The referee comments are italicized. Answers in regular typeface. Actions taken in red.

Interactive comment on "Physical control of the interannual variations of the winter chlorophyll bloom in the northern Arabian Sea" by M. G. Keerthi et al.
Anonymous Referee #1
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General Comments:
The Arabian Sea is one of the most productive regions of the world oceans which experiences phytoplankton blooms during boreal summer and winter. Both these blooms are reasonably well characterized and the physics controlling them are reasonably well studied. However, what is still largely least explored is the inter-annual variability of the summer as well as the winter blooms. The major reason why it has not yet been adequately addressed is the limitation in long time-series data. It is in this context that the present study assumes importance.

The present paper addresses the inter-annual variability of the winter chlorophyll bloom in the northern Arabian Sea (NAS) using both observation and simulations from a coupled biophysical ocean model. Towards the observational data, the authors used satellite-derived chlorophyll pigment concentrations, and Argo-derived mixed layer and thermocline depths. Using these, the authors aim at "better understanding the interannual variability of the NAS winter bloom" (lines 15).

The central theme of the study is the processes that leads to the phytoplankton bloom in NAS during winter and to ascertain whether the process suggested by Prasanna Kumar et al. (2001) or that by Wiggert et al. (2002) explains the bloom. The authors conclude that the winter blooms are strongly tied to mixed layer depth and the resulting modulation of nutrient entrainment into the mixed layer, a result similar to that of Prasanna Kumar et al. (2001). The inter-annual variability of NAS winter bloom amplitudes are controlled by the variation in the net heat flux at the air-sea interface, which in turn controls the MLD and nutrient entrainment.

It is a well-written manuscript and should be published, in my opinion, but only after consideration of some of the comments listed below.

We thank the reviewer for his positive comments and for his inputs on the paper. We provide a detailed answer to each of the comments below.

Major concerns: 1. What is the basis on which the authors use the months from January to April to define winter? This is not true in the case of the Arabian Sea and hence not acceptable. Let me explain. Based on the mean seasonal cycle of net heat flux both from observation as well as model presented in Figure 6 (c & f), the ocean looses heat from November until February. From March onwards the ocean starts gaining the heat and the net heat flux remains positive until October. Note that in April ocean gains heat as much as 100 w/m2 indicating the warming of the ocean rather than the prevalence of winter conditions. So *from the net heat flux point of view November to February defines the winter condition. Similarly, the mean seasonal cycle of surface chlorophyll from observation and model in figure 6 (a & d) shows an increase from November, peaks in February and returns to the November value by March. In view of the above, November until March could be considered as winter while dealing with chlorophyll response in the box under study.*

We agree with the reviewer that the definition we used for winter monsoon was not properly justified. As mentioned by the reviewer, Figure 6 clearly shows that the climatological heat flux is negative from November to February, which also corresponds to the period when the mixed layer depth (MLD) is deepest. The peak of the bloom is delayed by about one month relative to the deepest MLD due to the time it takes for the biomass to grow, and large chlorophyll values are still found in March. To better assess the seasonality of the amplitude of the winter bloom interannual variations, we calculated the monthly standard deviation of the interannual chlorophyll variations for each observed dataset. This new analysis is now included as Figure 3b in the revised manuscript (shown as Figure R1-1 below). This Figure clearly shows that the months of November and April correspond to minimum in the amplitude of interannual chlorophyll variability for most datasets. Based on this finding, we hence define the winter season as the period from December to March where interannual chlorophyll variations are larger. This is particularly true for February-March where interannual variations reach clear maximum amplitude. The revised figures and text now use this new season definition. This change of the seasonal window used to define the winter monsoon however does not change the overall conclusions of our study. This is illustrated on Figure R1-2 (similar to Figure 9 of the submitted version but for the new seasonal window considered).

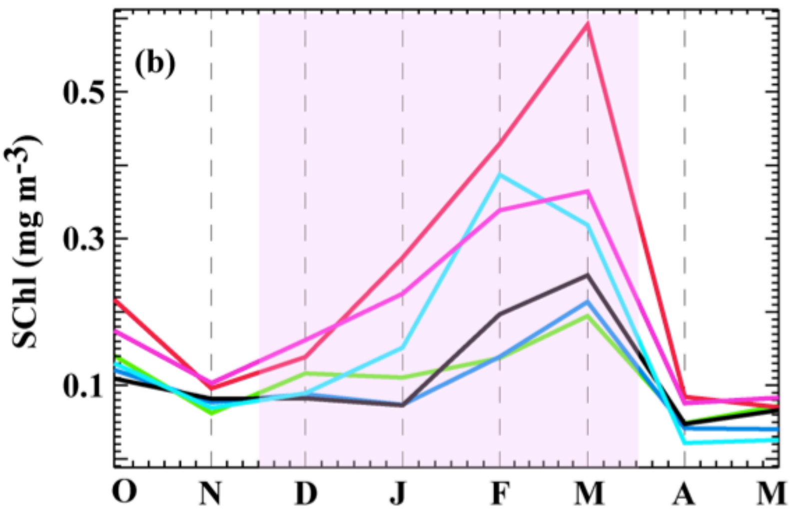

Figure R1-1: Monthly standard deviation of surface chlorophyll interannual anomalies for all satellite products.

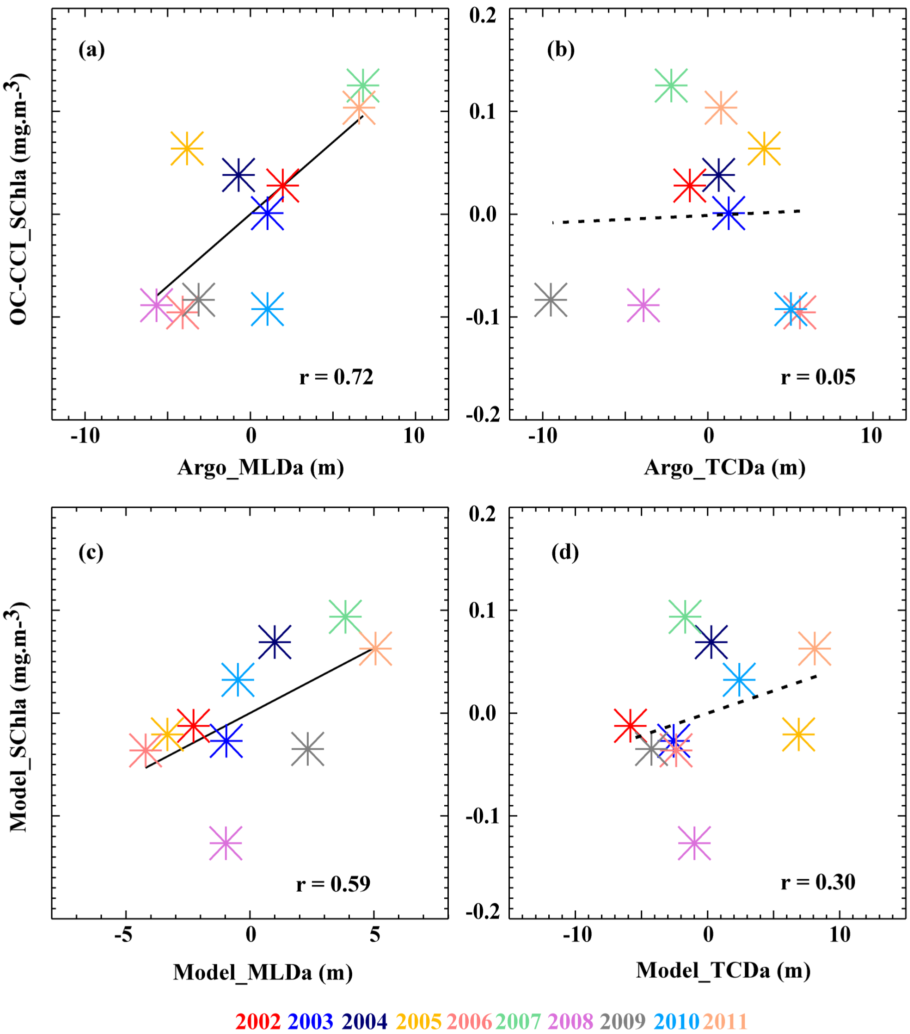

Figure R1-2: Scatterplot of winter (DJFM), NAS-averaged OC-CCI surface chlorophyll anomalies (OC-CCI_SChla) against observed (a) MLD anomalies (Argo_MLDa) and (b) thermocline depth anomalies (Argo_TCDa). (c-d) Idem for Model.

2. It has been shown in recent years that episodic dust storms that occur during winter are important in driving the interannual variability of chlorophyll in the Arabian Sea through the atmospheric input of nutrients, especially iron. See for example the studies of Wiggert and Murtugudde (2007), Patra et al (2007), Naqvi et al (2010) and Banerjee and Prasanna Kumar (2014). The authors need to at least address the role of dust-induced Fe input in driving the inter-annual variability of chlorophyll in their study region.

We agree that this should have been discussed in the manuscript. In the revised manuscript, in the model description section we first clearly acknowledge that the model forcing uses climatological iron inputs. We will also include a paragraph in the discussion section that acknowledges the potential influence of iron fertilisation by dust storms, quoting the papers listed by the reviewer (Section 5.2). In this paragraph, we further mention that, despite the fact that non-seasonal iron aerial deposition is not included in our model, it is able to accurately capture the observed interannual variability of the chlorophyll bloom in winter (see Figure 7a), suggesting that this mechanism is unlikely to play a dominant role in the interannual variability of the bloom. We will also mention modelling results of Aumont et al.

(2008) that suggested that the variability of surface chlorophyll induced by the interannual variability of aerosol iron is likely to be very small everywhere, especially relative to the impact of the ocean dynamics because largest fluctuations of surface iron produced by dust occur in oligotrophic regions where phytoplankton growth is not primarily controlled by iron availability. We will further suggest that the mismatch between model and observations for a couple of winters (like in 1997-1998) may be related to the absence of interannual variability of iron deposition, which is not included in the model. We also mention in the updated discussion section that the mismatch for some years between observed MLD and chlorophyll variations may be related to interannual variations of iron deposition during those specific years.

Related References:

Aumont, O., Bopp, L., Schulz, M.: "What does temporal variability in aeolian dust deposition contribute to sea-surface iron and chlorophyll distributions?" Geophysical Research Letters, 35, 7, 2008.

Minor concerns:

3. Page 7 Line 15 " The simulation captures the surface chlorophyll seasonal cycle in the NAS. . ...". While it is so during winter (even in winter note that the model SChl does not capture the increase from November to January seen in the observation), the model completely misses the declining SChl trend from July to August. Instead model depicts the continuation of monotonic increase from May to August. Authors need to point this out while discussing the simulation results.

We point this out when describing Figure 6 in the revised manuscript.

4. Page 8 lines 7-9 "The figure illustrates that the observed interannual SChl. . ..". The authors need to explain in the text/discussion what the inverse relationship between SChla and MLDa during 2003 & 2005 means.

Thank you for pointing this out. 2003 and 2005 actually behave inconsistently relative to other years in both the model and observations (Figures 7ab and 8a), suggesting that another mechanism the one that we describe could be at work during these years. We now point it briefly here but expand the discussion of other possible mechanisms that could contribute to chlorophyll interannual anomalies in the discussion section, including the potential influence of anomalous iron deposition, as discussed above.

5. Page 9 lines 2-3 "As a result, the mixed layer nitrate. . ..."Though the MLD nitrate concentration during 2008 Feb is double than that of 2007, the Chlorophyll did not show a commensurate increase. The authors need to explain this in the discussion in the of Redfield ratio and carbon fixation.

There is no reason for the nitrate variations to be proportional to the chlorophyll variations as biogeochemical models are highly non-linear. In the model, the relation between the phytoplankton and nutrient growth rate is only linear for weak concentrations. In addition, even if the growth rate is increased by a factor two, there is no reason for the chlorophyll concentration to be doubled. It depends on plenty of other factors, including light availability and grazing rate.