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Interactive Comment

Interactive comment on "Challenges in modelling spatiotemporally varying phytoplankton blooms in the Northwestern Arabian Sea and Gulf of Oman" *by* S. Sedigh Marvasti et al.

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I would like to express our gratitude to Referee #2 for reviewing this manuscript. The comments will result in increasing the quality of the manuscript. I have responded to all comments and either provide a more detailed explanation or have changed the manuscript with regard to the comments.

A) General comments:

1) It lacks analysis about the origin of the asymmetry between the large September bloom and the small February bloom. I think a real fundamental insight about the controls on seasonal phytoplankton blooms could be gained from such an analysis.

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Response :

We agree that this is an interesting question. As the introduction already makes clear, the difference between the blooms in the observations is thought to lie in differences in nutrient supply (largely advective in the summer and through mixing in the winter) (Wang and Zhao, 2008; Al-Azri et al., 2010 and 2013; Kawamiya and Oschlies, 2003; Murtugudde et al., 2007; Shalapyonok et al., 2001; Veldhuis et al., 1997). We present evidence below that the models fail to reproduce the asymmetry through having too much mixing of nutrients in the wintertime. In this case the reason for the failure to reproduce both the eddy bloom interaction and the mean cycle would be the same.

2) Furthermore, the current analysis focuses too much on the possible roles of cyclonic and anti-cyclonic eddies, culminating in a long-winded speculation without a satisfactory resolution.

Response:

We have revised the manuscript to highlight the role of deep mixing in the winter in also producing the seasonal asymmetry in bloom conditions We also agree that our initial manuscript did not do enough to motivate looking at the eddy-cyclone interaction. In Fig. 2 of the response to Reviewer 1, (which has been incorporated into the revision) we show that eddies modulate the location of blooms during the wintertime. Given the interest within the region in understanding such blooms and their behavior in the future, we think it is essential to characterize this source of interannual variability.

B) Specific comments:

1) February bloom is larger than the September bloom in the CM2.6 (miniBLING) model. What could be the fundamental origin of the asymmetry between the large September bloom and the small February bloom and why do the models fail to reproduce it? Could it be due to problems with the representation of the mixed layer and nutricline? or could it be the results of ecological interactions that are not represented

by the models? None of the biogeochemical models used by Sedigh Marvasti et al. includes an explicit representation of zooplankton; TOPAZ has implicit grazing through quadratic phytoplankton mortality. Specifically, Goericke (2002) has argued that the main control on phytoplankton abundance in the Arabian Sea is in fact top-down, that is, through grazing by zooplankton.

Response :

Reviewer 1 made a similar point about zooplankton and we address it in two ways. First, our model does of course parameterize grazing, and does so based on a global synthesis of data. We will add the following paragraph Grazing is highly parameterized in our global models. This is because these models were designed to reproduce the effect of grazing on size structure and biomass across ecosystems rather than trying to explicitly simulate zooplankton and introducing sensitivity to poorly known parameters (such as handling efficiency or grazing half-saturation). Instead, the grazing formulation was fit to ∼40 field sites to produce a size structure that transitions realistically between being dominated by small phytoplankton at low growth and large phytoplankton in nutrient and light-replete conditions. The resulting parameterization produces biomass that is a function of growth rate. The model also reproduces the scaling in particle size seen across ecosystems by Kostadinov et al. Second, however, we note that the idea that grazing becomes weaker in deeper mixed layers (as proposed by Marra et al. 1995, and noted by the other reviewer) would actually produce a larger February bloom, as mixed layers are much deeper in February than in September. So it cannot be the case that ignoring this aspect of explicit zooplankton dynamics is causing the winter bloom in the model to be larger than the summer. Indeed both the CORE-TOPAZ and COUPLED-TOPAZ models (which also have parameterized zooplankton) produce much less asymmetry than does miniBLING.

However there may be other aspects of the biological model that are important. We have revised Fig. 7 in the paper, as shown in Fig. 1 below, to include results from the BLING model run in the coarser resolution ESM2M code. The differences between

BLING and miniBLING in this code are just due to having fixed iron in miniBLING. The light field in these ESM2M runs is computed from using TOPAZ-derived chlorophyll, so that all three models see identical physical conditions. Both BLING and miniBLING in ESM2M produce an asymmetry in chlorophyll between February and September that is similar to that produced in CM2.6 miniBLING. This asymmetry is not seen in TOPAZ. Analysis of what drives this asymmetry shows that it is not straightforward. All of the model runs show an asymmetry in the nutrient concentrations that is in the opposite direction as the observations, with higher nutrients in February than in September. However, in TOPAZ this does not produce an asymmetry in chlorophyll, while in BLING and miniBLING it does. There are two possible reasons for this: 1) The equilibrium assumption, which means that biomass in both BLING and miniBLING is not directly simulated. In TOPAZ, the growth of plankton during the spring is limited by the biomass of phytoplankton, whereas in the fall TOPAZ continues to have higher heterotrophic biomass (diagnosed from growth rates over previous months) that then grazes the plankton. In BLING and miniBLING, by contrast, the biomass responds instantaneously to changes in growth conditions. 2) Different handling of light limitation. In TOPAZ light limitation is calculated using the instantaneous local light, whereas in BLING it is calculated using the mixed layer average light. Preliminary results with a very coarse resolution model using BLING show that this reduces the summer-winter asymmetry slightly, but is not sufficient to make the February bloom smaller than the September bloom. It is likely that all three of these factors- too deep winter mixed layers, too little light limitation and instantaneous response to changes in growth conditions, are all responsible for the overly strong blooms in boreal winter in the Arabian Sea.

2) (Sub) mesoscale eddies can impact nutrient transports and phytoplankton growth in many different (often very subtle) ways (see for example Martin & Richards, 2001; Flierl & McGillicuddy, 2002; Omta et al., 2008). Every eddy is different and will interact differently with the biota. Therefore, it comes as no surprise that the authors do not reach a clear compelling conclusion regarding the role of the eddies, even though they spend many pages speculating. Again, my suggestion is to shift the focus away from

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the eddies to the general seasonal pattern which can provide much more fundamental insight in how the Arabian Sea ecosystem works.

Response:

When we examined the region of interest in the observations, we found that whether or not a bloom was found in the Gulf of Oman depended almost entirely on whether there was a cyclone or anticyclone there. Fig. 5 and 6 (in the paper) are the evidence of this in a statistical sense but we will add two additional figures for more clarity.

The first of these (Fig. 2, below) shows sea surface chlorophyll and height during the months of November. The relationship between blooms and SSHA is clear and striking. Note particularly the difference between 1998 and 2001, when the location of high and low chlorophyll regions relative to the Ras al Hadd is opposite, and this difference is captured by the SSHA.

Additionally shown in Fig. 3 (Also Fig.3 in the response to the referee #1), low-resolution models (Core and coupled Topaz) provide an almost uniform seasonal coefficient of variation (mean C.o.Vs are 0.1 and 0.09, respectively), while both data and eddy resolving CM2.6 models show higher interannual variability and seasonal changes (mean C.o.Vs are 0.14 and 0.2, respectively). Together with the Fig. 2 (Also Fig.2 in the response to the referee #1), this statistical analysis suggests that eddies are indeed necessary to explain the variability. We will make this clearer in the revision.

3) The Introduction is too long and shifts between too many topics; I suggest the authors take a good look at how to focus it more sharply .

Response :

We have rewritten the introduction to make this clearer, moving this paragraph to the start of the paper. The general structure of the introduction is now, 1. There is a lot of interest in red tides and harmful algal blooms in the Northeast Arabian Sea. 2. We want to understand whether anthropogenic climate change or variability could drive such changes in chlorophyll and productivity. (Goes et al.) 3. Large-scale climate models represent a key methodology for exploring such questions. . . 4. But only if they get the physics and biology that drive variability in nutrient supply in this region.

4) It is unclear how Figure 4 was made and what is meant by a "Qualitative eddy chlorophyll a correlation".

Response :

We will replace Figure 4 with Figure 2 (see below) showing chl and SSH fields during the winter in different years, illustrating how on a small scale (which is the scale that many biological oceanographers focus on) different locations will have a bloom or not depending on whether there is a cyclonic eddy there. As shown in Fig. 2 below (Also Fig.2 in the response to the referee #1), there is a very clear relationship during the month of November, particularly in years 1998, 2001, 2002, 2004 and 2005. By contrast, in February (Fig. 8 in the response to the referee #1) the relationships are less clear, with blooms seen in both cyclones and anticyclones.

C) Technical corrections:

p. 9660, l. 1: are simulated with of five -> are simulated with five

Response:

Thank you for this comment. The sentence will be changed in the revision to address the grammatical problem.

p. 9672, l. 3: nutrients to euophotic zone and -> nutrients to euphotic zone and

Response:

Thanks a lot for the catch. The typo will be fixed in the revised manuscript.

*Figure captions:

Figure 1. Monthly variation of organic matter in satellite data between 1998 and 2005

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and GFDL models (8 characteristic years) within 56–66E, 15–26N: (a) chlorophyll from GFDL models and GSM5 algorithm. (b) PO4 from the BLING and miniBLING simulations, NO3/16 from the TOPAZ simulations and observed PO4 from WOA09. (c) NO3 from the TOPAZ simulations and observed NO3 fromWOA09.

Figure 2: Chlorophyll-a in mg/m3 (colors) and sea surface height anomaly (SSHA, contours) in meter in Gulf of Oman in November. (Also Fig.2 in the response to the referee #1)

Figure 3. Average monthly coefficient of variation of Chlorophyll a in satellite data between 1998 and 2005 and GFDL models (eight characteristics years) within (56–66E, 15–26N) and within the south region (56–66E, 15–19N) for CM2.6 (miniBLING) (Also Fig.3 in the response to the referee #1)

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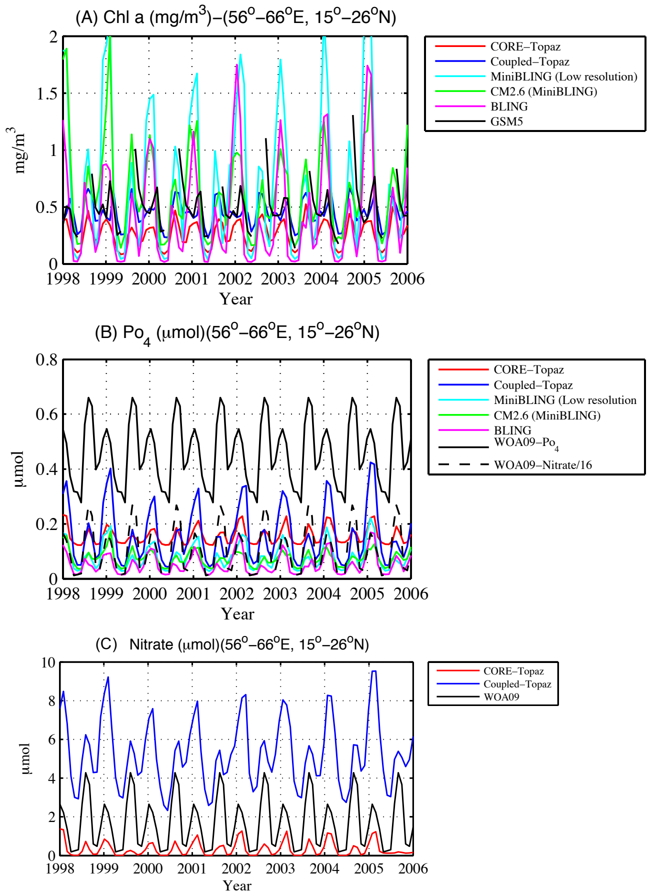

Fig. 1. Monthly variation of organic matter in satellite data between 1998 and 2005 and GFDL models (8 characteristic years) within 56–66E, 15–26N

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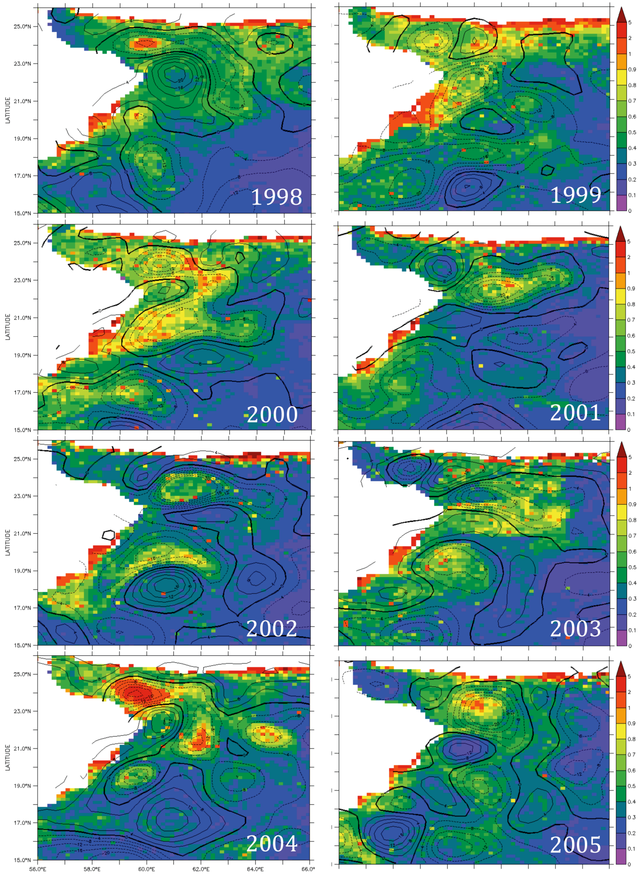

Fig. 2. Chlorophyll-a in mg/m3 (colors) and sea surface height anomaly (SSHA, contours) in meter in Gulf of Oman in November. (Also Fig.2 in the response to the referee #1)

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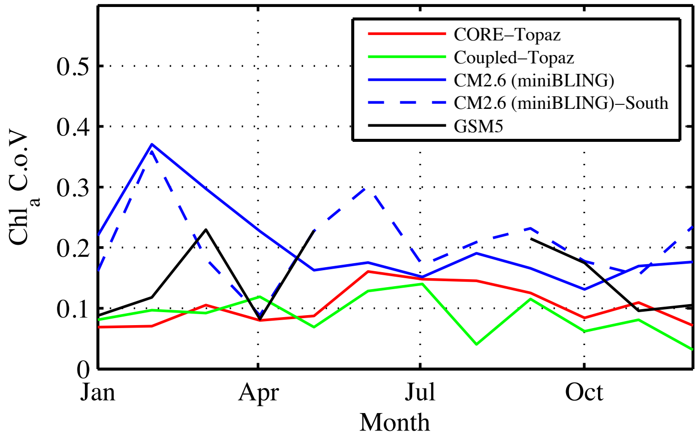

Fig. 3. Average monthly coefficient of variation of Chlorophyll a in satellite data between 1998 and 2005 and GFDL models (eight characteristics years) within (56–66E, 15–26N) and within the south region

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