Anonymous Referee #1
Received and published: 14 February 2018

We do thank Referee#1 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 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 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 are just synthetic distributions which have no representation in the real world.

In re-reading the submitted manuscript (ms.) we agree that it was too long, with some unnecessary and/or redundant information. We also agree that parts of the ms. may look like a report, although it is not that clear to us where the frontier between a report and a scientific research paper is. Whatsoever it was, a report-like or scientific paper, we appreciate that it generated a lot of interest with more than 400 views and downloads since early January.

In agreement with referee#1, we noted in the submitted ms., lines 343-345, that "The interpretation of the annual mean surface wind in a region that is highly influenced by strong seasonal wind reversals due to the monsoon cycles (see section 4) does not mean very much". To better consider the issue in the revised ms., we have removed all paragraphs dealing with the mean patterns (and standard deviations) of the surface winds, SST and precipitation, and rather focused on the two most contrasted seasons, JFM and JJA. We chose to keep the description of the altimeter-derived mean dynamic topography and mean water discharge (and related standard deviations) because, to our knowledge, it was never documented in the literature.

In doing this, we have deleted Figures 2a-b, 3a-f, 4a-f, 5, and 6a-f, and add the new Figure 4a-f showing the mean JFM and JJA surface wind (in vector form), SST and Precipitation. That new figure is shown below. We also have combined the former Section 3 'ECV means and standard deviations' and former Section 4 'Seasonal variability' into a unique Section 3 mostly providing a quick description of the two JFM and JJA seasons as a background. The combination of the former Sections 3 and 4 into a unique and more concise Section 3 results in a reduction of 6 pages.

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.

As noted in the submitted ms., lines 85-86, the standard deviation maps were presented because they denote the "overall variability", i.e., mostly the seasonal and interannual changes in our study. The similarity between the standard deviations and the seasonal EOF of the SSTs, u- and v-winds is indeed clear from the presented figures, although these have to be shown first. We agree we should have stressed that similarity, indicating that the overall variability mostly results from the seasonal variability. The point no longer arises for the surface wind, SST and P since we have removed the standard deviation and seasonal EOF figures. The similarity is mentioned for the SLA in the revised ms.

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 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.

As we have drastically reduced the analysis of seasonal changes, removing EOF analysis and concentrating on the JFM and JJA seasons only, discussion of the second seasonal EOF mode on SST no longer appear in the revised ms.

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.

We agree with that comment. Note, however, that the former Figure 9 (now Figure 3) not only provide information on the timing of the seasonal water discharge but also on the amplitude. Moreover, as was discussed in the last paragraph of Interannual Variability section, it is likely that the regulation of water flow by the barrages further impacts the time lag between P in the catchment areas and the river discharge. A comprehensive study of the relationships between the monsoon dynamics and the seasonal river discharges is beyond the scope of our ms.

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.

Following the reviewer’s comment, we drastically reduce the analysis of mean values, standard deviations, and seasonal variabilities of the five analysed ECV, concentrating now on interannual (ENSO) variability and long-term trends.

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.

We agree with the reviewer that we are not in a position to solve the question of the onset time of the monsoon with the monthly data sets we have in hand. To the best of our knowledge, this is a long-standing question, and we had no pretention to even partly solve it in our ms. We however better linked our results to the main ‘usual’ monsoon features when addressing ENSO and long-term trends in the Abstract, in Sections 4 and 5, and in the Summary and Conclusion section with a better description of the former Figure 8 and the new Figure 10. For instance, in the revised abstract, we note in line with:

i) the former Figure 8 (shown below): ‘….. The winter N-NE and summer S-SW monsoon winds weaken during El Niño events. Opposite wind anomalies are observed during La Niña events, enhancing both winter and summer monsoons….’,

ii) the new Figure 10 (shown below): ‘ ….. Increasing trends in northerly winds are observed in both winter and summer seasons, potentially intensifying/weakening the winter/summer monsoon, respectively…’.

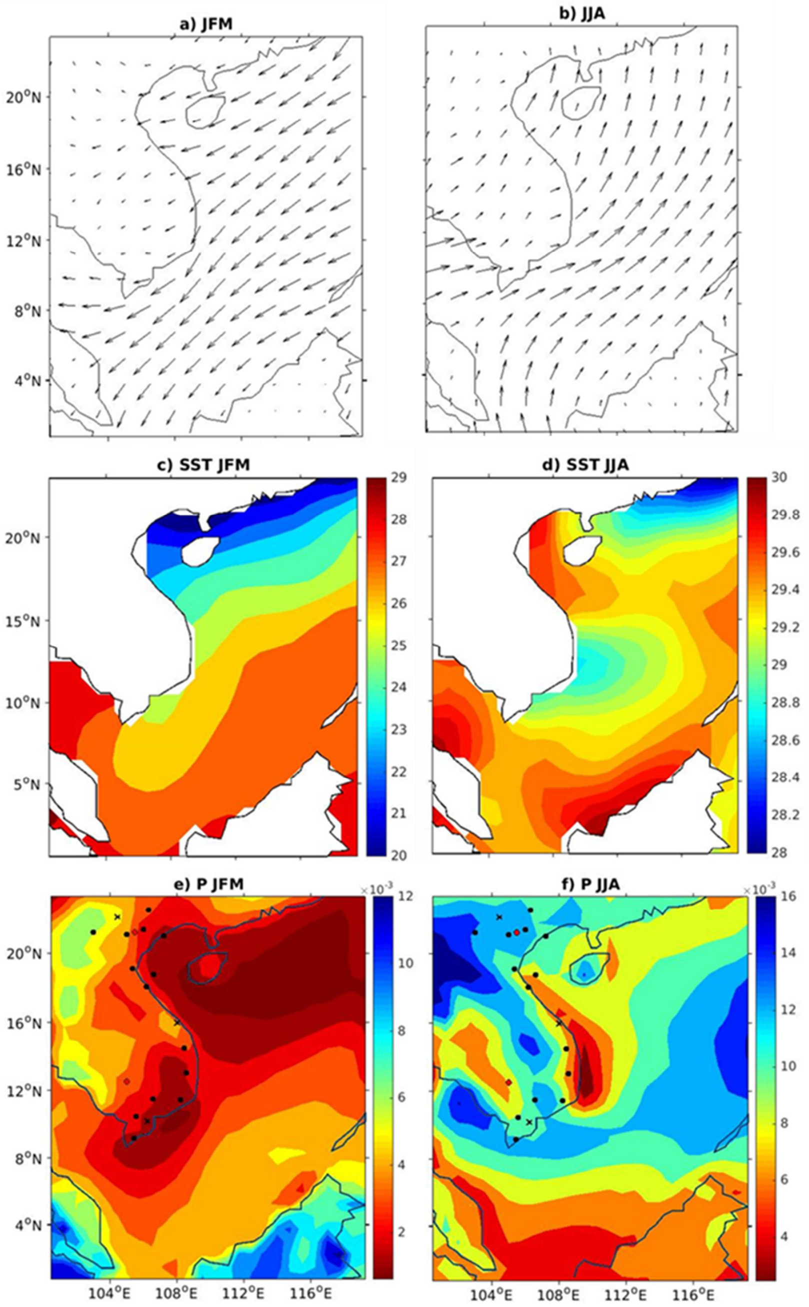

Figure 4. Mean surface wind (top panels), SST (middle panels, units are °C) and P (bottom panels, units are mm/day) in January-February-March (left panels) and June-July-August (right panels). The arrows in a-b denote the wind vectors, and the longest arrows equal 15 m/s. The black markers on panel (e, f) denote, from north to south, the location of the 17 inland rainfall stations and the crosses denote the location of Lục Yên, Đà Nẵng and Mỹ Tho discussed in the text (see Table 1). The red dots denote the location, from north to south, of the gauge stations at the Red River ST and Mekong CCV stations. The color codes differ between the figures.

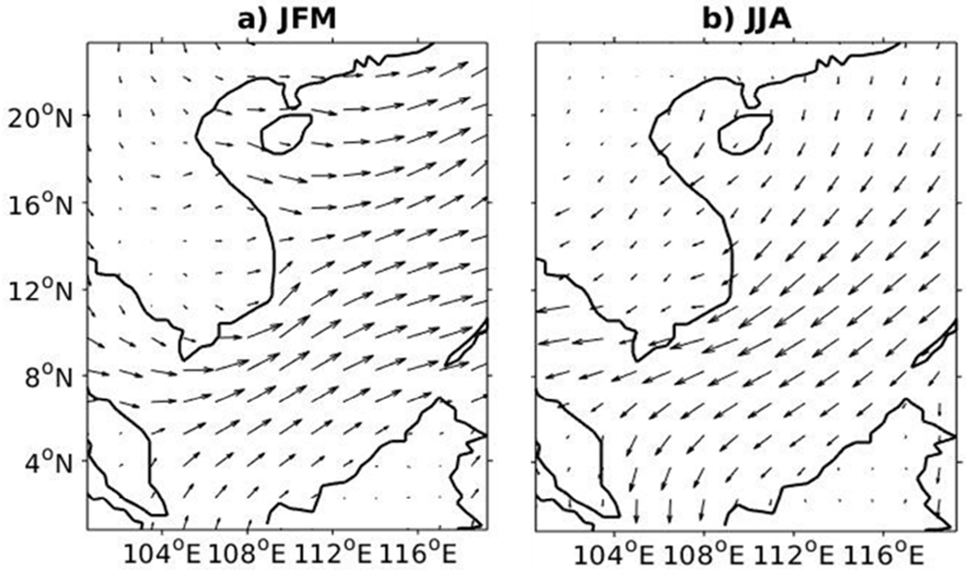

Figure 8. El Niño composites of the anomalous surface wind vectors in JFM (a) and JJA (b) computed for 1983, 1987, 1992, and 1998, representing the 1982-83, 1986-87, 1991-92, and 1997-98 El Niño events, respectively. The arrows denote the vectors, with the longest arrows equal to 1 m/s.

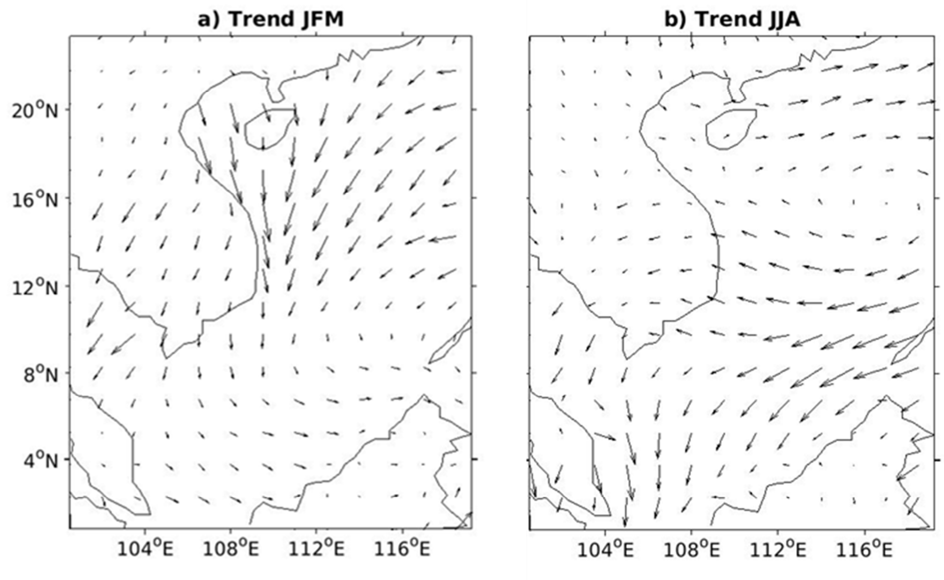

Figure 10. Surface wind trends in (a) JFM and (b) JJA. The longest arrow is equal to 1.5x10-2 m/s/decade.