Dear Reviewer:

We very much appreciate your time spent on reviewing our manuscript and providing us constructive comments. We make the revisions based on your comments correspondingly.

Comment:

I have read thoroughly the paper ‘Salt intrusion dynamics in a well-mixed sub-estuary connected to a partially well-mixed main estuary’ by Lin et al. The paper presents and discusses results of a study that endeavors to shed light on salt intrusion dynamics in the upper reaches of an estuary where channelized networks can be developed. This is an interesting topic that undoubtedly requires further attention especially because climate change will exacerbate salt intrusion problems. In this context, I reckon that the current paper makes a significant contribution for scientific progress. It considers robust numerical and analytical methods, provides results that have been well presented and adequately justified, and comes up with some very interesting conclusions.

Response:

We are grateful for your positive evaluation of our work. Yes, the salt intrusion has become more serious under climate change and requires further attention.

Comment:

In an observational data analysis for the Pearl River, the authors detected that salinity and tidal range were in phase in the main estuary but out of phase in the sub-estuary. To explore the underlying physics of this phenomenon, they built an idealized model resembling the Pear River Estuary bathymetry and geometry and managed to reproduce successfully this relationship between salinity and tidal range. Then, they compared their results with analytical solutions. In their conclusions, they attributed the in-phase relationship between salinity and tide inside the sub-estuary to changes in horizontal dispersion and the modulation of freshwater release by river-tide interaction.

The manuscript is concise, very well written and appropriately structured. I deem that only a few improvements may be needed before publication. Below, a list with my comments:

Response:

We modify the manuscript following your comments in a point-to-point way.

Comment:

1. Line 25: *'..and salinity rise and fall exhibited more symmetrical'*. This sounds like a phrasal error. I would suggest rephrasing into something like 'salinity rise, and fall were more symmetrical'.

Response:

We change it into "the rise and fall of the salinity were more symmetrical"

Comment:

2. Line 262: I think it would be good to explain further how equation 2 was determined.

Response:

$$H(x,y) = H_{min} + (H_m - H_{min})\frac{y}{L} + (H_{max} - H_{min}) \times \left(1 - \frac{y}{L}\right)\left(1 - \frac{4x^2}{B^2}\right)e^{-C_f\left(\frac{4x^2}{B^2}\right)}$$

In Eq. (2), the first term in the right side is to set a minimum water depth, the second term is to increase the cross-sectionally averaged water depth linearly with the landward distance from the estuary mouth, which is mostly a correction for the third term to maintain a constant mean water depth (8 m) along the estuary, and the third term is to set the water depth in the lateral direction. Note that the amplitude of the third term decreases in the landward direction. This formula is modified from Wei et al., 2017. In the revised text, we modify the text as "Following Wei et al. (2017), we roughly mimicked this feature by setting the bathymetry of the convergent part as:"

Reference:

Wei, X., Kumar, M., Schuttelaars, H.M., 2017. Three-dimensional salt dynamics in well-mixed estuaries: influence of estuarine convergence, Coriolis, and bathymetry. Journal of Physical Oceanography 47, 1843-1872.

Comment:

Line 283: I gather that the s-grid layer refers to the vertical transformations in ROMS. I would recommend to briefly mention this in the text or to point to a citation. It may not be clear for non ROMS users or confused with sigma layers.

Response:

In the ROMS model, the vertical coordinate is expressed as:

$$\sigma = \frac{z+H(x,y)}{\eta(x,y,t)+H(x,y)} \quad ; \quad -1 \leq \sigma \leq 0$$

$$z(x,y,\sigma,t) = \eta(x,y,t) + [\eta(x,y,t) + H(x,y)]S(x,y,\sigma)$$

$$S(x,y,\sigma) = \frac{h_c\sigma+H(x,y)C(\sigma)}{h_c+H(x,y)}$$

in which $C(\sigma) = C_s(\sigma) = \frac{1-\cosh(\theta_s\sigma)}{\cosh(\theta_s)-1}$ for the surface refinement, and $C(\sigma) = C_b(\sigma) = \frac{exp(\theta_b C_s(\sigma))-1}{1-exp(-\theta_b)}$ for the bottom refinement.

We add a reference in the revised text.

Reference:

Shchepetkin, A. F., McWilliams, J. C., 2005. The regional ocean modeling system (roms): A split-explicit, free-surface, topography-following coordinates oceanic model. *Ocean Modeling* 9, 347–404.

Comment:

3. Figure 3: I would recommend mentioning explicitly in the caption that the monthly results are given for the winter months between November and March. Also, the river discharges in 2022 are comparable to those of 2009 but the effect on salinities is dramatically higher. Is there any reason for this?

Response:

We add words "Note that the river discharges in 2022 are comparable to those of 2009 but the effect on salinities are dramatically higher." in the caption for Figure 3.

Comment:

4. Lines 418-422 and 454-459: I believe what you also see here is a slower salinity response of salinity to flow decreases than increases. This is something that is mentioned in a paper that the authors cite as well (Chen 2015) and has been discussed further in the literature (Hetland & Geyer, 2004; Savenije 2005; MacCready, 2007; Uddin and Haque, 2010; Monismtih 2017). For example, it can be seen in Figure 5 that it takes about 7-8 days after the storm for the salinity to recover to is pre-storm levels in the main and almost a month in the sub-estuary. Perhaps, this is something worthy to mention.

Response:

Yes, we note this asymmetry in the salinity response to rising and falling river discharge, and have read most of these references before. We add words "Note that it takes about 7-8 days after the storm for the salinity to recover to its pre-storm levels in the main estuary and almost a month in the sub-estuary" in the caption of Figure 5.

Comment:

5. Line 475: In relevant studies, salt intrusion is usually measured by monitoring the 2 psu rather than the 5 psu bottom isohaline (Monismith et al.1996; 2002; Andrews et al. 2017; Bellafiore et al. 2021). Why was the 5 psu chosen instead? It is also suggested to use g/kg instead of psu as salinity units.

Response:

The choice of 2 psu as the salt intrusion has been used before by a lot of research, as you mentioned. The 0.5 psu isohaline has also been used as the limit of salt intrusion in many studies, as it is the criterion for drinking water. In this study, we have chosen different isohalines for the salt intrusion, and noted that the results are quite similar. The final choice of 5 psu is based on the consideration that the lower limit of the salinity around the mouth of the sub-estuary is approximately 5 psu.

We change the unit of salinity to g/kg, though psu is more widely used now.

Comment:

6. Lines 501-505: This is very interesting indeed and shows similarities to what I mention in comment nr 5.

Response:

Yes, this is the asymmetry for the increasing and decreasing tidal strength.

Comment:

7. Section 5.2. I wonder if the authors would consider showing results for Case 2 in the Results (section 4) so that the modelling work is presented all together at once and discussed later in the Discussion (section 5.2).

Response:

Thanks for your suggestion. We take this comment seriously, and note that if this part is moved to the results part, there will be very little material in the discussion part. So to maintain the balance of the text, we keep the model results of Case 2 in the discussion part.

Thanks again.

Dear Prof. Savenije:

We very appreciate your comments for our manuscript. These comments and insights are helpful for improving the quality of our paper. We address your comments point-by-point as follows.

Comment:

The paper reads well and contains interesting new observations. It is also good that the authors present empirical proof that salt intrusion is larger during spring tide in well-mixed estuaries, while in partially mixed estuaries the highest intrusion is achieved during neap tide.

In general, I think the paper can be accepted for publication. I have a few observations, though, which I trust the authors will address in a revised final version.

Response:

We thank for the constructive comments from the reviewer.

Comment:

1. In Figure 10b we see that the salt flux is always directed upstream. This cannot be the total salt flux because in that case there would not be a steady state: the estuary would continuously become more saline. I suspect that the

authors mean the dispersive salt flux, as described by the third term in Eq(3), or the righthand side of Eq (4). In steady state the total average flux should be zero (the advective transport matches the dispersive transport). Please clarify.

Response:

This is a great comment. We double-check our subroutine for calculating the salt flux, and note that we made an error to misrepresent the water depth for the grid cells. We recalculate the fluxes and the new results are presented in the new Fig.10.

The new results indicate that the salt flux is generally positive during the periods from neap to spring tides, showing a net salt import, and negative from spring to neap tides, a net salt export.

Comment:

2. Then there is an issue with the way how the authors describe the condition for steady state. They compare the 14 days tide scale of the neap-spring cycle with the time that a water particle takes to travel through the estuary. If that time scale is T_f, then we see that during low flow, this time scale is very long, particularly in wide estuaries. But this is not the right time scale to consider. What should be considered is the system response time scale, such as for instance is described in Section 5.6.1 of Savenije (2012), which is a book cited

by the authors. Referring to Table 5.6 in Savenije (2012), one can see that the system response time scale (whether T_K or T_S) is substantially smaller than T_f (with a factor 2 to 10 depending on the channel geometry and river flow). This explains why the analytical model provides quite good sub-tidal salinity estimates even for low river discharge.

Response:

We carefully read the relevant chapter in Savenije (2012), and recalculate the response timescale of Ts ($T_S = -\frac{1}{Q_f S(x)} \int_X^L ASdx$). The thus obtained timescale is 16.22 day, which is comparable to the spring-neap tidal cycle.

We revise the text accordingly.

Comment:

Finally, the observation in line 606 "justifies ignoring the steady shear part in Eq. (3)" is not at all clear to me. What is ignored in Eq.(3)? And how does this connect to "steady shear"?

Response:

The landward salt transport is generally decomposed into two parts: one is the steady shear induced by estuarine circulation and salinity stratification, and the other one is tidal oscillatory transport (McCready and Geyer, 2010). As the sub-estuary is generally well-mixed, the estuarine circulation and salinity stratification are both weak, thus the steady shear is minor. The dominant landward salt transport is thus the tidal oscillatory. In some literature, the salt transport equation is written as:

$$\frac{\partial(A\overline{S})}{\partial t} = -\frac{\partial}{\partial x}\left(A\overline{u}\overline{S}\right) + A\overline{u}_v\overline{S}_v + \frac{\partial}{\partial x}\left(AK_x\frac{\partial\overline{S}}{\partial x}\right)$$

Where the second term in the right side denotes the steady shear. In Eq. (3), the steady shear is lumped into the horizontal dispersion.

We modify the text accordingly.

Comment:

I wish the authors success with submitting their revised version.

ref.:

Savenije, H.H.G., 2012. Salinity and tides in alluvial estuaries. Second Edition <www.salinityandtides.com>.

Response:

Thanks again for your constructive comments.