

Anonymous Referee #2

Received and published: 27 February 2018

We do thank Referee#2 for his/her careful reading of our manuscript and relevant comments. Below are his/her comments (in italics), followed by our responses and description of related changes in manuscript.

General comments:

The manuscript by Piton and Delcroix analyze the variability of 5 parameters (sea surface temperature, sea level anomaly, surface wind components, precipitation and water discharge) over the South China Sea using relatively long datasets. They have found the impact of ENSO variations to the analyzed variables with the corresponding timelag between the impact.

In general the quality of the paper is good – the description of data and methods are followed by the analysis of the parameters on the seasonal scale (Section 4) and then on interannual scale (Section 5). The trends of the variables are presented in Section 6 and afterwards Conclusion and discussion is presented in Section 7.

We thank the reviewer for his/her nice and encouraging words.

I think the authors have done great work collecting and presenting (discussing) results from other's work relevant to this study, but in order to be "true" review paper for the area, emphasize should be on other's results instead of the (new) findings that are presented in this manuscript. I have put parentheses around new as if there are already so many papers from different authors about the trends and variability of essential climate parameters in the area, what is new in this paper? Coherent methodology or newer dataset should not be considered worthy for new knowledge – perhaps it is also important to emphasize new findings in the text. Although the paper was interesting to read, it needs some revision before it can be accepted for publication in Ocean Science.

We removed the term "short review" in the Abstract and replace it by "integrated analysis" which we believe is more relevant to what our ms. really is. We still do believe our integrated analysis is very valuable despite the fact – and even due to the fact – there are already many papers dealing with similar themes. As a matter of fact, as we noted for Reviewer#1, we appreciate that our ms. generated a lot of interest with more than 400 views and downloads since early January despite the fact "there are already some many papers". Moreover, we all know that conclusion of scientific papers may sometime be dependent on used methodology and/or time series duration. Our conviction is then that the coherent methodology and enhanced time series duration we used are clearly appropriate and beneficial to a better understanding of the regional climate variability. We admittedly did not submit a fully original ms., but rather provide a concise co-analysis of five key ECVs (never done to the best of our knowledge) with recognition of already published results, if any.

I am not very satisfied how the Conclusion and Discussion is presented – the authors repeat most of the already presented results and discussion (from Sections 3-6) in a shorter way. I would suggest renaming the section to "Conclusions" and bring out important information or conclusions in this section. Considering that the manuscript is already written in a way, where the discussion is embedded in the results section, I think it is reasonable to skip the discussion from the conclusions.

We agree with that comment, and renamed the ‘Conclusion and Discussion’ section into ‘Summary and Conclusion’ section. The discussion of some of the results that appeared in previous sections has been moved to the Summary and Conclusion section.

Some comments:

Lines 5-6: Please rephrase “..., and P increases in the north”. In the beginning of the sentence the authors discuss the changes of SST and SLAs that occur in the summer and then end the sentence about changes that occur in geographical space.

Results about the seasonal variability are much reduced in the revised ms. (we removed the seasonal EOF analysis and focus only on the JFM and JJA seasons, see our reply to referee #1), and hence no more appear in the Abstract (where Lines 5-6 were).

Line 17: “: : : Pacific Decadal Oscillation (PDO) : : :” I missed the description of PDO in later manuscript.

“...linked to the phase of the PDO...” was replaced by “linked to the possible influence of ENSO phase in the computation of long-term trends”

Fig 1: Please add coastline preferably with thick black line.

It is a question of appearance. We tested to draw thick black lines for the coastline and found out the figure becomes less clear. Sorry, no change has been made here.

Line 51: “: : : as well as by the water exchange with the surrounding ...”

Changed.

Table 1: I would suggest another plot about the location of the stations colored either by the mean or standard deviation and other important information shown as a text close to the station.

The locations of the 17 selected inland rainfall stations are now plotted on Figures 4e-f for clarity. The modified Figure appears in our reply to referee #1. Note that the 17 selected stations are represented with black markers (the crosses represent the 3 stations discussed in the text, the 15 remaining are represented by the dots), the water discharges stations remain represented by the red dots.

Line 151: “: : : were obtained ...”

Corrected.

Line 163: I assume correlation or determination coefficient was meant instead of slope of the regression.

We confirm we meant ‘the slope of the regression line’ which represents the rate of change in y (satellite data) as x (in situ data) changes.

Line 192: “: : : EOF modes greater equal to two ...”

We meant equal to – or greater than – two.

Line 201: “: : : are available both over the ocean and land.”

This line disappears, as we no more focus on mean and standard deviation.

Line 206: Please rephrase “: : : in the winter in the area.”

This line disappears, as we no more focus on mean and standard deviation.

*Fig 3: I would suggest adding mean and std of wind-speed velocity as $\sqrt{u^{**2}+v^{**2}}$*

We agree this could be useful. We however removed all paragraphs dealing with mean and standard deviation of the winds, following all reviewers’ comments who noted that mean structures are meaningless in the SCS given the strong seasonal monsoon reversal.

Fig 4: Why to write in the caption: "The product between spatial and temporal functions denote anomalous SST (in °C) and SLA (in m) respective to the mean values." These products are never shown in the paper and as expected, the largest EOF mode describing the largest part of the variance, should show variance. I assume anomalous SST and SLA respective to the mean values is the variance.

That Figure 4 is removed in the revised ms. as we now focus on two contrasted seasons only (JFM and JJA). The sentence in the caption is however maintained in other figures when addressing the interannual EOF modes. Given the way we compute the EOF, we believe the sentence is necessary to indicate the units of the anomalous fields.

Lines 309-314: I miss the importance of this section. Although the authors refer to Qu (2001) and the methodology using mixed layer depths, they present the time-series of SST. I do not expect mixed layer depths to be positively correlated with SSTs.

These lines and the corresponding Figure 5 are removed in the revised ms.

Line 327: “: : : correspond to the maximum: : : ”

We meant ‘.... the timing corresponds ...’

Line 354: Where can I see the value 6 mm/d ? I assume it is seen from the Fig. 3f, please indicate this in the text.

This value could actually be view in computing ‘the product between the spatial (Fig. 6e) and temporal (Fig. 6f) functions’ in the seasonal EOF analysis. Line 354 no longer appears with the removal of the seasonal EOF analysis in the revised ms.

Table 2: Where did you get the values for Niño1+2, Niño3.4, Niño4, SOI and EMI? I saw references to different authors on page 9 , did you get the values from those papers?

Yes, these ENSO indices were referenced on page 9 in the paragraph called “climate indices” and the indices were extracted from these sources.

Line 409: Where can I see increasing values 0.7 and 1.0 oC?

The maximum value in Figure 10a was about 0.06, and the EOF time function is about 8 in 1986-87 and 12 in 1997-98. Then the product between the space and time functions, quantifying the anomalies, is 0.48 (and not 0.7) and 0.72 (and not 1°C). This is corrected (we noted 0.5 and 0.7°C).

Line 425 and 426: Where can I see values 0.1 and 0.2 oC?

The maximum value in Figure 10c was about +0.05 in the eastern half and -0.1 along the coast and south of Vietnam. The EOF time function in Figure 10d ranges within (-2, +2). Then the product between the space and time functions, quantifying the anomalies, is about 0.1 (0.05 x 2) in the east and 0.2 (0.1 x 2) along the coast.

Line 451: I guess values 5-10 cm are seen on Fig. 2d, please indicate in the text.

This is the same issue as above, and the reason why we noted in the caption that anomalies could be quantified computing the product between the EOF time and space functions. Looking at Figure 10e, maximum changes of the order of -0.025 occur in the east at about 15°N. Looking at Figure 10f, the EOF time function ranges within about -2 and +2. Hence the product between the space and time function reach about 5 cm (0.025 x 2 m). We changed 5-10 cm to 5 cm.

Fig. 13: I am not sure about the panel (e) as the authors only mention it on lines 619-621. The authors do not discuss the vector representation – are there some changes in the directions of the winds as well? What can we learn from the vector representation of the trends?

This vector representation was designed to ease the interpretation of trends in the figures showing trends in zonal and meridional components. We think this representation provide us with information about possible changes of the monsoon winds over time. From Figure 13e, and looking at Figure 7, there is in fact a hint for an intensification of the winter monsoon in the central and southern parts of the SCS.

To further our analysis, we plotted the trends for JFM and JJA over the period 1979-2015 (see the new Figures 10ab in our reply to Referee #1). As we note now in the text: “It appears that in winter, when comparing to Figure 7, there is an intensification of the northern winds along the coast of Vietnam, in the northernmost part of the SCS and in the Gulf of Thailand. These results seem to confirm the suspected intensification of the winter monsoon over the area. On the other hand, the linear trend of winds in JJA show an increasing tendency to winds blowing in opposite direction to monsoon winds, leading to a decreasing trend of the southern winds in summertime (when comparing to Figure 7), especially in the southern part of the SCS. These results are suggesting a decreasing in strength of the summer monsoon over the period considered.”

Line 601: PDO comes in with no previous indication what this is. I assume it is Pacific Decadal Oscillation from the Abstract.

Thanks for noting this. The acronym is detailed in the revised version, and a reference is given.

Line 629: Satellite? I thought the precipitation was from ERA interim re-analysis, which is model product.

Yes, this is from a re-analysis. Corrected.

Section 7 is definitely too long and repeats already shown results in not a good matter. I suggest rewriting it in a short and concise way, where the authors bring out the most important results or conclusions of their work.

We removed the whole paragraph dealing with the seasonal cycle, and removed some sentences and related references in the last paragraph.