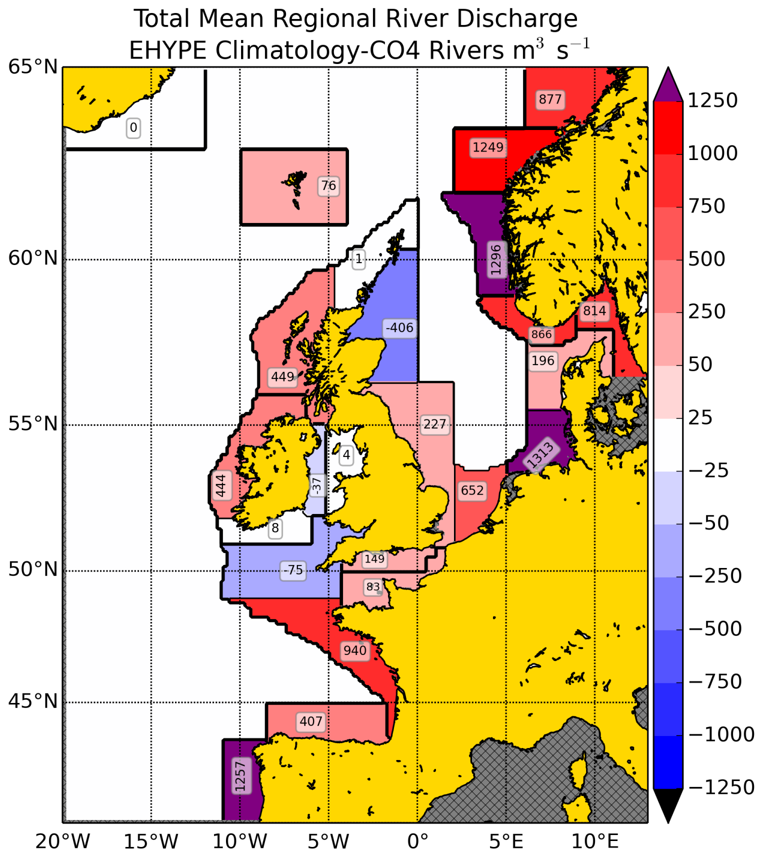

Fig. 1. New Figure or difference of river inputs between E-Hype (CO5) and Climatologies used in CO4

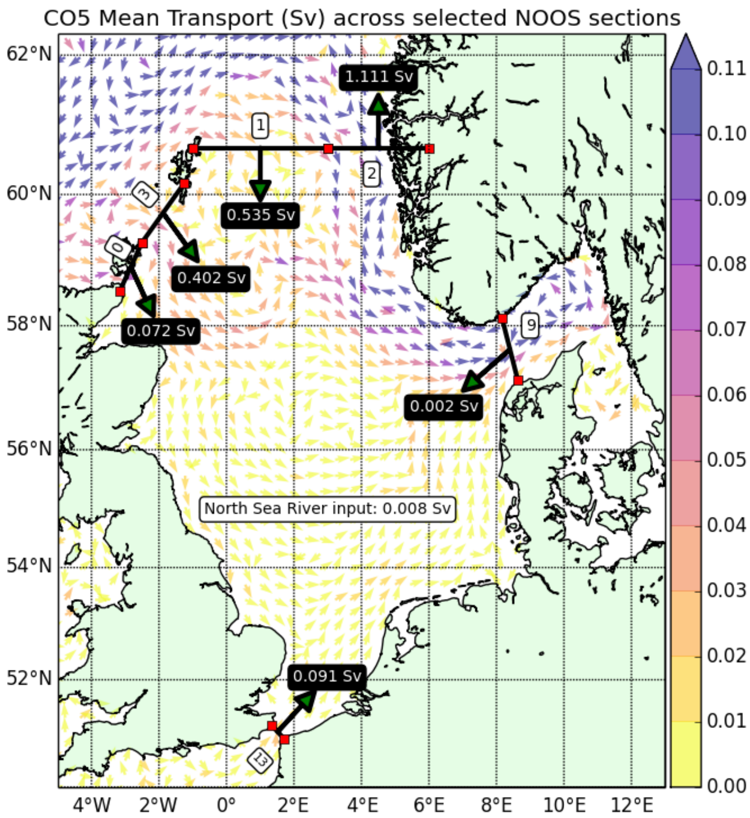

Fig. 2. Mean Transports in Sverdrups across NOOS sections + Mean EHYPE freshwater inputs inside the transects