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Interactive comment

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.

Anonymous Referee #1

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The authors present a modeling study of the North Sea and the Baltic Sea with the NEMO model. The strong point of this modeling is the representation in the same high-resolution domain of these two seas and their exchanges through the Danish strait. The paper presents a number of comparison with observations validating the model. I believe this paper will be of interest to the community of NEMO users and more generally to researchers interested in the North and Baltic Seas. It can therefore be published with minor revisions. I propose (see following) some remarks that I leave to the appreciation of the authors to improve certain aspects of their article.

lines 40-55: a figure with a map of the main currents and key elements of local dynam-

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Discussion paper

ics could be welcome

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.

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

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

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

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.

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.

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)

line 112 "adopted" "adapted"

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

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).

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.

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?

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?

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.

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?

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?

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.

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?

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?

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 giving the cloud of red dots.

Note in passing that the altimetry is a powerful tool for validation of the tide in the regional or global models, not subject to the possible strong local specificities (harbor installation, particular bathymetry) which characterize eventually the coastal gauges and that a 4km resolution model can not represent (see Toublanc et al, 2018, kodaira et al, 2016).

Finally it would not be useless to present a map of the amplitude and the phase of the main wave M2 (and possibly the M4 wave which allows to appreciate the accuracy of the effects of non-linearity M2-M2 in a model) to allow quick comparisons with previous studies (NEMO in Maraldi 2013, T-UGOm 2D in Pairaud et al 2008, etc, etc ...).

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? 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?

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

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)?

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

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 ...?

line 393. Specify the number of the Figure

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.

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

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

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