Ocean Sci. Discuss., referee comment RC3
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Comment on os-2021-23

Anonymous Referee #3

Referee comment on "A tidally driven fjord-like strait close to an amphidromic region" by Sissal Vágsheyg Erenbjerg et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-23-RC3, 2021

General comments

This manuscript reports about model simulations of the hydrodynamics in a narrow and deep sound situated between two islands of the Faroes. The sound is bounded by narrow and shallow sills at both sides. Tidal flow is complex due to the proximity of an amphidromic point for the M2 tide.

This study is highly site-specific, and for an article in general journal of "Ocean Science" it would be required to better motivate the study and to derive generalisations to other flow situations.

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. 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.

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. 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. Just initialising 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.

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). With this, the model results are nit validated at all, and do probably not reflect the dynamics of the sound under consideration.

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.

Altogether, a non-validated model is used that obviously does not reproduce the hydrodynamic regime. The key question of the regime (deep water renewal) is not addressed. The authors admit that the data situation is poor for the sound and they "recommend that a more targeted field experiment is implemented that can reveal more of the strengths and weaknesses of the model". I agree with this statement and do additionally recommend to extend the simulation period to reproduce deep water renewal events. It would be good to deploy long-term moorings including CTDs and ADCPs that can monitor such inflows events.

Therefore, I recommend to reject this manuscript at the present time and repeat the model study once a better data situation is given.

Specific comments

3: Specify for which partial tide you have the amphidromic region. You probably mean the M2 tide, but please specify.

5: I would prefer "volume transport", because I think "flux" is reserved for "transport per unit area".

9: How can you verify transports with sea level observations?

10/11: reformulate this as a sentence.

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?

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

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.

39-45: The review article by Farmer and Freeland does actually discuss tides as an important process of fjord dynamics (see their section 4).

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.

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.

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.

82/83: Could you also give the runoff in m3/s which is more common.

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.

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?

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.

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.

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.