Ocean Sci. Discuss., referee comment RC1
https://doi.org/10.5194/os-2021-23-RC1, 2021

Comment on os-2021-23

Manuel Diez-Minguito (Referee)

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-RC1, 2021

This manuscript addesses tidal propagation and subtidal circulation in a fjord-like strait. Authors adopted a modeling approach to estimate flow and salinity fields. The proximity to an amphidromic system induces fortnightly variations in sea level at both ends. Subtidal variations in exchange flows are attributed to fortnightly variations in the barotropic pressure gradient.

I liked the topic and I enjoy readying the manuscript. I think the topic is appealing and of interest to OS readers. The manuscript have the potential to become a relevant contribution, but model output analysis and discussion is quite superficial in many instances (e.g. statements about mixing/stratification). Authors have a powerfull tool, which is ROMS, but my feeling is that they do not take all the advantage of it.

The manuscript is mostly well organized, altough it is not terribly well written. Objectives could be better stated. The edition and selection of figures could be much improved. They sometimes fail to support rightly the story the authors want to tell.

Overall, a substantial revision is needed before I can recommend publication.

See specific comments below.

Abstract

I rather focus the abstract on physics, processes, etc. more than the model implementation itself. (The same occurs elsewhere in the manuscript, mainly in introduction, discussion and conclusions.)

L1. 'describes the implementation'. I think this manuscript does more than that.

L2. Not sure to call this 'estuary', as it is not semi-enclosed body of water. It wouldn't be more like a 'strait'?
L9. 'Surprising'. I suggest to ommit this kind of valorative adjectives. (here and elsewhere)
L1 and L12. Rephrase "describes the implementation...". "We recommend that..."

Introduction
L29. 'estaury'. Correct here and elsewhere (L30, L38,...)
L30. Please indicate in which ways.
L37-38. Please describe what are "normal conditions" are. Are those described in the next paragraph? What is "non-sill estuary circulation"?
L46. Perhaps would be convenient to show the location of the amphidromic point in a Figure.
L50. "To understand how these various forcing mechanisms affect mixing and circulation within and out of the estuary" This is the objective, am I right? Please state it clearly. The use of one numerical model or another is part of the methodology.
L60-61. Is this also part of the objectives? General objective, perhaps?
L50-66. I suggest the authors to reorganize this part of the introduction. General and specific objectives/aims should be clearly and logically stated. And then, describe how are they addressed (methods, numerical model).

The Model
L83. I do not understand this. Does this mean that the daily input to ROMS is 1.7e+8/365 m3/day and 6.3e+7/365 m3/day?
L85. Is it enough one day to spin-up the model? How do you determine that?

I suggest the authors to move Section 3 to Section 2.1. After all, model calibration and validation is part of the set-up process.
L98. Averages for the whole sampling period? Monthly? Please indicate the time span in which averaged are performed. Also in Figure 2 and 3.
L101-102. Please explain briefly the 'modeling details'.
Please consider to show one or two panels that show time series of observations confronted to model output.

Last two paragraphs in section 3.2 may shed doubts if the model is correctly validated in terms of salinity. Should not salinity variations in the shelf and freshwater discharges time series be included as input in the model?

Model results
L132-134. Move these lines to methods part.
L135-. Too late, I think. Please mention this or move the paragraph above.
L140. I think Results section should start here.
L142. I guess qN and qN are cross-sectionally integrated. Please indicate. Are tidal variations in the sea level (elevations) also considered or only the mean cross-section?
L146. How the one-hour lag compares with the tidal wave celerity?
L147. Not always. There are some intervals in which the volume flux is unidirectional both

during ebb and flood.
L159-161. Table 1. The cross-correlation between qS and qN has a 1 hour lag. If the barotropic pressure gradient is driving the volume flux, why there is no lag in the cross-correlation between qS and (hN-hS)? Any clue? Perhaps there is a baroclinic mode which is also driving the net volume flux in the estuary?
L165. 'Speed' vS. Cross-sectionaly averaged velocity?
L167. Assuming that there is no correlation between A and vS.
L169. It would be nice to test the sensitivity of the fit to the locations of hS and hN.
L170. I don't understand. Friction is always there. You mean when friction is included in Bernoulli equation?
L175. This is quite usual in many estuaries.
Figure 7. Define the daily averages <...> in the main text. is consistently lower in magnitud than . Why? I suggest to plot in the same panel qN, qS, hN, hS and their differences. It seems that and are slightly out of phase?
Figure 8. Indicate that the horizontal axes is along-estuary. Label northern and southern sills (or simply S and N). Instead of "grid numbers", use km.
L192-193. Denser water? What are the mixing rates?
L194-196. Although it seems plausible, it would be good to provide mixing rates to support this statement.
L201-202. Please clarify this. Shouldn't be a sign of this also in the salinity color map?
L187-207. Overall, there are some unsupported statements here. In my opinion, authors' arguments on mixing, vertical water movements, effect of discharges, etc. do not seem to be supported by any data. Could the authors provide additional numerical evidences? (mixing rates, vertical and lateral velocities?)
Figure 9. Is this figure really necessary?
L212-214. Both time series can be corrected for the barometric effect.
Figure 10. I guess panel a conveys the same information as correlations in Table 1. I suggest to remove panel a. (or substitute Table 1 by lagged cross-correlation plots)
L223-241. This is interesting, but I think it deserves to be much more elaborated. At which depths occur the inversions? Above or below the picnocline? More reliable/informative (but still simple) measurements for stratification could be Brunt-Vaisala frequency or potential energy anomaly. How they would compare with the turbulent kinetic energy? How much mixing induce then these inversions? Do you think this inversions could have a (baroclinic) influence on the net exchange flow?

Discussion
L247. Please discuss the consequences of it.
L253. As I mentioned above, it would be nice to see where the amphidromic point is. Also, I would expect that the tidal wave propagates faster around Faroe Islands than through the fjord. Is it so? If yes, there would be probably a superposition of both waves at a certain location near the south sill. Have you looked into this?
L257-258. I think the authors analysis here is based on the Bernoulli equation. If I'm not wrong, the determination of gamma only indicates that friction is overall important, but not that friction occurs mainly in the southern sill. Could you elaborate more on that, please?
L258-260. It is always so, I guess. Not sure what the reader should conclude from this statement...
L265. If the amphidromic point is located near Tórshavn, why the tidal wave enters the estuary from the north (L252)? Notice that this is about timing, not amplitudes.
L265-269. For me this is something that should have been mentioned in the introduction, not in the discussion section. This would have helped to better undertand the qN and qS variability.
L276-285. A harmonic analysis of sea levels could shed some light on this. Probably a mutual non-linear interaction between M2 and S2 produces a fortnightly compound tide

MSf, which is (almost) in phase with the spring-neap tidal cycle.
L290. Again, computation of mixing rates and TKE along the estuary (or better computation of terms of the momentum) could support this.
L311-317. I think this would be easy to check out by comparing (observed or modelled) elevations and currents at both sills.
L332. Figure 12. Nice figure!
L338. What is then the flushing time estimated from this? (The same in L341)
L344. There is no two layer circulation in the southern sill. However, the two layer circulation in the northern sill persists during both periods, altough it varies in magnitude. I think this is remarkable. It would be very interesting to comment a bit more on this. Is there any sign of this "fortnightly pumping" in the shelf, out of the estuary? Or inside the estuary, in its deep waters? Maybe because of this "fortnightly pumping", stagnant conditions are not observed during winter? How is the circulation during summer? Is this pumping effect also present when stagnant conditions near the bottom are observed? Are there other fjords or straits that show similar circulation patterns? BTW, please consider to put the study in a wider (global) context. Most references are local.
L351-. Again I find mixing discussions somehow "loose"

Recommendations
Why not Conclusions instead of recommendations? I suggest to rewrite Section 6 to frame it properly as Conclusions.