Anonymous Referee #3
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We do thank Referee#3 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.

The paper discusses the so called "Essential Climate Variables" in the area of the South China Sea over the last three decades. Included are mean and standard deviation, seasonal and inter-annual variability, and trends for five ECVs. The ECVs included are: SST, SLA, precipitation, surface wind and water discharge. Although the paper definitely contains some interesting aspects, which might be worth a publication I cannot recommend publication in its current version for several reasons.

My main criticism is that the authors seem to be not convinced of their own results and somewhat repeat and hide behind the results/finding of others, which is mirrored in the length of the paper and many redundancies. I do think also it is because of the repetitions too long and it contains too many figures, which are partly meaningless, because nothing new is shown. This is strikingly clear in case of the e.g. reversing signals in SST and wind on annual scale. In a monsoon driven system not really surprising, worth presenting, no, because nothing new. This is just one example. I miss also connection (in presentation and/or discussion) between the parameters studied, since they are not disconnected from each, but instead the reader get presented much information which is known, established and therefore the detailed presentation of their results e.g for the SST and wind pattern, is again redundant. Both the annual mean SST and wind speeds are presented despite the circumstance that in a monsoon dominated area these quantities are more or less meaningless, since in one year both the oceanic and atmospheric system alternates between two dominant phases.

We confirm we are convinced by our own results. We agree with the reviewer the ms. was too long. To shorten it, as responded to the other reviewers, we drastically reduce the number of pages, especially the ones appearing in the two former sections dealing with mean and standard deviations and seasonal EOF analysis. A brief description of the two JFM and JJA contrasted seasons is now given as a background in the revised ms.

We agree with the reviewer that the parameters are not disconnected from each other and we do agree that understanding their relationships is of particular interest. However, finding a realistic scenario seems very complex, in particular since the study deals with five ECV in a coupled system continent-ocean-atmosphere, involving both thermodynamic and dynamic processes. Such coupled systems imply chicken-and-egg relationships between variables that might be difficult to emphasize and understand. Only sensitivity tests with forced and/or coupled models, supposed realistic, could shed the light on the relationships between the variables. As mentioned in the Summary and Conclusion section, a precise quantification of the mechanisms responsible for the observed variability will be dealt with in an anticipated companion paper, as a second step, based on model outputs (from the hydro-dynamical model SYMPHONIE, in which for instance for the SST, all terms of the mixed layer temperature budget are available on regular gridded points).

Moreover, we note that proposing a common mechanism between variable might be sometimes counter-intuitive. We discussed the case of the inverse response of SST and SLA to ENSO in the SCS (lines 658-663).

My another main criticism is regarding precipitation and river discharge, as the authors just include Vietnamese data. Why contributions from China, which limits SCS to a huge extent, are not included at all? Not a single word is mentioning this, and I do think Pearl River might have a significant river discharge into SCS?

We understand precipitation changes in the southern part of China could have been analyzed as well. Because the ms. is quite long, we decided to put the emphasis on Vietnam in situ stations only, and Vietnam is the first author's affiliation. The Pearl River mouth is located in East China Sea, not in the South China Sea. We understand its fresh water flow likely influences the sea surface salinity distribution away from the ECS in the SCS. While sea surface salinity is also an ECV, it is not analyzed in the present ms.

I have noted a number of additional minor points, but since I'm not recommending publication at this stage it is not worth listing them now, as the authors need to rewrite the entire manuscript in a short, precise way omitting all redundancies, but highlighting their results instead.

We will be happy to have the list of minor points, assuming they are still relevant for our revised ms., and are confident these will further help us to improve our ms.

I'm sorry I cannot be more positive at this moment.

Thanks again for your comments, anyway.