Reply to Referee #1

We thank the Anonymous Referee #1 for the effort in reviewing the manuscript and for her/his positive evaluation. The posted comments and suggestions helped us to improve the manuscript.

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.

Many thanks to the Reviewer for her/his time and effort to provide us with comments, they are valid and very helpful. Below, you will find our responses to each comment. The comments received concerning language are all accepted and changed accordingly in the main text; therefore, they are not further discussed.

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?**

The LOWESS is a non-parametric fitting technique, no assumptions about data distribution are necessary. It gives a better overview of trends in data with complex patterns. In the case of temperature, we see that we have periods with different linear trends, so the lowess method gives us this overall trend considering all these different periods. The trade-off is that lowess in more computational expensive, and doing this analysis for the whole gridpoints would be very time consuming. We included the original reference of Cleveland (1979) and Cheng et al. (2022) describes the usefulness of the technique.

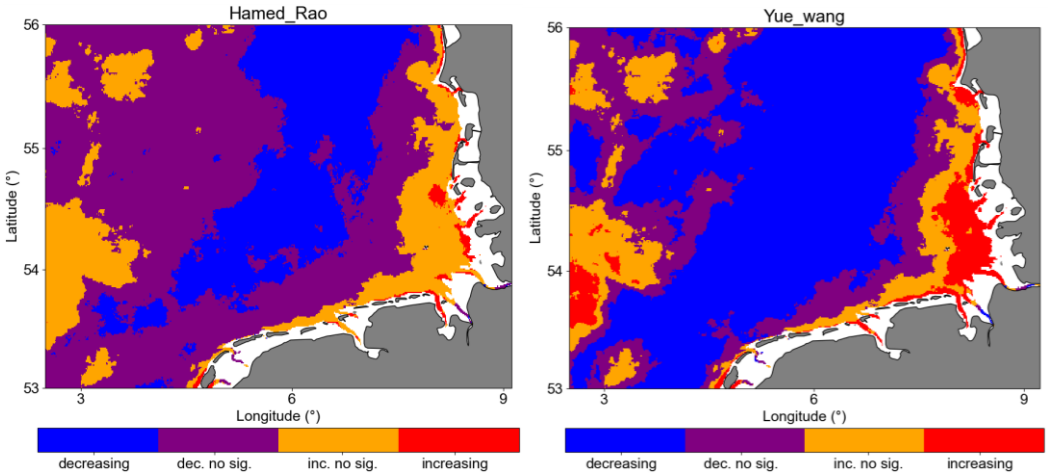
Line 234: “Specifically within the German Bight, the mean SST anomaly trend, as estimated by the locally weighted scatterplot smoothing method (LOWESS; Cleveland, 1979; Cheng et al., 2022) indicated an increase of 0.77°C from 1998 to 2020 (Fig. 5).”

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.

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

The two-tailed Wald test is inherent from the tool used to calculate the linear trends, and following the description in https://docs.scipy.org/doc/scipy/reference/generated/scipy.stats.linregress.html. It is applied with t-distribution to compute the p-values.

The Mann Kendall test was used as a more robust calculation of trend significance, as it was already used in several works. The linear trends had the aim to give the first descriptive results about the spatial chl-a in the German Bight. Besides the Hamed and Rao modified Mann Kendall test, there is also the Yue and Wang modified MK test (Yue and Wang, 2004), which agrees better in the coastal areas with the original Mann Kendall test. Considering that the Yue and Wang also corrects for serial autocorrelation, if necessary, we would change to Yue and Wang results.



Yue, S., & Wang, C. (2004). The Mann-Kendall test modified by effective sample size to detect trend in serially correlated hydrological series. *Water resources management*, 18(3), 201-218. doi:10.1023/B:WARM.0000043140.61082.60

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.

Thank you very much. We added the requested information in the Methodology Section.

Line 148: For analysis, coastal and offshore areas were defined by the isobaths of 30m, following the description results obtained by the temporal mean and standard deviation, where areas with Chl-a mean higher than 1 mg m-3 and standard deviation higher than 2 mg m-3 define coastal areas (see Figure in the Supplementary material).Consequently, the shallow Dogger Bank was considered in the offshore region.

The significance is based in the p-value obtained by the significance tests and considers the p-values lower than 0.05 as significative (95% significance level).

The number of the EOF and MCA modes is based in the curve of explained variance, defining the inflexion of the curve as a limit for significant modes.

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.

Thank you very much. The idea of figure 4D is to give the first description of the Chl-a trends in the German Bight, while figure 12 is a more robust assessment of the statistical significance of the observed trends using the Mann Kendall trend test.

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 ...

Thank you, we accepted the Reviewer's suggestion. Section 3.6 was merged with Section 3.1. We changed the 3.1 heading for "General findings and Chl-a overall trends".

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).

Thank you for the suggestion and we accept it as it will improve the clarity of our manuscript. For now, we defined Figures 7, 10 and 13 to be Supplementary material.

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.

The reviewer is right that the year was selected based on the Chl-a peak observed in 2008.
We discuss that this follows the positive NAO winter index pattern.

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

Thank you, for the suggestion. We applied the Pettitt test to detect the change points in Chl-a time series for March, April and May in coastal and offshore regions. April and May Chl-a offshore, and also May Chl-a coast showed 2010 as the possible shift point. For March, both Chl-a coast and offshore showed 2007 as probable change point. For April Chl-a coast, the year of 2012 was defined as probable shift point.

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.

The idea of Figure 8a and b is to test the EOF analysis for processes related to Chl-a variability that we already know, i.e. seasonal variability. In this case first mode is explained by the two phytoplankton blooms observed in the region and the second mode is the decrease of phytoplankton during summer and winter.

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,

To clarify, we applied the EOF analysis to seasonal Chl-a (monthly climatological means) and Chl- anomalies (seasonal signal removed by subtracting the climatological monthly means from the absolute Chl-a concentration). Figure 8 (bottom) is the temporal pattern of the second mode (PC2) applied to the Chl-a climatological means. We decided to remove the line of Chl-a mean from the PC2 graph.

Figure 8 does not provide any new results and can be part of the supplementary material.

We moved Figure 7 to Supplementary material and will keep Figure 8, if there is no opposition from the Reviewer.

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).

We applied the spectral analysis to each of the PCs because they represent the temporal modes of variability for each of the spatial modes (EOFs). We could calculate the averaged spatial mean and apply the spectral analysis, but we believe information would be lost due to the lack of the spatial component.

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.

The title is based on the findings of Amorim and Wiltshire et al. (2023) and also the results showed in Figure 5 of the manuscript. If necessary, we will follow the Reviewer's suggestion in changing 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)?

Thank you. We will present the rephrased objectives in the revised PDF.

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.

Thank you for this suggestion. We added the references pointed by the Reviewer and compared with the values observed in this study and Amorim and Wiltshire et al. (2023).

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

We created the SST trend map and it is homogeneous in the whole German Bight for the analysed period.

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.**
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Thank you, we accepted the suggestion.

- **Figures 15,17, and 18, Shaded regions make these figures unclear, I suggest removing these shaded regions.**

We accepted the suggestion.

- **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).**

Thank you very much and we accepted the suggestion.

- **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.**

Thank you very much. We will present a improved and shorter Abstract in the revised PDF.

- **Line 13: Please use "comparing with the in-situ data" instead of "comparing with the Helgoland Roads Chl-a in situ data".**

When shortening the Abstract, we removed this part of the sentence.

- **Line 19, "A significant long-term positive trend was observed close to the Elbe estuary and adjacent area". The trend of what?**

Thank you, we included "chlorophyll-a".

- **Please indicate the source of the bathymetry data used in Figure 1.**

Thank you. We included the GEBCO team reference.

Line 90: “Bathymetry of the German Bight (GEBCO Bathymetric Compilation Group 2023, 2023).”

- **Line 96: Please add the position of the Elbe estuary in Figure 1.**

Elbe position added to Figure 1.

- **Line 100: please use “flow” instead of “inserted”**

Change done.

- **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.**

Thank you, we included the doi and references and added the Data Availability Section.

- **Line 120: which products are used for SST and MLD? It is not clear to me. Please add more details about these products.**

We clarified this in the Methodology.

- **Line 163 ‘‘the two-sample Kolmogorov Smirnov test.’’ please add the reference for this test.**

Reference added.

Massey, F. J. (1951). The Kolmogorov-Smirnov Test for Goodness of Fit. *Journal of the American Statistical Association*, *46*(253), 68–78. https://doi.org/10.1080/01621459.1951.10500769

- **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.**

Thank you. We followed the reviewer’s suggestion.

Line 189: Evaluation of in situ and Remote Sensing Chlorophyll-a

Thank you, we removed it.

- **Line 190: “Both time series showed significant negative trends”. Please add the values of these trends.**

We added the trend values of 0.031 and 0.025 mg m^{-3} for in situ and remote sensing, respectively.

- **Lines 214-226: please refer to fig4b, fig4c, and fig4d in this section.**

Done.

- **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.**

Thank you for pointing this. We refer as temporal mean/std because is the mean/std in the time dimension of a spatial data. If it is confusing and the Reviewer prefers the term “climatological”, we would change without hesitation.

- **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?**

No, we estimated the significance of the MLD trends as we did with SST (Fig. 5), but because it was not significant, we did not show. Besides, the spatial MLD trend analysis with Mann-Kendall test (not shown), did not give significant trends.

- **Line 290: I suggest starting the sentence with something else instead of the number.**

Thank you. We modified the text.

Line 290: “Considering the German Bight area here analysed, 96% had a maximum…”

- **Lines 402-408: What if the authors apply spectral analysis to the original data? Do they expect to get the same results?**

- We applied the spectral analysis to each of the PCs because they represent the temporal modes of variability for each of the spatial modes (EOFs). We could calculate the averaged spatial mean and apply the spectral analysis, but we believe information would be lost due to the lack of the spatial component.

- **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?**

Thank you, we changed the colorbar range. No we did not apply longer time lags because, by our knowledge, scales longer than one month, in an intrannual scale, will decrease in correlation.

- **Please move lines 432-439 to the methodology section.**

Thank you, we accepted the suggestion.

- **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.**

Thank you, we will include in the revised PDF how Chl-a changes will impact in a more general and holistic way the ecology in the German Bight. This goes beyond the scope of our study, but it is clearly a discussion that cannot be left outside when discussing changes in Chl-a consequently in marine primary production.

- **Line 494: Balkoni et al (in prep.)?!**

We will ask the Editor's help to provide a better citation format when we discuss manuscripts in preparation.

- **Although the work is very well written, a linguistic check would be very helpful, especially with the very long sentences.**

Thank you very much. We will put more care on it.