

Comments to authors' reply

For research, the most important thing is honesty and credit. In previous comments, I have pointed out several unreliable presentations. In authors' reply, there is still unreliable presentations such as the following reply to major issue 2. So, I cannot be sure if the modelling process was proper and the data used was correct. In addition, about some authors' explanation, I disagree. Authors did not seriously modify the manuscript. Therefore, I think the manuscript is not proper for publication. My main specific comments are as follows.

1. Reply to major issue 1

What authors should do is adding introduce about previous related research, instead of only adding several references.

2. Reply to major issue 2

Authors added the wind speed curve at weather station in figure R3-2 and said that the data used was 2 minutes average. I also have the wind observations data. At this land-based station inside estuary, it is impossible that the 2 minutes average wind speeds are so large, persistent more than 10 m/s for long time and even larger than 15 m/s. In figure R3-1, why did authors present the modeled wind directions and speeds off the Subei coast, instead of off the Changjiang Estuary?

3. Reply to major issue 4

About why the water level rise inside estuary is small, authors said that I misunderstood and water level rise at Sheshan and Luchaogang stations in Figure 2d was almost same as the one in

Figure 5a. Authors clearly said in the manuscript that the water level rise at Sheshan and Luchaogang are distinct with a peak value more than 0.5 m (line 91), which is shown in plot d of figure 2 as well. But at Baozhen the relatively large rise during neap tide 7-11 is about 0.15 m (line 135), which can be seen from plot a of figure 5 as well. So, what is the real situation?

About the method used calculating water level rise in plot d of figure 2, authors said that they subtracted the data in the tide table from the measured water level value. The obtained water level rises based on this method could have much error because the forecasted water levels in tide table have error as well.

About much more water level rise inside the estuary in plot b of Figure 4 and Figure 7, authors said that in plot b of Figure 4 and plot b of Figure 7, the time-averaged water level was shown, not the water level rise. But it can be seen clearly that "water level rise" was labeled in the legend.

4. Reply to major issue 6

About already presented in previous work and unmentioned mechanism proposed in this manuscript, authors argued that the previous work was pure wind-driven and the work in this manuscript was not only wind-driven. But the proposed mechanism is the same. There is no the new thing, such as interaction between wind, tide, and river discharge. If you thought they were different, why did you not mention the previous work? Even if they are the same, it is ok only if you introduce and discuss the work. But you did not do this.

Authors said that if the wind directions were not always northerly, even southerly in some periods, the saltwater intrusion would be

more severe and more serious impact on the Qingcaosha reservoir. Why? If it is true, the mechanism should be shown as well.

In addition, it can be seen clearly from figure R3-4 that at Chongxi station the increase of salinity relative to the normal situation on 3-4 was more than the "extreme event" period. On 3-4 the strong northerly or northeasterly winds occurred as well. Why was the saltwater intrusion in the North Branch during the extreme event period is not extremely serious? During 13-17, salinity was similar to the normal situation, which means that there is no increase of saltwater intrusion. However, saltwater intrusion at other stations did not occur on 3-4, but was very serious on the "extreme event" period. What is the difference between mechanisms of winds influencing the North Branch and the North Channel?