

Interactive comment on “Seasonal and interannual (ENSO) climate variabilities and trends in the South China Sea over the last three decades” by Violaine Piton and Thierry Delcroix

Anonymous Referee #2

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Comments on "Seasonal and interannual (ENSO) climate variabilities and trends in the Southern China Sea over the last three decades"

Authors: Violaine Piton and Thierry Delcroix

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 time-lag between the impact.

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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.

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. 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.

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.

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

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

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

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.

Line 151: "... were obtained ..."

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

Line 192: "... EOF modes greater equal to two ..."

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

Line 206: Please rephrase "... in the winter in the area."

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

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.

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.

Line 327: "... correspond to the maximum. . . "

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.

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

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papers?

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

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

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

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?

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

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

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.

Please also note the supplement to this comment:

<https://www.ocean-sci-discuss.net/os-2017-104/os-2017-104-RC2-supplement.pdf>

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2017-104>, 2018.

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