Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-2-AC2, 2018

Geoscientific Model Development Discussions
Open Access
EGU

Interactive comment on "Nemo-Nordic 1.0: A NEMO based ocean model for Baltic North Seas, research and operational applications" *by* Robinson Hordoir et al.

Robinson Hordoir et al.

robinson.hordoir@hi.no

Received and published: 29 October 2018

Revised Version of the Manuscript in File Attached

First, we want to thank you for your work on our manuscript, we have tried to correct the many typos and hope this latest version will meet the quality standard expected for publication in GMD.

I think within the introduction (L85) or the Model set up needs mentioning of the two differing resolution models that are referred to later in the paper. It is not immediately obvious which version of Nemo-Nordic is being assessed at any one time, especially

Printer-friendly version

Discussion paper

as both are later compared against each other. I think it would help the reader if there was some way to make this clearer, e.g. Nemo-Nordic 1nm/2nm etc. or some other similar labelling strategy early in the paper and a description of these. In the model description there is only a description of the 2nm version. Perhaps restricting nautical miles to metric equivalents will be more in line with GMD.

Yes, indeed. We have now written clearly in the introduction that there are two configurations of different resolutions. Each time nautical miles units are used then the equivalent resolution in meters is now also written.

L105. With regards to the 2nm grid description, it might be useful to state if the grid is rotated, otherwise it would be hard to see how the stated grid resolution would be retained at a relatively high latitude.

We have made this more precise. The grid is un-rotated, it is a simple geographical grid.

L113-L116 The stated vertical resolution is surprisingly coarse in a regional model. I appreciate there is a need to focus resolution with regards to the overflows but 3 m surface resolution seems quite low. I refer the authors to Stewart et al. with regards to what would be an optimal vertical resolution for a z-level model in a global context. K.D. Stewart, A.McC. Hogg, S.M. Griffies, A.P. Heerdegen, M.L. Ward, P. Spence, M.H. England,Vertical resolution of baroclinic modes in global ocean models,Ocean Modelling,Volume 113, 2017,Pages 50-65,https://doi.org/10.1016/j.ocemod.2017.03.012. Towards the end of the paper there is an analysis compared to an SST product. The bias is surprisingly large and cold given the warm bias in the atmospheric forcing, could the surface resolution play a part? What is defined as SST in this context ?

The vertical resolution is a compromise, which allows to have an acceptable resolution close to the surface but also closer to the bottom in order to resolve as best as possible the Baltic overflows. The overflows are driven by barotropic processes, and the halocline/thermocline is usually located far below the level the first grid cells, so we do not

believe this kind of bias is linked with the resolution. However there is a clear problem linked with the Galperin parameterization, which we believe needs to be applied only to haline stratification, we are working with this issue.

L124 The issue of model resolution and the Danish straits is correctly brought to the attention of the reader and the method by which the barotropic flux can be maintained by retaining the same cross-sectional area. However, this must be problematic with regards to the baroclinic part of the flow. Particularly so as one of the main motivations of having the interconnect is to model bottom saline intrusions form the North Sea that enter the Baltic. Perhaps there is justification here for some more comment on the effects on the baroclinic flows by attempting to retain the barotropic flux.

Indeed, we have added a few lines about this specific issue.

L136 It is mentioned that 'tuning' is done with regards to optimizing model SSH. It is not clear what the optimization is, perhaps this could be elaborated as it could potentially save others time in the future or suggest useful strategies. I wonder could the authors supply a graphic/map in the supplementary material with regards to the 2d varying bottom friction "following the barotropic Kelvin wave", what is the physical grounds for this?

We can provide the input file that is used for this tuning, but the entire model is available on demand actually, including this file. The underlying idea is that barotropic waves entering the North Sea have a high energy that they lose while propagating cyclonically along the coastline. The bottom roughness tuning is done to fit the loss of energy.

L174-L175 The use of variable diffusivity and viscosity appears to be an interest- ing pragmatic engineering solution to the model difficulties concerning mixing and the dense water overflows. That is an interesting solution and appropriate for short time scale like a forecast model, but I wonder if it is appropriate for climate scales? That is this strategy assumes a-priori what the structure of the water column is, but on climate time scale that could change but the model may in effect be imposing it as it is, could

the authors comment on this. It seems that as the authors note, a hybrid z* with sigma at the bottom is a much better vertical framework for the problem at hand. Could the authors comment on why such a huge viscosity is required at the boundary region? This is likely to cause severe issues for any coupled biogeochemistry model here. I suggest it is worth investigating what is happening to vertical velocity and tke here.

Actually this idea is made more for climate time scales (i.e.: several decades) which affect the Baltic Sea salinity. As long as the Baltic Sea is a semi enclosed basin with low turbulence this would work. We have added a few lines in the manuscript. Sigma coordinates would be better at the bottom indeed but would have dire consequences at the level of the halocline, the best would be hybrid coordinates but long term simulations would be very costly. z* coordinates are not perfect but a good compromise. High viscosity at the OBCs does not affect areas of concern for biogeochemistry so far, which we always manage as for the physics to be far away from these OBCs.

L210 There are a number of chl products that are available. Do the authors consider using say even just a monthly climatology rather than a uniform value domain wise.

Indeed, but we have not investigated this issue yet. So far our only concern with chlorophyll data is to get a light penetration that corresponds to reality, especially in the Baltic Sea.

Fig 2,3,4 I think the use of Taylor plots here is not appropriate, as there are only two data points. It could save a lot of space to reduce the Taylor plots to numerical tables. Taylor plots are beneficial when analysing a large 'cloud' of data. In a model sensitivity, they are useful if tuning say one parameter a number of times. In this case, there are just 2 model resolutions, it might be more appropriate if there are several model resolutions to intercompare. Enabling the modeller to visualise if there are say competing trends between rms, correlation and standard deviation. However, with just two data points there can be no trend to discern. In conclusion, a table might be quicker to interpret and save considerable space I do not think the Taylor plots here bring

any advantage. With regard to the tides in the North Sea, the inclusion of a Co-tidal amp/phase plot of say M2 could be useful to give a quick look at how overall the model is doing in space with regards to tides.

The other reviewer made the same remark, we replaced the Taylor diagrams by arrays.

Fig 5 The climatological currents from North to South along the boundary in the English channel are very odd and suggest some problem in the bdy implementation here. Could the problem be related to using clim. TS in a highly tidal area? Ignoring the general cold bias in Fig 17, there is still an obvious bdy issue both in the north and in the south in JJA, again perhaps relates to the bdys provided. Too much vertical mixing??

Basically what the figure shows are the mean currents of a place where there is a huge variability compared with the mean value. Along the Western side of the English channel opening towards the Atlantic ocean, the mean currents are not entirely along the main direction of the channel as it is the case in many places in the channel itself. The flow being almost entirely barotropic in such a region, if there was such a mistake in the model it would show immediately in the sea level.

L256 It is noted that the north sea underestimate lower frequencies but these are unbiased in the Baltic. (due to amplification?) Is there a case to be made that the model Is overdoing amplification of waves that are initially underestimated in the North Sea? If so could that have other adverse effects?

This is a very interesting question, which is difficult to answer here because it is a research topic in itself. Basically our understanding is that some low frequency waves coming from the Atlantic ocean enter the domain and are not included in our set of open boundary conditions. The effects on the Baltic Sea are difficult to estimate, so far it seems we are able to reproduce all the major Baltic inflows.

L 425 the authors show a large freshwater bias at the Frisian front location. May I also suggest that the riverine input from HYPE could be a possible issue here, Have the

authors made an assessment of the HYPE model along this coastline? The accuracy is assessed for the Baltic Basin but not for the North Sea?

Actually we have computed the mean value of of the E-Hype flow for the North Sea and it looked quite correct although there was a positive bias indeed. We have tried a lot of different forcing datasets for hydrology, including trying to correct biases in HYPE, all lead the same salinity bias (too fresh) . We believe a bias of circulation and/or mixing is the cause of this issue.

3 Minor Technical points/errata These are likely but a subsection of minor points that need further editing.

Thanks for this re-reading work, which helps a lot to correct the manuscript. We have taken one by one all the points and corrected them.

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2018-2/gmd-2018-2-AC2-supplement.pdf

---

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-2, 2018.