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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Primary productivity and its variability in the equatorial South China Sea during the northeast monsoon" *by* S. H. Ooi et al.

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1. General Comments:

The title of the paper is 'Primary productivity and its variability in the equatorial South China Sea during the northeast monsoon', however, I don't find any primary productivity data in the manuscript. Although the authors try to use chlorophyll concentration as an index, 'chlorophyll' and 'primary productivity' are totally two different things, one is a biomass term, and the other one is a rate term. Even this, ocean color satellite derived chlorophyll concentrations (SeaWiFS and MODIS) are known for their bad performances in coastal regions (case 2 waters) with magnitudes of differences compared with measurements due to the influence of coastal CDOM, sediments, and bottom

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reflection etc. This study seems rely heavily on the coastal chlorophyll data, thus, without in-situ measurements validation, it is hard to convince the readers the results presented here are significant. Also the 'cloud' issue seems pretty serious in the study region, how to account for these uncertainties from the satellite data? From this state, I cannot recommend publication in ACP.

- We regret that the title was misleading. In fact we never state that we quantify primary productivity, which seems to be the essence of your criticism. We focus on coastal variability and its drivers and among other data products use satellite derived chlorophyll-a concentrations to investigate how many factors come together and accumulate in a response that could be relevant for the biosphere. In addition we describe how this response might be modulated but teleconnected, in particular by ENSO. To avoid future misunderstanding, we propose a change of title: "Coastal variability in the equatorial South China Sea during the northeast monsoon"

However, we note that satellite based remote sensing has largely focused on quantifying near-surface chlorophyll-a concentrations. These are therefore used as a proxy for interpretation in terms of rates of primary production or growth, as well as in terms of phytoplankton abundance. Our sole objective is to provide a climatological overview and fundamental understanding of essential physical (not biological and chemical) forcings on the distributions and changes shown by the satellite observations of chlorophyll-a in the equatorial South China Sea during the northeast monsoon. Of course, such an approach cannot quantify the chlorophyll-a budget. We agree with you that the quantification (which we do not attempt in the paper) must rely heavily on supporting in-situ measurements.

We agree with you that cloudiness is a problem for the observations. Therefore we use carefully assembled monthly mean fields for our general argument. We are aware that there is a trade-off between the temporal resolution we would like to achieve and the spatial scales involved in investigating coastal upwelling. We believe that we managed a reasonable compromise and are able to present a physically consistent description

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of interannual coastal variability and ENSO impacts. We hope that clarifying this fundamental misunderstanding and the change of title will make it possible for you to reconsider your judgement and we would welcome further comments on how to avoid any misunderstanding in the main text if, after the revision, you have remaining concerns.

The authors present wind data, wave data, and chlorophyll data to support their conclusions. While most of the data are with very coarse resolution in space, which cannot even resolve mesoscale structures in the ocean, then how can they provide meaningful representations for the coastal dynamics, and further ecosystem response?

- For climatological overview of the equatorial region, we have to look at the synoptic instead of mesoscale aspect. In terms of ecosystem response, we need to conduct detailed case studies using field data. However, data from case studies are few and far between. Such limited temporal and spatial coverage is not adequate for a full understanding of the processes involved. For our intended objective, we think the 0.75o x 0.75o reanalysis data and 9 km resolution satellite data will suffice.

The authors mention the 'dilution' effect for observed low chlorophyll concentrations. However, this is purely 'physical', have you ever considered the 'biological' effect, like nutrient depletion, high grazing pressure?

- Our model of physical forcing is consistent with the chlorophyll-a changes we see. Perhaps, wherever possible, we would mention in our paper that other effects beyond the physical forcings will play a role as well.

2. Specific Comments:

P21574, L6, and also in the main text, please indicate what satellite data is 'new'?

- Yes, you are right. Ocean color satellites were only recently launched (in the 80's). The data should be "recent" and not "new".

P21575, L27, chlorophyll concentration is an index of phytoplankton biomass.

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- Sorry. We are not clear about this particular comment. We adapted the statement by Stewart (2008).

P21576, L7, you probably mean 'from phytoplankton'

- We think "phytoplankton biomass" – a collective noun, is more appropriate.

P21577, L21, what is the resolution of OSCAR data?

- 0.33o by 0.33o

P21578, L5-13, I think this paragraph is not necessary.

- We envisage that readers to our paper, if published, are likely to come from different disciplines. We hope this will help those readers who are not well versed with the usage of satellite data. We would like to retain this paragraph.

Eq(1), wind stresses are vectors, so it would be good to present u and v stress here. And in the following paragraph, there is no need to talk so much about the Ekman transport or dynamics, as they are just basic oceanography concepts.

- We feel that vector representation can convey our message better in this study. Also, details about Ekman transport are meant to assist those readers who are not oceanographers. If possible, we will make an attempt to shorten this paragraph.

P21581, L18, are you talking about summer upwelling off the Vietnam coast? Because from a lot of literatures, winter upwelling in that region is not significant. Moreover, wind stress only is not an indicator for the upwelling event.

- It might be small but non-negligible. The upwelling is noticeable off the Vietnam coast consistent with the wind stress and chlorophyll-a signatures we find in the data. In our paper, you have noticed that the wind stress indicates that upwelling in the equatorial South China Sea is generally weak.

P21582, L17, By 'North Equatorial Drift current', do you mean 'North Equatorial Cur-

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rent'?

- Yes.

P21590, L26, and also over the entire manuscript, nutrient distribution data or at least water turbidity data are needed to support the idea of 'wind-mixing (upwelling)-nutrient-chlorophyll'. Otherwise, it seems too fast to jump from the wind forcing to phytoplankton response.

- We hope our proposed change of title can clearly convey our intended message and justify our principal use of wind forcing to investigate climatologically the coastal variability during the monsoon in the region. Sarangi et. al (2008) and Lathuiliare et al (2008) not only use satellite data as practical means to monitor spatial and seasonal changes of chlorophyll-a concentrations (as proxy for phytoplankton biomass) but also stress the response impact due to the importance of wind forcing.

[References: Monthly variability of chlorophyll and associated physical parameters in the southwest Bay of Bengal water using remote sensing data by R.K. Sarangi, Shailesh Nayak and R.C. Panigrahy (Indian Journal of Marine Sciences, Vol. 37(3), September 2008, pp. 256-266.

Seasonal and intraseasonal surface chlorophyll-a variability along the northwest African coast by C. Lathuiliere, V. Echevin and M. Levy (Journal of Geophysical Research, Vol. 113, CO5007, doi: 10.1029/2007JC004433, 2008]

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 21573, 2013.