Reply to Referee #2

We thank the Anonymous Referee #2 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 MS "Analyses of sea surface Chlorophyll-a trends and variability in a period of rapid Climate change, German Bight, North Sea ", from Felipe de Luca Lopes de Amorim et al.

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.

General remarks

• The study is relevant and important wrt marine ecosystems in the context of the climate trends, and the method is adequate to explore the questions.

Thank you very much.

• Yet, I have problems to identify the take-home message. The paper is very long and contains 18+ graphs, which somehow blurs the message.

Thank you, we tried to reduce the amount of information focusing in the most important results and will move some of the graphs to Supplementary material.

Our take-home message, considering the dominant decreasing trends in chlorophyll-a concentration observed in this study, is that in a shallow German Bight, the impact of increasing temperatures are more important than stratification in the chlorophyll-a variability. But these

effects of temperature are mostly caused by indirect effects and temperature is an indicator of hydrographic changes in the German Bight. We will improve the clarity of our take-home message.

• The Authors may want to provide clearer explanations about some statistical methods (e.g., combined EOFs and PCs results are not always straightforward to interpret). Clearly, these statistical approaches are rich and provide good insights, but they remain somewhat cryptic still. Such paper may be the opportunity to share knowledge and familiarize the community on the used methods. This is a non- mandatory suggestion.

We have a paragraph in the text (Lines 374-380) explaining a general analysis of EOF results. Because the EOF is completely mathematical, the physical interpretation is done by the researchers related to their pre-knowledge about the variability processes they want to study.

• The language of the whole manuscript should be screened by an English-speaking colleague before publication (and, ideally, even before submission). I pinpointed some disturbing examples but did not underline all instances.

We are sorry and increased our effort in the text.

Abstract

This section describes with too many details the results, and could probably be shortened with a better summary of the results. What is the take-home message? There are some unclear sentences that should be corrected. For instance:

• L27. "The monthly chlorophyll-a concentration anomalies covaried 45% with sea surface temperature anomalies" should better be "Monthly chlorophyll-a concentration anomalies covaried by 45% with sea surface temperature anomalies"

Thank you, we accepted the suggestion.

• L28-29. "This study demonstrated that the […] product can assess mostly of the known processes" should be "This study demonstrated that the […] product can evaluate most known processes"

Thank you, we accepted the suggestion.

Introduction

1. L57 'dimension' instead of 'domain'?

Thank you, we accepted the suggestion.

2. L59 'enabling the assessment of Chl-a spatiotemporal variability.' … but only at the surface.

Yes, only in the surface.

Line 59: Satellite data offers a solution to this problem by providing comprehensive spatial and temporal coverage, enabling the assessment of surface Chl-a spatiotemporal variability.

3. L69-73 exhibit an argument that is between a discussion and an introduction. Having read it as it is written now, I am not sufficiently convinced that the approach is without flaws, as more questions are raised than answered. For instance, you mention a remote sensing (RS) sampling at depths comprised within 1-12 m (depending on turbidity).

However, considering the total depth at the Helgoland sampling site (~6-10 m) and its surroundings sampled by satellite (~30-40 m in the Elbe Glacial Valley), we see that these are different depths. Is there a difference wrt the interpretation of the RS signal of Chl? I mean, if the satellite Chl is calibrated at Helgoland sampling site, is it valid at deeper sites? And what do you do when the water column is stratified in summer (is it?)? When it is not stratified, it is well-mixed for dissolved substances, but not for particles (you even suggest this idea when you rightfully mention that turbulent mixing may enhance resuspension). What about that when it comes to analyze RS Chl signal? Do changes in turbulence only generate a small variability in Chl wrt the seasonal variability? Perhaps the Authors might want to be more affirmative in the Introduction (i.e., suggest less questions), and then discuss the details about RS signal, depth, stratification, resuspension, etc. in the Discussion? As far as I can see, it seems to be just a matter of presenting the argument.

Thank you for this suggestion. It would have been interesting to explore this aspect. However, in the case of our study, it seems slightly out of scope because we assume that the monthly means will remove the stratification effect, so we can consider a Chl-a response as the one observed in a vertically homogeneous water column. We hope this simple argument will be enough to answer Reviewer #2 comment.

4. L75 ‘Chlorophyll-a (Chl-a)’ This acronym was already defined above. Please, double check the whole manuscript for overall consistency.

Methods

1. L136 ‘60 km of the German coast’ Do you mean ‘60 km off the German coast’?

Yes, thank you. Fixed.

2. L138 ‘The samples are representative for the whole water column due to the well-mixed conditions’. Indeed, Wiltshire et al. say it in their paper of 2009 based on an earlier reference. Yet, isn’t there a vertical gradient of particles (Chl and SPM) in spite of the vertical mixed conditions? Is it negligible for the purpose of this study?

Thank you very much for this question. We used the monthly means of Chl-a surface exactly to overcome this problem with vertical gradients. Considering the time scale of mixing processes in the German Bight, the monthly means make the vertical gradients negligible.

3. L167 ‘As a pre-analysis, we calculated temporal mean and standard deviation (std) of the Chl-a anomalies.’ When writing ‘temporal mean’ (or std) do you mean ‘yearly mean’ (or std)? Please, specify here.

To clarify, we mean the temporal average of the whole analysed period.

Line 167: “As a pre-analysis, we calculated temporal mean and standard deviation (std) of the Chl-a anomalies for the whole analysed period.”

4. If you see Fig.3b, would you consider that Chla anomalies are normally distributed, or skewed? Is it important when calculating the mean and std?

Considering that the frequency of the highest Chl-a anomalies is very little compared to the whole sampling, the highest density of values are still around the 0 mean. We could consider the Chl-a anomalies, if not normally distributed, weakly skewed, which the mean and standard deviation are still valid.

5. L175 ‘1 time step lagged’ Is the lag one month, or is it another time length?

Yes, the lag is one month, clarified in Line 175.

Line 175: "…the direct anomaly fields and in lagged time step of 1 month Chl-a in relation to SST and MLD."

Results

1. L190 'Both time series showed significant negative trends, evaluated by the Mann Kendall trend test.' Difficult to see how this statement relates to Fig.2. It seems better linked to Fig.10…

We verified the values of the trends for both time series presented in Figure 2 using the Mann Kendall trend test, and both showed decreasing trends. Following comment from Reviewer #1, we changed Figure 2 and included the trend values for better visualization.

2. Fig.3b Is the green colour the superimposition of both in situ and RS Chl? Please, clarify or improve the plot.

Yes, it is the superimposition. We made clear in the caption.

Line 211: Figure 1: a) Boxplots and b) distributions of remote sensing (orange) and in-situ (blue) monthly Chl-a anomalies. The shaded areas in b) are the superimposition of in situ and remote sensing bars.

3. Fig.4d There is an increasing trend of Chla at the coast and a decreasing trend offshore. While any potential eutrophication/de-eutrophication trend may affect Chla, it would do it at the coast mainly. This is a very interesting result as it suggests that the (de-

)eutrophication trend is not the only (or even the main) controlling factor of the Chl trend. This result motivates the study.

Thank you, we will consider this comment to improve the take-home message of the manuscript. Please, also refer to the answer on comment 12 of the Results.

4. Fig.4 & 5 In this approach, attention is given to the spatial variability of Chl. It raises the question of whether the observed increasing trend in SST is also variable in space, or if it is homogenous in the G. Bight…

It is homogeneous in the whole German Bight.

5. Fig.6 caption. The last sentence of the caption should be in the text, not in the caption.

We accepted the request.

Line 245: It is possible to observe the intra-annual behaviour of Chl-a, with a positive gradient from open waters to coast, and the increase in Chl-a in April and August.

6. L256 'bellow' => 'below' Please, check the MS for this kind of misprint.

Thank you, we fixed it.

7. L260-261 'although the spatial averaged Chl-a remote sensing was overestimated during winter months, and the second bloom peak was delayed in offshore areas.' Dubious interpretation. It seems the Authors were expecting the same results for mean coastal RS

Chl and Helgoland in situ Chl profiles. I do not see an 'overestimate' or a 'delay'. Profiles are just different.

We consider the in situ data to be characteristic of a transitional zone in the German Bight. When we mentioned overestimation and delay, we referred to the capacity of the in situ data from one location to represent the whole German Bight area. To avoid doubts, we removed part of the sentence.

Line 258: "The in situ HRTS acquired in the transitional zone of the German Bight, between coastal and offshore areas, aligned well with the spatial averages of Chl-a remote sensing."

8. Fig.8 caption. Once again, clarify please. Understanding what is on a graph should be made easy by the Authors for the reader, especially in a paper showing 18+ graphs. An effort should definitely be provided on that aspect.

Thank you for the valuable comment. We gave more attention to the captions.

Line 285: "First and second EOF spatial pattern (top) and PC temporal modes (bottom) of monthly climatological means of Chl-a. Dashed thin line superimposed by the PC lines is the Chl-a spatially averaged at the study area. March-April period (green shaded) and August-September (pink shaded)."

9. Fig.8 Maybe I did not fully understand the EOF approach, but it is unclear to me why PC2 was averaged over the entire area and not over the two different areas (red and blue) identified with EOF2. As a side remark, PC2 shows a seasonal profile that reminds me the profile of SPM concentration in most coastal zones of the southern North Sea (high winter values, and low summer values due to TEP-enhanced flocculation of SPM).

In Figure 8 (bottom), we show PC1 and PC2 temporal patterns, superimposed by the Chl-a concentration averaged for the whole area. This was made to show that what dominates the variability of seasonal Chl-a is the presence of two phytoplankton blooms (peaks) and the decrease of Chl-a during summer and winter months. This is exactly what the spatial average of Chl-a shows. The PC2 result shows that the decrease during summer and winter months is not the same for coastal and offshore areas, because the decrease during summer in coastal areas is not as significant as in offshore areas when compared with the spring phytoplankton bloom.

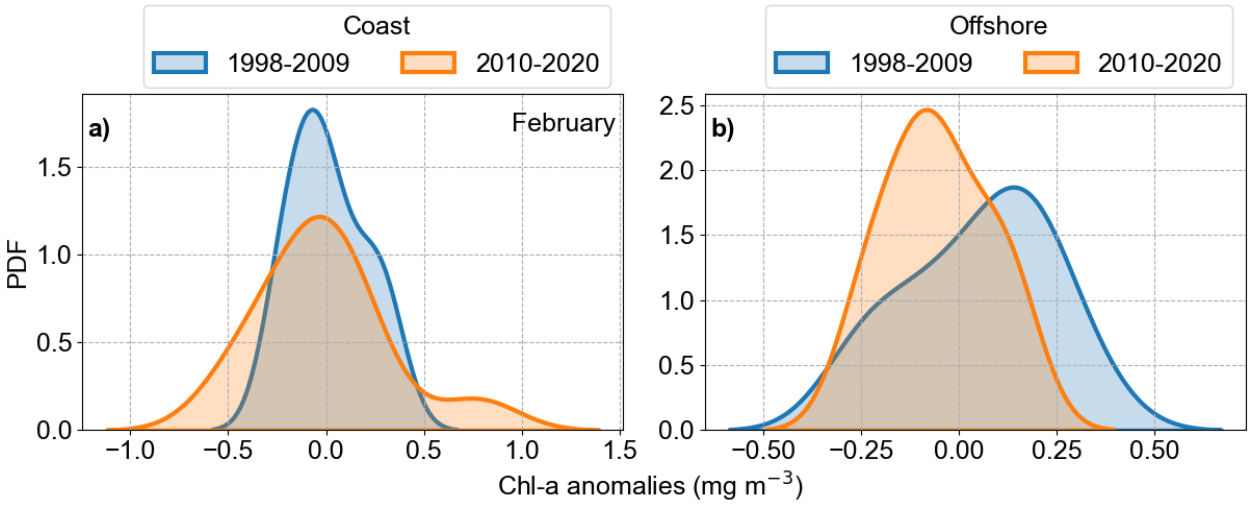
10. L316 'The peak in Chl-a anomalies in 2008 was related with a positive peak of North Atlantic Oscillation index winter mean (NAO)' (and sentences next to it). This is not a convincing demonstration. I would be convinced if Chl anomalies in April were in general more correlated with winter NAOi. But I do not think it is the case. Therefore this statement seems very dubious to me. This being said, I have nothing against dividing the period into two segments around 2010, as the Authors did. These two periods seem indeed different wrt their mean April Chl, for instance. Some impartial statistical tests might even be conducted to justify this separation.

Thank you very much. We conducted a Pettitt shift test suggested by Reviewer #1.

11. L329 'These results could be the response of earlier spring blooms in the period 2010-2020 compared to the years before.' Indeed, the results from March to May might indicate a forward shift of the spring bloom to earlier days in recent years. Did the Authors also have a look at the February distributions?

No, we did not look at the February distributions. We considered that light availability did not change in the analysed period and February primary production would still be limited by light.

Considering the Reviewer's #2 interest, we calculated the February Chl-a anomalies distributions:



What is observed is that in offshore areas, the distribution shifted to negative anomalies in the period of 2010-2020, characterized by values below the mean of the period 1998-2020. In the coast, the distribution in the period 2010-2020 increased variability for both negative and positive values (stretched in the x-axis), with a very week bimodal distribution due to a peak in positive anomalies.

12. L347-361 Interesting results! Yet, I find it odd that the Authors offer an interpretation of why coastal Chl anomalies tend to increase in recent years without even mentioning a possible trend in coastal nutrients (or adjacent river loads, at least the Elbe)…

This is a very interesting hypothesis proposed by the Reviewer #2. Schulz, G. (2023) shows that the amount of nitrogen being carried to the Elbe estuary (consequently the German Bight coast) decreased by both control in river eutrophication and less river discharge towards the estuary. The decrease in river discharge would also affect other essential nutrients like phosphate. Considering these findings, we could not consider that there is an increase in nutrients in the German Bight, at least not form the Elbe. I hope we understood the question and answered it properly.

Schulz, G., van Beusekom, J. E., Jacob, J., Bold, S., Schöl, A., Ankele, M., ... & Dähnke, K. (2023). Low discharge intensifies nitrogen retention in rivers–a case study in the Elbe River. Science of The Total Environment, 904, 166740.

13. Fig.16 caption. Now, we know that the lag is one month… It should have been said in Methods (or perhaps I missed it?)

Yes, the lag is one month. We clarified this in the Methodology.

Line 175: "…the direct anomaly fields and in lagged time step of 1 month Chl-a in relation to SST and MLD."

14. Fig 17 & 18. Improve caption please.

We improved captions as shown below:

Line 441: "Figure 17: Results of MCA showing the first co-varying mode between Chl-a and SST anomalies. Top images are the co-variability mode maps (Chl-a: left; SST: right). Bottom images are the temporal co-variability PC1 (left) and the corresponding spectra (right). PC1 is shown as rolling means of 6 points to better visualize the temporal co-variability of mode 1 between Chl-a and SST anomalies."

Line 456: "Figure 18: Results of MCA showing the first co-varying mode between Chl-a and MLD anomalies. Top images are the co-variability mode maps (Chl-a: left; MLD: right). Bottom images are the temporal co-variability PC1 (left) and the corresponding spectra (right). PC1 is shown as rolling means of 6 points to better visualize the temporal co-variability of mode 1 between Chl-a and MLD anomalies."

15. L492-3 is a direct repetition of L480-1

Thank you very much for pointing this mistake. We removed the sentence from lines 480-481.

16. L494-5 The information about nutrients comes much too late in this manuscript about Chla variability. I wonder if it shouldn't even take place in the introduction as it is not a proper result of the study and nevertheless an important element of the story.

We accepted the suggestion and moved the sentence to the introduction.

Line 54: "Changes in nutrient concentrations have profound impacts on phytoplankton productivity and species composition (Hickel et al., 1993; Topcu et al., 2011; Burson et al, 2016). Balkoni et al (in prep.) estimated nutrients decadal changes in the German Bight and the results point out that there is a decrease in nutrients."

17. L499 'decreasing trends and slight increase of Chl-a' Unclear sentence.

Thank you for pointing this. We rephrased as:

Line 499: "Alvera-Azcárate et al. (2021) showed the heterogeneity of the North Sea, with areas presenting decreasing trends and others indicating slightly increasing trends of Chl-a."

18. The discussion does not discuss the validity of the approach. It is not always mandatory but in this case it may be more convincing (see, e.g, my comment in the Introduction section).

Thank you. We will include a discussion of the approach validity in the revised PDF.

19. The conclusion seems a repetition of the Discussion with more numbers and less references. Where is the core message? When I see the results, I see a potential story. However, I do not find that story in the text.

Thank you very much. We will try to improve the clarity of our take-home message as discussed in the General Remarks.