Reply to comments from Referee 2 (R2) on: A tidally driven estuary close to an amphidromy by Sissal Vágsheyg Erenbjerg et al.

We thank the referee for positive and constructive comments.

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 R2, 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 R2

Comment: not clear how geographical names are to be spelled - please provide phonetic translation; similar comments apply to maps (Fig. 1) where mesh indices are shown instead of coordinates in meters: please present information such that it is easily accessible to be memorised and interpreted by the average reader
Reply: We have added phonetic translation for the relevant local names. We have also replaced the mesh indices (grid numbers) in Fig. 1 and elsewhere by geographical distance.

Comment: in the Introduction the study area is presented as an estuary or fjord, i.e. a land-ocean transition space, but obviously it is an ocean strait.
Reply: "Estuary" has been replaced by "strait" (see General Responses Bullet point 1, above).

Comment: Here a decent review on circulation in ocean straits is imperative, for example Danish straits and Bosporus (amongst others) have been studied well: Identify the knowledge that can be transferred from other straits to the local strait, identify the knowledge gaps and say how the gaps shall be closed using the methodology of this study.
Reply: In the revised version, we now refer to the review paper on shallow straits by Li et al. (2015) and list several features that distinguish our area from a typical strait (see General Responses Bullet points 2, 3, 4).

Comment: Model area, model validation: Why is model area so small? This creates several problems: As water level differences are substantial for the conclusions of the study, the model area should include both gauges shown in Fig. 1. Alternatively authors could validate the parent model against these gauges.
Reply: Doing a model study will always be a delicate balance between available computing resources, time and resolution. The present study is part of a PhD-project with main workplace in the Faroes, degree-giving university in Copenhagen, Denmark, while computations were run in Bergen, Norway, as in-kind contribution. This has put severe restrictions on computing resources. The model domain described here also has a very high resolution of 32m x 32m and even though the area covered is rather small we still have 785 x 185 horizontal grid points with 35 layers in the vertical. We have followed the advice and added a subsection on tidal validation of parent (800 m) model versus tide gauges (new Table 1).

Comment: Salinity validation reveals the model is too mixed - this hints at underestimated exchange flow/density driven circulation - here the area outside the sills could be crucial but it is excluded from the model which could be a serious dynamical flaw.
Reply: We agree that the salinity variation is underestimated by the model and too strong mixing in the model may be one reason. The large discrepancies are, however, characterized by the observed CTD profiles showing large amounts of freshwater mixed down to depths 10-30 m. This may be caused by storms that induce a lot of runoff combined with strong mixing. Since the model assumed constant

runoff, we cannot expect it to catch these events, but this ought to have been better explained in the text. We have now acknowledged that data does not really allow us to validate the salinity variations in the model, moved Fig. 4 to the supplement, and modified the text (see General Responses Bullet point 6, above).

Comment: Although tides are important the validation considers daily scale which does not make sense. Sub-tidal flows are usually a function of both overtides and density driven flows - it would make sense to start validation at the intra-tidal scale.
Reply: We have now added validation of the tidal constituents in the model (new Table 1). We have also added two figures with juxtaposed Hovmöller diagrams from ADCP and model to the Supplement and have clarified the text in the revised manuscript (see General Responses Bullet point 5, above).

Comment: Density inversion in a hydrostatic model - how is this possible?
Reply: Apparently, high-density/salinity water crosses the northern sill and flows southward where it may pass over less dense water. Rapid fluctuations of denser water above slightly lighter water then occur, and since we apply a model using the hydrostatic assumption, we will not be able to reproduce this in detail. However, the inflow of denser water from the north is probable, but in our simulations, the internal vertical mixing and turbulence parameterization will homogenize such waters leaving the southern part of the fjord system in a hydrostatic balance.

Comment: ll15 "results from a model" from a model simulation
Reply: The text has been modified

Comment: ll18 a sill is an elevation, maybe say depth is 4 m at the sill
Reply: Done

Comment: ll27 how large?
Reply: The Introduction has been changed so that this is no longer applicable (see General Responses Bullet point 2, above).

Comment: ll37-38 why study winter conditions when for the Stakeholders (aqua farms etc) summer dynamics are more relevant?
Reply: We would have preferred to have a full year of simulation as this would help us describe the annual cycle of water exchange in the fjord. Unfortunately, the access to computational resources was quite limited. We therefore started with the winter or more "normal" conditions to get a picture of the best case scenarios of water exchange in the fjord. In the new Introduction, this is hopefully better motivated. The aquaculture farms will also benefit from better information about flow during the winter, especially as regards transmission of parasites (salmon lice) between farms. Here, the suggested long-period (fortnightly and monthly) variations of net flow may perhaps be developed into a useful management tool.

Comment: ll62-66 Please provide a consistent description of the aims of the study, list the research questions
Reply: This is hopefully better in the revised version (see General Responses Bullet point 4, above).

Comment: Section 3: Why not use the ADCP data to illustrate intra-tidal dynamics, validate simulated currents?
Reply: Is now done in the new Table 1 and a new supplementary figure (see General Responses Bullet point 5, above).

Comment: In estuaries per definition river flow affects the salinity field, and Fig. 4 shows that freshwater input is probably significant
Reply: It is not quite clear to us, what the referee intended with this comment, but Fig. 4 has in any case been moved to the supplement (see General Responses Bullet point 6, above).

Comment: Fig. 2a: what is the data basis for the red and blue colors - model or observations? Specify in caption.
Reply: Done

Comment: Fig. 3: Consider illustration and validation on intra-tidal scales...consider showing these numbers in a table instead
Reply: Validation of tidal constituents and intra-tidal variations has been added (see General Responses Bullet point 5, above). Originally, we also tried to put the information in Fig. 3 into a table, rather than a figure, but found it difficult to show the same overall information.

Comment: ll247 no hydrographic observations during the simulation period" - what about using climatological data
Reply: This is what we tried to do in Fig. 4, although we admit that our climatological data set is not perfect. For the parent (800 m) model, a fairly comprehensive comparison was made between observed and modeled hydrography in a previously published manuscript. This ought to have been referred to in this text and we have now done that.

Comment: ll291 please specify what is meant by highly non-linear flows.
Reply: This was not well phrased. The sentence is hopefully clearer in the revised manuscript.

Comment: tidally-rectified currents: probably a tidal analysis of ADCP data and model results can be very helpful in this case
Reply: This explanation referred to processes outside the model domain, but we agree that it was probably too speculative and we have deleted this paragraph.