Review of "Analytical model captures intratidal variation of salinity in a convergent, well-mixed estuary" by Xu et al.

In this paper, the authors proposed an unsteady analytical model for salt intrusion to understand the spatial-temporal dynamics of salt transport under different riverine and tidal forcing. The model was applied to the Humen estuary, which is a tide-dominated and well-mixed estuary. And the modelled results correspond well with the observed data. The paper is interesting and of important scientific implications for estuarine dynamics. However, there are still some major concerns that should be properly addressed before the paper can be accepted in this journal. Thus, I would suggest the authors to have a substantial revision.

Major concerns:

1. Method in Section 2: It is noted that a rather similar approach for salinity intrusion in an unsteady state was proposed by Song et al. (2008) entitled "One-dimensional unsteady analytical solution of salinity intrusion in estuaries". It is better to clarify the main difference between the current model and the one proposed by Song et al. (2008) in order to highlight the new insights into the salt dynamics.
2. P8, Lines 19-24, estimation of the tidal excursion: Note that the tidal excursion is a critical parameter that links the salinity intrusion to the tidal hydrodynamics. In this study, the authors assumed that the longitudinal tidal excursion can be described an exponential function. However, such an assumption is only reasonable for a short channel (Let's say less than 10 km). I would suggest the authors to adopt an analytical hydrodynamics model to reproduce the longitudinal tidal excursion since there exists a long traditional analytical solution for tidal hydrodynamics in estuaries (e.g., Toffolon and Savenije, 2011; Winterwerp and Wang, 2013; Cai et al., 2016). The advantage of coupling the salinity intrusion model to the tidal dynamics lies in that it enables directly linking the salt dynamics into the tidal forcing (e.g., tidal amplitude imposed at the estuary mouth). Moreover, it allows to have a prediction of salinity intrusion for different tidal forcing conditions (e.g., neap-spring changes) for given tidal amplitude observed at the estuary mouth. The current model used the observed salinity to forecast the tidal excursion (i.e., Eq. 18 in the manuscript), which is not very practical if prediction is required.
3. P8, Lines 25-29, estimation of the wave celerity: Similar to the tidal excursion, I would suggest the authors to link the wave celerity to the tidal forcing imposed at the estuary mouth by means of an analytical model for tidal hydrodynamics.
4. For the time being, the authors only illustrate the proposed analytical model applied to the Humen estuary during the neap tide condition, when the salt intrusion length is approximately minimum. I would suggest the authors to adopt the model to the case during the spring tide condition when the salt intrusion really matters. In section 4.2 concerning the model validation, since the authors only used the dataset from Jan. 29th to Feb. 3rd, I think this is only kind of the model calibration rather than validation because the tidal hydrodynamics is more or less the same during the chosen period.
5. Sensitivity analysis: As mentioned by the authors, the proposed analytical model can directly reflect the influence of the tide and the interaction between the tide and runoff (see Abstract part

in Line 17). Hence, it is better to conduct a sensitivity analysis of the salinity distribution to both the tidal and riverine forcing imposed at both ends of the estuary.

Minor concerns:

1. P3, Eq. (3) and Eq. (4): Here please clarify the physical meaning of s_1 and s_2 coefficients. In addition, it is noted that the salinity and velocity are assumed to be in phase since they have the same initial phase, am I right? Please also clarify this important assumption.
2. P7, Line 18: Please clarify where the salinity was sampled. It was sampled in the central part of the channel or near side banks? Due to the fact that the model used the cross-sectional averaged salinity concentration, it would be better to clarify this point.
3. P9, Lines 14-16: It is better to illustrate the stratification or mixing during the studied period since the authors already collected both the surface and bottom salinity concentration.
4. P11, Lines 6-8: Due to the assumptions of Eqs. (3)-(4), the extreme values of salinity appear when the tidal velocity is zero.
5. Figure 1: Please use 'West River' and 'North River' instead of 'Xijiang River' and 'Beijiang River', respectively. Meanwhile, it is better to indicate the locations of outlets that were mentioned in the main text.
6. Figure 2: It is better to use the logarithm scale.
7. Figures 4, 6, 7: Please relocate the legend to a suitable place.

References:

Cai, H., Toffolon, M., Savenije, H.H.G., 2016a. An analytical approach to determining resonance in semi-closed convergent tidal channels, Coast Eng. J., 58(03), 1650009.

Toffolon, M., Savenije, H.H.G., 2011. Revisiting linearized one-dimensional tidal propagation. J. Geophys Res., 116. DOI:ArtnC0700710.1029/2010jc006616

Winterwerp, J.C., Wang, Z.B., 2013. Man-induced regime shifts in small estuaries-I: theory. Ocean Dynam., 63(11-12): 1279-1292. DOI:10.1007/s10236-013-0662-9