Response to Reviewer 3

August 29, 2023

In this response, we give short replies to the reviewer's comments and suggestions. The reviewer's points 1, 2, 5(i), 5(ii), 7, 9, 10, 11, 12, 13, 14, 15, 16, 20, 22, 23, 24, 25, 26, 27, 28(i) and 30 point to typos and suggested figure improvements. These will all be addressed in the revised version of the manuscript, but we will not address them specifically here.

3. It is referenced to a small degree in the discussion, but I think it would be nice if the authors could add greater discussion surrounding i) the choice of running the model in 2D, and ii) how the results of a 3-D simulation might differ.

Why did we choose to write the paper based on results from a 2D model?

- The velocity signal from the ADCP followed tidal frequencies closely, and with only a relatively weak depth dependence. These indicate that the flow is governed by sea surface elevation induced pressure gradients, i.e. barotropic dynamics.
- Having identified that the flow was quite barotropic, the cheapest (and simplest) next step is to use a 2D approach.
- After tuning the 2D model's horizontal diffusion for fit with the observations, the velocity signal matched the observations well.
- With the choice of parameters yielding best fit with the observations, the dynamical evolution in the channel did not resemble dynamical explanations we could find in the literature (see, e.g. Hench and Luettich (2003) – even though or fundamental assumptions were the same.

Hence; The observations motivated the use of a simple 2D approach. The results from the 2D model motivated a thorough analysis of the dynamical evolution, since the results differed significantly from previous similar studies. Presenting an alternative view of the fundamental dynamics using mathematically similar 2D barotopic dynamics felt like a logical step forward. Using a full 3D barotropic simulation would have introduced other uncertainties such as whether the model had sufficient vertical resolution, if the horizontal viscosity could be tuned to sufficiently low levels and retain numerical stability. We will add a truncated version of this to the discussion in the revised version of the manuscript.

A full 3D simulation is an obvious next step. It will open for assessments of processes we could not account for in our simplified setup may influence the evolution of eddy dipoles in tidal channels. We expect that a 3D simulation will differ from the simplified 2D view in several ways

- A boundary layer near the bottom represents a less direct energy loss from the bulk of the water column to bottom friction than implicitly assumed in a 2D model.

- A vertically sheared flow. We expect a mass flux toward the centre of the eddies near the bottom and away from the centre elsewhere – influencing the eddies longevity.
- Hydraulic jumps in stratified runs, consuming a significant portion of the energy provided to the flow by the tide rendering it unavailable for the eddies.

However, we speculate that such 3D effects, such as the barotropic effects reported by Albagnac et al. (2014), primarily influences the eddies' longevity on timescales greater than those we explore in this study (>1 hour). Furthermore, whether the physical interpretation presented herein will work for a 3D setting with the addition of realistic atmospheric forcing (surface winds and waves) would be speculative to comment on at the time of writing. We will further develop the "limitations of this study" section in the manuscript with this in mind.
However, our model results demonstrate that at this *specific* location, the 2D mode of the system dominates. We speculate that a full 3D model would not add processes that would significantly alter the flow for this specific case, and our conclusions regarding the temporal development of dynamical processes at the constriction.

4. L87: "the model performed better". Please elaborate on what 'better' means/how it was quantified.
"Better" in that sentence means that the tidal signal was in closer agreement with observations when forcing the model with TPXO7.2 rather than with the more recent version, TPXO9-atlas-v5. The modeled flow was too strong in Tromsøysund when using the TPXO9-atlas-v5 as forcing, and the sea surface elevation was out of phase with the tidal current. In contrast, the observations indicate that the flow and sea surface elevation are in phase (max flow speed at max tidal surface elevation). This likely shows that the tidal dynamics in the Tromsøysund are very sensitive to intricacies with the external forcing. The fjord connecting to Tromsøysund, Balsfjord, is connected to the open ocean at two distinct locations – in the south at 69.57 degrees north and in the north at 70.31 degrees north, exposing it to how well the model reproduces the Kelvin wave propagating northward, and spatial variations of the boundary conditions quality. We will modify the revised text with this in mind.

6. Fig. 3, right panel: Other than the M2 tide, the constituents appear to follow more flat lines than a 1:1 slope. This is, for example, most apparent for the K1 tide. Why are you not able to capture the regional variability in the smaller-amplitude tides like you are with the M2 tide? How significant is this discrepancy for your tidal channel? (It is briefly mentioned starting on L95, but I would appreciate more analysis).
This might seem a bit cliche coming from ocean modellers, but the rather flat curve for the minor tide components may indicate a problem with the observations, not the model. We believe these plots indicate that while the rigs were deployed sufficiently long to get a good estimate of the leading harmonic (M2), roughly one month was insufficient to get a good spectral estimate for the minor components. The minor components are much weaker than the M2 tide, and are more susceptible to noise. We will discuss this in further detail in the revised version of the manuscript.

8. Fig 5: Is the 'm below sea level' relative to a mean SSH, or is it dynamic? (i.e., does the figure take tides into account). Would it make more sense to refer to height above sea floor to have a constant reference point?

The z-axis is relative to depth below mean sea surface level. The instrument collects data from bins at constant depth relative to the sea floor, hence it makes sense to present the data relative to mean SSH. We will clarify that the data is presented relative to mean SSH in the revised manuscript.

17. The jet in Fig. 9 appears to be angled toward the right. Why? Is it simply because of the Coriolis force, or are there other factors involved?
This is an interesting point which was the subject of several discussions while we wrote this paper. We left it out of the manuscript to keep our focus on the initial development of the eddies, rather than the subsequent advection of the dipoles.
In our results, the cyclonic eddies generally "live" longer than anti-cyclonic eddies. The cyclonic eddies grow to sizes much greater than the constriction, and can introduce an eastward mean flow following the coastline, influencing the drift of the dipole. Whether this is the case in Fig. 9 is unclear.
One of our hypothesis on why cyclonic eddies are predominant, builds on the observation that *both* the cyclonic and anti-cyclonic eddies have low pressures in their centres. As they grow horizontally, the centripetal force decreases. In cyclonic eddies, the Coriolis force works in the same direction as the centripetal force, which combine to balance the pressure gradient force. In anti-cyclonic eddies, however, the Coriolis force and the pressure gradient force work against the centripetal force. This *may* explain why the anti-cyclonic eddies dissipate earlier than the cyclonic ones, but we haven't looked into this in more detail, or done a proper literature search to see if others have had the same idea.

18. Figure 10, with its clear message and simplicity is beautiful!
Thank you!

19. Fig 11 and elsewhere: m/s2 are units of acceleration, not a force. Please clarify terminology.
Good point. We will clarify that we express force per unit mass, i.e. acceleration.

21. I think there is a word missing from L240: 'on southward and inward what?' Flow?
Thank you for pointing out this typo; the sentence should say "on southward flow".

28 (ii): L352 on: I appreciate this nice, practical finish to the manuscript.
Thank you!

29. There is discussion around the alternating rotation of the eddies as well as the role mixing plays within the channels. Are the eddies large enough to lead to local differences in vertical mixing on either side of the channel (because of their alternate rotation), or would that be small compared to background mixing?
We are a little unsure of what the reviewer is referring to here. Presumably, the vortex motion will impact the exact lateral distribution of (shear-generated) turbulent mixing. It is an interesting idea, and we might flag this in the revised text (under discussion), but we admit that anything we could say on the topic would be speculative.

References

Albagnac, J., F. Y. Moulin, O. Eiff, L. Lacaze, and P. Brancher, 2014: A three-dimensional experimental investigation of the structure of the spanwise vortex generated by a shallow vortex dipole. *Environmental Fluid Mechanics*, **14 (5)**, 957–970.

Hench, J. L. and R. A. Luettich, 2003: Transient tidal circulation and momentum balances at a shallow inlet. *Journal of Physical Oceanography*, **33 (4)**, 913–932.