Geoscientific
Model Development
Discussions

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

Revised Version Attached

First we want to thank you for the constructive comments and the general work done on our manuscript. We answer each comment below. You ask a lot of question and show a great interest for our model and manuscript, which we appreciate very much of course. But sometimes we can not do all the work which you suggest, either for time related questions or because some questions (like for what generates the inflows for example) are still a subject of debate.

C1

lines 40-55: a figure with a map of the main currents and key elements of local dynamics could be welcome

Indeed. We will add them. However, since such maps have been produced numerous times in many articles, we will just reproduce pre-existing one and cite their origin accordingly.

line 55-65: the authors emphasize the importance of the transition between the two seas (North and Baltic). Is the horizontal resolution sufficient to represent all these small straits? A mesh of almost 4km seems a bit coarse to achieve this goal.

It is too coarse to make any detailed study of the Danish straits, but an appropriate tuning can reproduce the ´nÂăimpedanceÂăÂż of the Danish straits so that the transfers of volume, salt and heat between the two basins are represented. We mentioned this briefly in the text, but we will make it more clear.

line 67: if possible I would suggest replacing the nauticmiles by the metric system

We will put both.

line 96: I think we could be more specific: for example explain what are the physical processes represented by NEMO that allow NEMO to be better adapted than these other models cited in lines 95.96

We will detailed this part to a larger extent. Beyond the features present today in NEMO that other non-community models will likely never have, it's also about the yet non-exploited possibilities like wave coupling for example, or the future ones.

line 96: " the dense overflows that feed its very specific sill bounded estuarine circulation ". Unclear. . .. Detail a little more.

Done.

line 101: the authors specify that they use version 3.6 of NEMO. If this seems relevant to the authors, I would suggest to say what this version brings compared to previous

versions.

The biggest improvement was the new coupling between barotropic/baroclinic modes which enables a much better SSH representation, and since SSH variability is the driver of the entire system a better representation of almost everything. XIOS, and the ability to have only one output file and not one per processor anymore, made the model a lot nicer to handle for everyone. This is not a scientific argument but having motivated scientists to work on a model does help a lot. We will add more details about the scientific aspect in the text.

line 105: This area has a large overlap with the NEMO-IBI operational system domain (Maraldi et al, 2013). The horizontal resolution is ultimately worse than that of IBI but NEMO-NORDIC brings the connection with the Baltic, missing from IBI. The overlapping zone also offers interesting intercomparison possibilities between the two models. The authors could say a few words about the respective interests and the complementarity of NEMO-NORDIC and NEMO-IBI and if possible make a reference to the validation study of NEMO IBI by Maraldi et al, 2013.

Yes. The idea is that to have good North Sea baroclinic dynamics, you need a good Baltic Sea freshwater outflow, which most models don't have. And to get good Baltic Sea dynamics you need the North Sea variability. Not to mention the most interesting area of Nemo-Nordic which is Kattegat/Skaggerak. We have mentioned this aspect and added the relevant citation.

line 108: NEMO's recent advances on the sigma coordinate could be cited here. I think for example to the paper of Shapiro 2013 (or other if the authors see a reference more relevant)

Indeed, we have added this reference.

line 112 "adopted" "adapted"

Yes, thanks.

L132: the word "decouples" seems clumsy to the extent that there is in fact a coupling between baroclinic and barotropic modes.

Indeed, we'll correct this.

L135: the term "degree of conservation" implies that the conservation properties of tracers may not be strictly respected, which seems a priori not very compatible with the study of climate. If the model really respects the conservation properties of tracers the authors should say it more clearly (or can refrain from commenting on a fairly basic property).

Quite so. I think we shall refrain from commenting on this aspect.

L140: Is the roughness given by a length of roughness? Is it a constant or a mapped parameter? Since this parameter seems important it would be interesting to give its value, or its order of magnitude if it is not a constant.

In operational mode (1nm resolution), the roughness is mapped. It is constant for the longer term simulations. We will add a better description.

L145-150: without doubting the proper functioning of the BBL, is it still not unsatisfactory to have to completely remove the advective component of this parameterization? Independently of the numerical considerations that one has well understood, is it not less realistic from the point of view of physics? Would that not finally plead for the use of the sigma coordinate referred to online 107?

Actually the transition times of Baltic flows are several months, so a small advection speed might help. We need to investigate this matter further. Sigma coordinates would help for the overflows but would create many other problems, like destroying completely the Baltic Sea halocline. The best would be hybrid coordinates like in Getm. We'll add more lines about this.

lines 150-160 What is "tuning" mentioned by the authors is not very clear. Do the authors refer to the value of the Galperin coefficient? In this respect, the reference

to Galperin's paper is too vague. One could for example think that one refers to the functions of stability of Galperin? Is that the case? According to Reffray et al 2015 the choice of Canuto seems the most judicious... Or do you refer to the limitation of the mixing length (eq22 of Galperin)? What exactly is this coef mentioned above? The problem raised by the authors also refers to the choice of the thresholds of minimum values for the TKE of the closure scheme and their possible regionalization. Can the authors say a few words about the values used by NEMO?

We refer to the mixing length limitation, itself tuned with the Galperin's coefficient. We have made this more precise.

lines 165-175 The authors mention the drawbacks of the calculation of the horizontal mixing in z coordinate, which, even taking into account the NEMO rotation tensor, tends to introduce a significant diapycnal mixing. The authors correct this defect by means of a spatial adaptation of the coefficients of viscosity / diffusivity which seems a little artificial but which has the merit of working. There is certainly room for discussion of possible future improvement prospects for NEMO-NORDIC. Insofar as the saline intrusions evoked by the authors would follow the bottom, one can for example wonder if the sigma coordinate would not be better adapted. The work of Shapiro 2013 on the different forms that the sigma coordinates can take in NEMO could be a source of inspiration for a possible evolution of NEMO-NORDIC in this direction.

As said before, sigma would be better for the overflows, but would have dire consequences for the frontal structures and the permanent stratification. The best would be hybrid coordinates, but this is for the future. Meanwhile, we believe there are possible improvements to do with our viscosity/diffusivity coefficient.

line 183: the tide is apparently introduced as a boundary condition only. Does this mean that the internal generating forces in the numerical domain (astronomical potential and loading self attraction present in the NEMO version used by Kodaira et al, 2016) are not used here? If yes, why? Are they negligible in comparison with the influence of

boundary conditions?

Yes, we have neglected the tidal potential. NOAA on its website says US great lakes have a maximum spring tide of 5cm so it could be worth trying. So far our tidal signal is actually too strong, but we will try this sensitivity experiment in the future.

lines 184 The open boundary conditions seem to have a fairly high level of elaboration with respect to the barotropic processes (tide, storm surge). On the other hand, I am surprised, given the possible operational purpose, and also given the Copernicus context in which the NEMO-NORDIC model seems to be developed, by the great simplicity of the boundary conditions for the general 3D circulation. Only temperature and salinity seem to be concerned (nothing specific is said about SSH and currents, apparently). In addition, T and S would be climatological. This simplicity can be understood in the context of a climate projection, but for operational applications it is expected that the Coperninus operational system will serve to provide boundary conditions for regional models such as NEMO-NORDIC. I may have misunderstood the text which in this case should be a little clarified. Note also that the IBI operating system seems to have the capacity to forecast storm surges according to Maraldi et al 2013. Can the authors discuss a little more about their choices?

Indeed, thanks for this remark, this is a mistake of ours when drafting the manuscript. In operational mode, having proper OBCs has been a subject of great concern, especially for the barotropic mode. We will add more details.

line 185. The authors apparently use the TPXO tide atlas of Egbert et al 1994. In Maraldi 2013, the accuracy of altas FES is widely commented and finally used as a reference to validate the quality of the tide simulation obtained with NEMO. The present study could have been an opportunity to make a comparison between the different tidal atlases usable as boundary conditions. Which produces the best result? etc etc. . . That would be useful I think. This is a minor remark but if the authors deem it appropriate they might mention this fact as a possible prospect in future work.

Absolutely, it is just a question of time: one of the co-authors of this article has suggested we use FES. We just did not have the time to try, but will mention it.

line 195-205 The introduction and the abstract of the article suggest that NEMO-NORDIC is used for both climate studies and short-term operational forecasting. The description of the atmospheric forcing seems to correspond to the first point only. What is the authors' strategy for short-term operational forecasting? Does the hourly forecast of the sea level for example impose particular constraints with regard to the frequency of the atmospheric forcing? We can also think that the precise prediction of the sea level requires taking into account the effect of the waves. Preliminary developments have been made in NEMO on this subject (see, for example, NEMO and WW3 coupling by Clementi et al, 2017 for a better representation of the drag coefficient). What is the authors' strategy for this question?

Indeed, we had forgot to mention the forcing used in operational mode. This is now corrected. There has not been a thorough investigation on the influence of the frequency of the atmospheric forcing so far, the goal has been to find the best accessible data to obtain results which fit the quality requirements of the model in forecast mode, through a benchmark. About the coupling with wind waves, this work is ongoing at the Finnish Meteorological Institute.

lines 208-215 About taking into account a constant concentration of chlorophyll to improve the essential point of the penetration of light. Would there be an interest (perspective) in using Copernicus' global predictions of chlorophyll?

It would be interesting for future studies but we have not pushed our investigation so far. Basically we provided a value which is different than the NEMO default value to take into account the Baltic & North Sea turbidity.

line 243: I am surprised when the authors say that the correlation is mostly close to 0.99. I would have rather said 0.95. This difference of appreciation is probably subjective and attributable to the lack of readability of Figures 2-3-4. In fact it seems to

me that Taylor diagrams are not very suitable here. Figures 2-3-4 indeed occupy a lot of space for little information (only two points, a yellow, a blue) with a very low level of readability since each individual figure per tide station is finally tiny. We therefore lose a lot of time trying to see what are the RMS values, Standard deviation, correlation, when a simple table would immediately give this information, and allow a quick comparison with other authors (see for example Table 1 in Maraldi 2013). It seems to me that Taylor diagrams are appropriate when a single reference is compared to a scatter plot. For example, in Toublanc et al (2018) Figure 7, it is immediately understood that the simulations corresponding to green and blue point clouds are better than the simulation

We have converted this data into arrays.

line 247: The issue of the horizontal resolution is appropriately addressed in the Strait of Denmark. Some passages are indeed so narrow that a resolution of 2nm (almost 4km) seems clearly insufficient. For example the passage between Elsinore and Helsingborg barely fits a mesh. However, in NEMO, there is possibility for local increase in horizontal resolution, either by using an AGRIF nesting (Waldmann et al, 2016), or by using the NEMO curvilinear grid (Madec Imbard, 1996). To what extent can either of these two possibilities constitute a NEMO-NORDIC development perspective?

AGRIF is the way to go, but that is for the future.... Actually I am right now just back from the Nemo User's Meeting and it seems finally that Agrif works with the non-linear free surface in Nemo (key_vvl), which was not the case so far. The non linear free surface being an essential feature of Nemo Nordic, we did not spend too much time investigating Agrif before being should it would work.

The thresholding of the bathy (note that Maraldi 2013 also uses a threshold and discusses its consequences) also seems problematic: is not it a handicap for the forecast of surges? What is the technical reason that prevents lower bathymetry? Could the sigma coordinate overcome this problem?

We are not sure to understand, we do not limit the value of the bathymetry: the only

treatment of the original database we perform is to interpolate it on the Nemo-Nordic grid.

Table 1: Northern boundary, the "Inflow Observations" bounds are given in descending order. Is it correct?

It was not very clear indeed, and there was a bug in one of the values. We changed that. The values given are a range, and are now in ascending order.

Line 328. My next question is motivated by the authors' commentary on the Galperin coefficient and the fact that NEMO has two types of turbulent closure (TKE, k-epsilon, Reffray 2015). In the manner of Reffray et al 2005, have the authors made a sensitivity test of the SST bias to the turbulent closure scheme (TKE or K-epsilon)?

We have not made such an experiment with Nemo-Nordic, but we tried different values of the Galperin coefficient.

lines 353-355: In the description of the model it would therefore be interesting to say a few words about the state equation used.

Indeed, we have added a line when providing the runoff description.

line 370. It seems to me that the mechanisms responsible for the Major Baltic Inflows of 1993 and 2003 could be explained in more detail if possible (would there be no more things to say outside of the sea level? ?). Do these mechanisms come from open boundary conditions, or are they generated inside the modeling domain etc etc ...?

This is a very nice remark, but still a subject of intense debate among researchers. We have added a line

line 393. Specify the number of the Figure

Corrected.

line 415: the authors evoke the possibility of a validation of their boundary conditions.

But what exactly are the boundary conditions for T and S? How are the T and S fields constructed that force the open-border model? Climatology, ORCA25? Same remark for baroclinic currents.

The sentence is not very clear indeed, we changed it. The data source for T&S at the open boundary is a climatology for our long term hindcast, but can be anything else depending on the simulation. We do not use baroclinic currents data for the open boundary conditions, but a simple radiation condition.

Figure 14: Would a single color palette (as in Figure 15) be preferable?

We have tried to adapt the color scale to the salinity range, the idea was to visualize as best as possible the biases of the model when comparing with observations.

line 481: it seems to me that the acronym BSH is not defined.

Yes, this is corrected.

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2018-2/gmd-2018-2-AC1-supplement.pdf

---

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