

# Response to Reviewer 1

Tuomas Kärnä et al.

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In the following, all the comments raised by the Reviewer (in blue font) are addressed (in normal font). In the revised manuscript, all changes have been marked with red font color.

*Summary:*

*The manuscript describes an operational forecast model for the Baltic Sea and illustrates the applicability of the model by comparing a hindcast simulation to observational data from various sources.*

*Major comments:*

*The manuscript is well structured and the model seems well suited for the task at hand. I have only some minor points.*

*(1) The purpose of the model could be stated more clearly - which variables are of interest for the end-users and forecasted? In this context I would find it nice to motivate the model assessment metrics and the choice of the considered observations based on the model purpose (why are these of interest and which precision of the forecast is required for potential end-users?)*

We have revised the manuscript in this regard. The focus is now not only on forecasting but the development of a Baltic Sea model in general. The variables of interest are now mentioned in the abstract.

*(2) The assessment of the simulated sea ice is limited to a time series of sea ice extent. Some more assessment (including a visual impression of the spatial distribution) would be nice because I regard this aspect as very important for potential end-users.*

We agree. We have added a figure comparing sea ice concentration (Fig. 12 in the revised manuscript).

*More specific comments are listed below. Note that I am not an English native speaker either.*

*Specific Comments:*

*Line 1: The authors could add one sentence which model output they want to provide to the end user.*

Added.

*Line 7: I would suggest to add that the comparison is based on a hindcast simulation and the considered time period.*

We have added the time period in the sentence:

*The model is validated against sea level, water temperature, and salinity observations, as well as Baltic Sea ice chart data for a two-year hindcast simulation (October 2014 to September 2016).*

*Line 17: Please add Dietze et al. (2014) (already referenced later)*

Added.

*Line 50ff: For my feeling the part about spurious mixing is a bit long in the introduction and parts of it could be moved to the final discussion when referring to the representation of inflows from the North Sea.*

We have revised the introduction and shortened the discussion of numerical mixing. It is an important topic, however, and often overlooked by model users, and thus we feel that it is worth mentioning in the introduction.

*Line 64ff: The following parts seems a bit unrelated to the foregoing text. Maybe the authors could add a few sentences why operational forecasting is required in the Baltic and then come to the operational models.*

We have revised the introduction for clarity.

*Line 70: better "...ocean circulation model..."*

We have added the word ocean in the sentence.

*Line 80ff: I would suggest to add what should be forecasted/is of interest to potential end users.*

We have formulated the paragraph to emphasize the model configuration and its generic use in both hindcast and forecast tasks. This is now mentioned in the abstract as well. The fields of interest are mentioned in the subsequent paragraph, so we are not duplicating those here:

*The aim of this article is to validate the Nemo-Nordic 2.0 model configuration. The configuration is used in the EU Copernicus Marine Service for both near-realtime forecasts, as well as multi-year hindcast simulations for the Baltic Sea. The presented validation is based on a 2-year hindcast simulation that uses similar forcing as the operational configuration. [...]*

*Line 86: Instead of "skill metrics" I would rather use "model assessment metrics" - especially since the model is evaluated on a hindcast simulation and no real forecasts are considered. Note that this expression occurs rather often in the text and should be changed consistently.*

We have changed the terminology as suggested (also elsewhere in the paper). We do, however, continue to use the term "model skill" where appropriate: The term skill is not restricted to solely the forecast task.

*Line 90: I find this confusing. Better? "...is an updated version of Nemo-Nordic 1.0 based on..."*

The sentence has been revised:

*The presented model setup is an updated version of the Nemo-Nordic 1.0 configuration (Hordoir et al., 2019) implemented on NEMO version 4.0 (Madec et al. 2019; subversion repository revision 11281).*

*Line 93: horizontal resolution*

Fixed.

*Line 93ff: I would finish the description of the model grid before coming to the boundaries. Also, it should be specified which open boundaries are used.*

The location of open boundaries is a property of the horizontal grid being presented. We have thus not altered the text. The open boundary types are now mentioned in Section 2.3.

*Line 99: was -> were*

Fixed.

*Line 102: How was the bathymetry modified (what counts as "shallow")?*

We have revised the sentence:

*The bathymetry was modified along the west coast of Denmark by masking out shallow lagoons and channels (such as the Wadden Sea, Ringkøbing Fjord and Limfjord area; cut-off depth was about 10 m; Fig 1 c) to improve the propagation of tides to Skagerrak and Kattegat.*

*Line 106: How was the tuning done?*

The tuning process is explained in the same section. We have now revised the text for clarity: description of bottom friction tuning now follows the sentence:

*The model configuration was tuned to accurately simulate surface gravity waves and internal gravitational currents. Bottom friction is [...]*

*Line 135ff: I would suggest to finalize the description of the ocean part before describing the sea ice.*

Good idea, the order has been updated.

*Line 173: I would rather call this section “Boundary conditions”*

The section is now called “Boundary conditions and forcings”

*Line 174ff: The initial conditions could be described in a bit in more detail (e.g. from which conditions did the spin-up start?) because I expect at least some impact on the representation of the simulated deep water properties. Please note in this context that I see no perfect solution for their choice because the Baltic Sea virtually never reaches some kind of steady-state.*

We have elaborated the description of the spin-up run in Section 2.3.

*Line 178: Better? “The atmospheric boundary conditions are provided by ...”. Also, I would be interested in the spatial resolution of the atmospheric forcing.*

We prefer the more active voice wording (“model is forced by” rather than “conditions are provided by”). We have added the resolution of the atmospheric model (3 km) to the text.

*Line 182/183: Better move to line 180 (+ delete “in NEMO”)*

Text has been revised.

*Line 190ff: Better? Observational data were provided by ...*

Also here we prefer “obtained from” rather than “provided by”.

*Line 208: Better? “Model assessment metrics”*

Changed.

*Line 231: I am not sure what is meant by “datum” in this context.*

Vertical datum is the reference zero level in digital elevation models. We believe it is standard terminology.

*Line 235ff: The following part sounds like a lengthy excuse why SSH might not be captured perfectly in the model - which for me would rather refer to the discussion of the model results. Also, I would suggest to express this in a more positive way, i.e. what to expect from a SSH forecast. For my feeling the most important aspect for end users are deviations from the mean.*

We have shortened this part of the text:

*The exact vertical reference datum of the circulation model is not well defined. Consequently, SSH bias cannot be reliably evaluated and we therefore assess SSH performance with centralized metrics, i.e. CRMSD and Taylor diagrams.*

*Line 252: When mentioning the locations (Kiel Holtenau etc.) the authors could refer to Fig.1c.*

We have added a reference to Fig 1 in the captions of Figs. 2 – 5.

*Line 259: This reads a bit confusing for me. Suggestion: The agreement between model and the ssh observations is generally higher in the open Baltic Sea than in Danish Straits. In the open Baltic the NCRMSD is generally below 0.3 and correlations between model and observations are above 0.95 (exceptions are....). In the Danish Straits, stations Frediericia and Copenhagen show much lower correlations of ....., respectively. This local drop in the correlations is expected due to the complex bathymetry in the Danish Straits.*

We have revised the text as suggested.

*Line 263ff: The BSH provides 2D maps for SST. It might be nice to show an example snapshot – even though the statistical value is of course limited.*

The model is running operationally at CMEMS so examples of SST fields can be obtained from their services. As the Reviewer states, including a map of SST in the paper without a comparison or analysis does not seem, in our opinion, to add much value.

*Line 265ff: Maybe add "...mixing in the water column" because the other processes mentioned here refer to the atmosphere.*

Added.

*Line 265/266: ?*

It is unclear what the Reviewer is referring to by the question mark. Nevertheless, we have revised the wording in this paragraph.

*Line 281: The authors could add that they refer now to one of the ferries (for those who are not so familiar with ferry box data and the respective shipping lines). At which depths do the ferries measure?*

Ferries are mentioned already in the previous paragraph. TransPaper measures at 5 m depth, FinnMaid at 3 m. This information has been added to Section 2.4.

*Line 288/289: Which gradient?*

Horizontal, added.

*Line 294ff: I would expect that the deep water properties are still impacted by the initial conditions because Döös et al. (2007) report a residence times of almost 30 years for the Baltic Sea. This can in my eyes not be avoided here but could be mentioned (unless this issue was investigated somehow and ruled out).*

We agree. We mention this now in the discussion section.

*In this section I personally would include an assessment of the simulated mixed layer depths.*

See our reply to Reviewer 2 on the same topic: We agree that comparing the thermo- and halocline structure is surely valuable, but doing it reliably is out of the scope of the present paper.

*Line 299: I would use some other wording instead of "Temperature profile skill" (similar expressions are used several times.)*

Replaced by "temperature profile metrics", also elsewhere.

*Line 357ff: I regard the representation of sea ice as very important in a forecast model. I would find it nice to see a/some 2-dimensional map(s).*

We have added maps of sea ice concentration.

*Line 392ff: For my feeling the discussion could focus more on the applicability of the model for forecasts. I like the honest discussion about MBIs.*

The paper is now revised to not only focus on forecasting. Thus the discussion addresses model performance in general.

*Line 444: Why would a forecast model require a coupling with an atmospheric model? A coupled ocean-atmosphere model is not suitable for a 1:1 model-data comparison.*

We have changed the focus of the text: the model is not solely designed for the forecast task.

*Line 445: Code availability: Please provide doi and the link. I would find it nice if also the extra information to run the model was provided in addition to the code (such as initial conditions (restart), short test forcing/boundary conditions and specific model settings).*

The DOI and the link to zenodo.org are provided in the citation. The model input files are included in the code archive.

We fully agree that including initial conditions and forcings etc. would be very beneficial to the com-

munity. However, unfortunately due to licensing issues we do not have the rights to redistribute the data.

*Reference*

*Döös, K., & Engqvist, A. (2007). Assessment of water exchange between a discharge region and the open sea—a comparison of different methodological concepts. Estuarine, Coastal and Shelf Science, 74(4), 709-721.*