

## ***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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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 - 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.

AR – Accepted. In addition we have included one other reference from this region (Van Weering et al., Deep-Sea Res. II, 44, 1177-1193, 1997).

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RC # 2 - 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.

AC – Accepted. The citation has been deleted from this place.

RC # 3 - Page 31, line 4. Why are these time series categorized as “reconstructed” as opposed to “constructed”?

AR – Accepted. “Reconstructed” has been changed to “constructed”.

RC # 4 - 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.

AR – Accepted. This has been done.

RC # 5 - 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.

AR – We had committed a mistake and thank the referee for pointing it out. We have corrected it in the revision.

RC # 6 - 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 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

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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.

AR – Accepted. We have deleted the two sentences.

RC # 7 - 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.

AR – This is a little complicated. "Remote forcing" includes all forcings other than the local wind forcing of which Kelvin waves is one (see Shetye et al., 113, doi:10.1029/2008JC004874, 2008). We prefer to avoid details, but have slightly changed the sentence.

RC # 8 - 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?

AR – Accepted. We have added a couple of sentences to this effect.

RC # 9 - 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

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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.

AR – Accepted. We have rewritten the "Conclusions" as advised by the referee. The two statements about which the referee expressed his reservations in the opening paragraph as well as under specific points have been dropped, and the point about Fe limitation, missing in the first version, has now been included.

RC # 10 - 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 O<sub>2</sub> concentrations shown for the full profile, identifying low-O<sub>2</sub> distinctions between the two sites is difficult.

AR – Accepted. The figure now includes an inset – a map showing the station locations with reference to the suboxic zone.

RC # 11 - 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*

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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.

AR – We have included the first two citations, but not the third one in view of our aforementioned response to RC # 6.

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