We appreciate the reviewer for taking the time to make such detailed comments to our manuscript. Special thanks for the suggestions on AOT_{ma} calculation and the comment on including a figure with wind rose along with DOT. However, we expects the reviewer to be quantitative in referring to the actual publications making a comparable delineation of marine zones for the Arabian Seas, beyond what we regard as the state of the art and have quoted and based our publication on.

General comments: "Can't say I like this paper. The innovative information established by the authors is meager: all prime features of the phytoplankton field across the north Arabian Sea and their driving processes are known and the present research has not contributed to this knowledge. The authors regard as a major merit of their work a more fine delineation of marine zones in the north Arabian Sea as compared to the ones determined previously by other workers. First of all, the zones established by the authors are readily discernible in the spatial distributions of Chl, and secondly, the established contours of the zones are not proven."

Authors' comments: In order to "prove" the robustness of our delineation of the identified zones a new figure representing the seasonal average of Chl-a over the winter period (Nov-March) is included as Figure R1, which reveals distinct Chl-a characteristics for each of the identified ecological zones. Our objective classification based on winter average of Chl-a values from eleven winter seasons takes into account both spatial and temporal information. To say that the same result could be obtained by the authors by looking at the spatial distribution is highly uncertain and the result would probably depend both on the person doing the subjective analysis and how the data was presented in terms of colormap etc.. In the initial manuscript itself, the authors have compared Chl-a variability in six obtained zones with the well-accepted biogeographic classification of Longhurst falling in the selected area. As our study has utilised Chl-a concentration obtained from satellite sensors which has about 100 times finer spatial resolution used by Longhurst for regional mapping for classifying ecological zones in the northern Arabian Sea, this regional classification could delineate the spatial Chl-a variability better with more detailed regional information than obtained from Longhurst's classification. The objectivity of the methods used and the increased amount of information in modern ocean color products are the basis for author's argument about 'finer delineation of marine zones in the north Arabian Sea' is true.

Authors have analysed physical and chemical characteristics within each of the identified marine ecological zones, which relation between cooling deepening and production between six zones. In the analysis section, author has made use of the established knowledge on driving processes of Chl-a processes in the study area based on published information. Our information is based on surface-data and limited number of variables – hence we must utilize previous studies to better understand out results. However, the in-situ observation coverage in the Arabian Sea is lacking both spatially and temporally and the utilized literature base their result on observations from shorter periods compared with the to our study. Such long period of information is very essential for resolving inter-annual variability in the ecosystem characteristics. Our study contributes understanding of the temporal/spatial variability of phytoplankton and hence, authors disagree with the reviewer's comment on *'The innovative information established by the authors is meager: all prime features of the phytoplankton field across the north Arabian Sea and their driving processes are known and the present research has not contributed to this knowledge'*.

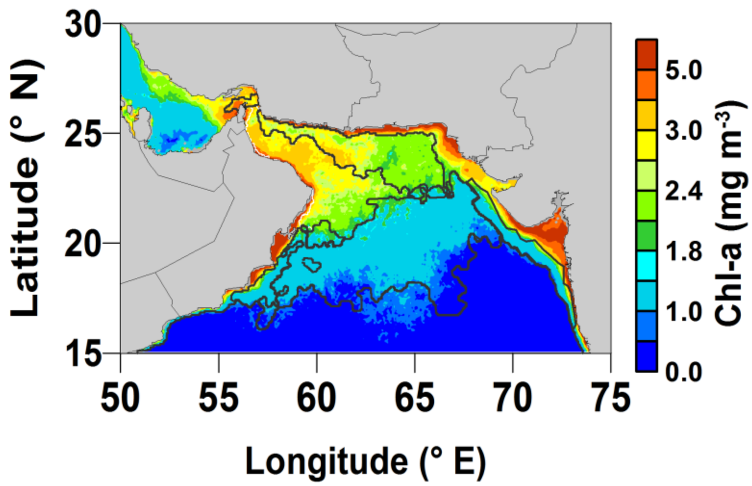

Figure R1: Annual winter climatology (seasonal average Chl-a concentration over the winter period (Nov-March) from 2002 to 2013) of Chl-a revealed from satellite data. The black line indicated the delineated zonal boundaries.

This thesis is underpinned by my comments to the text. The paper composition is also unsatisfactory: instead of partitioning the respective part of the paper into Results and Discussion sections, the authors mixed up the reporting on the results obtained and underpinning of the results' validity. This caused numerous repetitions and unnecessary lengthening of the text. The authors' English needs to be brushed up In light of the above and the comments below, I reckon that the paper should be subsumed under the category "major revision".

Author's agree to restructure the summary and conclusion section. If the editor provide an opportunity this part of write-up will be restructured to fit with the more traditional partitioning of scientific papers and the English will be revised and improved.

Authors do appreciate for the comments on AOT_{ma} calculation and wind rose. These two comments with several comments on poorly written statement has helped authors to improve the manuscript. However, we disagree on comment about the PCA is very drastic.

Specific comments

1. Specify the desert(s); [5 (page 2)]

Authors' comment: Arabian desert in the west and Thar desert to the east are the major dust contributing deserts.

2. It is insufficient to anticipate: this needs to be proven. [15 (page 2)]

Authors' comment: Agreed. The statement has been rephrased and the following references were included in the text that justifies our argument: Longhurst 1995, Longhurst 1998 and Longhurst 2006; Spalding et al. 2012.

3. Why the Chl concentration at 0.5 mg/m3 is used as a criterion? [25 (page 2)]

Authors' comment: The concerned statement is a general argument for Chl-a concentration for the study area in an annual cycle (Sarma et al. 2012; Ravichandran et al. 2012). Based on Chl-a monthly climatology for the study area, annual concentration considering all seasons comes around 0.5 mg m^{-3}.

4. Firstly, Mignot et al. reported solely on Pacific and Mediterranean oligotrophic waters (typically, Chl is significantly under 1 mg/m3). The actual location and degree of "weakness" of deep Chl maxima (DCM) are site-specific. For the locations within the study waters the assertion that DCM did not affect the satellite-borne Chl concentrations needs independent confirmation. The authors write that DCM in the study area is presumingly shallow because of the strong attenuation by surface Chl. A rather strange argument: if the DCM is shallow then it can be "sensed" by the satellite sensor. Besides, the Chl concentrations reported in your study are not likely to affect the downwelling light to a degree of eliminating the DCM optical influence. At least, a Hydrolight experiment can bring certainty in this issue. [30 (page 2)]

Authors' comment: We agree that deep Chl maxima are site-specific. However, some regions in the selected area show shallow DCM (24 m) during winter (Al-Niami et al. 2017), and concurrently regions with deeper DCM exist in the study area (Breves et al. 2003; Ravichandran et al. 2012; Kumar 2000). Since, it is clear that DCM is not shallow in the entire study area during winter, the statement 'DCM is shallow during winter' is deleted. However, it is to be mentioned here that in-situ coverage on Arabian Sea is not sufficient to give complete spatial and temporal variability on DCM and hence we have to accept the uncertainty on this issue (Barlow et al. 1996).

5. There are no assessments of Chl retrieval errors. This issue is essential, because of the above comment, and also because of the optical heterogeneity within the study waters. It is unnecessary to mention that the NASA algorithm used by the authors is valid (and produces really accurate values of Chl concentrations) only for case I waters (i.e. strictly oligotrophic). However, the authors haven't elucidated this issue with regard to the studied waters in view of the impacts produced by the river discharge, and dust fallouts. The observed variations in Chl could arise,

inter alia, from the inability of the NASA algorithm to retrieve Chl correctly in those parts of the study sea where waters are not strictly case I waters. In this case the zoning [in essence, based on Chl variations] might be compromised (at least the declared contours of six zones, which are supposed to be the main advantage of the study). That is why the realistic error bars relevant to the study sea are indispensable for all illustrations of Chl concentrations in the selected zones. The issue of retrieval error arises also with respect to other satellite-borne variables used in the study. [15 (page 3)]

Authors' comment: The NASA OBPG chlorophyll product that we used does not have values of uncertainties associated with each value of chlorophyll and, therefore, region-wise assessment of errors in the chlorophyll product is not feasible to perform. The validation shows that in oligotrophic waters the algorithm accuracy is quite high: r^2 = 0.86, RMS = 0.25 mg m^{-3} (Feldman, 2017; Hu et al., 2012). Large errors are presumably observed in the turbid waters of the Persian Gulf as well as the coastal areas. Our region of interest excluded coastal areas and included only phytoplankton dominated open ocean areas, where the standard algorithm of NASA would work well.

6. As a matter of fact: the coefficients taken from the literature are not necessarily relevant to the study area, e.g. fdu, and AOTm a (the later was determined by Smirnov et al., for Midway Island in the Pacific, located in waters located far away from the study area; meanwhile, it is known that AOTm a depends not only upon the above water surface wind but also on a number of other parameters, that is why there are many parameterizations suggested for specific marine locations). [5 (page 4)]

Authors' comment: Thanks for this comment. This question is a valid one, author was not aware of the stated scenario. It is clear now that in the Indian Ocean an exponential relation exist between wind speed and sea salt formation, where as in Pacific this relation is linear. As a result of which now, the author have replaced Smirnov et al. 2003 with Moorthy 1997 to estimate AOT_m. DOT such obtained is super-imposed in the manuscript figure below (pink line) while red represents DOT as computed with the old formula, as can be seen the values differ, but the temporal evolution is similar in zone 1, 2 and 5 (Figure R2).

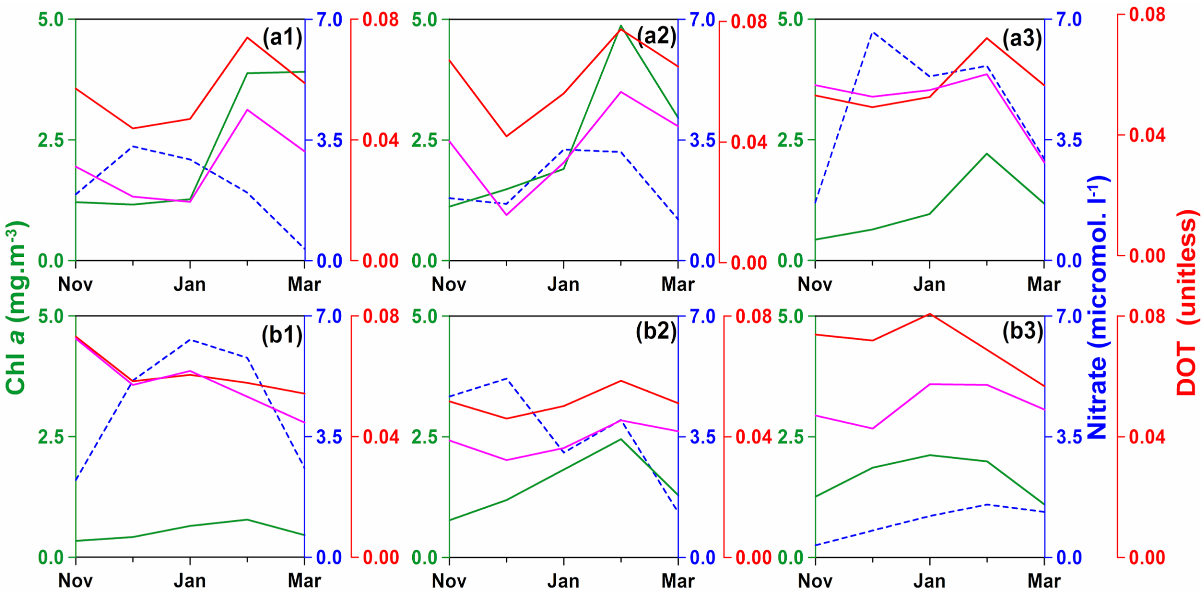

Figure R2: Averaged variability of surface Chl-a, nitrate and DOT in six ecological zones. Viewports (a), (b), (c), (d), (e) and (f) represents variability along first, second, third, fourth, fifth and sixth zones, respectively. Red and pink line indicates DOT computed with the old and new formula.

7. Please, give the major assessments of MLD simulation errors (results of validation by George et al., 2010). Error bars are indispensably required for all illustrations of MLD variations in the selected zones. [20 (page 4)]

Authors' comment: Statistical analysis cannot be carried out using George et al. 2010, hence a comparison of MLD modeled data with the recent Argo derived mixed layer climatology (http://mixedlayer.ucsd.edu/Argo) was carried out for winter months. On average a RMSD of 20 m and a 28 % error is observed between model output and argo dataset. It was found that adding error bars to the plots looked very messy, instead we will add a paragraph in the revised manuscript where the MLD simulation error in each region is described.

8. If only PC 1-3 are meaningful, why you provide illustrations for PC 4 and PC5 (fig. 2). The authors are reporting on the northwestern and southeastern gradients in spatial distributions of PC1 (that is the component that predominantly, accounts for 97% ofthe spatio-temporal variance in Chl) as one of the important findings. However, this finding could be attained without the PC analysis just by visual examination of the spatial distribution of Chl or/and SST, which is confirmed by the authors themselves. So there is nothing new in this finding. [5(page 7)]

Authors' comment: A simple visual examination remains a subjective approach complicated (or, even, disabled) by the fact that sequences of maps of several variables have to be visualized and analyzed simultaneously. Efficiency of such a manual method is not described anywhere in literature, nor proven to be correct. In fact, PC1 accounts for only 80% of the Chlorophyll variance, indicating that if only a single map of Chl (e.g. annual average) was used for zoning, it would be incorrect in 20% of the cases.

We attempted to develop an objective method of analysis of time series and provide exhaustive explanations of the methodology and description of several experiments to illustrate its sensitivity to various factors (number of PCs, number of zones, etcs).

PC5 is provided just for illustration of the speckle noise that contaminates the signal. In case of PC4 some of the signal is still present and, therefore, it is used, but PC5 appears to be useless. We respectfully disagree with the comment that there is nothing new in this finding.

9. First, the authors write that PC4 and PC5 are not informative (mostly noise) and then declare that the final delineation into ecological zones was obtained by combining the first 4 PCs. Please, explain. Also, please, explain what you mean saying “based on general Chl pattern in..”[5 and 10 (page 8)]

Authors’ comment: PC4 and PC5 were re-analysed and the corresponding paragraphs of the text were re-written incorporating the information on periodicity. Based on periodicity it is clear that PC4 and PC5 represent the intra-winter variability. Since, this work concentrates on intra-winter variability, these two PC’s cannot be considered as noise. The amplitude of PC4 is more than 10% of PC1, and PC4 is necessary to include it in the zoning to get the narrow coastal regions that we know exists and is not present when only 3 PCs are included (see argument in appendix A in the manuscript). These arguments show the reason for including PC4 in the final zoning.

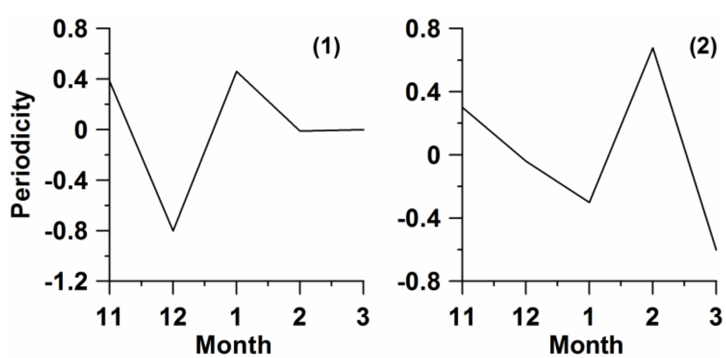

Figure R3: Periodicity for (1) PC4 and (2) PC5.

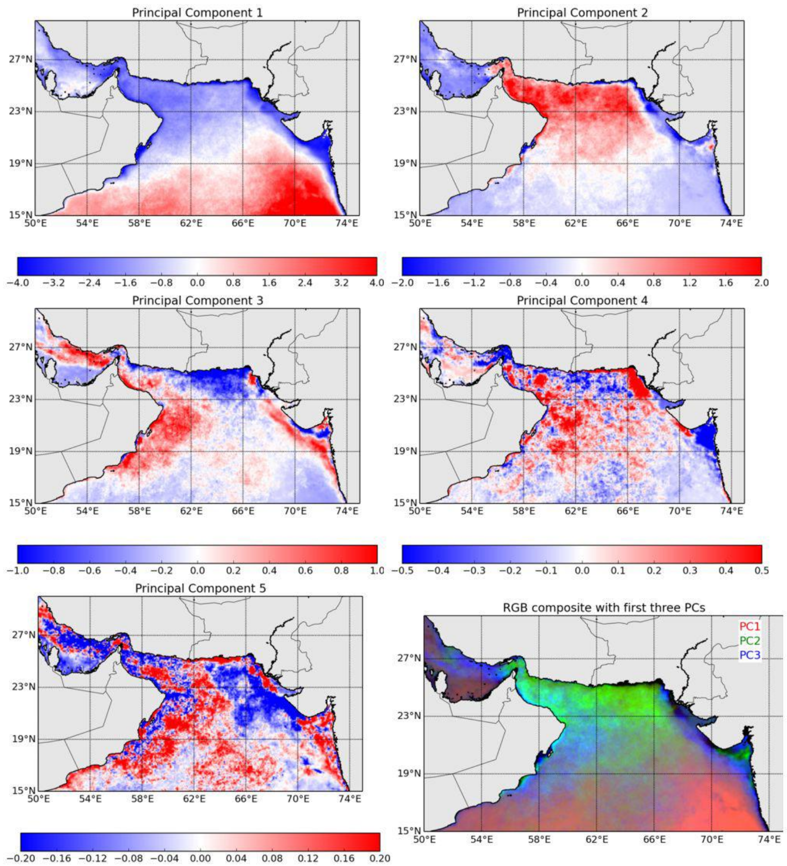

Figure R4 (Figure 2 from manuscript): Individual maps of principal components (PC 1 to 5) and RGB composite of the first three statistically significant components.

PC4 causes high Chl-a production for January, November months and minima during December (Figure 3R(1)). Most of the high variation during January / November occurs during December too, hence in this PC we can observe more regions with zero variation (indicated by white colour) (Figure 3R(2)). Therefore, the regions under this PC are highly scattered within north, central and eastern part of the study area. Next PC demarcates regions with high Chl-a production for February, and follows a decreasing trend for November to January months. Similar, to PC4, region coming under this PC is also scattered highly. However, this PC differentiates Persian Gulf and Pakistan and Gujarat coast from the rest of the north-central region (Figure R4).

Regarding second part of the question, a map (RGB composite of the first three statistically significant components) illustrating the significance of combined Principal Components (PCs) is described (line no. 5). This map is generated with the combination of first three PC's (Figure 2). First PC is represented using red, second by green and third by blue. Zones with similar colors have similar combinations of PC values and therefore this figure illustrates similar winter variability on Chl-a. This image is the application of a statistical clustering method to delineate the study region into areas with distinct Chl-a dynamics. This is based on the values of principal components (details is discussed in section 3 of manuscript).

The same method was applied with the PCs 3, 4 and 5. Clustering in the case, is done making use of the technique ‘k-mean Cluster Analysis’ (CA). Several combinations of PC and CA is carried out (described in Appendix A). Based on the available knowledge of Chl-a variability as well as oceanographic characteristics in the area the combination of 4 PC and 8 CA is selected (Figure 3 and Appendix A in the main manuscript).

10. Please, explain, on the basis of what it was decided that satellite-derived Chl values along coastal and shallow waters were erroneous.

Authors’ comment: Retrieval of Chl a concentration from optical satellite data near to the coast is complex. The water masses contain optical properties of riverine fresh water influx, containing terrogeneous dissolved organic compounds, and these contribute to an error in Chl-a retrieval. In addition, shallow water depth regions (depth < 30 m) may include signals from bottom reflections (with clear waters conditions), which can introduce additional errors in the retrieval process (Martin, 2003). Considering the fact that our present work uses a global Chl-a retrieval algorithm (OCI) to obtain Chl-a along with the above mentioned two points, we mask out the analysis of satellite derived Chl-a values near the coast. Additionally, in our response to question number 5 we explain that only regions classified as case-1 waters during winter in the selected study area, where the NASA algorithm will work well, are selected.

11. Please, explain in the paper what are the reasons to believe that “ the physical forcing affecting chl concentration along the two regions is likely to be different” … [10 (page 8) 15 (page 8)]

Authors’ comment: Based on knowledge available through published studies (Kumar and Prasad, 1994; Kumar et al. 2000; Shetye et al. 1994) it is concluded that the two regions are likely to be different. Accordingly, these references will be included in the revised manuscript.

12. The authors write that 1-3 zones (encompassed by Longhurst’s ARAB zone) are strong upwelling regions with high Chl in winter time, and then they refer to Longhust who defines the ARAB province as a zone with strong upwelling during summer and strong convective cooling during winter. Obviously, some phrase is required to follow these statements in order to clarify the actual hydrodynamic situation therein. [5 and 10 (page 9)]

Authors’ comment: We apologise for this mistake, and will correct the text accordingly. Zones 1-3 are regions where strong convective overturning occurs during winter (page nos 9 (line number 8, 12 and 19), 10 (line number 1) and 11 (line number 20). Hence, the comparison of Chl in the convective zones identified with Longhurst province during winter is been carried out.

13. Please, specify 1. what is known about the atmospheric deposition on nitrogen (there is no respective reference), and 2. why this mechanism of nutrient supply acts only in zone 6 (or, at least, is not mentioned with regard to other zones). Also, specify the annual cycle of stream flow of the Narmada and Tapi rivers to support your thesis that nutrient supply from Narmada and Tapi rivers as well as atmospheric deposition of nitrogen enhances marine production in zone 6. This additional information might clarify the authors' statement that in zone 6 "peak Chl-a is observed during January" as opposed to other zones.[5 (page 10)]

Authors' comment: A relevant reference on atmospheric deposition on nitrogen in the study area is Singh et al. 2012. He has reported the contribution of atmospheric nitrogen during winter is 0.06 mmol N m-2 day-1 based on 43 in-situ measurements. Assuming that all the six zones are exposed to this level of atmospheric deposition of nitrogen and comparing this with concentration available for the study area (Figure 9 of manuscript), it is clear that the contribution is low. The complexity of zone 6, in particular, can be explained by additional sources of nitrogen supply eg., likely from rivers discharges (see Figure R5). The sentence will be restructured accordingly.

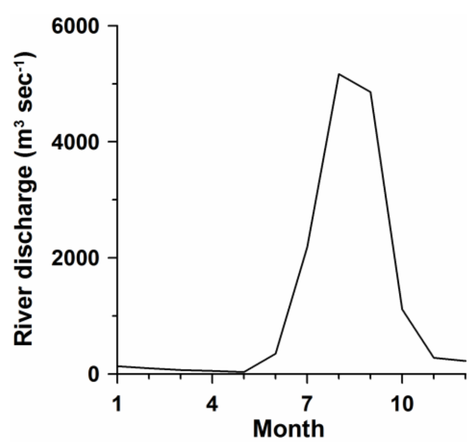

Figure R5: Annual river discharge from Narmada and Tapi. (Data source: http://nelson.wisc.edu/sage)

14. First, the authors write that the inverse relationship between SST and Chl-a have weak correlation coefficient 1 in zone 1 (r = 0.39, n=60) and zone 2 (r = 0.55, n=60). . Then a bit further: "However, MLD and Chl-a in zone 1 and 2 are moderately correlated (correlation coefficient, r = 0.28)". What are your criteria in this regard? [15 (page 11)]

Authors' comment: Thank for this observation, as you pointed out a criteria based on r value is defined as follows:

- r> 0.50 is high,
- r>0.35 is moderate
- r< 0.35 is low.

Which will be introduced in the manuscript. Hence, the above mentioned statement will changed as ‘the inverse relationship between SST and Chl-a have moderate correlation coefficient 1 in zone 1 ($r = 0.39$, $n=60$) and zone 2 ($r = 0.55$, $n=60$).’ “However, MLD and Chl-a in zone 1 and 2 are poorly correlated (correlation coefficient, $r = 0.28$)”.

15. The authors write “Mean wind speed in zone 1 is highest during January (3 m s−1) and in zone 2 during December (> 3 m s−1) (Figure 5a”). Does fig. 5a collaborates this statement? Further on: “During November to December, low PAR (33-36 E m−2 day−1) prevailed in the study area, corresponding to low temperature and enhanced mixing, deepening the MLD. But according to fig. 5 in November –December MLD is still rather shallow, especially in November. [25 (page 11)]

Authors’ comment: Thanks for the critical observation. The sentence is rephrased now as ‘Wind speed fluctuates strongly for zones 1 and 2. In zone 1, maximum variability (0.5-3.0 m s^{-1}) is seen during November and December and for zone 2, wind is varies strongly throughout winter, with maximum wind speed (0.5-3.0 m s^{-1}) for December and January months.’ This sentence is corroborated by the data presented in Figure 5A.

Regarding the second suggestion, the sentence is changed to ‘Decreasing pattern in PAR (33-36 E m^{-2} day^{-1}) prevailed in the study area during November to December for both zones, which corresponds to a reducing trend in temperature and deepening MLD cycle.’

16. The fig. captions are poorly written: ”Time series of the monthly average concentration of wind speed and PAR (a1 and a2) SST and MLD” [5 (page 12) and 5 (page 13)]

Authors’ comment: The figure caption has been rephrased:
“Temporal variability of wind speed and PAR (a), SST and MLD (b) and surface Chl-a (c) averaged for zone 1 (left, denoted by suffix 1) and zone 2 (right, denoted by suffix 2) during the winter period for the years 2002–2013. Pink colour is used to represent Chl-a, SST and wind speed and blue to represent MLD and PAR. Thick lines represents mean and the shaded areas the standard deviation for each parameter. The time series for the individual years are shown using thin lines. Vertical dotted lines represent the timing (month) of peak algae blooms in each zone.”

17. Please, comment on your finding that PAR and Chl for zone 5 are not correlated at all, and for zone 6 they are inversely correlated. Also, some interpretational comments are required for the phrase “For zone 5, wind and Chl-a production are weakly correlated ($r =0.30$, $n=60$), while in zone 6, these parameters are not correlated ($r = -0.09$, $n=60$)" [5 (page 16)]

Authors’ comment: MLD in Zone 5 are ~30 - 40 m shallower than in zone 6 and hence strong winds for the entire month will have triggered mixing, supplying more nutrients than by convective mixing alone to the mixed layer enhancing Chl-a production. In zone 6, wind fluctuates strongly compared to zone 5. Zone 6 is classified as INDW in Longhurst’s classification, where wind induced blooms are observed. However, the time scale of wind induced bloom, will be of the order of days / weeks and not months and hence on monthly scales, the wind's influence will not be resolved.

18. why the regression equations do not include such variables as MLD, concentration of nitrates nitrates and iron. It would be much better to do so instead of discussing the relations between Chl and the above variables apart from the variables reflected in Table 1. [Table1]

Authors' comment: Linear regression as well as the multiple regression analysis is done utilizing monthly data. Whereas, the nitrates and iron data is available only as monthly climatology. Therefore, regression analysis on monthly scale cannot include nitrate or iron concentration using the available data.

19. Caption for Fig. 8 lacks the designations of colours. [Page 17]

Authors' comment: Sentence provided in quotes will be included in the Figure caption. "Zones 1 to 6 are represented by violet, blue, green, light green, yellow and red lines respectively."

20. Please, give (at least in the Appendix section) the rose of winds in winter in order to let the reader better understand why in some parts of the sea DOT is higher than in the others. It would be good to give alongside it the field of DOT over the study area. [15 (page 13)]

Authors' comment:

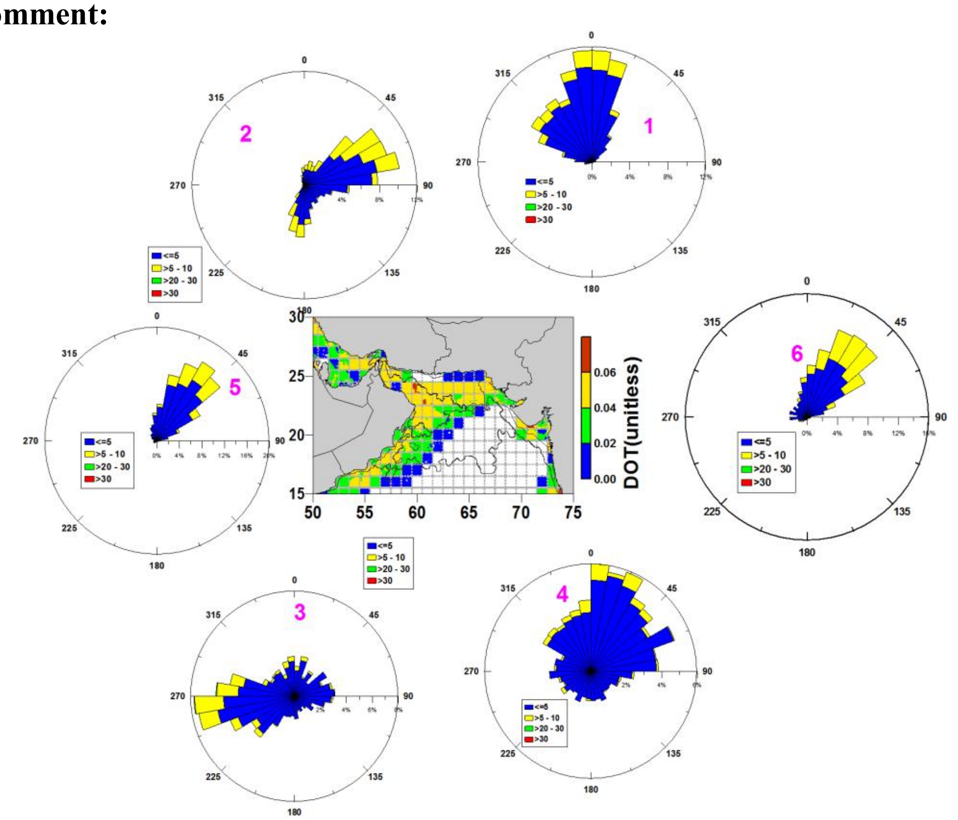

Figure R6. Wind rose diagram for the six zones. Zone number corresponding to wind rose plot is provided.

As suggested we have plotted the wind roses for the respective zones in order to reveal the possible source locations of DOT (Figure R6). For Zone 1, both the Thar desert and Arabian desert contribute to DOT, as the strong wind have directions between northerly to north-westerly. Similarly for zone 2, both these zones can be significant. While, for zone 3, its the Arabian desert contribution more to DOT enhancement as revealed from wind rose diagram. While for zone 4, it's the continental wind from Indian sub-continent available in the area. This is consistent with Patel et al. 2017.

21. As was commented above, the reported finding on the north-south gradient in Chl is stale and had been established without any complicated processing procedures. The same comment can be made with regard to the identified number of [10 (page 19)]

Authors' comment: The north-south gradient in Chl-a is visible in satellite images (individual and binned data, however identification of other PC's contributing to Chl-a distribution during winter cannot be done with subjective / visual analysis. An objective method is required to handle it and in this paper we have elaborated a method using the combination of principal component analysis and cluster analysis. The number of differentiated zones in the region is consistent with what is found in literature for other marine areas.

22. The reported finding that "The increased amount of Chl-a production in the open ocean zones are found to be directly related to sea surface temperature variability (ie. cooling) and the deepening of the mixed layer " is neither an unknown phenomenon for the study area. [5 (page 20)]

Authors' comment: The sentence has been modified: "In consistence with other studies, an increase in the concentration of Chl-a in the open ocean zones (zones 1, 2, 3 and 4) are found to be directly related to the variability of the sea surface temperature (ie. surface cooling) and the deepening of the mixed layer.

23. "The combined analysis of DOT and nitrate suggests that the variability of the algae blooms depend on both sources in these zones. The variability of Chl-a in the northern and northwestern parts of the Arabian Sea is correlated strongly with the atmospheric deposition of iron from January to March" The two statements appear kind contradictory. [15(page 20)]

Authors' comment: The initial sentence is in general for all zones, dependence of both parameters is observed in six zones. However, in the north and northwestern Chl-a production and DOT follows similar trend of variation and hence in these zones [zone 1, 2, 3 and 5] strongly is observed with the atmospheric deposition of iron from January to March.

Clarification of the sentence, according to comment; "The combined analysis of DOT and nitrate suggests that the variability of the algae concentration depend on both sources of nutrient supply in all six identified ecological zones. However, the variability of Chl-a in the northern and northwestern parts of the Arabian Sea (zones 1, 2, 3 and 5) is predominantly correlated with the atmospheric deposition of iron during the period from January to March."

24. It is difficult to agree with the authors' statements that "This study provides a more comprehensive understanding of the environmental factors controlling the spatio-temporal variability of the marine chlorophyll a concentration in the northern Arabian Sea during winter conditions", and further on "Additionally, this study reveals the need for better understanding of factors controlling the marine primary productivity in other coastal upwelling zones". Indeed, to justify/prove the validity of each zone the authors refer to the relevant publications of other workers who investigated in depth the factors and mechanisms controlling the spatiotemporal variability of the marine chlorophyll a concentration. Also, in many studies of the north Arabian Sea the need of further investigations, and more thorough sampling/in situ determinations of physico- and biogeochemical variables. [30 (page 20)]

Authors' comment: Distinct Chl-a characteristics for each of the identified ecological zones clearly indicates the spatial variation of Chl-a during winter is better brought out in this work (Figure R1). The temporal variability in Chl-a in the six delineated zones is the best way to study spatio-temporal variability. This comparison clearly indicated the significance of present classification.

Authors have correlated the surface cooling and mixed layer deepening with Chl-a production, in the delineated six ecological zones, which is required to explain Chl-a characteristics in these zones. Distinct physical and chemical characteristics within zones are identified. For the first five zones, it's the cooling followed by MLD deepening which enhances nutrient availability resulting in increased production, in zone 6 while MLD deepening / Chl-a production is followed by maximum cooling. In the analysis section, author has made use of the established knowledge on driving processes of Chl-a processes in the study area based on published information. Such comparison is well accepted part of research and development. Also, the complex influence of both nutrient and DOT between north, north-east as well as south and southeast part of the study area is brought out in this work. Though, similar study are carried out, it is been done for smaller area / shorter period, while present study has covered eleven year data with for the region where maximum winter bloom occurs in the Arabian Sea. We have observed the influence of DOT and nitrate in all zones. While, in the north and northeast production is strongly influenced by high DOT, than nitrate availability. Therefore, the authors disagree with this comment.The authors believe this study provides a more comprehensive understanding of the environmental factors controlling the spatio-temporal variability of the marine chlorophyll a concentration in the northern Arabian Sea during winter conditions.

Furthermore the in-situ coverage in the Arabian Sea is not great, hence we argue that the spatial and seasonal distribution of physical mechanism coupled with production in the Arabian Sea is fully known yet.

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