

Interactive comment on “Seasonal to interannual variability of Chlorophyll-a and sea surface temperature in the Yellow Sea using MODIS satellite datasets” by Chunli Liu et al.

Anonymous Referee #2

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This paper described DINEOF and wavelet analyses done on CHL and SST data from the Yellow Sea. However what is missing from the paper is any sense of why this should be done, or what scientific questions they hope to address by applying these statistical analyses. Their three stated objectives are very vague – “identify spatial and temporal patterns”, “investigate interannual trends”, and “explore temporal correlations”. Consequentially their conclusions include “findings” such as “the SST mode was dominated by a seasonal cycle: warmest in summer and coldest in winter” (line 398/399). Surely a DINEOF analysis was not needed to come to that conclusion! They mention water depth, currents and sedimentary nutrients as factors explaining their results, but they do not show or analysis any of this data. Given the lack of a clear scientific question they are trying to answer, I can not recommend publication of this work.

Author: We thank the referee for taking the time to review our

manuscript and for making constructive criticisms of it with detailed suggestions that we have used to improve it. In the conclusion section, the main conclusions have been refined to reflect the objectives stated in the introduction. We answer the other comments in the appropriate responses below.

Introduction They need to give details about what previous work has done. While they cite relevant literature they make empty statements like XX analyzed relationships and YY examined distributions, etc, however they fail to tell the reader what those researchers found that they will be building upon. There is also an inconsistency with how they describe these previous studies. They usually state that the researchers looked at CHL and SST but for the some of the studies they mention the name of the sensors and even the level of data, details which are not relevant. The introduction should describe what has already been done on the subject, and outline what questions remain that they will be addressing. While they give some objectives at the end of the Introduction these mainly seem to be perform EOF and wavelet analysis and describe the results without giving a solid scientific question that they are looking at. For example their first objective is to identify seasonal and spatial patterns of CHL and SST in the Yellow Sea, however I suspect that some of the papers they have cited have already done this, so is there really a need to

do it again? Their second objective is to investigate the interannual trends in SST and CHL, but in lines 83-86 they mention several researchers already done this, so again why should it be redone here?

Author: Thank you for your constructive criticism of the introduction section. We have revised the introduction section according to your suggestions. A new scientific question has been presented, which this research was designed to address.

In lines 83-86, we have mentioned that several researchers had focused on the Chl-a trend, but there was no research that addressed Chl-a and SST trends systematically. In the Yellow Sea, although some research on spatial and temporal variations of Chl-a and SST has been done, the study periods were shorter than ours, and there is lack of research for the whole Yellow Sea. According to referee #1 comment, in our revised manuscript, this part in the results section had been simplified.

Figure 1 (and other maps). Show the boundaries of the maps in (b) and (c) on the map in (a). Why is the high-resolution coastline used in maps (b) and (c) not used on map (a)? Expand the eastern boundary so the coastline is more visible.

Author: Since the data sources which were used to make the Fig.1 (a) and (b) and (c) maps were different, the resolutions of coastlines were different.

We have redrawn Fig.1 using the same data source. Also, the eastern boundary has been expanded.

Lines 137: “These images show clearly the utility of the DINEOF method in reconstructing monthly, high-resolution imagery from datasets with large amounts of cloud cover”.

I do not necessarily agree with this statement. The DINEOF reconstructed image (Fig 2b) is nearly “complete”, whereas the original data was approximately 80% missing. However clearly there is no way to know how accurate this filled in data is. The features shown in Fig 2b are presumably features present in months before or after this image, but if the intent is to look at variability, doing analysis on datasets with a lot of “reconstructed” data will be biased towards less variability. It would be preferable to use techniques that do not in effect “make up” a lot of data.

Author: We apologize that we have not describe the DINEOF method in detail for lack of space. DINEOF method has been used to reconstructed the missing data effectively in many early research (please see line 122-129). Before using the DINEOF, we removed the images where the missing pixels accounted for more than 95% of all pixels. We ensure that the total missing percentage was less than 40%. Much study showed the spatial and temporal variability of the parameters constructed by the DINEOF method could be well kept not

only for the whole imagery also for the details. The accuracy for the constructed dataset is much ideal.

In the revised manuscript, the accuracy validations of the reconstructed datasets in Chl-a and SST have been added.

Lines 192/193: What does it mean that “the largest spatial coverage of Chlorophyll was in spring”? Presumably they are referring to high chlorophyll, or a bloom? What level of chlorophyll constitutes a bloom?

Author: Probably the sentence was not very clear leading to possible misunderstands. It referred to the high chlorophyll in Spring compared with the Summer where the Chl-a concentration was smaller than $\sim 2 \text{ mg.m}^{-3}$.

Line 195: They refer to a subsurface phytoplankton bloom, that is not visible by satellite data – how then do they know about it? No reference was given.

Author: The following reference has been added.

Platt, T., White, G., Zhai, L., Sathyendranath, S., Roy, S., 2009. The phenology of phytoplankton blooms: ecosystem indicators from remote sensing. Ecol. Model. <http://dx.doi.org/10.1016/j.ecolmodel.2008.11.022>

Lines 196-200: Are they certain the spring/summer bloom chlorophyll in the Yangtze river plume is chlorophyll? Could it be CDOM or sediments

that are mischaracterized by the global algorithm that is tuned to case 1, not case 2, waters? Surely there has been research on this subject, but none is cited.

Author: Thank you for your suggestions. It is debatable about whether it was Chl-a or CDOM, or sediments in the Yangtze river plum by the global algorithm. But in the coastal waters in the other research, the Chl-a imagery showed the similar distribution and variation. More discussion involving references to other papers has been added in this part.

Section 3.2 Figures 5 and 6 are not very useful. It is rather pointless to expect to see interannual differences in annually averaged data. Listing the range of values (to two decimal points!) is reminiscent of a field report, rather than a scientific paper. How is this scientifically relevant?

Author: Thank you for your suggestion. According to referee #1, the results sections have been rearranged. In this section, Fig.7(e) and 7(f) have been moved here to show the inter-annual variability. Fig. 5 and Fig.6 which showed the spatial variations of annual Chl-a and SST have been deleted.

Line 245: Since they discuss chlorophyll concentrations relative to water depth they should show bathymetry contours on the maps being

discussed.

Author: Thank you for your suggestions. The Fig7a and 7b had been redrawn with the bathymetry contours added.

Line 248: I think they mean “sedimentary nutrients” rather than “deep-sea nutrients”. Given the that depths in the YS are less than 100 m they can hardly be classified as “deep-sea”.

Author: We are agreed with you. The word “deep-sea” has been replaced with “sedimentary nutrients”.

Figures 3 -6 are very redundant, not terribly illuminating and could probably be captured by one or two images showing the extent of the variability (ie a climatological winter and summer image of chlorophyll and SST). Most of the discussion associated with describing these figures, ie the seasonal distribution of chlorophyll and SST, has probably already been described in previous literature. If so this information should be given in the Intro and spend more time in the paper discussing whatever new results this study has generated.

Author: Thank you for your suggestions. According to referee #1 comments, the results sections had been simplified, and combined with the discussion section. For the monthly imagery, the Apr, Aug, Oct and

Jan were selected to represent the spring, summer, fall and winter, respectively.

Line 253/4: “This phenomena would require more observations of subsurface nutrients: : :”

This statement implies that there are some observations of subsurface nutrients, but none have been discussed in this paper, even as citations to other work.

Author: Thank you for your suggestions. The reason that Chl-a presented an increasing trend has been given more explanation about subsurface nutrients by supplemented citation work.

The cited paper is “Wei, Q.S., Li, X.S., Wang, B.D., Fu, M.Z., Ge, M.Z., Yu, Z.G. Seasonally chemical hydrology and ecological responses in frontal zone. Journal of Sea Research, 2016, 112:1-12”

Line 392/393: “Chlorophyll variability was dominated generally by a spring bloom followed by a secondary fall bloom throughout the entire YS region”

Where was this shown and discussed?

Author: This conclusion could be seen in line 250-251. We have already added more information in the line 289-292 to make it more

clearly.

Line 400-402 “Further analyses showed that the magnitude of the seasonal cycle in different regions was a result of the water depths and water currents in the YS” While this was discussed, there was certainly no analyses put forth that showed this. They do not show or analyses any water depth or current data in this paper so I fail to see how they come to this conclusion.

Author: In Fig 3-7, the bathymetry contours have been added in order to show the relationship between water depths and the spatial distribution of Chl-a and SST. The water currents here mentioned referred to the current showed in Fig1b and Fig1c. In this manuscript, the qualitative relationship was only focused on between water currents and Chl-a/SST. Also, this sentence was reconstructed to make it more clearly.