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Biogeosciences Discussions

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.

Anonymous Referee #2

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General comments

This analysis presents new findings from the Arabian Sea that suggest the establishment of an HNLC regime in the northern upwelling region, within which iron limitation of photosynthesis is identified as the controlling factor. The authors provide an excellent interpretation of broader implications and impacts to the Arabian Sea ecosystem and biogeochemical processing. They also incorporate an insightful contrast of these processes, as they are affected by the Omani upwelling region, to the regime that is in force in continental shelf waters along the west coast of India. The other principal component of this analysis consists of examining whether the long-term trend in regional production reported elsewhere can be corroborated, and with both in situ and more complete remote sensing time series at their disposal, the authors argue strongly that the previously reported increase must be reconsidered.

In summary, this is an outstanding analysis that introduces significant new insights to the current understanding of biogeochemical processes in the Arabian Sea. There are a number of relatively minor points needing clarification and to some degree I feel that the authors have exceeded the reach of their analysis in some of their conclusions. So I have made suggestions for how these could be addressed and consider that with minor revision their manuscript would be suitable for publication.

Specific comments

Page 27, line 24. Suggest inclusion of a citation for the paper by Hitchcock et al. [2000], which is another key (and more contemporary) contribution to the literature on biochemical variability within the Somalia upwelling region.

Page 29, line 28. I question whether it is necessary to cite Gregg et al. here for the SeaWiFS data. I think defining the acronym is sufficient, especially as the web-source of the ocean color data is subsequently provided.

Page 31, line 4. Why are these time series categorized as "reconstructed" as opposed to "constructed"?

Page 31, lines 27-28. I think it would be worthwhile to reiterate (i.e., clearly spell out) that this concluding remark is based on the SST, as well as, the chlorophyll time series.

Page 32, line 3. The analysis by Prakash and Ramesh presents monthly SeaWiFS chlorophyll time series through 2005. So it is unclear to me why their results are noted as relevant to wintertime given that the Gregg paper against which it is being contrasted reports on annual primary production estimates.

Page 34, lines 22-27. The implication of these last sentences is that the Wiggert et al. (2006) model over-predicts the severity of the iron limitation in the waters upwelled off the Arabian Peninsula. However, the new observations that clearly indicate this to be so are not presented until the subsequent paragraph. So the possibility of dFe

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contributions from upwelling over the coastal shelf is not particularly relevant here since it is not a component of that model and, at this point in the narrative, quantitative evidence that the modeled degree of iron limitation is in question has not been given. As for the root of the model's shortcoming, there are several possibilities (e.g., iron requirement for growth, remineralization length scales or bioavailable component of aeolian iron) in addition to Ks. Indeed, in a follow-up analysis the model's sensitivity to which atmospheric deposition field was applied [Wiggert and Murtugudde, 2007] is a clear indicator of the general need for more comprehensive information with which to formulate iron biogeochemistry in marine ecosystem models. So I would question whether such a remark on model implementation of iron biogeochemistry outside of the broader formulation issues is worth making.

Page 36, lines 4-6. What mechanism with link to Bay of Bengal winds is being referred to here? I would conjecture it relates to coastal Rossby waves propagating around from the Bay into the eastern Arabian Sea that carry the Bay's freshwater signal. But the specifics should be given so the meaning is clear for the reader.

Page 36, lines 9-12. The introduction of significant dFe through the actions of a highly reducing environment is an intriguing mechanism. Can the authors offer any suggestion as to how persistent the resulting elevated dFe concentrations would be if oxygenation via either mixing or ventilation were to subsequently occur?

Page 40, Conclusion. The evidence presented in this report clearly suggests that the long-term trend in primary production (or rather phytoplankton biomass) in the Arabian Sea reported elsewhere (Goes et al. 2005) is not corroborated. However, the other two concluding statements that follow are enigmatic to me. Alterations in upwelling and dust delivery may indeed be decoupled; however, there is no evidence presented here that addresses whether/how upwelling in the Arabian Sea might be changing (if in fact it is). Thus I would content that the last two remarks appear to be extrapolating far beyond what this analysis can support and are venturing into intellectual speculation. If they were to be retained as part of this manuscript, these points would seem better suited for the discussion that precedes, with more explicit supporting arguments so that the meaning/connections are clear.

I am furthermore surprised that the spatio-temporal mosaic of limiting nutrient in the northern upwelling region is not highlighted through reiteration in these concluding remarks. The suggestion by their observations of iron limitation in the northern Arabian Sea despite the considerable aeolian dust fluxes is a highly significant result and challenges one of the canonical paradigms of biogeochemical cycling in the Arabian Sea (cf., "Mother nature's iron experiment", [Smith, 2001]). I would suggest to the authors that this contribution of their analysis is a seminal result that is worth emphasizing.

Figure 8. An inset that focuses in on the suboxic/anoxic portion of the profile would be useful to include. Given the overall range in O2 concentrations shown for the full profile, identifying low-O2 distinctions between the two sites is difficult.

Technical comments

None.

Suggested Literature

Hitchcock, G. L., E. L. Key, and J. Masters (2000), The fate of upwelled waters in the Great Whirl, August 1995, Deep-Sea Res. II, 47, 1605-1621.

Smith, S. L. (2001), Understanding the Arabian Sea: Reflections on the 1994-1996 Arabian Sea Expedition, Deep-Sea Res. II, 48, 1385-1402.

Wiggert, J. D., and R. G. Murtugudde (2007), The sensitivity of the Southwest Monsoon phytoplankton bloom to variations in aeolian iron deposition over the Arabian Sea, J. Geophys. Res., 112, doi:10.1029/2006JC003514.

END OF REVIEW

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