

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 #1

Received and published: 14 February 2018

General Remarks:

The paper discusses the development of five so called “Essential Climate Variables” which have been observed for the South China Sea over the last three decades. Four aspects: i) mean and standard deviation ii) seasonal variability iii) inter-annual variability iv) trends and five parameters: i) SST ii) SLA iii) precipitation iv) surface wind v) water discharge are considered for this study. All of these data are based on observational data, if one accepts the point that an optimal interpolation or a re-analysis is also closely related to observations. Although the paper definitely contains a lot of interesting material which would deserve its publication, unfortunately, in the current

C1

state of the manuscript it must be rejected.

The main criticism is that the paper is written like a report and not like a scientific research paper. The authors follow a very rigid structure, where for each of the four aspects each of the five parameters is discussed separately, providing insufficient or only scarce overarching information. Due to this rigid scheme, a lot of unnecessary or even redundant information is provided. For this reason the paper in relation to its content is much too long and boring to read with in total 48 manuscript pages. A good example of the problems which arise from the report-like structure is the fact, that annual mean SST and wind speeds are presented despite the circumstance that in such a monsoon dominated area these quantities are more or less meaningless, since in one year the oceanic and atmospheric system switches between two dominant modes. In this case a mean for the summer and winter monsoon situation would be much more valuable. The annual mean patterns just are just synthetic distributions which have no representation in the real world.

Another very striking problem which arises from this rigid structure can be seen, when looking at the standard deviation of the SSTs or the u- and v-winds (Figs. 2b, 3b, 3d). These figures are nearly similar to those of their first EOFs (Figs. 4a, 6a, 6c), which certainly has to be expected. Interestingly, this close agreement between these figures has not even been mentioned by the authors. This is a good example how the authors just treat each aspect separately, without considering any of the cross-connections, which in many cases would provide a lot of additional scientific information. Another general criticism concerns the way how the authors interpret the results of their analyses. Most of these explanations are just speculations or even platitudes, which are rarely being substantiated by adequate information and/or literature. Here are just two examples:

Lines 293-301: the development of the second SST mode is explained basically by the monsoonal variations, although it only explains 5% of the total variance. Moreover, solar radiation and ocean heat transport are both brought into play, without giving any

C2

indication of their particular contribution. One would expect that also the first EOF is governed by monsoonal variations. It seems that the monsoon triggers two orthogonal modes at the same time. However, this interesting question has not even been mentioned by the authors.

Lines 377-381: It is obvious that the river discharge depends on the monsoon dynamics. Therefore the peak run-off should occur during the rainy season, which is no surprise. However, a time lag may occur, since firstly, it will take a few weeks from some of the catchment areas to the Mekong River mouth, and moreover, the onset and peak time of the rainy season will vary for the different parts of the large Mekong catchment area.

I have noted a number of further minor points of criticism regarding some specific details of the manuscript. However, since according to my opinion the entire structure of the paper has to be changed, at present it makes no sense to list them here.

In conclusion, I would propose that the authors rewrite the paper using the style of a research paper. This means that they construct the paper around their major finding. For this reason only information supporting these findings should be presented in a stringent way. Otherwise the reader just becomes confused or bored by too many unnecessary details.

The other big drawback of the paper is, that due to its report-like structure it was not possible to answer the most obvious and pressing questions for this research area, i.e., whether the summer or winter monsoon have increased or decreased in strength over the last decades, and if the onset time of both monsoon phases has changed, as speculated by many authors. I can imagine that the monthly resolution of the data could be problematic to answer the latter questions. However, since this question is of vast interest for the entire region, the authors should at least make an attempt to answer this question.

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