Rebuttal letter

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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.

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| 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. | You are right. Water levels from the reginal model are nested as mean sea level, and then the astronomic tide from TPXO is added to it.  This is now clarified in the text. |
| 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. | The NorKyst800 daily means of vertical profiles are applied.  This is now clarified in the text. |
| 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. | Karina (etter samtale med Nils): Nils skal undersøke hvorfor vi egentlig gjorde som vi gjorde. Vi tror det var noen tekniske utfordringer som avgjorde. Vi kan kanskje argumentere med at vi ønsket å sjekke begge atlas.  (TPXO Atlantic database slutter rett nord for Saltstrømmen.)  En ide er å plotte gitteret for databasene… Dette kan jeg gjøre i romjulen. |
| Section 4, line 25. How many components are used for the analysis with T-Tide? | The ‘standard’ set of 69 components are applied in T\_tide, but only the relevant components (i.e. included in or derived from the tidal forcing) are described in the manuscript. |
| 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. | Surely, the quality of the tidal representation away from the boundary depends not only on the tidal forcing, but also strongly on the representation of the propagation, which is highly influenced by the bottom topography. The diurnal components improve for Oscarsborg, but the diurnal and the quarter diurnal are expected to be more sensitive to the representation of the propagation. In order to improve the diurnal and quarter diurnal components, a better representation of the propagation is needed, which is not the scope of this study.  A short discussion on this matter is now included in the text. (Karina: ikke gjort enda) |
| 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. | Figure 5 is the resulting M2 amplitudes and phases once the boundary forcing is corrected (run 2).  This is now clarified in the text. |
| 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. | Good point. A short discussion is included. (Karina: ikke gjort enda) |
| 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. | Karina: Å bruke en “surge event” innbiller jeg meg ikke har noe å si, men kanskje snarere en styrke? Vi drar jo uansett bare ut tidevannsbidraget og endrer det. At metoden er robust nok til å takle en “surge event”, ser jeg på som en fordel fordi vi uansett må hanskes med andre effekter. Mulig vi må fokusere mer på tidevannsbidraget før og etter. Og kanskje regne ut noen RMS’er. Hva tenker dere? |
| 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. | The modelled depth averaged time series are constructed based on the tidal analysis. The observed depth averaged time series are calculated directly from the observations.  This is now clarified in the text. |
| 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). | Karina: Her bør vi legge til noe mer diskusjon. Vi har begrensning ved at metoden fungerer og er testet på fjorder med small åpning og tidevannsbølgen bortimot vinkelrett på åpningen, men vi kan ikke si noe om andre tilfeller.  Dessuten bør vi skille mer mellom å korrigere tidevannet ved randen og det at tidevannsbølgen propagerer riktig innover i modellområdet. Det siste har vi ikke sett på og det er heller ikke hensikten å gjøre. Det bør likevel kommenteres. |
| 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. | Karina: Usikker på om jeg liker denne måten å vise resulatene her. Hva synes dere andre? |

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| 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. | Karina: Igjen har vi to utfordringer. En ting er å bedre tidevannspådraget. En annen hvordan tidevannsbølgen propagerer i modellområdet. Det siste har vi ikke sett på. Og jeg innbiller meg at det er dette som gjør at han ikke blir overbevist.  Han ønsker tilsynelatende flere simuleringer, men jeg ser ikke hvilke han er ute etter. |
| 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. | Karina: Vi vurderte å bruke bade Helgeroa og Viker, men forskjellen mellom disse var så liten, at det ikke var hensiktsmessig. Dette kan jeg skrive litt om i romjulen når jeg får tid til å grave i analysene jeg gjorde den gangen. Det bør ikke være vanskelig å argumentere for. |
| 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. | Karina: Usikker på hva slags figurer han er ute etter her. Vi har tabeller som viser amplitude og fase. |
| 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. | Karina (etter samntale med Nils): Vi ønsket å vise metoden brukt på et annet område og at det har impact på resultatene. At målinger mangler, er en svakhet. |
| 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. | Karina: Denne skjønte jeg ikke. Strømmen i tidevannspådraget er dybdeintegrert og innholder derfor ingen info om lagdelingen. Derimot innføres lagdeling fra NorKyst-data’en. Og vi legger inn begge deler. |
| 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 ? | The currents are in general difficult to predict in shallow waters due to spatiotemporal challenges. The fact that the amplitudes shows good agreement is actually surprising. Local features such as eddies may cause problems with phases.  A short comment is added in the text. (Karina: ikke gjort enda) |
| Minor comments: |  |
| Figures 2 and 3 are unclear and should be replaced by colored versions. | Karina: Dette fikser jeg i romjulen |
| P.3, line 38: as a “nature” type. Please reformulate “nature” into something more understandable for the reader. | “Nature type” is now exchanged by “habitat” |
| P.9, line 3: replace “forcin” by “forcing” | Corrected |