Response to reviewer 2

August 29, 2023

In the model description, the authors mention the parameterization of the bottom friction coefficient Cd (line 75). However, it is not specified what D represents in this context. I presume it refers to Manning's n formulation. It would be beneficial to provide a reference or an explanation regarding the utilization of these coefficients.

Thank you for pointing this out; you are correct in assuming it relates to Manning's n formulation. More specifically, the bottom friction coefficient C_d is computed using the Chézy-Manning formulation;

$$C_d = g\frac{n^2}{\sqrt[3]{D}} \tag{1}$$

Where g is the earth's gravitational acceleration, n is the Manning coefficient (which, by default in FVCOM, is set to 0.02), and D is the dynamic water column thickness. We will write the definition of C_d in this form in the revised version of the manuscript.

This formulation is based on the assumption that the water column depth can approximate the hydraulic radius. It's unclear how true this is in the open ocean, but we understand it is commonly used in coastal ocean circulation models. We notice from other publications that $n \in [0.013, 0.023]$, with most reported choices of n being in the higher end of the range (see, e.g. Blakely et al., 2022; Lee et al., 2020; Lyu and Zhu, 2018; Mayo et al., 2014; Kerr et al., 2013).

If D represents depth, taking into account the U-shaped channel configuration, there would be more friction at the channel's edges compared to the center. How does this parameterization influence the formation of eddies? Does the flow structure remain unchanged, and do the article's conclusions hold true if the coefficient is assumed to be constant? I believe this aspect could be added to the discussion or the section on "Limitations of this work."

It is important to note that the bottom roughness coefficient varies with depth (D) as $C_d \propto 1/\sqrt[3]{D}$. Bottom friction, as formulated in the shallow water equations, is also depth dependent; $F \propto C_d/D$. The depth dependence of friction is thus not dominated by the choice of drag coefficient. While the parameterization influences the formation of eddies, the fundamental physical mechanism still relies on stronger flow attenuation near the side walls, leading to flow separation and the dynamical evolution as described in our manuscript. We therefore expect that the dynamical system will exhibit a very similar behaviour if keeping the drag coefficient constant. We will incorporate this discussion into the revised version of the manuscript.

Despite the intricate nature of fjords and straits, the authors have succeeded in relatively well modelling tidal dynamics in the study area. However, when comparing the tidal characteristics of the model with observations, the authors do not present a phase comparison. This comparison is essential for a comprehensive validation of the tidal model across the entire area. Since

this study focuses on a single channel, the main objective of the setup is to establish accurate boundary conditions specific to this channel. Thus, it would be appropriate to restrict the comparison of model phases with observations to the channel area.

We agree with the reviewer's suggestion to compare the modelled sea surface elevation (SSE) to observations on either side of the channel. However, we do not have access to SSE measurements on the southern side of the channel constriction, just far into the nearby fjord (which also connects to other straits). If we had measurements near the choke point, we would have been able to assess whether the tidal forcing of the channel – the SSE difference across the choke point – was well captured.

However, Figure 7b shows that the modelled SSE variability over the period we model fits the observed variability, which together with the fact that the modelled- and observed speeds were similar indicates that the potential energy made available by the tide for driving the tidal jet should be reasonably well captured by the model. We therefore think a phase plot would be redundant, given the reasonable match with observations as shown in Fig 7 b.

Regarding line 161, the statement "..by some earlier..." needs clarification. Which specific reference is being referred to?

Thanks for pointing this out. In this sentence, we refer to Hench et al. (2002); Hench and Luettich (2003); Vennell (2006), and we will change the manuscript accordingly.

References

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