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.

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.

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.

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

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.

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.

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.

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.

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?

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

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.

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.

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.

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.

Technical corrections

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

Ln 34: perhaps "structures of circulation" ?

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

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

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

Ln 91 ageostrophic dynamics

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

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

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

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)

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 ?

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

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

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

Ln 254: in very good overall agreement

Ln 285: Choose a better title.

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

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

Ln 328: 10-year long simulation.

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

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

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.

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.

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

Ln 532: these differences

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

571: zonal location is preferable to position