General comments:

I can see the improvement of the manuscript. However, I am still confused on some critical points. Therefore, I recommend the publication of the manuscript after major revision.

Major comments:

1. The logic of the manuscript is still unclear. In section 3.1 and 3.2, the authors showed the temperature and salinity features and circulation pattern of the cold water, trying to demonstrate the evolution of the cold water and the related mechanisms. However, there is a lack of supported evidence. I will list those points out later. In section 3.3, the authors tried to prove that geostrophic balance is not applicable for the maintains of the anticyclonic circulation in lower layer in May due to the non-negligible friction in the shallow water. Isn't it obvious in a shallow shelf? I am not quite sure why the authors spent a section to demonstrate this question. It may be better to merge this section into section 3.1 and 3.2? In section 3.4, the authors demonstrated that the anticyclonic circulation is NOT caused by the cold water mass which induces changes in baroclinic conditions. So, why not just to demonstrate which factor cause the anticyclonic circulation? As I observed from the figures shown in the manuscript, there are at least two compensate currents (secondary currents) which, I think, are important for the anticyclonic circulation development and the cold water mass evolution, i.e., the landward current at near bottom layers (westward current) and the upwelling system. Also, the background current system outside the shown region is very important. In section 3.5 and 3.6, the tidal and wind effects on the anticyclonic circulation are studied. But I am not quite satisfied with the explanation provided. I will list my concerns later. In section 4.1, the upwelling effects on the cold water mass is discussed, which I think is very important and should be move to section 3.3. More focus should be put to this part. And I found it is hard to follow the tidal effects on the upwelling and the mixed layers (lines 334–360). I will list my questions. Section 4.2 is too short to be a section alone. Instead, as supporting evidence for the tidal and wind effects, it may be better to merge this section into the sections discussing the tidal and wind effects (section 3.5 and 3.6). The same problem raises to section 4.3.

2. I am a bit confused as to why the authors devoted so much effort to comparing all the forcing terms. Based on Figure 5, the barotropic term appears to dominate in the near-bottom layer, which I believe is likely due to water piling up on the

eastern side of the domain under the influence of the southwesterly monsoon. As a result, the landward flow in the near-bottom layers along the coastal region is primarily driven by this pressure gradient force, with minimal influence from other forcing terms. This aligns with what I mentioned earlier regarding the compensating current in my previous comment. So, it seems that too much discussion on other terms (baroclinic, Coriolis, friction) is not necessary. Could you please clarify the rationale and necessity for such extensive discussion of these terms (as presented in Figures 5, 6, 7, 9, and 13)?

3. There are still some findings or points listed in the manuscript that are contradicted to each other.

Detailed comments:

1. Figure 1. Please overlap the bathymetry used in the model and also provide the coverage of the entire computation domain.

2. Section 2.2. There is a lack of description of the lateral boundary conditions and riverine forcings, which I think are very important to shelf dynamic simulations.

3. Line 103. Please use the specific years here, e.g., use November 1st, 2017 instead of November 1st of the 9th year. The latter expression may confuse the readers if they count the 2008 as the 1st year.

4. Lines 141–142: Evidence is needed to support the claim that the merging of warm and cold water (i.e., the deformation of the cold water mass) is primarily due to increased solar radiation and not other factors, such as mixing caused by the sustained monsoon. Additionally, doesn't surface heating contribute to maintaining the cold water mass near the bottom by reinforcing strong stratification?

5. Lines 142–145. Although the author added my understandings on the cold water mass formation, it is just the knowledge from my previous studies in physical oceanography. The authors need to provide evidence to support this statement. I see that Figure 12 is a helpful clue. And also, the author may need to add more plots in the appendix showing the evolution of this cold water mass from April to June (may be evolution of the transect as depicted in Figure 12?). So, it is better to move Figure 12 to here.

6. Lines 141–142 contradict to Lines 142–145. The former one emphasize the importance of solar radiation, while the latter emphasize the role of mixing and upwelling.

7. Section 3.2. Please confirm the direction of the summer monsoon. According to the Figure A3, it is southwesterly wind, but southeasterly wind is used during the discussion, e.g., Line 166. The inconsistence is found almost all over the manuscript. Please double check.

8. Lines 159–161. The southwesterly wind induces the northward current on surface, but the authors indicate that the near bottom northward current is also induced by this wind pattern. I doubt that. This is the reason why I asked, "Is the southeasterly wind strong enough to induce vertically-homogenized the northward currents?" during the last round of revision. It is better to show the current pattern from surface to near bottom. Now, I would argue that it is the compensate current that induces the northward current near bottom but not the direct effects from the surface winds.

9. Lines 166–167. This sentence is an incomplete one grammatically. The "southeasterly" contradicts to the previous description and Figure A3. The causality shown confused me. Why do the shallow water and southeasterly monsoon lead to stronger current at the west part of the anticyclonic circulation? Here, the authors may miss the boundary effects but only focusing on the local wind effects. So, how does the current system like over the entire computational domain?

10. Lines 171–174. What is the purpose to show the circulation below 25m?

11. Line 174–175. I don't think the baroclinic pressure gradient forces are contribution to the vertical structure of the current patterns near bottom. As I observed from the Figure 4, current patterns are quite similar over these near bottom layers (northwestward and northward). Isn't is due to the landward compensate currents or the current from the south boundary? A well-known Subei current system usually intrudes into the Yellow Sea in summer and merge with the cold water mass. That is, a larger picture of current systems needs to be discussed.

12. Lines 175–176. I don't understand why the anticyclonic circulation contribute to the formation of the cold water mass near bottom? Does it contradict to Lines 142–145?

13. Line 194–195. The vertical friction shown in figure 5 is the friction at depth of 25m. So, the wind stress at the surface layer does not contribute to this friction term but the current speed gradient around the 25 m depth does.

14. Lines 195. I doubt that the surface wind can impact the current pattern near bottom through direct dragging. If so, the whole water column can be well mixed with not cold water exists at near bottom.

15. Lines 190–199. I see that the discussion is conducted mixing the current patterns at surface and near bottom layers, which should be discussed separately as they should not be the same as shown by Figure 3 and Figure A3.

16. Line 200. The expression of the vertical friction force is wrong and same for Line 219.

17. Section 3.4. I don't think discussion on geostrophic balance is useful for linking the anticyclonic circulation and the cold water mass.

18. Line 227. The term "significant" has statistical meaning. So, "significant" usually come along with statistical tests. Please use another term. Please check throughout the manuscript for this issue.

19. Lines 227–232. The geostrophic balance should not be the theory discussed here as in this region, wind forcing cannot be ignored. Instead, it is the wind stress that causes the pressure gradient forcing (higher SSH on the east than on the west evident by the westward barotropic term) and the compensate current in lower layer but not the violation of geostrophic balance by tidal forcing. The explanation here conflicts with my knowledge. Holp the authors can convince me if I am wrong.

20. Lines 233–236. What does the "dynamic reason" refer to? And I don't see the anticyclonic circulation contribute to the evolution of the cold water mass, rather, based on my study on the figures show, both of which (anticyclonic circulation and cold water mass) are the results of the compensate currents induced by the surface wind forcings. Again, please convince me if I am wrong.

21. Section 3.5. In-depth discussion is lacking. I suspect that the elimination of tidal forcings result sin the changes of the background current systems which affect the anticyclonic circulation near Qingdao. The authors, however, only provide the description of how the anticyclonic circulation changes due to the absence of tides.

Additionally, when removing the tidal forcing (that is in the no-tide experiment), how the boundary conditions are configured? The tidal forcing here should be the tidal signal generated within the computational domain, right? If tidal signal is not removed from the open boundary conditions, the no-tide experiment still contains tidal signal generated from outside.

22. Line 265. Please be consistent of the difference terms. Based on the caption, Figure 8c is flow differences of no-tide and control runs (no-tide minus control), Figure 8d is flow differences of control and no-wind runs (control minus no-wind). And what does the colored patches represent? Magnitudes? Or directions?

23. Section 3.6. The authors demonstrated that the wind is the dominant driving force for the anticyclonic circulation, which I agree with. But I don't agree with the rest. Firstly, there is a westward shift of the Qingdao cold water mass when wind is removed. It should be pointed out and discussed but the authors did not. Instead, the authors mentioned that the temperature of the cold water decreases by 2^{o}C which is due to the weaker wind mixing when wind is removed. Rather, I think, it should be related to the location changes in cold water mass.

24. Line 309. Based on Figure 12a, the upwelling does not reach the surface but stop at depth around 10m

25. Lines 312–313. As I observed, the upwelling occurs over the entire transect but with different strength.

26. Lines 313–315. Yes, it is true. But the convergence is not pronounced. Instead, the most remarkable point is the eastward surface current and the associated secondary westward current at lower layers over the east of the 122E and the strong upwelling system.

27. Line 317–318. Based on Figure 12a–12b, the upwelling system is strengthened over studied transect but not just the east side when tides are considered, right?

28. Lines 320–321. The westward or eastward flows in the rectangle zone are not the most remarkable point but the upwelling, instead, westward current at lower layers over the east of the 122E may be more interesting.

29. Line 325. Southeasterly monsoon? The upwelling is contributed by tidal forcings, not by wind forcing?

30. Lines 309–326. These paragraphs should be more concise as they only highlight two points: the upwelling system and how it is related to wind and tide. Apparently, the authors did not provide convincible evidence of how tide affect the upwelling here.

31. Lines 334–360. I lost here. Instead of believing the tide induce thermal fronts, I would rather believe that the background current system (like the Subei coastal current) is affected by the tides. So, when tides are removed, the background current system change a lot leading to the changes in upwelling and also anticyclonic circulation around the Qingdao cold water mass. Also, I don't know why the authors focus on the relationship between tide and baroclinic pressure gradient force which is a very small term comparing to the barotropic pressure gradient force. Again, the tidal effects can be reflected by changing the background current pattern which, I guess, results in more pronounced changes in the barotropic term than the baroclinic term.

32. Figure 13. Please double check the caption.

33. Line 369. No. Oceanic modelers use statistic tests a lot.

34. Lines 370–380. Is t-test an appropriate test for this study? You only have 4 ensemble members. That is, for example, for the temperature at a given grid point, you are testing if the 4 temperature values in the control run are significantly different (assuming two-side testing) from the 4 temperature values in the no-wind (or no-tide) run. So, sample size is very small. Hope my understanding to your t-test is correct.

35. Lines 377–380. I don't get it. Could you provide more detailed explanation?

36. Lines 394–396. The anticyclonic circulation discussed in this study locate at near bottom, right? The northeastward current induced by summer southwesterly monsoon is at the surface. So, how does this surface current affect the near bottom current system?

37. Section 4.3. This section is weak as the authors did not do a great comparison between the finding from this study and the previous studies, e.g., which points agree with the previous findings, and which do not.

38. Figure A1. The term "evolution" is not expected as only one map is shown.

39. Figure A3. Blue arrow is for wind while the black for the wind stress, right?