Reply to comments from Referee 3 (R3) on: A tidally driven estuary close to an amphidromy by Sissal Vágsheyg Erenbjerg et al.

We are of course sorry about the overall verdict (rejection) of R3. We still feel that the results of our study are sufficiently special and interesting to justify publication in OS, especially the fortnightly variation in net flow through this strait and its effect on the fjord-like circulation. We acknowledge, however, that this was not well emphasized in the original text and that one might have to read the manuscript very thoroughly to get the full impact. In the revised version, we have tried to rectify this and both the abstract and Introduction are more or less completely re-written. In spite of the overall verdict, R3 has several constructive and useful comments, for which we thank her/him.

General responses to comments from all or two of the referees

In the reviews from the three referees, there are a number of points addressed by all or two of them. They necessitated comprehensive revisions of the manuscript and, here, we give a general overview of these points and our responses to them. For more specific comments from R3, see below.

1. One of these points is our use of the term “estuary”, criticized by all the referees. We have followed the recommendation of R1 and R2 to use “strait” throughout the manuscript, instead. We still feel that this strait in many ways behaves like an estuary, but we acknowledge that this was badly motivated, especially in the Introduction. In the revised version, this question is now addressed more thoroughly in the Introduction. Other points of criticism were a too superficial treatment of the tides and also our lack of clearly stated objectives.

2. To address these points, we have re-written the Introduction completely. There, we now emphasize that the freshwater supply is sufficient to lower the salinity appreciably and that the cross-sectional area of the southern sill is so small that it only allows slightly less than half of the water entering the strait across the northern sill during flood to pass through the strait, on average. This makes the strait behave much like an estuary and motivates the new title in the revised version: “A tidally driven fjord-like strait close to an amphidromic region”.

3. In the new Introduction, we also address the tidal regime more comprehensively, referring to a supplementary figure with maps of the amplitudes of the main semidiurnal and diurnal tidal constituents, based on the parent (800 m) model. We stress that the amphidromic character of the region south of the strait includes the four dominant semidiurnal and to some extent also the two dominant diurnal constituents.

4. In the literature, we have not found any water body that shares this combination of fjord-like topography (sills) and competition between freshwater and tidal forcing. In the new Introduction, we argue that this justifies a closer study even though this strait is small compared to most better-known straits. Based on this motivation, we have re-phrased the objectives and methodology of the study, hopefully to be clearer.

5. Another common point of criticism from all of the referees was in regard to model validation. We have now added a new section comparing the characteristics of the main tidal constituents as measured at two locations on either side of the strait with those in the parent (800 m) model (the southern location is not within the domain of the high-resolution model). The comparison (including the new Table 1) verifies that the parent model reproduces the dominant tidal characteristics fairly well. We have also added a new supplementary figure with Hovmøller diagrams comparing simulated velocities in the strait with those measured by ADCPs to compare velocity profiles at intra-tidal time scales as requested by all of the referees.

6. We also acknowledge that the lack of hydrographic observations during the modelling period and constancy of freshwater supply in the model make our attempt at validation of salinity fields in the model rather unrealistic. We have therefore moved the old Fig. 4 to the supplement and modified the text on this matter. Following the recommendation from R1, we have furthermore moved model validation from being a separate section (old Sect. 3) to a subsection in Sect. 2.

7. As motivated in the new Introduction, we feel that the special features of this strait distinguish it from the typical strait and make it worth a study. In our opinion, the main result of the study is, however, the long-period (fortnightly and monthly) variation of the daily-averaged (25 hour) net flow through the strait, which changes systematically between northward and southward flow with periods on these time scales. When combined with the abovementioned special features, this example of long-period tidal forcing is to our knowledge sufficiently unique to justify publication in OS. Unfortunately, we have to acknowledge that we did not discuss or emphasize this message adequately. In the revised version, we have exchanged old Fig. 10 with a new figure (new Fig. 9) that better documents that this feature is not an artefact of the model, but is also to be found in the measured sea level data. We have tried to clarify this point in the new Results and Discussion sections, we have re-written the abstract to more clearly emphasize the results of the study (as recommended by R1), and we have converted the Recommendations section to a “Conclusions and Recommendations” section (as recommended by R1).

Specific responses to comments from R3

Comment: I am irritated about denoting the sound between the two islands as an estuary although you even write in the introduction that this is not an estuary in the classical sense. It would be better to call this here as a "narrow sound with strong freshwater run-off" or so.
Reply: We now use the term "strait" throughout as suggested by R1 and R2 and have changed the title (see General Responses Bullet points 1 and 2, above).

Comment: Here the major runoff (from a hydro power station) occurs at the open end of the sound and thus it can be expected that the general behaviour is much different than in an estuary where the freshwater run-off occurs at the closed end.
Reply: As we have now stressed in the new Introduction, the naturally occurring freshwater runoff (excluding the contribution from the hydropower plant) is sufficient to lower the salinity appreciably and give a salinity distribution that looks quite fjord-like (e.g., the old supplementary Fig. S3).

Comment: In fjords with sills, one of the major topic is the ventilation and renewal of the deep water. Since we have here a sound that is bounded by two narrow straits with sills, there is a large body of deep water residing near the bottom. To my opinion, it is a major limitation of this study that this topic is not discussed.
Reply: We agree that a longer simulation period would have been preferable, but the computing resources available to the study were limited and a study of deep-water stagnation would have had to include a substantially longer simulation period.

Comment: What are the deep water renewal processes? Is it tides or wind or surges? How often does it happen? The model system used here should be able to reproduce those dense water overflows.
Reply: We have tried to address this topic and noted that this process also is affected by the fortnightly variations, see old Figs. 8 and 9, but this should have been better emphasized in the text, which it hopefully is in the revised version.

Comment: Just initialiing the salinity and temperature fields at some instant of time and simulating for a short period might completely miss the dynamics. Here, just one day is used to let the model adjust to the initial fields, a time span that should be by far shorter than the deep water renewal time.
Reply: The 32m model is nested within a 160m model, which was run for two weeks before start of the 32m model and the parent (800m) model was run for four weeks before the start of the 160m model. The 160m model has many points within the estuary and we expect both upper and deeper layers to be approximately spun-up by the start of the 32m model. Also, no spin-up effects are seen in the 32m model. This has now been better explained in the revised version.

Comment: As for the validation, the results are very poor. Tidally resolved velocity measurements are not compared to model results, and the comparison between simulated and observed residual velocity profiles is very bad. Salinity observations are not available during the simulation period. A comparison to observed salinity profiles obtained during several other years is made, shows big differences to the model results and a high variability (and makes no sense anyway).
Reply: We now have included validation of tidal constituents and added a supplementary figure showing tidally resolved velocities, but the lack of observed near-surface velocities (which are stronger than the deep currents) makes this less interesting. We have also modified the text on salinity validation to admit that we do not have the data to do such a validation satisfactorily (see General Responses Bullet points 5 and 6, above).

Comment: With this, the model results are nit validated at all, and do probably not reflect the dynamics of the sound under consideration.
Reply: This is in our opinion an exaggeration. Although not perfect, some of the relationships in the old Fig. 3 are sufficiently significant to support the model results. With the added content in the revised version, this is strengthened.

Comment: The paper is lacking motivation. In the introduction, a clear scientific problem needs to be presented on the background of the state of the art. Here, however, very little state of the art is given, a problem is not clearly identified and hypotheses are not offered.
Reply: We agree that the Introduction (and abstract) were not sufficiently informative and concise. This is hopefully better in the revised version.

Comment: 3: Specify for which partial tide you have the amphidromic region. You probably mean the M2 tide, but please specify.
Reply: In the Introduction, we now emphasize that the amphidromic character applies for all (four) the dominant semidiurnal constituents and also to the two dominant diurnal constituents at least partly. We have also added a supplementary figure to illustrate this (see General Responses Bullet point 3, above)..

Comment: 5: I would prefer “volume transport”, because I think “flux” is reserved for “transport per unit area”.
Reply: “Volume flux” has been replaced by “Volume transport” throughout the manuscript.

Comment: 9: How can you verify transports with sea level observations?
Reply: The original text did say that (modeled) “variations in sea level differences” (not transports) were verified by observed sea level variations.

Comment: 10/11: reformulate this as a sentence.
Reply: This text has been modified in the new abstract.

Comment: 26: “reducing the runoff into the southern part of the sound while the northern part has received more freshwater”. What is the mechanism here and how do you know?
Reply: The water supply to the hydropower plant is partly through tunnels that redirect water that would have gone into the southern part so that it enters the northern part instead.

Comment: 29/30: typo “estaury”, here and at many other locations.
Reply: Corrected.

Comment: 37/38: “when the circulation is more similar to that of a non-sill estuary”: At this point the reader has no idea of the salinity distribution in this sound. A better motivation is needed for the choice of the winter for this case study.
Reply: This statement has been removed from the revised version and the motivation clarified.

Comment: 39-45: The review article by Farmer and Freeland does actually discuss tides as an important process of fjord dynamics (see their section 4).
Reply: We no longer refer to Farmer and Freeland in the revised version.

Comment: 48-49: You can also have “a strong periodically varying barotropic pressure gradient through the estuary” when the amphidromic points are far away. So, at this point I do not see any special influence of the proximity of the amphidromic point apart from the fact that the M2 tide is weak.
Reply: This is correct, but in our case, it is the proximity of the amphidromic region that creates the large sea level differences between both ends of the strait and the strong tidal currents.

Comment: 56-61: I would move this paragraph to the “Materials” section, since the introduction should serve more general purposes and introduce the problem, give hypotheses, etc.
Reply: Since the model is the main method used in this study, we feel that it should be mentioned in the Introduction, although we have moved some of the details to Sect. 2.

Comment: 64/65: “One aim of this study was therefore to validate the model against these observations.”: This is not a sufficient aim for a study to be published in a peer-reviewed international journal. Also the next sentence is not sufficient as motivation.
Reply: We have now emphasized that model validation is a secondary aim of the study. The main aim and its motivation should also be clearer now.

Comment: 82/83: Could you also give the runoff in m3/s which is more common.
Reply: has been done.

Comment: 83: What do you mean with constant daily run-off? I suppose that the run-off has to be given a every barotropic model time step which is much shorter than one day.
Reply: The text has been clarified.

Comment: 84/85: Not clear how the spin-up of the model can be as short as one day. How are the initial conditions for the high-resolution simulation been initiated? I guess from the level-2 nest. This needs to be explained. Since the residence time of the deep water in the sound must be much longer than one day, I wonder how good the quality of the initial condition is. Have they been validated by observations?
Reply: As explained in the response to a previous comment, the parent models have been spun up over much longer time and no spin-up effects were seen in the run of the 32m model. Unfortunately, we do not have observations for validating the initial conditions.

Comment: 96-113: I do not see any agreement between observed and simulated velocity profiles. The model results show a residual flow that is directed northwards, but the observations do not show that at all. I find it also strange to report on a study of tidal flow, have tidal flow observations at hand, but state that "a model-observation comparison of instantaneous velocities is not very meaningful". The key issue in tidal simulations is to reproduce tidal phases and amplitudes. This requirement is not met here.
Reply: Our intention with the quoted sentence was to emphasize that a point-to-point correspondence between model and observations requires that the phases of the tidal constituents are accurately simulated, whereas this may not be necessary to simulate the processes in the model adequately (except for the exact timing). But, we agree that this was not well phrased and this text has been modified. We also now include validation of tidal constituents (new Table 1) and have added a figure with Hovmøller diagrams (see General Responses Bullet point 5, above).

Comment: 115-130: Simulated salinity is here compared to observations that have been made outside the simulation period. Since salinity at the bottom should vary substantially with deep water renewal events, any similarity between observed and simulated salinity would be pure random. With this, no validation of the salinity field has been made. I wonder, if the bottom-mounted ADCP's should have included a CTD such that at least bottom salinity and temperature could be validated.
Reply: The salinity observations used for validation were from winter (see caption for old Fig. 4) and during that season the deep water renewal is continuous (not in events) on daily time scales or longer although variable (e.g., old Fig. 12), but we agree that our salinity validation lacks observational data to be satisfactory.

Comment: I am stopping here with my detailed review, since I do not think that it makes sense to deeply analyse results of a non-validated model.
Reply: As previously mentioned, we find this to be an exaggeration. We also note that there are many model studies to be found in the literature with little validation because adequate observations are not available, as is the case for our study.