

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.

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 have already done this, so again why should it be redone here?

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.

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

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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.

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?

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.

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.

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?

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

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”.

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

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given in the Intro and spend more time in the paper discussing whatever new results this study has generated.

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.

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?

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.

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