Answer to 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 ocean model" by Tai To Duy et al. in Ocean Science

In this document, the reviewer's comments appear in black, and our answers in blue.

This is the second round review of the manuscript. The authors have tried to addressed my concerns in the last review. Some major comments are still needed to be addressed before being acceptable.

We warmly thank the reviewer for this careful and positive review of our paper. We addressed all the comments below in our revised version of the manuscript. Changes done in the manuscript following the comment of the reviewer are highlighted in blue in the revised version of the manuscript. Line numbers correspond to the highlighted version.

Major Comments:

1. Section 2.3:
Regarding the definition of the upwelling index, I have three points of criticism.

a) In lines 229-232, "The threshold temperature below which upwelling happens is defined from the analysis of the occurrence of cold surface water: it is defined as the temperature that allows to cover the largest number of upwelling occurrences but avoids to include cold water advected between areas. We obtain To = 27.6°C". The authors obtained T0 based on the two conditions: (1) "the largest number of upwelling occurrences" and (2) "excluding advected cold water from other boxes"; however, no further explanation about how they dealt with two two conditions was given. Did the authors conduct any sensitive tests for the value of T0? Da et. al (2019) set up the threshold of T0=27.5 by applying sensitive tests of the frequency of occurrence but did not mention advected water. Did they use the same method or have any improvements?

We used exactly the same methodology as Da et al. (2019). SST-based upwelling index is a widely used method. Moreover, during his PhD, Da (2018) studied carefully the choice of upwelling criteria (Da et al. 2019, and see pages 85-86 of the PhD thesis of Da (2018) available on http://lotus.usth.edu.vn/uploads/PhD_NguyenDacDa_2018.pdf). We therefore did not redo this part of the study, and directly used the methodology of Da et al. (2019). Following this methodology, To is defined from the frequency of occurrence of cold surface water, as shown on Figure A below. For that, we vary To from 26.0°C to 28.0°C every 0.1°C to select the threshold temperature. The goal of those tests is to select the temperature that allows to cover the largest number of upwelling occurrences, but avoids to include cold water advected between areas. From our tests, 27.6°C is the optimal choice (Figure A).

→ Following this comment we modified the text, referred to Da (2020) and Da et al. (2019), added the part about the tests between 26°C and 28°C, and rewrote and rearranged the text to be more clear (Section 2.3, lines 223-242).

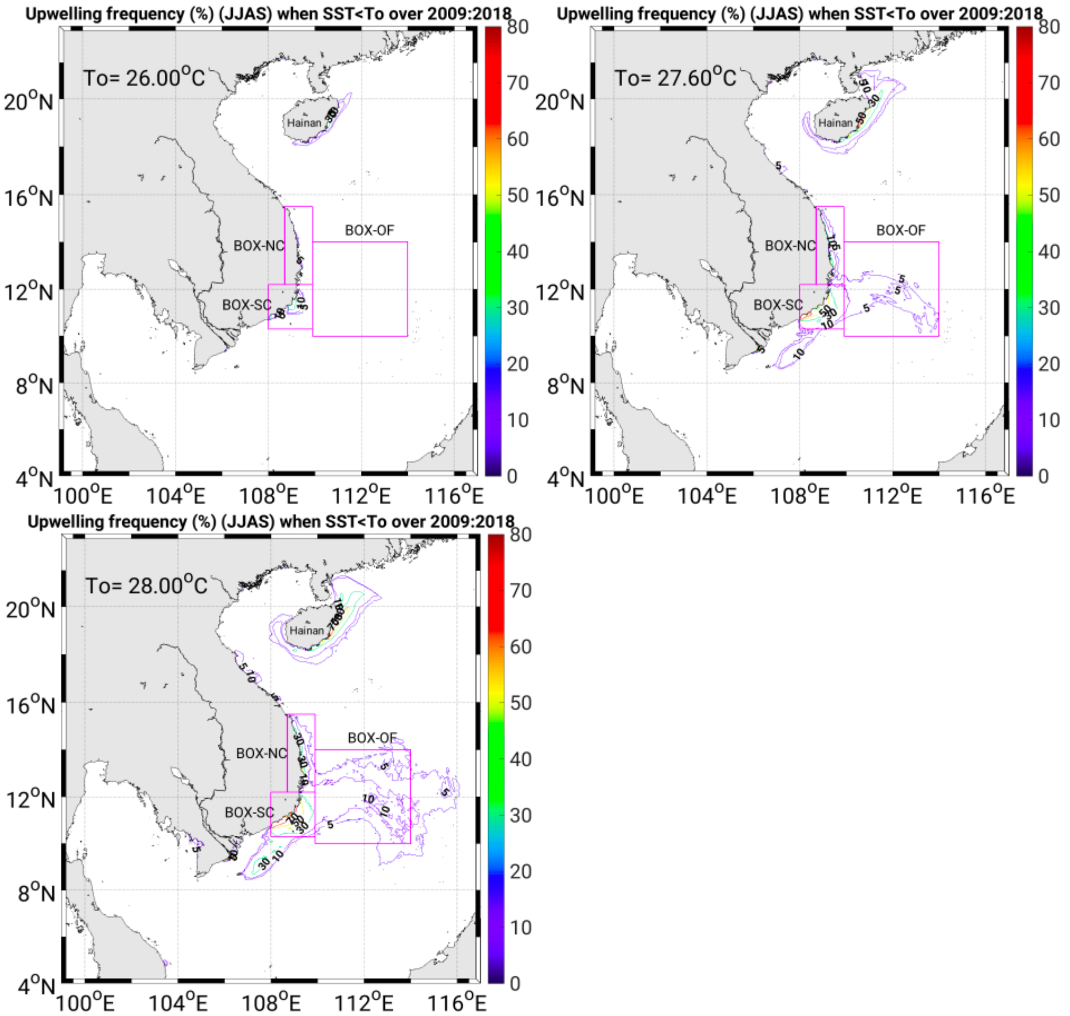

Figure A : Upwelling index frequency (% of June-September (JJAS) period) defined as the frequency of events for which SST < To for different choices of To (color contours).

b) In the revised manuscript, the authors decided to use one Tref for the four boxes of BoxSC, BoxNC, BoxOF and BoxMK to help UIy change continuously in space. And, the authors stated that the difference between the upwelling indices using old and new Tref is small (2%) based on a simple calculation (Tref,new - Tref,old)/Tref,old = (29.2-29.7)/29.7 = -0.017 ~ -2%). I think this argument is not correct. In fact, from Table A of Response, I find that difference between new and old UImean in BoxNC is 19.6% ((0.163 – 0.195)/0.163) and the difference between the standard deviation of new and old UI in BoxNC is 18.6% ((0.118-0.14)/0.118). In sum, the difference between the upwelling indices using old and new Tref is around 20%, not as small as they thought (just 2%).

Indeed, computing carefully the relative difference between upwelling index computed using the “old” and “new” Tref gives :

$$\mathrm{Delta(UI)} / \mathrm{UI} = \left(\iiint (T_{ref,new} - T)\, dxdydt - \iiint (T_{ref,old} - T)\, dxdydt\right) / \iiint (T_{ref,old} - T)\, dxdydt$$

$$= \iiint (T_{ref,new} - T_{ref,old})\, dxdydt / \iiint (T_{ref,old} - T)\, dxdydt$$

$$= (T_{ref,new} - T_{ref,old}) \iiint dxdydt / \iiint (T_{ref,old} - T)\, dxdydt$$

Since since $T < T_o$, then $|T_{ref,old} - T| > |T_{ref,old} - T_o|$, and

$$|\mathrm{Delta(UI)} / \mathrm{UI}| < |T_{ref,new} - T_{ref,old}| \iiint dxdydt / |T_{ref,old} - T_o| \iiint dxdydt$$

$$= | T_{ref,new} - T_{ref,old} | / | T_{ref,old} - T_o | = (29.7-29.2)/(29.7-27.6) = 0.23$$

So finally $|\text{Delta}(UI) / UI| < 23\%$ (and not 2% !)

We thank the reviewer for pointing out this a mistake in the previous answer. Our corrected calculation shows that the relative difference between the upwelling index computed with a different Tref or a unique Tref is smaller than 23%, which is consistent with the 20% difference underlined by the reviewer. This mistake done in the previous answer to the reviewer's comment actually did not have any consequence on our computations of upwelling indexes and related conclusions in the paper. In the previous version of the paper, we indeed did not mention this computation. Moreover we showed in the previous answer that using a common Tref instead of a specific Tref for each box did not induce any significant change in the computed correlations.

→ Following this comment, we did not need to correct the manuscript itself, but mentioned the impact of this choice of common vs. specific Tref for each box in the text (Section 5.1, lines 650-655).

c) In Response and Section 5.1 of revised manuscript, the authors have addressed reasons why they chose the constant Tref but not the interannually varying Tref: (1) "Tref can be influenced by the advection water from the other box, not only the interannual variation of SST", (2) "a varying Tref would increase the weak values and decrease the strong values, hence reducing the interannual variability", and (3) "differences between UIs using constant Tref and interannually varying Tref are small". I think the main reason of the authors should be the third one and is reasonable; however, I still have two comments on the the choice of Tref.

- First. Tref in SST-based upwelling index represents the reference SST that the upwelling effect on this value is small. That is, the authors should re-consider the location of the reference box if "Tref is still strongly influenced by the advected upwelling water".

The reference box was chosen as far as possible from the upwelling zone and over the same latitude region. However, this choice was also constrained by the computational domain, i.e. the location of the eastern border. Figure 3 of the paper shows that on average, BoxTref is outside of the upwelling area. However we indeed showed that BoxTref could be influenced by the eastern advection of cold upwelled water during years of strong upwelling (Figure 17a of the paper and Figure B below). This would affect the computation of Tref, hence of the upwelling index. Taking a climatological average (i.e. a constant Tref) instead of an interannually varying Tref contributes to reduce this effect.

→ Following this comment, we added a paragraph in the text (Section 5.1, lines 626-629)

- Second. I don't agree with the second reason proposed by the authors that "using the interannual Tref will reduce the interannual variability of the upwelling". The interannual variability of the SCS SST is closely related to the ENSO events. In the cases of summers 2010 and 2018 that the authors had mentioned, summer 2010 was on the decay phase of the El Nino event 2009/2010, while summer 2018 followed the La Niña event 2017/2018 [Liu and Chu, 2019]. This fact explained why SST and Tref in 2018 were cooler than those in 2010, and ENSO is considered the main factor. If the same Tref is used for these years, upwelling is underestimated in 2010 but overestimated in 2018. Therefore, subtracting SST by time-varying Tref can help to eliminate climate variability coexisting in SST and Tref. In addition, due to the fact that the input SST for UI is daily data, using daily Tref consistently could make UI more accurate.

Indeed, the effect of ENSO on SST is clear and has been showed before (Qu et al. 2004, Wang et al. 2006) (note that despite our efforts we were unfortunately not able to find the paper of Liu and Chu 2019), with warmer surface following El Niño periods. However, Figure 17a of the paper also shows that the advection of upwelled water influences SST around the upwelling area. Moreover, previous studies also showed that ENSO influences the SVU, with weaker SVU following El Niño periods due to weaker monsoon winds (Da et al. 2019), implying weaker advection of cold water. Using a yearly-varying Tref partly eliminates the effect of climate variability, but not the effect of advection of upwelled water. Conversely, using a constant Tref reduces the effect of upwelled water advection, but does not take into account the influence of large scale factors like ENSO. Choosing a constant or a yearly-varying Tref therefore involves making tradeoffs between those influence factors.

The choice of a seasonally averaged vs. daily varying Tref involves similar tradeoffs (lateral advection of upwelled water vs. seasonal variability). Using a daily Tref would indeed help to take into account the background variability of SST induced by large scale forcings, but it would also increase the effect of lateral advection. Figure B below indeed shows the daily SST for days 48 (peak of OFU, mid-July), 61 (end of July, period of OFU weakening) and 75 (2nd peak of OFU, mid-August) of the JJAS 2018 period : the SST in BoxRef is clearly influenced by the advection of water upwelled over BoxOF for in mid-August, and to a lesser extent, mid-July, and much less during day end of July. Using a daily varying Tref would therefore result in an overestimation of upwelling, over all boxes, during the weak upwelling period vs. an underestimation during upwelling peaks.

→ Following this comment of the reviewer, we acknowledged and discussed the fact that those choices of Tref involves tradeoffs in the revised version of our paper (Section 5.1, lines 632-641).

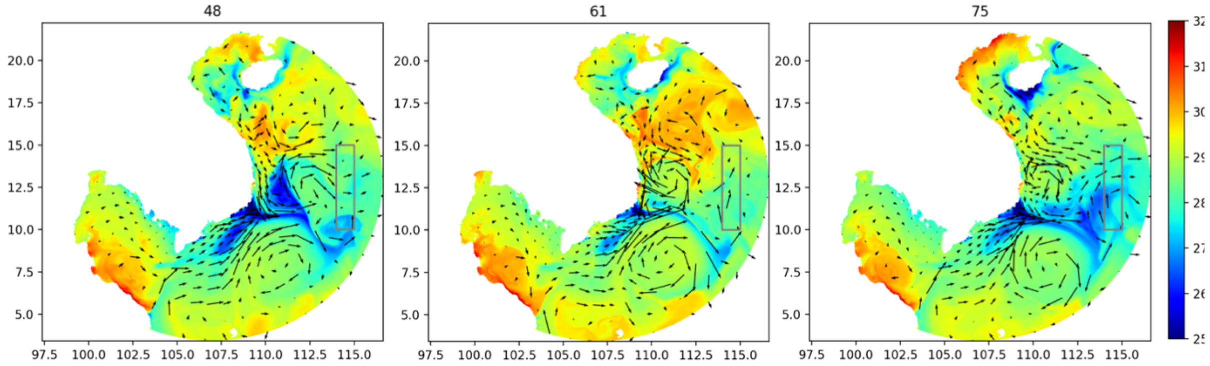

Figure B : Daily SST simulated during day 48 (mid-July), 61 (end of July) and 75 (mid-August) of the 2018 June-September period (°C).

2. Section 3

a) The authors used several sources of data to prove the occurrence of upwelling in BoxMK, and they convinced me. However, there is an issue that the percentage of covering days of JAXA over BoxMK in summer 2018 is low (below 60%), and on some days such as July 16th, the number of valid pixels in the satellite image is inadequate for a significant statistics, making values of minimum SST meaningless in Figure 5b. For this reason, I suggest the authors to use a scatterplot for JAXA as they did in Response, and mark values derived from the data with a low percentage of coverage by another color.

There are more than 15 000 JAXA pixels in BoxMK. We show in Figure C the daily time series of the percentage of pixels of quality level ≥ 4 over BoxMK. We also show the scatterplot of minimum SST, with days with a coverage lower than 50%, higher than 50% and higher than 60% highlighted. There are more than 60% of pixels of quality level ≥ 4 during 68 over the 122 days of summer 2018, i.e. 56 % of the time. Figure C shows in particular that at least 60% of BoxMK is covered with data of quality level ≥ 4 during the periods of low SST (mid-June, Mid to end July, Mid to end August). Those results show that at the first order, it is statistically meaningfull to use JAXA observations to assess the evolution of minimum SST during the summer period over BoxMK.

→ Following this comment, we included the figure with scatterplot in our revised version of the paper, and added a few lines of comments in the text (Section 3.1, lines 266-269 and Figure 5).

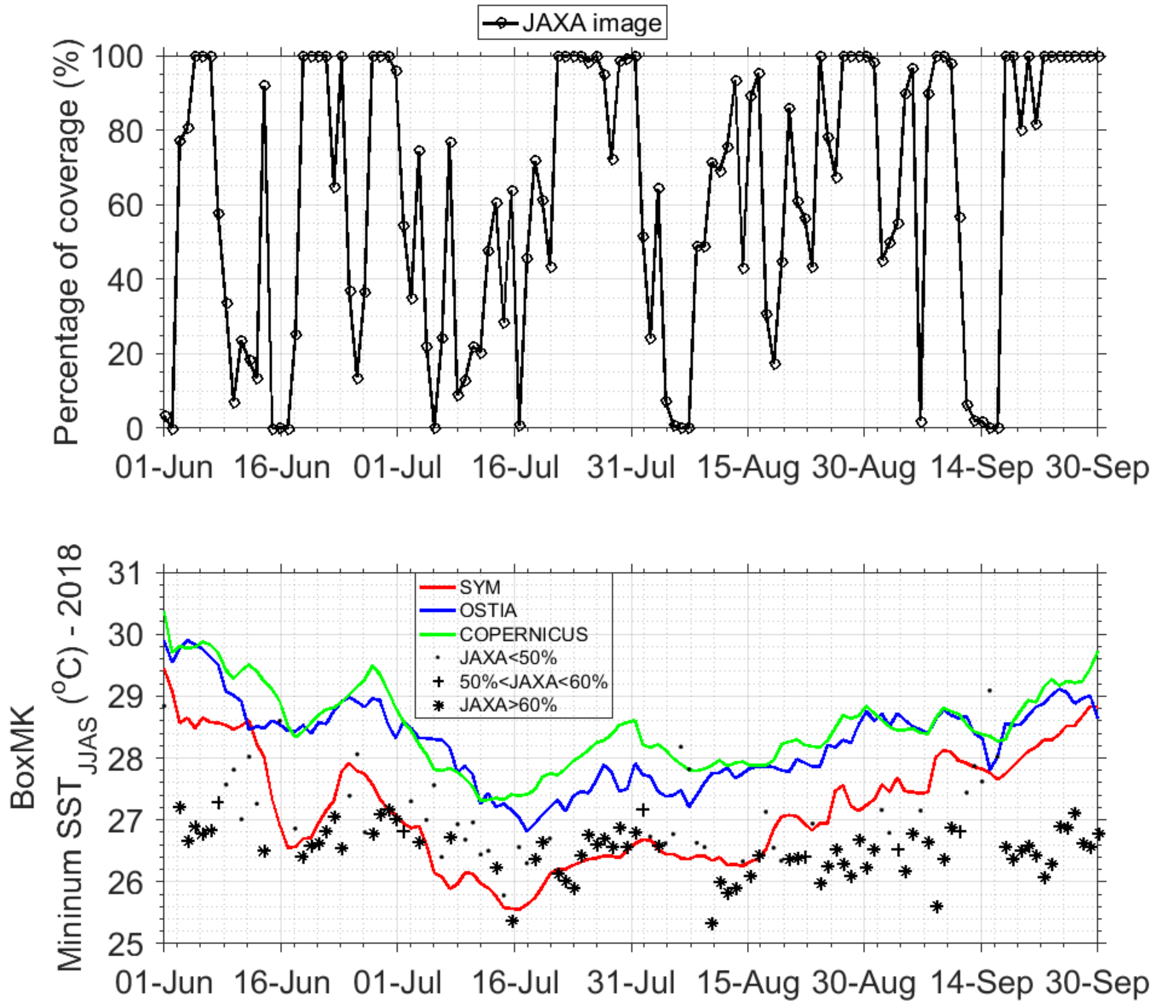

Figure C : Daily time series of JAXA coverage of BoxMK (top, %) and of minimum SST over BoxMK in SYMPHONIE, OSTIA, COPERNICUS and JAXA (bottom, °C). For JAXA, we only consider data of quality level ≥ 4. Dots, crosses and stars correspond respectively to a coverage smaller than 50%, between 50% and 60% and above 60%

b) Section 3.2: The length of this Section can be optimized by reducing the repetition of information that has been mentioned in the Introduction.

→ Following this comment, and the detailed comments below, we reduced as much as possible the repetition of information already provided in the introduction (Section 3.2.1, lines 295-298 and 327-330).

c) The authors presented TS characteristics simulated by SYMPHONIE in the whole Section 3.3 (Line 353-401), with the aim of highlighting the capability of SYMPHONIE in simulating water masses in the coastal areas and open-sea as well. The description did not make any further contribution to upwelling analysis afterwards, which turns this Section into a disconnection message from the general structure of the paper.

This section was written in order to show that the model is able to realistically simulate water masses in the coastal and open-sea areas of our domain. It was even developed during the first revision, following the request of the 2nd reviewer. The 2nd reviewer indeed insisted on the fact that even if we use surface variables to quantify the strength of the SVU and investigate the factors involved in its interannual variability, it is relevant to show that the model reproduces correctly the tridimensional water masses in the computational and study areas : this is a guarantee that the model reproduces realistically the SCS dynamics, including the surface dynamics that are influenced by vertical stratification and vertical dynamics. Note that including this evaluation in the paper will also be useful if the model is used, by us or by other authors willing to use our simulations, to investigate other processes than the SVU.

→ Following this comment, we decided that the comment of the 2nd reviewer was also reasonable and decided to keep section 3.3. We however added some text in the revised paper to justify this and take into account this comment of the reviewer here (section 3.3, lines 353-357)

3. Section 4

a) The authors used the spatial integral of the positive relative vorticity over BoxOF in JJAS ($\zeta+$) as an indicator of the intensity of the summer circulation (AC/C dipole + eastward jet) in the offshore region. I don't think this is a good indicator. The negative and positive vortices of a dipole do not always develop in conjunction, so the intensity of a dipole cannot be evaluated just by one side. For example, in the years 2011, 2013, and 2015, values of $\zeta+$ were similar but the dipole evolved in three different patterns. As a result, the direction and intensity of the jet in these years were various.

Circulation in the SVU region is characterized by the dipole structure and eastward jet. To represent this circulation, one can indeed use several indicators, and even combine them : positive relative vorticity over BoxOF but also negative vorticity, speed of the eastward jet, meridional position of the eastward jet, e.g. Ngo and Hsin 2021.... We chose the $\boldsymbol{\zeta}_{+,OF}$ indicator because 1) it is an indicator of the intensity of the cyclonic activity that develops northern of the jet over BoxOF ; 2) it is an indicator of the strength of the jet (a stronger jet is characterized by a higher positive vorticity on its northern flank) ; 3) the cyclonic activity (both related to eddies and eastward jet) has been shown to influence OFU and SCU (Da et al. 2019).

We completely agree with the reviewer that this choice, as every choice of an integrated indicator, is associated with limitations. In particular, as underlined by the reviewer, it does not account for the spatial characteristics of the positive vorticity and does not allow to fully represent the full range of situations. This can be seen by examining the cases of years 2011, 2013 and 2015 in Figure 12 of the paper. All years show similar $\boldsymbol{\zeta}_{+,OF}$ and similar JJAS wind stress intensity (Figure 13). However, the spatial distribution of this positive vorticity and the values of UIy bof BoxSC and BoxOF differ. 2011 shows a double dipole structure favoring the upwelling. 2013 shows an eastward jet located in the south and weak activity northern of the jet, not particularly favorable to upwelling. 2015 shows an eastward jet located in the north with a strong anticyclone located over BoxOF, preventing the upwelling.

To go beyond the limitations of this indicator, it is therefore necessary to examine into details the impact of spatial patterns of the mesoscale circulation on the upwelling. We began to do this in the present paper, and will go into much more details using ensemble simulations to examine the impact of OIV in a coming paper.

→ Following this comment, we added a paragraph to discuss the impact and limitations of the choice of this integrated indicator (Section 5.3 lines 705-712 and 721-722 and Section 4.1.2. line 443)

b) One of the important conclusions (or new findings) is that NCU is not primarily driven by the intensity of the summer wind over the SVU of BoxNC region, which is drawn due to the low correlation coefficient between UI of BoxNC and WS averaged over the same area (I "assume" the area is around [108.5-110E, 12-15N] because no detailed information was provided). Besides some issues in the definition of UI as mentioned above, I can try to figure out certain differences between their results and Ngo and Hsin (2021), which is the study they compared with.

Ngo and Hsin (2021) used the wind-based upwelling index to find that wind stress-induced upwelling-favorable condition changes correspondingly with the change of orientation of the coastline along the southern Vietnamese coast, and the roles of two components of wind stress (WSU and WSV) also change in driving Ekman transport along the coast. In the NCU, the favorable condition of wind stress for the upwelling is mainly contributed by the WSV because the WSV induces Ekman transport pushing water

offshore. Whereas, the WSU plays a role in restricting the development of the coastal upwelling (i.e. pulling water onshore). Thus, it appears clearly that the role of wind stress on the coastal upwelling cannot be illuminated if the total wind stress is just considered.
In addition, Ngo and Hsin (2021) analyzed the spatial distributions of correlation coefficients between SSTUI in their NCU with both WSU and WSV over the whole SCS, instead of only presenting the averaged WS over a chosen region, whose size or location is sensitive to the result of correlation analysis. Their results showed that the the SSTUI in the NCU is positively correlated with the local WSV (R=0.4~0.67) and has a negative remote connection with WSV in the southern SCS (R=-0.4~-0.7).
Based on the above two points, I think that the role of wind on NCU has been investigated carefully by Ngo and Hsin (2021) and are more reliable than the the authors' argument unless the detailed evidence can be provided. Besides, the dissimilarity in the number of upwelling events in NCU (in other upwelling regions as well) also contributes to the variance in results between the two studies.

We warmly thank the reviewer for this comment : it encouraged us to examine into more details the relationship between wind stress and NCU. Following this comment and the analysis of Ngo and Hsin (2021), Figure D shows the map of average JJAS wind stress over the 2008-19 period, and the maps of correlations between yearly time series of UIy (for SCU, NCU and OFU) and JJAS wind stress intensity over BoxOF on one side, and the (spatially dependent) times series of JJAS wind stress components and intensity on the other side.

First, the intensity of JJAS wind stress over boxOF is highly significantly ($p<0.01$) positively correlated with the zonal and meridional components and the intensity of wind stress over most of the domain (Figure D-e). But it is negatively correlated with the zonal component and intensity of wind stress in the northeastern part of the domain (including BoxNC). The intensification of the summer monsoon wind over the region is thus associated with an intensification of the wind over most of the area, but a weakening in the northwestern part of its intensity and meridional component, i.e. a less northward and more eastward direction; and vice versa.

Second, correlations maps for UIy for OFU and SCU are very similar to the correlations maps for JJAS wind stress intensity over boxOF, with larger areas of highly significant correlations for SCU than for OFU (Figure D-c,d). This confirms the link, stronger for SCU than for OFU, between the summer intensity of upwelling over those regions and of the intensity of summer monsoon wind.

Third, the correlations maps for NCU are of opposite signs to the other maps. When the summer monsoon wind intensifies (favorable to SCU and OFU), the wind stress and its meridional component weaken in the northeastern region, where it becomes more eastward. The wind is therefore less parallel to the coast over BoxNC and more offshore oriented, i.e. not favorable to NCU. It is the opposite when the summer monsoon wind weakens. Wind conditions favoring OFU and SCU (and MKU) therefore prevent NCU, and vice versa.

Last, correlations are much more statistically significant ($p<0.01$) for SCU and, to a lesser extent, OFU, than for NCU ($p<0.05$ or 0.01 only in very small coastal areas over BoxNC). Those results therefore suggest that wind also participates to the interannual variability of NCU, with favorable conditions opposite of those favorable to SCU, OFU and MKU, as already showed by Ngo and Hsin (2021). However, they further reveal that the influence of wind is not as important as for the other areas, and that other factors may be playing roles of equal or stronger importance, in particular OIV and mesoscale structures.

→ Following this comment, we added a figure (Figure 16) in the document and paragraphs about this in the paper (Section 4.4, lines 540-564 and Section 5.4, lines 745-759 and 766-767), and modified the abstract (lines 37-38) and conclusion accordingly (lines 823-829).

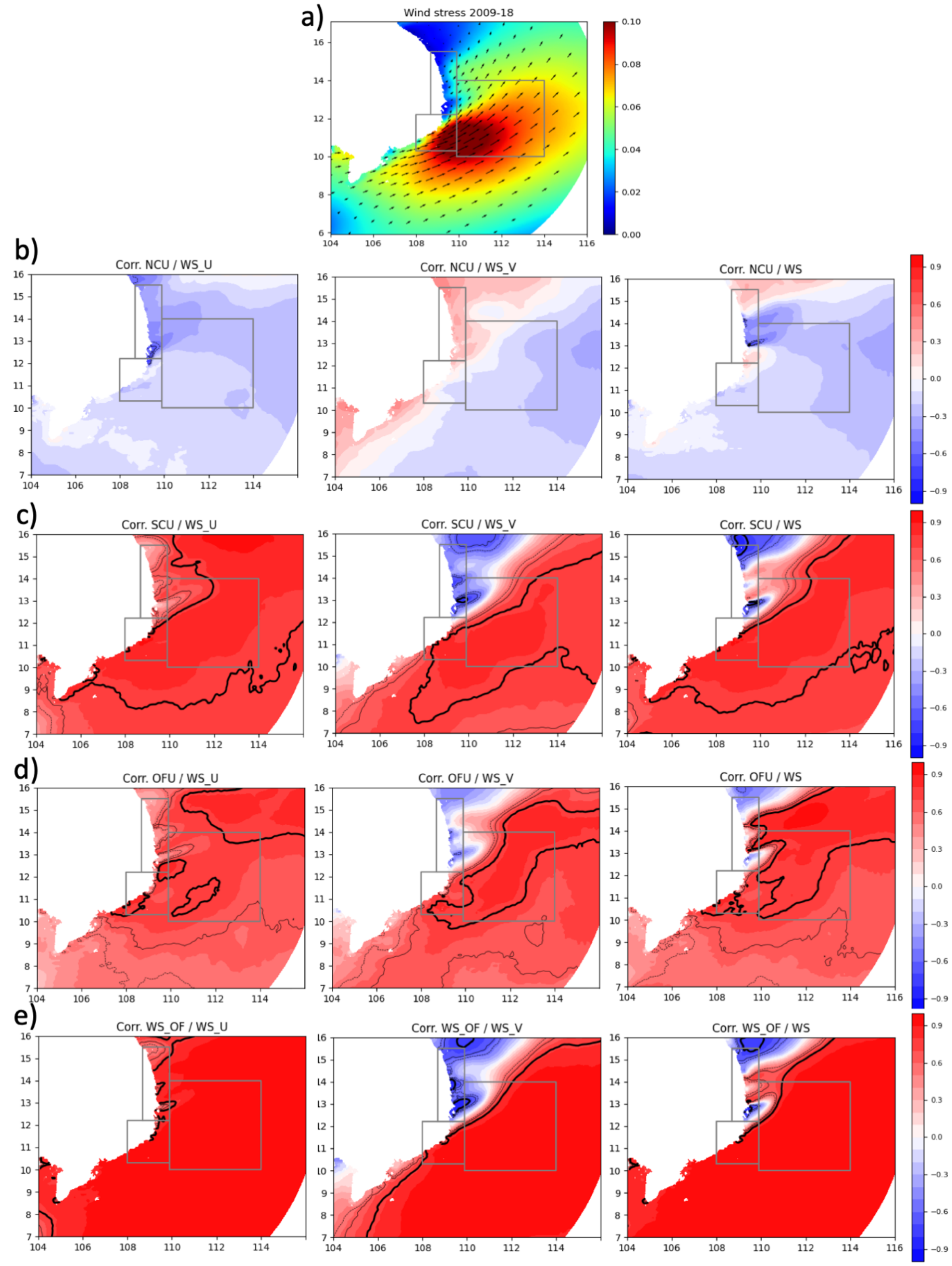

Figure D : (a) direction (arrows) and intensity (N.m-2) of average JJAS wind stress over the SVU area during 2009-2018 from ECMWF analysis. (b) to (e) Correlation between yearly time series of the zonal (left) and meridional (middle) components of wind stress and of its intensity (right) and (b) UIy,NC (c), UIy,SC (d), UIy,OF and (d) JJAS wind stress intensity over BoxOF. Thick, plain and dotted lines correspond respectively of the isolines p=0.01, p=0.05 and p=0.10.

c) Influences of the alongshore and offshore currents on the NCU have a better presence. The finding that the enhancements of SCU and NCU could relate to the appearances of the secondary dipole east of BoxNC and the associated jet in-between is novel and interesting. However, the condition for the appearance of the secondary dipole and the associated jet is still unclear and needs more quantitative analysis to get a solid conclusion.

Indeed, in the previous version of the paper we did not investigate into details the conditions that favor/prevent the development of a secondary dipole and of the NCU. However, the answer to comment 3b above were we investigated into more details the link between NCU and the wind comments contributed to better understand this : wind in the NCU area with a strong eastward component contribute to prevent the development of NCU. Moreover, we performed sensitivity simulations on the 2018 case study that revealed that the ocean intrinsic variability also strongly triggers the development of those structures : with the same external conditions (atmospheric, oceanic and continental), simulations with perturbed initial conditions can result or not in secondary dipoles, due to the chaotic behavior of the submesoscale to mesoscale dynamics. For the sake of conciseness and clarity, those results were not presented here but a second paper is being written to present them.

→ Following this comment, we added a sentence in the text to mention the need to perform further studies to better understand factors triggering the development of the circulation, and secondary dipole, along the northern coast (Conclusion, lines 828-829).

Besides, the authors used the current in the upper 1m depth for their analysis, which is primarily driven by wind forcing. Can the current at the sea surface demonstrate properly (sub)mesoscale circulation, OIV, or ocean dynamics in the SCS?

To answer this question, we explored the vertical extension of surface mesoscale structures. Figure D below shows the example of August 1st, 2018. At this date, a large anticyclonic eddy develops near ~11°E / 8°N, and a smaller one near ~13°N, with a strong eastward current in between. We plot the horizontal velocity normal to sections A and B that cross those eddies : those eddy structures extend vertically, with high velocities (between 0.5 and 1 $m.s^{-1}$) until 50 to 100 m depth, and significant velocities (up to 0.2 $m.s^{-1}$) down to 500 m depth. Exploring other days of the summer period shows similar results. The surface mesoscale currents simulated by our model is therefore representative of the circulation over the surface and subsurface layer, and can therefore be used to examine (sub)mesoscale dynamics in the SCS.

→ Following this comment, we added a paragraph in the revised version of our paper (Section 5.3, lines 723-727), and added this figure in the supplementary materials (Figure SM2).

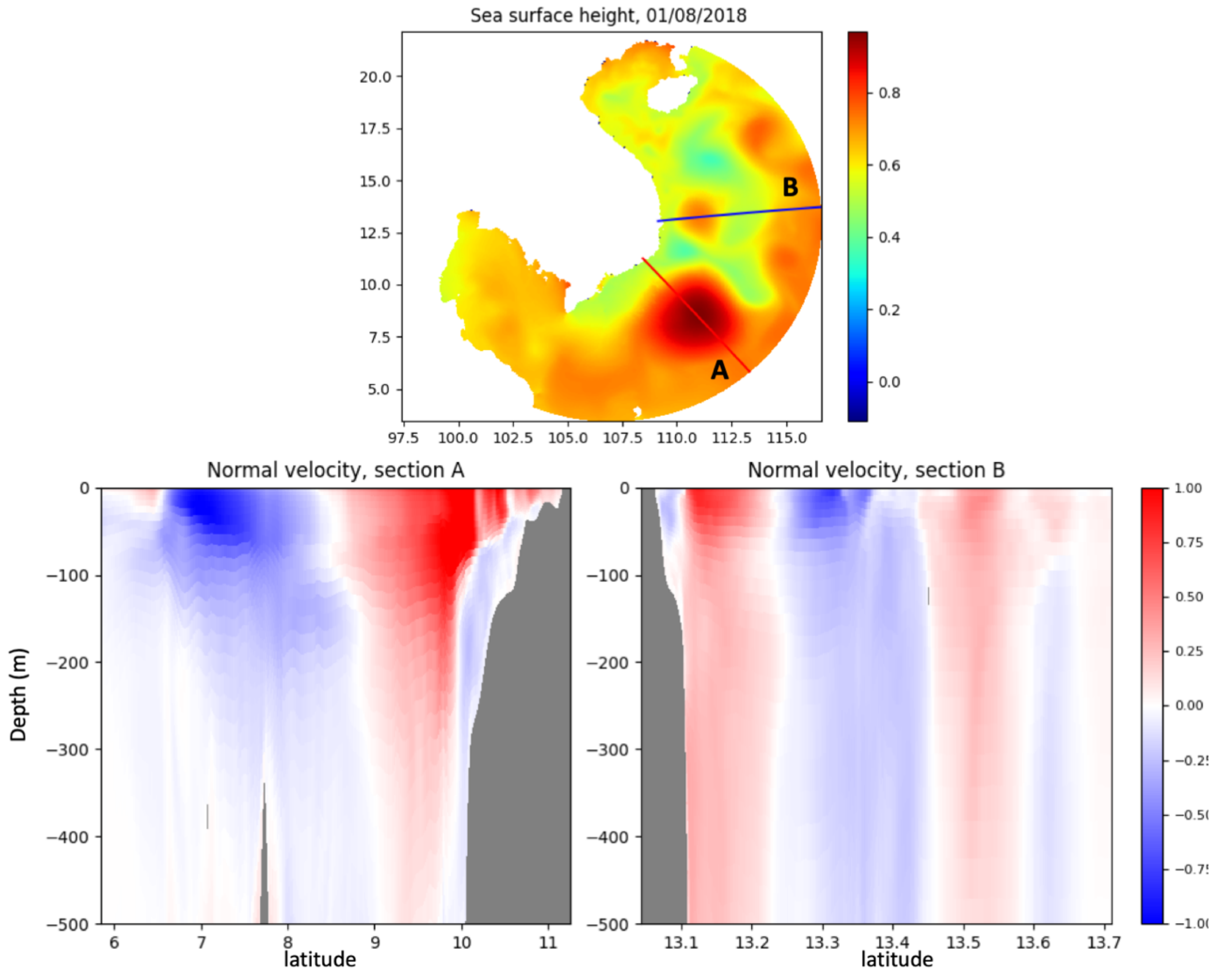

Figure 17 : Simulated daily sea surface height (m, top) and velocity (m.s^{-1}) normal to sections A (bottom, left) and B (bottom, right) on August 1st, 2018. Positive (negative) values correspond to a normal velocity oriented to the north (south).

d) In the MKU, the authors showed a strong relationship between UI and summer wind and summer offshore circulation based on high correlation coefficients; however, physical mechanisms for this upwelling region is lack.

Indeed, in this study we focused on the interannual scale, and wanted to cover the four boxes. For the sake of conciseness, we therefore chose not to explore here the functioning of the MKU, that our study revealed for the first time. However, it is indeed fundamental to perform detailed studies dedicated to the understanding of the physical mechanisms of this upwelling. This is an undergoing work in our group, where we are investigating the effects of different factors (wind, currents, topography, rivers, tides…). Our results will be presented in a coming paper that we are presently preparing.

→ Following this comment, we acknowledged the need to understand into details the physical mechanisms involved in the functioning and variability of the MKU (Conclusion, lines 841-843)

e) The authors revealed that “the daily to intra-seasonal chronology of wind stress contributes to the summer average of SCU and OFU intensity, and their interannual variability”. I totally agree that but this is not a new finding. The summer SCU or OFU is calculated by averaging the daily SCU or OFU which has a strong correlation with the daily wind stress. Thus, summer SCU and OFU obviously are influenced by daily or intra-seasonal wind stress.
Indeed, previous authors already showed that upwelling develops under the influence of wind peaks. Liu et al. (2012) for example showed that on the intraseasonal time scale, SST cooling develops with the

evolution of the southwesterly wind anomalies with nearlyone week delay, and revealed the influence of MJO but also typhoons. We further show in our study that the chronology of those peaks matter : with the same summer average (see 2018 vs. 2009 and 2012), and even with the same July-August average (see 2012 vs. 2009 and comment below), involving strong wind peaks for the 3 cases, the upwelling is stronger if wind peaks occur regularly during the July-August period, with peaks spaced by a sufficient period to let the upwelling develop and maintain it.

→ Following this comment, we acknowledge the fact that previous authors already highlighted the role of intraseasonal to daily chronology, and better explain that we further highlighted here the role of the timing of the wind peaks itself (Introduction, lines 96-98, Section 5.2, lines 661-669 and Conclusion, lines 816-818 and 821-822).

f) By examing three summers of 2009, 2012, and 2018, the authors concluded that the summer OFU is stronger when wind peaks occur during the core of the summer season (July-August) than at the beginning (June) or end (September). My question is whether it true for every year when the summer monsoon matures in July-August?

This comment is partly related to the comment above. Indeed, at the second order, other factors are certainly involved, as can be deduced by considering again the cases of 2009 vs. 2012 (Figure 15 of the paper). The wind stress over Box OF is stronger in June and August in 2012 than in 2009, and similar in July, and the JJAS vorticity $\zeta_{+,OF}$ is similar (Figure 15 of the paper). However, OFU is 50% weaker in 2012 than in 2009 on the yearly average (Figures 13,15 of the paper). A first factor explaining this difference could be the daily chronology of wind inside the July-August period : 2009 shows 2 distinct wind maximum periods in mid-July and mid-August, whereas 2012 shows 2 close peaks during the 2nd half of July. Those results suggest that the wind chronology not only at the intraseasonal scale but even at the daily scale inside the July-August period affects the development of the upwelling. The fact that the summer monsoon matures in July-August is therefore important, but is not the only factor. This result is supported by previous studies (*Xie et al. 2007, Liu et al. 2012),* who highlighted the effect of intraseasonal wind variability including the effect of Madden Julian Oscillation and typhoons. The impact of OIV (ocean intrinsic variability) could also contribute to the difference, as shown by Da et al. (2019). Studies including sensitivity ensemble simulations testing in particular the role of those factors will allow to better understand the intraseasonal variability of the upwelling. This will be the scope of coming studies based in particular on the study of the strong SVU 2018 case study.

→ Following this comment of the reviewer, we developed the discussion concerning the impact of intraseasonal to daily variability of wind, underlining the need to explore those factors in dedicated sensitivity simulations (Section 5.2, lines 661-669 and Conclusion, lines 821-822). The need to develop studies about OIV was already mentioned in the previous version of the paper.

4. Section 5.2
The authors performed a simulation using summer wind stress in 2018 to exam role of intraseasonal variability of atmospheric forcing on the SVU. I think this part should be presented in another paper with more details on input wind data and the modelling results.
This simulation was performed following the recommendation of the other reviewer, who requested us to quantitatively highlight the importance of intraseasonal variability of wind. Following this comment, we therefore did not removed completely this part, but reduced it and explained that it will be examined into more details in further studies (Section 5.2, lines 671-681).

Minor Comments:

Line 70: “Ngo et al. 2021” should be “Ngo and Hsin 2021”
→ this was corrected

Line 220-221: “both in observed and simulated …” should be “both in observed and in simulated …”
→ this was corrected

Line 255: “vs.” is not suitable here.

→ this was replaced by "whereas it reaches"

Line 281: "the SSH temporal average over 2009-2018" should be "the averaged SSH over 2009-2018"
→ this was corrected

Line 413-414: should be " … horizontal surface current rotation"
→ this was corrected

Line 288: remove "very"
→ this was removed

Line 291-299: "Under …2000" and "In winter … datasets". These sentences prolong the paragraph unnecessarily.
→ These sentences were written to comment the main patterns of SST. Following this comment they were strongly shortened in the revised version of the paper

Line 327-331: "As explained …Figure 2k-l". I think this part should be shortened.
→ those sentences were shortened

Line 424: "….and related to the summer monsoon wind". This is an obvious argument.
→ indeed, but we kept it for the sake of readers who are not specialist of this area

Line 469: "all" should be replaced by "both"
→ this was corrected

Line 485: "double" should be removed.
→ this was removed

Line 485: "classically". This word is not appropriate when describing the dipole and the eastward jet.
→ this was removed

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