Rebuttal letter

Manuscript Number: ODYN-D-16-00155

First we would like to thank the reviewers for their valuable and clarifying comments. The manuscript is revised accordingly. We have amended all sections in order to meet the requirements from the reviewers.

Below is our rebuttal against each comment from the reviewers.

|  |  |
| --- | --- |
| First reviewer | Our comments |
| In section 3, line 24, it is mentioned that a set of variables are nested from the NorKyst800 model, including water levels. But the tidal elevations forced at the open boundary are retrieved from the TPXO database. So, the water levels from the regional model are nested as mean sea level, then, and the astronomic tide from TPXO is added to it? This is no clear to me. |  |
| Are the temperature and salinity daily means nested from NorKyst800 daily means of vertical profiles, or daily means of depth averaged quantities? If stratification is strong at the location of the open boundary, I would expect that imposing the right vertical profiles at the boundary can have significant impact on the baroclinic flow throughout the domain. |  |
| Section 3, lines 28 to 31: Why are two different datasets (TPXO Atlantic database and TPXO atlas respectively) used for the two fjord models Oslofjord and Saltfjord? Specially for the Saltfjord model which has a very short open boundary, the use of a even lower resolution boundary forcing data(1/4 degrees) seems inadequate to me. |  |
| Section 4, line 25. How many components are used for the analysis with T-Tide? |  |
| Section 5, line 10: Here it is mentioned that table 2 shows that the tides, once corrected at the boundary, are distributed as intended in the inner parts of the fjord. An improvement at the open boundary forcing is expected to have a positive effect throughout the domain, but the quality of the tidal representation away from the boundary and especially in shallower waters will also strongly depend on the representation of the propagation, which is likely to be highly influenced by bottom friction, and on the representation of the shallow water constituents (e.g. quarter diurnal). The latter ones specially show quite a poor representation in Oscarsborg, but also diurnal constituents like K1 and P1. I think a discussion is needed on the factors affecting the representation away from the boundary which are separate from the boundary forcing quality. |  |
| Section 5,line 25: Is the Figure 5 the resulting M2 amplitudes and phases once the boundary forcing is corrected?It is not clear from the text. If so, it is a good picture to show how although M2 representation at the boundary is very good, in the innermost stations representation deteriorates. |  |
| Section 5, table 1: I think it is worth discussing how this table shows the big correction factors that had to be used in shallow water constituents, and therefore showing the very poor representation of these by the global forcing and the need for the correction. Maybe also discuss it for the other frequencies. |  |
| Section 5, page 10, lines 21 to 28: For assessing the effectiveness of the method (which focuses on tide) on currents, I would focus on a period of the measurements in which there is no surge event if possible, since as you mention other effects dominate on currents during these events, and the depth averaged current doesn't look like a tidal signal any more. It would be also useful to show some RMSE values to assess the quality, and have a discussion on the possible sources of the resulting errors. |  |
| Figure 9: Is this showing depth averaged currents subjected to tidal analysis for both model and observations? It is clear in the text and description of the figure that this is the case for the modeled timeseries, but not for the observations. |  |
| GENERAL COMMENTS:  I am missing more discussion on the limitations of the method and other sources of modelling errors that should be take into account on the interpretation of the results and impact of the method (like misrepresentation of the propagation from boundary to inner parts of the fjord). For the presentation of the tables with amplitude and phase per constituent for the 2 runs, I would use amplitude error and phase error instead to show how the error reduces (or not) from Run 1 to Run 2. |  |

|  |  |
| --- | --- |
| Second reviewer | Our comments |
| In this study a new method is proposed to improve tidal predictions in fjords by applying corrections to the open boundary conditions. Contrary to more complex and CPU time-consuming data techniques such as data assimilation, the method has the advantage that it is much easier to implement in currently available models. However, after reading the manuscript, I was not really convinced that the proposed method really improves the quality of the predictions. I have also some doubts about the usefulness of the method. Much improvement could be made if more details are provided about the validation and additional simulations are performed. Detailed comments are given below. I can therefore only recommend publication if the authors are prepared to make a major revision. |  |
| A major shortcoming is that the method for correcting the amplitudes and phases at the open boundaries is based on harmonic data from one station only. In the case of the Oslo fjord there are two stations (Viker and Helgeroa) available. It would be better, in my opinion, to obtain the harmonic corrections from e.g. a least-squares fitting using the data from the two stations.  Not unexpectedly, the method performs well at the Viker station where the corrections have been derived. No validation is given for the other stations. There is therefore no evidence that the proposed method improves model performance further away.  The reason why the Viker station is selected for making the corrections and not the other one (Helgeroa) is unclear to me. To show that model results do not largely depend on data location, an additional simulation should, at least, be performed using corrections from the Helgeroa station and validated for the same stations used in the previous runs. |  |
| Additional figures showing differences in amplitudes and phases between the uncorrected and corrected runs, are needed to see the impact of the open boundary corrections on tidal prediction over the whole domain. |  |
| The study for the Saltfjord does not seem to be very relevant as no data are available inside the fjord. The only useful information could be to compare the difference between corrected and uncorrected model results inside the fjord. |  |
| Contrary to most commonly used tidal models, the ROMS model uses both elevation and current data as open boundary conditions. As no current data are available in this study, a linear dependency (presumably using the equations for a linear surface gravity wave) is taken. This neglects possibly non-linear effects such as stratification, The importance of stratification (inside the domain) is clearly observed in the vertical phase shift for the current as shown in Figure 8. Please comment. |  |
| The validation for the currents at the Filtveldt station (Table 4) shows good agreement for the amplitudes. No agreement is found at all for the phases predicted by the uncorrected and corrected runs. Is there an explanation for this ? |  |
| Minor comments: |  |
| Figures 2 and 3 are unclear and should be replaced by colored versions. |  |
| P.3, line 38: as a “nature” type. Please reformulate “nature” into something more understandable for the reader. |  |
| P.9, line 3: replace “forcin” by “forcing” |  |