Comments on " Role of wind, mesoscale dynamics and coastal circulation in the interannual variability of South Vietnam Upwelling, South China Sea. Answers from a high resolution" by Tai To Duy et al. in Ocean Science

In this study, the authors attempted to explore the dynamics governing the interannual variability of South Vietnam Upwelling based on model simulation. Befere discussing the modelling results, they first compared the general patterns of simulated SST, SSS, and SLA with the satellite-remosed data, and compared the simulated temperature and salinity profiles with the in-situ measturements of ARGO, Seaglider, and R/V cruise. After reviewing, I think the manuscript needs an subsustainsial revision before being acceptable based on the following comments.

Major Comments:

1. Methodology:

a) The authors explored the South Vietnam Upwelling in four areas: BoxOF, BoxNC, BoxSC, and BoxMK. For the first three areas, the cold temperature are evident in both the simulated SST and satellite SST. However, for the BoxMK, the cold temperature seems only appear in simulated SST, but not in satellite SST, althougth the authors have referred to the finding of two literatures. This argument does not make sense, because a reader cannot directly confirm the rationality of considering BoxMK a spereate area to be explored. If this is not a natural phenomenon, the discussion becomes meaningless.

b) The authors performed the simulation of the model SYMPHONIE from 2009-2018 by comparing output data (SST, SSS, SLA, and T-S profiles) with high-resolution satellite data and in-situ observations, showing that this model is an innovative tool that can reproduce oceanic dynamics properly not only at the surface but also at deeper sub-layers, and at wide-range time scales. To investigate the daily-to-interannual variability of the VNU, however, they employed only the surface data (SST and velocity) and the discussions are all statistically, which brings not many new results in the comparison with previous studies using satellite data. In other words, this study can be performed by the satellite data without SYMPHONIE. I think the authors should utilize the advantage of modelling to conduct numerical experiments to examine whether the proposed factors are really factors controlling the interannual variablility of South Vietnam Upwelling in each area.

c) In Section 2.3, the author introduced several SST-based upwelling indicators (daily, yearly and spatial upwelling index), which are applied for 4 upwelling areas. Each area uses different reference boxes, which is taken as the areas not impacted by surface cooling. However, the boxes (besides RefOF) they chose may be highly possible to be influenced by other upwelling areas. For example, RefNC could be impacted by the offshore upwelling if the offshore upwelling have more northern extension. In addition, the authors use the time-averaged Tref in each Reference box, but the temperature in the SCS suffers interannual variations, e.g., Figure 3b. This could make a great impact on the calculation of SST-UI, and result in a large dependency as discussed in section 4.5.

d) Another concern is that the spatial upwelling index could be not a continuous field as shown in Figure 8 because the authors use different Trefs.

e) Some calculations have been done but not defined (i.e. wind stress, vorticity, coefficient of variation, …). Specifically, the authors adopted wind stress for many places, but they did not define the wind stress: meridional wind stress, zonal wind stress, along-shore wind stress or cross-shore wind stress.

2. Result and discussion:

a) The authors wrote long paragraphs to describe known results and few lines for un-solid conclusions. For example:

Section 4.1: The impact of intra-seasonal and inter-annual variability of wind forcing on SCU has been revealed. I suggest the authors reconstruct this section by referencing known results in the introduction, using several sentences to describe the similarity with previous outcomes and highlighting new finding they have discovered. In the case of oceanic factors, word usage is not direct to point, for instant, “background coastal circulation” and “mesoscale structure”. Quantitative assessment is missing for the oceanic factors.

b) Similar comments for Sections 4.2, 4.3, and 4.4.

Section 4.3: The authors proposed 4 situations that help/prevent NCU occurs.

(1) Strong southward alongshore current prevents NCU.
(2) Strong northward alongshore current weakens NCU.
(3) Secondary dipole and the relating secondary offshore jet strengthens NCU.
(4) Weaken dipole structure and offshore jet strengthen NCU.

These situations seem to conflict with each other and no further quantitative analyses are employed to prove their hypothesis.

c) Figure 4i-4l, The authors compared the basin-scale SCS circulation based on the sea level anomaly field, which only expresses the anomalous flow field. This is not proper for describing basin-wide circulation, because it should include both mean flow and anomalous flow. Besides, the authors claimed an eastward jet appears in the modelled and satellite-derived anomalous flow field (L239-243); however, I cannot see that!

3. **Conclusion:**
Factor like wind stress curl has not been carried out in the analyses but still appear in the conclusion.

Minor Comments:

L18 "mesoscale ocean dynamics" should be more concise or direct to the point.

L63 "influences"

L73 "varies"

L158 The mean bar notation should be put overline

L197 "The fourth area"

L226 For accuracy, comparison between the spatial-mean simulated and observed SST, SSS and SLA could be done over a smaller area such as the VNU rather than the whole VNC domain.

L254-257 "Though SYMPHONIE is overall ... Woo et al. (2020)". Quantitative assessment of the overestimating of the surface cooling in the southern Vietnam coasts is missing, which is important for evaluating SST in the upwelling region. The reader is left wondering, the SST overestimating is caused

by SYMPHONIE output or OSTIA? It further raises the question that if upwelling occurs in BoxMK in reality.

L233 L245, L261, L270, L275, L280 Inconsistencies in describing NRMSE, sometimes use "%", sometimes use decimal.

L235, globally?

L292 - L299 Long description

L307 Figure SM1 is not found in the manuscript.

L341 "... the lowest of the 4 boxes...". The lowest of what?

L391 Definition of "OIV" haven't been mentioned.

L357-L358 Values of UI need to be checked again.

L356-L365 The authors compare the differences between 2009 and 2012 of the daily/monthly wind stress and daily upwelling index and conclude that the daily to intra-seasonal variability of wind forcing modulate the SCU interannual variability. However, this analysis does not make sense to me because they are the different time scales. Similar comments for the OU and NCU.

L372, L373 "is (not) related to" should be " (does not) relate(s) to"

L393 Vorticity calculation has not been described. What kind of vorticity? How do the authors define the surface current? Which depth layer of velocity do they use for the calculating?

L503-509 “This current constitutes …the stable position of MKU”. This inference needs more evidences.

Figures:

Figure1: VNC configuration should be described in detail, especially the coastal region, rather than locations of 4 upwelling areas, which are displayed again in Figure 2c.

Figures 3 and 4: There is an inconsistency that exists between the order of figures and the text's description, which makes the reader hard to follow.

Figure 5: "... ARGO (a, black dots and purple for mean), GLIDER (b, black dots and cyan for mean), IO-18 (c, black dots and green for mean) observations and from SYMPHONIE (yellow dots and red for mean) colocalized outputs..." should be "... ARGO (a, black dots and purple line), GLIDER (b, black dots and cyan line), IO-18 (c, black dots and green line) observations and from SYMPHONIE (yellow dots and red line) colocalized outputs...". However, the caption should be better clarified.

Figure 7: Legendaries of x and y axes are overlaid (year 2018)

Figure 8: Purple contours in a,b,c,d,i,j,k,l,q,r and blue contours in e,f,g,h,m,n,o,p,s,t have no explanation.

Figure 9 and Figure 10: The ranges of y-axis should be fixed with the presented data