Reviewer 2

Comments on the paper entitled "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 To Duy, Herrmann, Estournel, Marsalex, Duhaut, submitted to Ocean Science (os-2021-121)

This paper investigates the interannual variability of the South Vietnam upwelling by using a modeling approach. The high resolution coastal circulation model is extensively validated by comparison with the data from different sources which makes the results convincing. The modeling approach seems appropriately designed for studying the upwelling events and their variability in a wide range of time scales: from daily to interannual. By considering high-frequency variations of the wind stress at the regional scale the authors have clearly demonstrated that **the magnitude of the wind variability at scales of days to weeks can partially explain large differences in upwelling intensity observed during years with rather similar mean wind forcing. This is the first valuable result of the study.** The second issue addressed is **how some specific features of the regional circulation can impact the interannual variations of upwelling by modifying the Ekman transport, precisely by adding a not wind-driven component to the total current velocity**. It was shown that the surface currents act differently in four considered sub-regions of a vast upwelling system of the South Vietnam. The background current can weaken of reinforce the upwelling intensity thus affecting the interannual variability.

The authors furnished an effort in analyses of modeling results, the data form observations, and they overall made up nice figures. I believe that conclusions of their work are interesting and could contribute to the general knowledge on scales of variability of the upwelling circulation in this part of the ocean and in other ocean regions.

I am convinced that this paper is worth publication after some major revisions. I provide below a list of the most important comments.

We warmly thank the reviewer for this careful and constructive review of our paper. We addressed all the comments below in our revised version of the manuscript. In this document, the reviewer's comments appear in black, and our answers in blue. Changes done in the manuscript following the comments of the reviewer are also highlighted in blue in the revised version of the manuscript. Line numbers and pages in this document refer to the highlighted version.

Major comments

- Abstract

The text after line 30 should be rewritten in order to demonstrate the forceful results. In the present version, I don't feel the major findings are presented in appropriate way. There a lot of generalities without precision and quantification. For example, it is difficult to understand what the authors mean by " … the impact of the … temporal organization of mesoscale ocean structures and atmospheric forcing". What is the message addressed in the last two lines: "… an interannual variability of upwelling (in Mekong box) is mostly determined by the summer wind and summer driven circulation in the region". I agree, but what novelty is behind this statement? I would suggest to avoid this kind of sentences and make the presentation of the results sharper, more incisive.

→ The abstract was completely rewritten to take into account this comment, writing things in a clearer and more precise way (**p 2**).

- Introduction

The scope of the study needs a more clear definition (ln.94-100). This research didn't start from zero. The role of the background current variability in the interannual variability of upwelling was already highlighted by Da et al. (2019). What was discovered before and what is focused in particular in the present study should be better introduced. This concerns the "processes", which were not clearly defined, and "scales" which are targeted.

Ln 99: The text should be reworded with respect to my previous comment. "The objective is … scientific" should be removed.

→ Following this comment, and the comment of the other reviewer, we developed in the Introduction the part about the existing knowledge about SVU (areas of development, role of wind, eddies and intrinsic ocean variability, **lines 70 to 107**). We also detailed the scope of the present study (**lines 114 to 129)** which fundamental objective is to better monitor, represent and understand the behavior of upwelling at smaller scales (meso to submesoscales), and over detailed areas (coastal to offshore) and the role of high frequencies (daily to intraseasonal).

Perhaps a clear definition of the numerical tool should be provided here. If it is different from the numerical model, it should be specified.

→ The numerical tool is actually the numerical model implemented over the area of study. This was not very clear so we removed this expression "numerical tool" and used "model" everywhere

- Section 2

The numerical model is briefly presented in this section. I think some terms require clarification. The first concerns "the biharmonic viscosity of momentum" and the second concerns "nudging". I assume the authors mean how the tidal motions were prescribed at the open boundaries. But the word tidal is missed in the text.

Following this comment we corrected the sentences to be more precise and rigorous :

→ "the biharmonic viscosity of momentum" was changed to "the viscosity of momentum associated with this biharmonic scheme" (the viscosity *per se* is not biharmonic) **(lines 144-145)**

→ Open boundary conditions are prescribed for the temperature, salinity, and total (i.e. including effect of tide but not only) currents and sea surface height : "The lateral open boundary conditions, based on radiation conditions combined with nudging conditions, are described in Marsaleix et al. (2006) and Toublanc et al. (2018)." was changed to "The lateral open boundary conditions for temperature, salinity, current and sea surface height, based on radiation conditions combined with nudging conditions, are described in Marsaleix et al. (2006) and Toublanc et al. (2018)." **(lines 146-148)**

Ln 126: the authors use the term "zoom on the VN coast". I don't have impression that the technique of zoom was implemented in the model. This needs clarification.

Indeed technically it is not a classical zoom with 2 configurations at different resolutions.

→ We replaced "zoomed" by "with a refined resolution" **(lines 155)**

Presentation of the data sources. Sometimes, the information provided is absolutely useless: for example the program code, the name of PI. On the contrary, some acronyms need clear definition such as IO.

→ Text was modified to take into account this comment (remove useless information and add definitions such as IO) in **Section 2.2.2**, **p7)**

The term "hydrological characteristics of water masses" is misleading. The temperature and salinity are used for water masse characterization. What role the hydrology plays (the freshwater input, as I understand the term) in modifying T,S characteristics is unclear.

Indeed, some authors use the term "hydrological characteristics" for temperature and salinity characteristics (eg. *Criado-Aldeanueva et al. 2006*), but we agree that it is quite uncommon and may be confusing.

→ We consequently changed the occurrences of "hydrological characteristics" in the manuscript to "Temperature and salinity (TS) characteristics" **(lines 193, 353, 359, 384)**

My major concern is about the definition of the upwelling indicators and the choice of the reference box which area is much smaller than that of the corresponding upwelling box. The authors should justify their choice of the reference box size and the temperature values. When the size is small and the reference location is close to the upwelling region, the reference temperature is obviously dependent on the temperature observed during the upwelling event. To what degree this quantity is independent? This needs clarification as the results could be sensitive to the choice of the reference value. What could be the difference if an overall mean temperature (space and time mean) is used as the reference value?

Following this comment and a comment from the other reviewer, we slightly modified (and justified) our computation of Tref.

First, we completely agree that the SST over the reference boxes chosen for BoxMK, BoxSC and BoxNC may be influenced by the propagation into the reference box of water upwelled in other areas. This is all the more the case when the reference box is small. Moreover, using different Tref makes the upwelling index field spatially discontinuous. We therefore recomputed everything with the same Tref for all the boxes, taking Tref for BoxOF, which can be considered as large and far enough from the upwelling areas. We obtain a value of Tref=29.20°C. Using different boxes, our former values were $Tref_{NC}$=29.66°C, $Tref_{SC}$ = $Tref_{MK}$= $Tref_{OFF}$ 29.20 °C. Except for BoxNC, using different references boxes actually was thus equivalent as using a unique reference box. Note also that even for BoxNC, the difference of upwelling index value induced by the new choice of Tref will be very small. Given the formulae used for UId and UId (Equations 1 and 2 of the revised manuscript, see below), the relative difference between UIy and UId computed using the new and the old Tref will be equal to (Tref,new - Tref,old)/Tref,old = (29.2-29.7)/29.7 = -0.017 ~ -2%).

$$UI_{d,boxN}(t) = \frac{\iint_{(x,y) in\ boxN\ so\ that\ SST(x,y,t)<To}(T_{refN} - SST(x,y,t)).dx.dy}{A_{boxN}} (1) \quad \text{and} \quad UI_{y,boxN} = \frac{\int_{JJAS} UI_{d,boxN}(t)dt}{ND_{JJAS}} (2)$$

Table A below shows the updated values for Table 2 of the manuscript. Logically (since Tref only changes for BoxNC, by less than 2%), UIy (mean, std, CV) values do not or only very slightly change. Moreover, most of the correlation values remained unchanged. Those which changed changed by less than 0.03 (see table below). Our conclusions concerning the relationships between the different factors were therefore robust to this choice of reference temperature.

Table A : Modified Table 2 with a constant and unique Tref=29.2° (values that were modified compared to the previous manuscript are highlighted in red and italics). From 1st to last line : temporal mean and standard deviation of UIy,boxN over 2009-2018 for each box and coefficient of variation CV (which is the ratio between STD and mean), correlations (correlation coefficient and associate p-values) between time series of significant factors : yearly upwelling index over each box vs. yearly upwelling index over other boxes, vs. average wind stress averaged over June-September (JJAS) and July-August (JA) over each box, vs. integrated positive vorticity over BoxOF. Correlations significant at more than 99% (p<0.01) are highlighted in bold.

	BoxNC	BoxSC	BoxOF	BoxMK
$UI_{y,boxN}$ mean (°C)	0.163 *vs. 0.195* *(0.14 in the previous manuscript was a typo)*	0.798 *(0.42 in the previous manuscript was a typo)*	0.074 *(0.09 in the previous manuscript was a typo)*	0.065
$UI_{y,boxN}$ STD (°C)	0.118 *vs. 0.14*	0.423	0.093	0.055
CV (%)	72 *vs. 71*	53	126	85

Correlation between :	$UI_{y,NC}$	$UI_{y,SC}$	$UI_{y,OF}$	$UI_{y,MK}$
$UI_{y,SC}$ (°C)	0.00(0.99) *vs. +0.01(0.98)*	**1**	**+0.73(0.02)**	**+0.83(0.00)**
$UI_{y,OF}$ (°C)	-0.26(0.47)	**+0.73(0.02)**	**1**	**+0.92(0.00)**
$UI_{y,MK}$ (°C)	-0.19(0.59)	**+0.83(0.00)**	**+0.92(0.00)**	**1**
$WS_{JJAS,NC}$ ($N.m^{-3}$)	-0.09(0.78) *vs. -0.10(0.77)*	-0.41(0.24)	+0.23(0.53)	+0.07(0.85)
$WS_{JJAS,SC}$ ($N.m^{-3}$)	-0.13(0.72)	+0.85(0.00)	+0.76(0.01)	+0.83(0.00) vs. +0.84 (0.99)
$WS_{JJAS,OF}$ ($N.m^{-3}$)	-0.19(0.60)	+0.81(0.00)	+0.77(0.01)	+0.80(0.01)
$WS_{JJAS,MK}$ ($N.m^{-3}$)	-0.08(0.81)	+0.78(0.01)	+0.63(0.05)	+0.72(0.02)
$WS_{JA,NC}$ ($N.m^{-3}$)	+0.04(0.90) *vs. +0.07(0.92)*	+0.18(0.62) *vs. +0.12(0.62)*	+0.54(0.11)	+0.38(0.28)
$WS_{JA,SC}$ ($N.m^{-3}$)	-0.15(0.69)	**+0.70(0.03)**	**+0.84(0.00)**	**+0.84(0.00)**
$W_{JA,OF}$ ($N.m^{-3}$)	-0.15(0.67)	**+0.69(0.03)**	**+0.84(0.00)**	**+0.82(0.00)**
$W_{JA,MK}$ ($N.m^{-3}$)	-0.11(0.77)	**+0.72(0.02)**	**+0.78(0.01)**	**+0.82(0.00)**
$\zeta_{+,OF}$ (s-1)	-0.28(0.43)	**+0.60(0.07)**	**+0.69(0.03)**	**+0.74(0.01)**

Table B : Modified Table A with a varying Tref (values that were modified compared to Table A are highlighted in red) :

	BoxNC	BoxSC	BoxOF	BoxMK
$UI_{y,boxN}$ mean (°C)	0.171	0.743	0.060	0.055
$UI_{y,boxN}$ STD (°C)	0.140	0.344	0.065	0.040
CV (%)	82	46	108	73
Correlation between :	$UI_{y,NC}$	$UI_{y,SC}$	$UI_{y,OF}$	$UI_{y,MK}$
$UI_{y,SC}$ (°C)	+0.11(0.78)	**1**	+0.63(0.05)	+0.73(0.02)
$UI_{y,OF}$ (°C)	-0.28(0.42)	+0.63(0.05)	**1**	+0.81(0.00)
$UI_{y,MK}$ (°C)	-0.09(0.80)	+0.73(0.02)	+0.81(0.00)	**1**
$WS_{JJAS,NC}$ ($N.m^{-3}$)	-0.01(0.97)	-0.43(0.21)	+0.21(0.56)	+0.09(0.80)
$WS_{JJAS,SC}$ ($N.m^{-3}$)	-0.25(0.48)	+0.72(0.02)	+0.77(0.01)	+0.76(0.01)
$WS_{JJAS,OF}$ ($N.m^{-3}$)	-0.30(0.39)	+0.66(0.04)	**+0.77(0.01)**	+0.69(0.03)
$WS_{JJAS,MK}$ ($N.m^{-3}$)	-0.20(0.57)	+0.66(0.04)	**+0.63(0.05)**	+0.65(0.04)
$WS_{JA,NC}$ ($N.m^{-3}$)	+0.05(0.89)	+0.14(0.70)	+0.53(0.11)	+0.30(0.39)
$WS_{JA,SC}$ ($N.m^{-3}$)	-0.23(0.52)	+0.54(0.11)	+0.82(0.00)	+0.73(0.02)
$W_{JA,OF}$ ($N.m^{-3}$)	-0.23(0.52)	+0.53(0.11)	+0.82(0.00)	+0.70(0.03)
$W_{JA,MK}$ ($N.m^{-3}$)	-0.20(0.59)	+0.59(0.07)	+0.77(0.01)	+0.74(0.02)
$\zeta_{+,OF}$ (s^{-1})	-0.37(0.30)	+0.43(0.22)	+0.68(0.03)	+0.64(0.05)

Second, we chose a constant Tref, as done previously by *Da et al. (2019)*, whereas other studies, like *Ngo and Hsin (2021)*, used an interannually varying Tref, to take into account the fact that SST can also vary on an interannual basis. We justify our choice of a constant Tref by the fact that even if the reference box was chosen outside the upwelling area, the SST in this box can be influenced by the eastward advection of water upwelled in the other boxes. This can be seen on Figure A that shows the JJAS map of simulated SST for summers 2010 (year of weakest upwelling) and 2018 (year of strongest upwelling). In 2018, the SST in the reference area is cooler than in 2010 due to the eastward advection of upwelled water. The upwelling index computed from the difference *SST(x,y,t) - SSTref*, would therefore be smaller in 2018 and larger in 2010 using a varying Tref vs. a constant Tref. In other words, a varying Tref would increase the weak values and decrease the strong values, hence reducing the interannual variability. Note however that the impact on weak values is actually limited by the use of the threshold temperature T_0. We investigated the influence of this choice on our results. Figure B shows the time series of Tref computed annually instead of a constant Tref, and the resulting yearly time series of UIy for each box. Table B provides the modified values of Table A. The resulting UIy values slightly vary, in particular for stronger values (see year 2018), but that the change is not significant. The interannual variability remains nearly the same, though it slightly decreases, as expected. Correlation coefficients consequently also slightly decrease (by at most ~0.10). However correlations that were statistically significant (or not), remain statistically significant (or not). Our conclusions are therefore still valid.

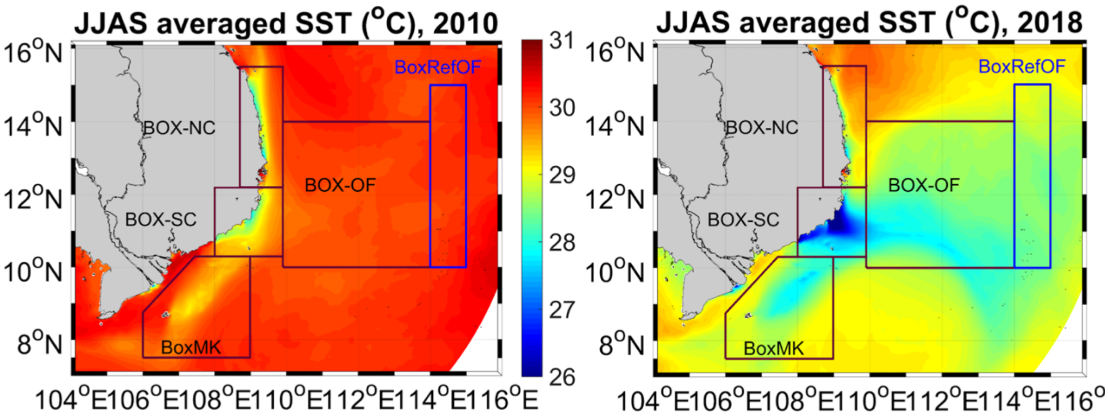

Figure A : JJAS average simulated SST in 2010 and 2018 (°C).

→ Following this comment**, Figures 12, 13, 14, 15, Table 2 of the revised manuscript** were modified after using the same and constant Tref for all boxes. The definition of Tref was modified (**Lines 225-229 in Section 2.3**). A new section **(Section 5.1, p19)** and a new figure (**Figure 16**) were moreover added to discuss the sensitivity of our results to a varying vs. constant Tref.

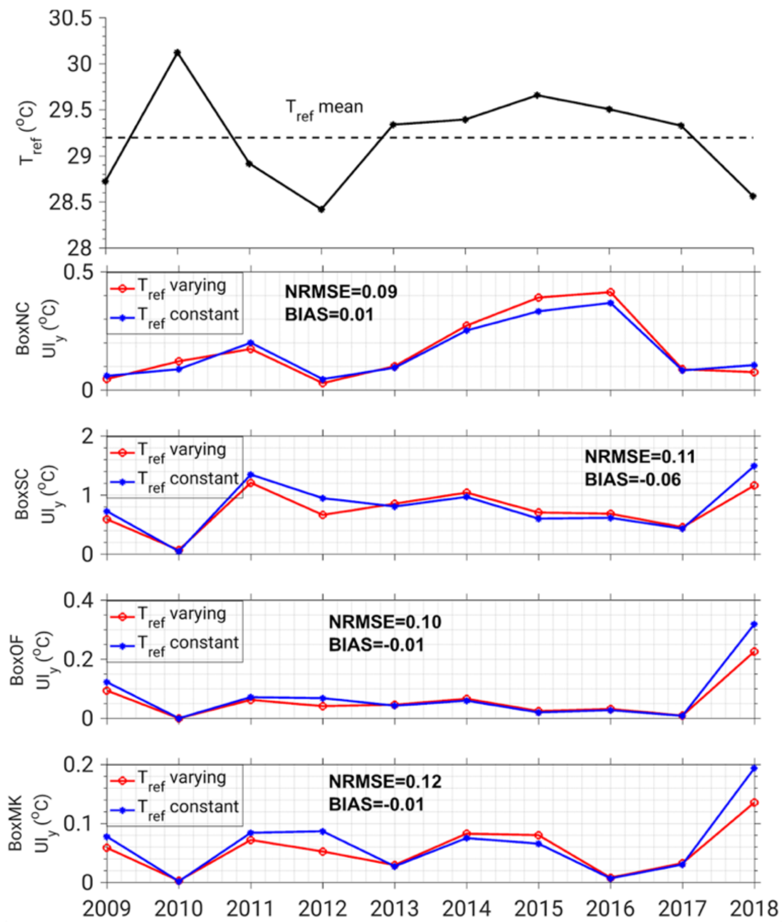

Figure B : (1st row) Yearly time series of interannually varying summer averaged SST over the reference box (i.e. =varying Tref, full black line) and value of the climatological summer averaged SST over the reference box (i.e. =constant Tref, dashed line). Yearly time series of UIy computed using a varying (red) vs. constant (blue) Tref, for BoxNC (2nd row), BoxSC (3rd row), BoxOF (4th row) and BoxMK (5th row).

Presentation of the results

- Section 3

As I indicated above, the authors furnished an effort in analyses of the data from different sources and in validation of the modeling results. The results are convincing. However only spatial distribution of different quantities at the surface is used in comparison. But the model is three-dimensional and high resolution. A demonstration of the **model capability in reconstructing the upwelling circulation (and related water properties) in the vertical plan** can be an added value. This can support the choice of high-resolution in the horizontal and also in the vertical.

The reviewer underlines the need to demonstrate the model capability in reconstructing the upwelling circulation (and related water properties) in the vertical plan. This was indeed our objective when comparing observed and simulated TS diagrams over the region (ARGOS, Figure 8a of the revised manuscript, and GLIDER, Figure 8b) but also in the upwelling area (IO-18 data, Figure 8c). Those data are not restricted to the surface, and go until ~200m to ~1000m, i.e. covering vertically the extent of the upwelling.

There are only few data that allow to observe the vertical dimension of the SVU during its period and over its are of development. IO-18 CTD profiles collected during September 2018 allow to explore this dimension. Figure C below shows the T and S profiles observed by IO-18 and simulated by SYMHONIE at each station of the IO-18 cruise. IO-18 TS diagram and profiles reveal two types of profiles in the deeper regions (i.e. reaching 150 m depth), corresponding to the nearly vertical part of the TS diagram. The first type of profiles was sampled along the section at ~12.7°N (points 2.1 to 2.5, located in BoxNC) and in the coastal part of the section along 10.5°N (points 3.2, 3.4, 3.5, located in BoxSC). It shows high salinities (> 34.5) and low temperatures (< 25°C) below a pycnocline shallower than ~30 m. It corresponds to location where upwelling still occurs at this period. The second type of profiles is sampled in the offshore part of a section at 10.5°N (points 3.6 to 3.8, in BoxOF). It has deeper haloclines and thermoclines, reaching 90 m, with SSS between 32.8 and 33.6 and warmer SST around 28°C : the upwelling already ceased in this region at this period. Both the TS diagram (**Figure 8c** of the revised document) and the TS profiles show that the model is able to reproduce this diversity of TS profiles in the coastal and offshore upwelling regions in very good agreement with IO-18 data, without any significant bias, even after 9 years of simulation.

→To take into account this comment and better highlight this vertical aspect, we therefore also show the vertical TS profiles of simulated and observed during IO-18 cruise (**Figure 10 of the revised manuscript)** and commented those figures in the revised paper of the manuscript (**end of section 3.3, lines 386-401**).

Last, collecting current data as well as high resolution observations in the SVU region and period, for example from glider campaigns, would allow to evaluate more precisely the ability of the model to reproduce the dynamics and water masses over the vertical dimension.

→We added a sentence to underline this need for future observations in the Discussion (**lines 731-735 in Section 5.4).**

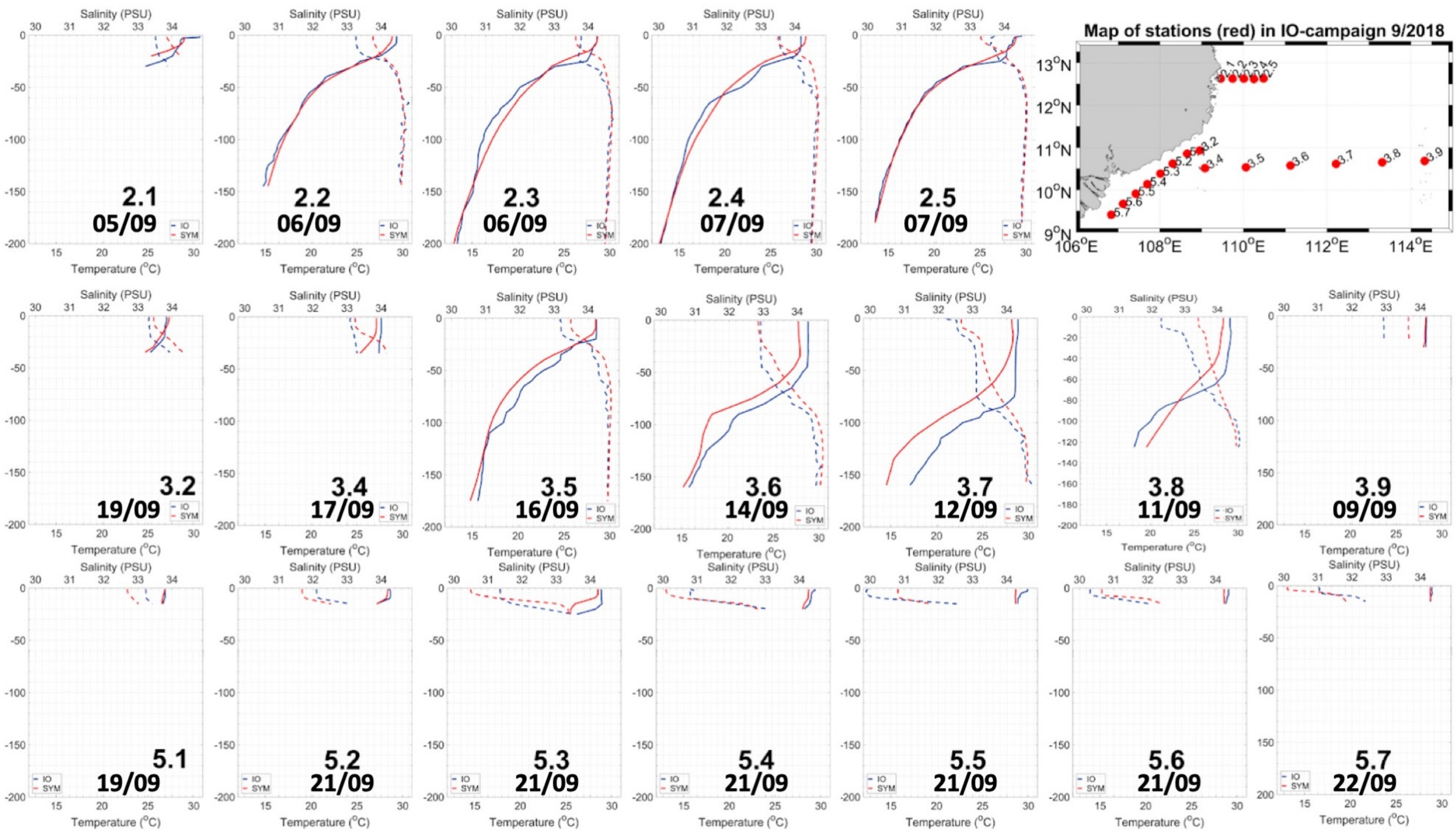

Figure C : Temperature (°C) and salinity profiles sampled during IO-18 campaign (blue) and simulated by SYMPHONIE (red) between September 12 and 25 in the SVU region.

The authors often use the term "coastal scale". The word coastal is not appropriate for scale definition. The scale needs clarification.

→ Indeed, we sometimes actually more precisely meant “coastal area”, or “coastal circulation”. We modified the text accordingly everywhere where “coastal scale” was used before.

- Section 4

In this section the authors explore in detail the upwelling variability for each year. Nine years in total are considered with strong and weak upwelling events. The effect of the mean wind in interannual variability of upwelling was verified but this is not a novel result. Mechanisms which can explain large difference in upwelling intensity between years with a similar mean wind are identified.

I have two major points of criticism regarding this part of the study.

The first point concerns the interpretation of the variability of the wind stress at high time resolution which is presented as the "other factor modulating the wind induced interannual variability of SCU" (ln 354).

The idea behind seems clear, but what is less clear how to quantify and interpret the effect of high-frequency variability. A method or a metric should be used in this demonstration. A visual inspection of the wind stress curves given in Fig. 9,10 is not sufficient. The authors use CV, the coefficient of variation. But how it helps in quantification of the contribution of high frequency compared to low frequency variability? This needs clarification.

The conclusion that the intraseasonal chronology of the wind forcing influences the seasonal average of the upwelling over Box SC and BoxOF was indeed obtained from the study of four summers (2009, 2011, 2012 and 2018). To quantitatively confirm this conclusion, we performed an additional simulation from June to September 2018 (the summer that shows the strongest wind stress and upwelling over BoxSC, BoxOF and BoxMK, Figure 13 of the revised manuscript). We prescribed during the whole summer 2018 a temporally constant (but spatially varying) wind stress to the model: for each point of the model, this constant was computed as the JJAS average of the 2018 daily wind stress at this point. Initial conditions for this simulation were taken as the conditions of June 1st, 2018 of the 2009-2018 simulation described in section 2. The summer average of summer wind in this simulation is therefore equal by construction to the average of summer 2018 wind in the 2009-2018 simulation, but it does not show any daily to intraseasonal variability. In this sensitivity simulation, the surface cooling is much weaker than in the 2009-2018 simulation, and no upwelling develops on any of the boxes during the whole summer. This quantitatively highlights the fundamental role of wind intraseasonal variability in the development of upwelling and in its summer average intensity.

Additional simulations would now be required to more quantitatively assess the role of intraseasonal variability vs. seasonal average of wind stress on the yearly upwelling intensity. In particular, ensembles of simulations with wind of same seasonal average but different daily chronology, and conversely, would help to estimate the variability of summer averaged upwelling induced by the variability of the intraseasonal chronology vs. by the interannual variability of summer average wind.

→ Following this comment, we added a section in the discussion (**Section 5.2 Role of intraseasonal variability of atmospheric forcing, p19**) in the revised manuscript where we discuss this point, present the sensitivity simulation and call for sensitivity simulations.

I see a small inconsistency in the interpretation of the "other factor modulating the wind induced variability". First, in this particular case, the high and low frequency variability of upwelling is wind-induced. The physical process involved in upwelling generation is the same. Second, I cannot imagine how the high frequency can modulate the low frequency signal. Different averaging techniques can provide different values of the mean quantity. But this is not sought as modulation. The authors should find a better formulation.

Indeed, the term "modulation" is a specific technical term in the domain of frequency signals, and using it here was a misuse of language.

→ We removed this term, and to replace it by more appropriate expressions : e.g. "contribute to", "influence" everywhere where it was used in the previous manuscript.

The second point of criticism concerns the role of the wind stress and surface currents in upwelling variability in different years. From my point of you, the **surface current velocity and the current velocity curl are tightly related to the wind stress curl** (example of anti-symmetric eddies and the eastern current). I have impression that only the wind stress magnitude is used in analysis. The added value of the study will increase if **analysis of the wind curl and perhaps the wind stress vector field can be introduced and comparison with current velocities be made**. This will help in interpretation of the surface current variability. What part of the current variability is wind-induced? and what part is remotely induced? The choice of the method of quantification is an important issue. And this is related to the statement (used for the second time) "another factor of non-wind origin" controlling the variability of upwelling. A part of the background current variability, independent of the wind, should be clearly identified and characterized. This requires a method of identification. I didn't see a clear description of such a method in the manuscript.

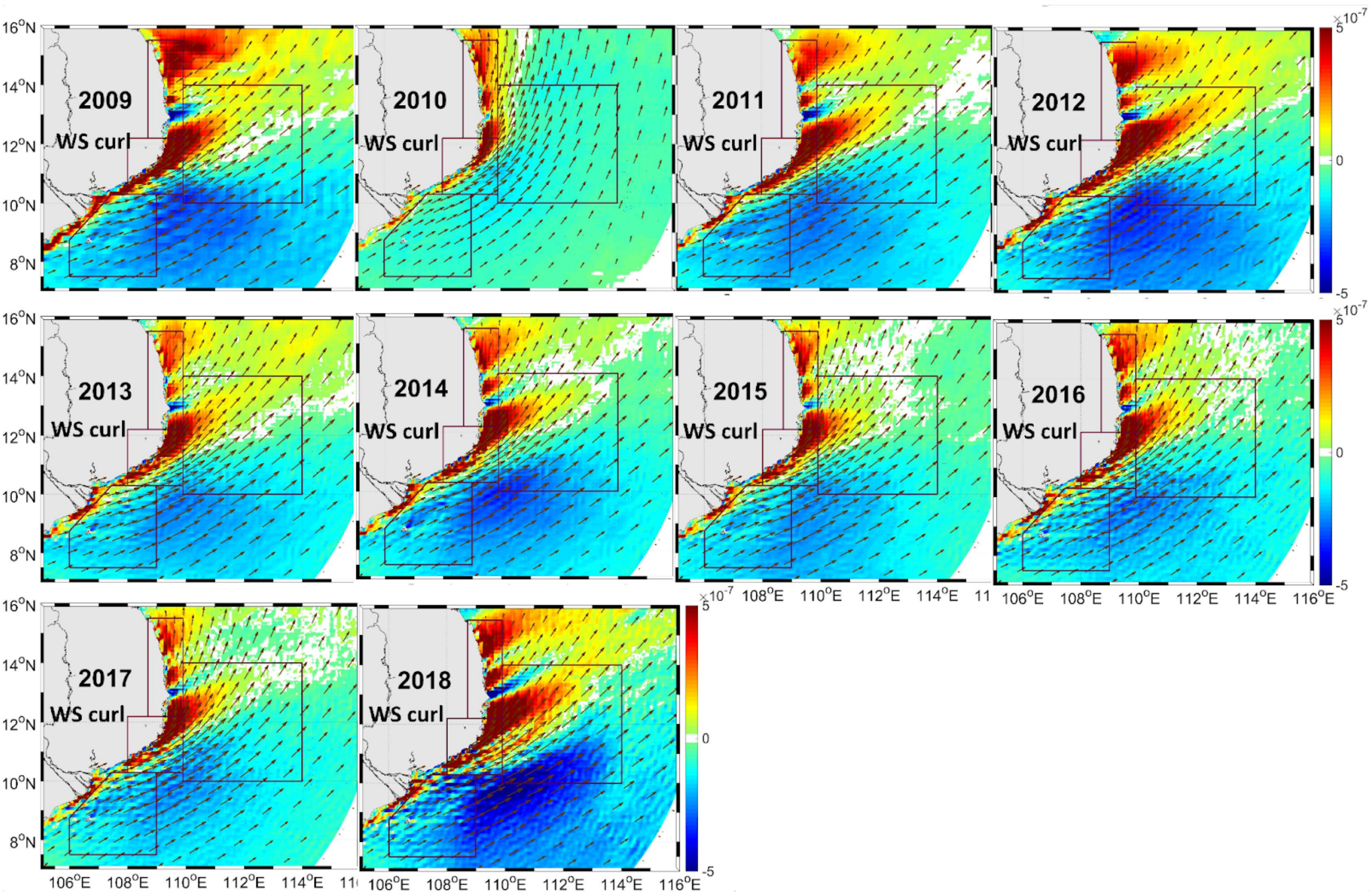

Figure D: Maps of JJAS averaged wind stress (arrows, $N.m^{-2}$) and wind stress curl (colors, $N.m^{-3}$) for each year of the simulation.

Following this comment, we developed the description and discussion of link between wind stress, wind stress curl, vorticity and upwelling, over the different areas of development :

1) Figure D above shows the maps of JJAS averaged wind stress and wind stress curl for each summer of the simulation, computed from ECMWF atmospheric forcing. The intensity of wind stress and wind stress curl shows a strong interannual variability, but their spatial patterns are quite similar from one year to another and related to the summer monsoon wind : a wind stress curl dipole develops offshore Vietnam, with an area of strong positive curl along and off the Vietnam coast in the north (covering BoxSC and a part of BoxOF), and an area of strong negative curl in the south (covering BoxMK and a part of BoxOF). Time series of the summer wind stress

over BoxSC and BokMK are almost equal to the summer wind stress over BoxOF (Figure 13) and completely correlated with it (>0.97, $p<0.01$, Table 2). The intensity and interannual variability of summer wind over BoxSC and BoxMK is thus completely driven by the large-scale wind over the region.

Correlations between JJAS wind stress (WS), wind stress curl (WSC) over each box and ocean vorticity over boxOF ($\zeta_{+,OF}$) where also computed (see Table C below). There is a highly significant correlation (> 0.95) between wind stress over BoxOF, BoxSC and BoxMK, and wind stress curl over both BoxSC (area of maximum positive wind stress curl of the wind dipole) and BoxMK (area of maximum negative wind stress curl of the wind dipole). The intensity of wind stress over the regions of upwelling is therefore strongly related to the intensity of the wind curl dipole located along the Vietnamese coast. We already showed that the interannual variability of JJAS regional wind stress partly drives the regional scale circulation (current jet + AC/C dipole, correlation of 0.89 between $\zeta_{+,OF}$ and $WS_{JJAS,OF}$, Table 2 of the manuscript). This circulation is also related to the wind stress curl (correlation of 0.82 and -0.81 between $\zeta_{+,OF}$ and $WSC_{JJAS,SC}$ and $WSC_{JJAS,MK}$, respectively).

Table C : correlation (and p-value) between JJAS average of wind stress curl (WSC) over each box, and JJAS average of wind stress (WS) and of BoxOF positive vorticity

Correlation between :	$WSC_{JJAS,NC}$	$WSC_{JJAS,SC}$	$WSC_{JJAS,OF}$	$WSC_{JJAS,MK}$
$WS_{JJAS,SC}$	0.40(0.26)	**0.95(0.00)**	**0.69(0.03)**	**-0.96(0.00)**
$WS_{JJAS,OF}$	0.42(0.23)	**0.96(0.00)**	**0.71(0.02)**	**-0.92(0.00)**
$WS_{JJAS,MK}$	0.24(0.51)	**0.96(0.00)**	**0.83(0.00)**	**-0.91(0.00)**
$\zeta_{+,OF}$	0.59(0.07)	**0.82(0.00)**	0.46(0.18)	**-0.80(0.01)**

→ A section was added in the revised manuscript (Section 4.1, Interannual variability of wind and large scale circulation), where this question regarding the link between wind stress, wind stress curl and large scale circulation and vorticity is addressed. Correlations between wind stress, wind stress curl and ocean vorticity where added in Table 2. Maps of wind stress vector field and curl was added in Figure 11 of the revised manuscript.

2) $\zeta_{+,OF}$ quantifies the intensity of cyclonic circulation over the offshore are, including both the intensity of the eastward jet, and of the cyclonic eddy circulation that develops northern of the jet. This eddy circulation is partly induced by wind, as shown by the highly significant correlations between $\zeta_{+,OF}$ and $WSC_{JJAS,SC}$, $WSC_{JJAS,MK}$ and $WS_{JJAS,OF}$. However it has by nature a strong chaotic part (see previous studies of *Waldman et al. 2018, Sérazin et al. 2016, Da et al. 2019*, cited in the Introduction). For example, wind is similar for 2014 and 2016 ($WS_{JJAS,OF} \simeq 0.08$ N.m^{-2}, Figure 13 of the revised manuscript), with similar patterns of wind stress curl (Figure D above), however $\zeta_{+,OF}$ is twice larger (~2.6×10^{-6} s^{-1}, Figure 13 of the revised manuscript) than in 2016 (~1.5×10^{-6} s^{-1}) and a very different circulation develops for both years (Figure 12 of the revised manuscript). This confirms that the variability of surface circulation and associated vorticity over the SVU region has a forced component driven by summer averaged regional wind, but also a chaotic component related to OIV. Ensemblist approaches that allow to distinguish and quantify the effect of the chaotic vs. forced component of ocean dynamics at different scales, are now required to understand which part of variability of the current and its vorticity, and of the upwelling, is wind-induced and which part is related to the formation and propagation of eddies of strongly chaotic nature

→ A section was added in the revised manuscript (**Section 5.3 Role of forced vs. chaotic variability, p20**) where this question about the forced vs. chaotic part of surface circulation and upwelling is discussed.

- Discussion and conclusions

Section 4.5 and section 5 (Conclusion) should be reorganized. Section 4.5 contains the discussion of the modeling results and should be entitled "Discussion". A part of section 5 also contains the discussion and should be relegated to "Discussion".

Conclusion section should contain the major and novel results in a condense form. The comparison with previous studies is already done (in principal) in Discussion section. The form is important. Please avoid sentences of five lines difficult to follow. Highlight what knew knowledge the study brought and in what it is different compared to the results of previous studies.

→ Following this comment, we deeply reorganized and rewrote the discussion (now the whole **Section 5, pages 19 to 22**) and conclusion (**Section 6, page 23-34).** In particular, we added in the new Section 5 a discussion about the sensitivity of our results to the choice of the reference temperature (**Section 5.1**), the role of intraseasonal variability of atmospheric forcing (**Section 5.2**), the role of forced vs. chaotic variability (**Section 5.3**), the role of local upwelling vs. lateral advection of cold water in OFU (**Section 5.5).** The conclusion was shortened and sharpened in order to highlight the new results of this study (it is now a little bit longer than one page).

Technical corrections

Ln 30: move "driving" to different location … our results confirm the role of the … in driving the interannual …

→ This was corrected, **lines 31-32**

Ln 34: perhaps "structures of circulation" ?

→ This was indeed not clear, and disappeared after the revision of the abstract

I would recommend replacing the word ability, when you talk about the model skill, by capability, in the whole text.

→ This was corrected (9 occurences in the text)

Ln. 103-105: When describing the paper structure, it is better to use the word Section, not Part.

→ This was corrected everywhere in the document

Ln 45-46: Please reword the text concerning the CSS contribution.

→ "contributes" to was replaced by "influences", **line 48**

Ln 91 ageostrophic dynamics

→ This was corrected, **line 112**

Ln 151-152 … level 3 SSS derived from SMOS … (MIRAS) measurements at 0.25° resolution

→ This was corrected, **line 184**

Ln163-164: The modeled outputs were spatially and temporally co-localized with observations and used for comparison.

→ This was corrected, **line 195**

Ln 165: put a dot after "area". The data are available …

→ This was corrected, **line 197**

Ln 201: I think there is a conventional way how to refer to a Figure in another publication: (cf. Fig. 1, Da et al., 2019)

→ This was corrected, **lines 243-244** (we also guess that this will be formatted in the journal format at the final stage of the publication)

Ln 206-2020: The text should be edited to make it clear. Frequency should be removed. I suggest : … from the analysis of the occurrence of …

Please choose the right order in index definition: UI should match upwelling index

Perhaps is it more simple to use a number 122 (days in four months) instead of NDjjas ?

→ The text was carefully edited to make it clearer, **lines 229-239**

Ln 224: the title: Surface circulation, temperature and salinity in the SCS. Perhaps it is better than hydrological characteristics.

→ This was corrected**, line 276**

The text in four lines following the title should be rewritten. I don't understand "interannual yearly averages". Do you mean" monthly mean and yearly mean values" ? Please check the sentence structure and articles.

→ Indeed it was confusing : we replace this by "Figure 3 shows the time series of climatological monthly averages and of yearly averages of simulated and observed SST" **, lines 278-279**

Ln 221: Title : cycle ("c") an variability of what?

→ We detailed the title : *Annual cycle and seasonal variability of surface circulation, temperature and salinity* , **line 287**

Ln 237: remove "the" before Introduction and put "the" before northern monsoon wind.

→ This was corrected, **lines 327-328**

Ln 254: in very good overall agreement

→ This was corrected, **line 299**

Ln 285: Choose a better title.

→ We detailed the title : Interannual variability of surface circulation, temperature and salinity », **line 333**

p9 "coastal scale" is used in some places but the scale is not defined.

→ Following the comment above the expression"coastal scale" is not used anymore

Ln 323-325: This text and the text in ln 326-330 is repetition. Lines can be removed.

The text at the end of section 3.2 focuses on the observation and simulation of vertical profiles during the period and in the area of upwelling in 2018, whereas the text at the beginning of section 4 is a synthesis of the conclusion of section 3.

→ The text at the end of 3.2 was modified following the comment above about the model capability in reconstructing the upwelling circulation (and related water properties) in the vertical plan, **lines 386-401**

Ln 328: 10-year long simulation.

→ This was corrected, **line 406**

Ln 329: prefer " in four regions" to 4 regions

→ This was corrected, **line 407**

Ln 350: UI=1.49. I read 2.0 in Fig. 7.

On figure 7 (now Figure 13), we plot the relative values UIy,boxN/mean(UIy,boxN) to make the values readable. Indeed, ranges of absolute values of UIy,boxN vary between ~0-0.2°C for boxMK to ~0-1.5°C for BoxOF. We provide the values of (meanUIy,boxN) in Table 2.

→We carefully checked the values of UIy,boxN that we provided in the text, especially after changing to a common Tref for all boxes.

Ln 374-375: the next needs rewording. If possible, provide the exact location of each of four eddies in Fig 8. Put a symbol for example, or provide coordinates in the text.

→ We added black arrows on Figure 12 to show the position of cyclonic and anticyclonic structures and the resulting jets, and rewritten this with shorter and clearer sentences**, lines 486-488**

Ln 390: perhaps the word "chaotic" is not appropriate if the structures are visible in the mean field. Do you mean large scale turbulent structures? If not, clarify the meaning.

Indeed those structures themselves are not chaotic, but their development and propagation has a strongly stochastic part (this is related to the comment about the forced vs. chaotic part of the vorticity above)

→ This was rewritten (see **Section 5.3, and lines 669, 707, 711, 781).**

Ln 411-415. Perhaps reword the text. Too long and difficult to follow.

→ This was rewritten, with shorter and clearer sentences, **lines 504-507**

Ln 532: these differences

→ This was corrected, **line 691**

Ln 541-542: the SCS. The text should be rewritten.

→ This was rewritten, **lines 738-745**

571: zonal location is preferable to position

→ This was corrected, **line 765**

References

Criado-Aldeanueva, F., García-Lafuente, J., Vargas, J. M., Del Río, J., Vázquez, A., Reul, A., & Sánchez, A. (2006). Distribution and circulation of water masses in the Gulf of Cadiz from in situ observations. Deep Sea Research Part II: Topical Studies in Oceanography, 53(11-13), 1144-1160., doi:10.1016/j.dsr2.2006.04.012

Da, N. D., Herrmann, M., Morrow, R., Niño, F., Huan, N. M., and Trinh, N. Q. (2019). Contributions of wind, ocean intrinsic variability, and ENSO to the interannual variability of the south vietnam upwelling: A modeling study. Journal of Geophysical Research: Oceans, 124(9), 6545–6574. https://doi.org/10.1029/2018jc014647

Sérazin, G., Meyssignac, B., Penduff, T., Terray, L., Barnier, B., & Molines, J. M. (2016). Quantifying uncertainties on regional sea level change induced by multidecadal intrinsic oceanic variability. Geophysical Research Letters, 43, 8151–8159. https://doi.org/10.1002/2016GL069273

Waldman, R., Somot, S., Herrmann, M., Sevault, F., & Isachsen, P. E. (2018). On the chaotic variability of deep convection in the Mediterranean Sea. Geophysical Research Letters, 45, 2433–2443. https://doi.org/10.1002/2017GL076319

Ngo, M., and Hsin, Y. (2021). Impacts of wind and current on the interannual variation of the summertime upwelling off southern Vietnam in the South China Sea. Journal of Geophysical Research: Oceans, 126(6). https://doi.org/10.1029/2020jc016892