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. 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".

Specific comments

paragraph	comment(s)
1. Introduction	
5 (page 2)	Specify the desert(s);
15 (page 2)	It is insufficient to anticipate: this needs to be proven.
25(page 2)	Why the Chl concentration at 0.5 mg/m3 is used as a criterion?
30 (page 2)	Firstly, Mignot et al. reported solely on Pacific and Mediterranean oligotrophic waters (typically, Chl is significantly under 1 mg/m^3). 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.
2. Data	
15 (page 3)	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..

5 (page 4)	As a matter of fact: the coefficients taken from the literature are not necessarily relevant to the study area, e.g. f_{du}, and $AOT_{m\ a}$ (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 $AOT_{m\ a}$ 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).

20 (page 4)	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.
4. Objective delineation of ecosystem zones in the northern Arabian Sea	
5(page 7)	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 and 10 (page 8) 10 (page 8) 15 (page 8)	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.." Please, explain, on the basis of what it was decided that satellite-derived Chl values along coastal and shallow waters were erroneous. 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" …
5 and 10 (page 9)	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 (page 10)	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..
5.1 The ecological zones in the northern and most productive part of the Arabian Sea	
15 (page 11)	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?
25 (page 11)	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.

5 (page 12) and 5 (page 13)	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 16) Page 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)" Table1: 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. Caption for Fig. 8 lacks the designations of colours
7. Impact of nutrients and iron on Chl-a production based on the analysis of climatological nutrient and dust optical thickness	
15 (page 13)	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.
8. Summary	
10 (page 19)	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
5 (page 20) 15(page 20)	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. "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.
30 (page 20)	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 spatio-temporal 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.