

Interactive comment on “The Arabian Sea as a high-nutrient, low-chlorophyll region during the late Southwest Monsoon” by S. W. A. Naqvi et al.

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Received and published: 27 May 2010

We thank the referee for his/her very positive and constructive comments. Almost all of the suggestions made by him/her have been accepted as described in the following point-to-point response:

RC = Referee's Comments; AR = Authors' Response

RC # 1 - Some of the conclusions and organization could be improved, and I offer a few comments to help the authors with this. 1. The summary and abstract are not completely consistent. As stated above, there are two noteworthy points: long-term changes in chl are not significant, and Fe plays a central role in the C cycle of the region. Both should be stated clearly, and in the same order, in both the abstract and

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summary. I'd encourage the satellite chlorophyll analyses to be second in both (as well as in the results section).

AR – We have rewritten the Abstract as well as Conclusions parts following the referee's advice. The two main thrusts are now dealt with in the same order in the Abstract, Results and Conclusions sections. However, we prefer to keep the satellite analysis ahead of iron limitation simply because we can connect various sections better that way. However this is a minor point.

RC # 2 - In Figure 3, what is the red line? Black line?

AR – This is given in the figure (just above the lower panel). However, since the referee missed it (and others may similarly miss it too), we have also explained it in the figure caption now.

RC # 3 - On page 33, top, it is argued that stations experienced intense upwelling. By what measure?

AR – Low temperature (<23 C) and high nitrate content (>11 micomolar) of surface waters. It has now been clarified.

RC # 4 - Same page – comment about getting Fe samples off a CTD rosette system may not be justified, as an intercomparison showed little difference between the MLML and UH collections.

AR – This comment has been deleted.

RC # 4 (continued) - Regardless, concentrations of 1 nM certainly are enough for phytoplankton, in my experience. If you get values less than 0.3 you have a strong potential for Fe limitation. The data in Fig. 6 should be viewed as a potential for Fe limitation, given the biological variability of this ratio.

AR - We agree and have made suitable changes in the text.

RC # 4 (continued) - The authors also talk about the addition of aerosol Fe, but don't

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talk about the variability in dissolution (or kinetics) in this, which would influence the temporal pattern of Fe limitation.

AR – We agree. Unfortunately, there are no data on labile Fe availability in, or dissolution from, dust, but we have added new text addressing this issue.

RC # 5 - Another aspect that I found interesting is the spatial variability in Si/N ratios as induced by Fe. I think this may be a robust tracer of Fe limitation, but some discussion on the role of Si limitation might also be justified. The spatial variability of this ratio also has parallels in the upwelling region off NW Africa.

AR – We believe we already have sufficient discussion on this – the whole of page 39 of the BGD manuscript is devoted to the potential role of Si depletion on Arabian Sea biogeochemistry.

RC # 6 - Comments on atmospheric input of N are interesting but speculative, as it is unknown what form green Noctiluca uses.

AR – We agree. We have slightly rephrased the text, merely indicating this possibility along with the caveat mentioned by the referee.

Interactive comment on Biogeosciences Discuss., 7, 25, 2010.