

Review of “Analyses of sea surface Chlorophyll-a trends and variability in a period of rapid Climate change, German Bight, North Sea”.

In this work, the authors provide a comprehensive analysis of sea surface chlorophyll-a trends and variability in the German Bight, a coastal area in the North Sea, using satellite remote sensing data and in situ measurements. The paper aims to understand the relationship between chlorophyll-a, sea surface temperature, and mixed layer depth. The paper presents some interesting and novel findings, such as the significant positive trend of chlorophyll-a near the Elbe estuary and the negative trend in most of the central German Bight, the changes in the distribution of chlorophyll-a anomalies before and after 2009, and the contrasting modes of co-variability between chlorophyll-a and sea surface temperature or mixed layer depth in coastal and offshore areas. Overall, the paper is well-written and structured, but I think some of the figures could be improved before publication. I have some major/minor comments and suggestions, but I could not see any scientific flaws, and think the manuscript is a good addition to the field.

1. In this MS, the authors have used several statistical methods such as EOF, MCA, linear correlation, trend analysis, probability density function, and different types of tests such as the Mann-Kendall trend test, Kolmogorov-Smirnov test, and two-sided Wald test. However, the authors do not examine/illustrate these statistical techniques in detail. For example, what is the LOWESS trend, and how can it be estimated? What is the two-tailed Wald test? Is it different from the t-test? Could you please add more details about this test? What is the difference between this test and the Mann-Kendal trend test? Also, I strongly recommend using the modified Mann-Kendal test (Hamed and Ramachandra Rao, 1998), which takes into account the serial correlation between observations. The authors should also provide more details and justifications for some methodological choices, such as the definition of coastal and offshore regions, the criteria for significance tests and confidence levels, and the number of modes used for the EOF and MCA analyses.

[https://doi.org/10.1016/S0022-1694\(97\)00125-X](https://doi.org/10.1016/S0022-1694(97)00125-X)

2. The MS is overloaded with content, analyses, and details, which can be reduced in some places for better understanding and easier to follow at times. For example, figure 4D and figure 12 are identical because the authors have already superimposed the significant and non-significant regions in Figure 4D. In addition, the entire section (Section 3.6) in the description of Figure 4D can be moved to the main body of Section 3.2 so as not to interrupt the story. In another example, from Figure 6 and Figure 7 and their description, the authors came to the same conclusion that the highest chlorophyll concentrations are found in the coastal region in April and May. And so on ...

I strongly recommend adding a file of supplementary material that includes these figures (6 and 12) and others that are not discussed in detail in the main body of the MS (e.g., fig. 13).

3. The MS does not provide a clear explanation for the choice of 2009 as the breakpoint for the analysis of chlorophyll-a anomalies distribution. It seems that this year was selected based on the peak of chlorophyll-a anomalies observed in 2008, but the paper does not discuss the potential causes or implications of this peak. It would be helpful to provide

more justification and context for this choice and to explore the sensitivity of the results to different breakpoints. To detect the abrupt change in chlorophyll-a concentrations, I highly recommend using the Pettitt homogeneity test (Pettitt, 1979).

<https://doi.org/10.2307/2346729>

4. Some parts of the paper are a repetition of the others, for example, Figure 8b does not bring any new results than those in Figure 7. Also, I wonder why the authors estimated the seasonal cycle of each principal component at the seasonal (Figure 8b) and interannual scale (Figure 15 E, F, G, and H) although it is supposed to use the PCs to look at variability during the whole study period. In my opinion, Figure 8 does not provide any new results and can be part of the supplementary material. In particular, the authors have already applied the EOF to the Chl-a anomalies (Figure 14 and Figure 15). Also, all spectral analyses applied to each principal component (Figure 15 I, J, K, and L) could be removed and applied the spectral analyses to the original data (Chl-a).
5. The authors mention "a period of rapid climate change" in the MS title. It is not clear to me whether the authors consider the whole study period as a rapid climate change or whether they defined this period in their MS using a specific test. Please add more details on this point or support it with a reference in MS or change the title.
6. Objectives: In Lines 80-85 the five main objectives of the study are stated. For me, objectives (ii), (iii), and (iv) seem to be identical to the main objective (line 75). I would suggest rephrasing/rewriting the main goals concisely and clearly. I would also suggest that the authors put these in the final section of the paper when summarizing their findings in the conclusion. What is the difference between objectives (ii) and (iv)?
7. Lines 233-237: In this section, more details on the SST time series in Figure 5 are needed, e.g. which year has the highest and lowest SST anomalies and variability. In addition, the SST trend values obtained should be compared with previous studies in the same region to highlight differences and similarities. Furthermore, I suggest creating the spatial trend maps of SST. This will give the reader a clear picture of the spatial and temporal variability of SST trends in different locations of the study area, which can be compared to the chlorophyll trend map.

<https://doi.org/10.3389/fmars.2023.1258117>

<https://doi.org/10.5194/nhess-22-1683-2022>

## Other comments

- Figure 2: For the comparison, it would be better to draw a two-line time series in one panel instead of drawing the positive and negative anomaly for each one, which makes the comparison unclear.
- Figures 15,17, and 18, Shaded regions make these figures unclear, I suggest removing these shaded regions.
- As far as I know, it would be better to limit the acronyms in the abstract and introduce them in the text (from the introductory chapter onwards).
- The abstract is very long and contains a more general and longer sentence, which can be shortened or moved to another section (e.g., introduction). For example, a sentence starts in line 12 and ends in line 15. The same for the next one (lines 15-18). Please try to shorten the abstract to be concise and focus on the most interesting results, of which there are many in your MS.
- Line 13: Please use “comparing with the in-situ data” instead of “comparing with the Helgoland Roads Chl-a in situ data”.
- Line 19, “A significant long-term positive trend was observed close to the Elbe estuary and adjacent area”. The trend of what?
- Please indicate the source of the bathymetry data used in Figure 1.
- Line 96: Please add the position of the Elbe estuary in Figure 1.
- Line 100: please use “flow” instead of “inserted”
- Line 114: Please provide the doi and a reference to the data, if possible, instead of using the general link of CMEMS and the product name. Especially, the same link has been repeated in line 120 and line 125. Also, I suggest removing Table 1.
- Line 120: which products are used for SST and MLD? It is not clear to me. Please add more details about these products.
- Line 163 “the two-sample Kolmogorov Smirnov test.” please add the reference for this test.
- Line 189: I suggest removing the acronym HPLC from title 3.1. I understand that it was used previously and refers to "high performance liquid chromatography" but should not be used in the title.

- Line 190: “Both time series showed significant negative trends”. Please add the values of these trends.
- Lines 214-226: please refer to fig4b, fig4c, and fig4d in this section.
- Line 229: In the caption of Figure 4, I think the authors should use the spatial mean instead of the temporal mean. Or they can use spatial climatological means.
- Line 236: “However, when it comes to the averaged MLD, no significant trend was observed.” On what basis do the authors come to this conclusion? Do they estimate the trend of temperature at MLD?
- Line 290: I suggest starting the sentence with something else instead of the number.
- Lines 402-408: What if the authors apply spectral analysis to the original data? Do they expect to get the same results?
- Figure 16; please use an appropriate range for the color bar, say between -0.4 and 0.4. It is not clear how the trends are significant in some regions and not significant in others, while both have the same trend values. Have you tried testing these correlations with different time lags and not just one month?
- Please move lines 432-439 to the methodology section.
- The MS does not provide a clear link between the observed chlorophyll-a trends and variability and the broader implications for the marine ecosystem and biogeochemical cycles in the German Bight. It would be interesting to discuss how the changes in chlorophyll-a may affect the food web structure, the carbon fluxes, and the ecological status of the region, and to compare the results with other studies in similar or contrasting regions.
- Line 494: Balkoni et al (in prep.)?!
- Although the work is very well written, a linguistic check would be very helpful, especially with the very long sentences.