DETAILED RESPONSE TO THE REVIEWERS' COMMEMTS

We would like to thank the editor and the two reviewers for their comments, which we believe have significantly improved the manuscript. We have made a number of changes in response.

1. The introduction has been significantly rewritten to de-emphasize the importance of HABs, although we feel it is important to include some discussion of this as it was a motivation for this study.

2. The analysis has been reordered and a number of figures have been replaced.

3. Discussion of Gaube and Resplandy has been added, though we feel that our paper is complementary to these studies for reasons outlined in the response.

4. Discussion of an additional set of model simulations at low-resolution has been added.

We hope these changes will meet with your approval, as well as that of the reviewers.

Sincerely

Safoura Seddigh Marvasti and *Anand Gnanadesikan*

Response to reviewer 1

I would like to express our appreciation to Referee #1 for the comments. I believe that the comments will improve clarity of the manuscript, especially as regards the global coupled models.

Below, I respond to every single one of the comments, providing more elaboration or changing the manuscript if applicable.

1) The analyses are very qualitative and in many instances, the authors jump to conclusions without sufficient support.

Response : We have revised the manuscript to clarify both the motivation for our study and provide quantification for the reasoning behind our conclusions.

2) A large part of what is shown regarding the data analysis has already been published elsewhere and these papers are not referenced here (Gaube et al., 2014),

The anti-correlation between satellite Chl_a and SSH was already reported by Gaube et al.

Gaube et al, 2014, JGR, looked at Regional variations in the influence of mesoscale eddies on near-surface chlorophyll and the cross-correlation between SLA and Chl at the global scale, including the Arabian Sea where they find a negative correlation

Response : Thank you very much for mentioning this reference. It was definitely our mistake not to see this paper. This paper will be highlighted in the literature review and references. However, we note that Gaube et al. show only the overall correlation while we examine the correlation between SSH and Chl as it varies over the course of the year in the Arabian Sea. Additionally, we show that this variation in the correlation also holds for backscatter and CDOM (which supports the idea that it is driven by nutrients and not by photo-adaptation).

Moreover, as discussed in our paper, based on the numerical results the diffusive flux of nutrient mirrors the chlorophyll. It also shows that for the few cases where the bloom is clearly associated with eddy passage diffusive flux is actually larger and positive at the center of the cyclone and negative in other parts of the region. Please see section 4.2.2 and Fig. 13. This mechanism is not one of the ones on which Gaube focuses.

We have added the following text to the manuscript on lines 106-113

Gaube et al. (2014) provide a global overview of how eddies influence chlorophyll blooms. They find that the effect of mesoscale eddies on the chlorophyll bloom varies both temporally and spatially. They identify four particular mechanisms that can be distinguished by linking sea surface anomalies to chlorophyll, namely eddy stirring, trapping, eddy intensification, and Ekman pumping. Although Gaube et al. (2014)find a negative correlation between chlorophyll and SSH in the Arabian Sea, they do not analyse which of these mechanisms is involved in this region, nor do they quantify the extent to which this correlation varies over the course of the season.

We have added the following paragraph to lines 551-562

The bloom associated with eddies E1 and E2 do not fit with any of the mechanisms highlighted in Gaube et al. (2014). We first consider the mechanism of trapping. Eddy E1 is generated in the ocean interior, not as a result of coastal upwelling. As shown in Fig. 15, the nutrient supply rate ranges between 5 and 8 mmol/m^2/month in the eddy. The concentrations in this eddy are only 0.01 mM (5 mmol/m^2) over the top 50 m. It cannot be the case that the nutrients in the eddy can last for several months as a result of "trapping", there must be a continuous supply. Moreover although eddy E2 shows a horizontal advection signal in November (with a positive ring around the edge in Fig. 12a), the signal in December has the opposite sign. Eddy intensification is also an unlikely mechanism for explaining the blooms, as dSSH/dt is relatively small (particularly if we track the minimum SSH associated with E1 in Fig. 12c or E2 in Fig. 12d). Finally, Ekman pumping signatures in Gaube et al. (2014) have the opposite sign as what is seen in E1 and E2.

We also contrast the results here with Resplandy et al. The focus in Resplandy is on the productivity driven by horizontal and vertical advection in summer and mostly vertical advection in winter. This would appear to us to contradict our finding of a primary diffusive source of nutrient in winter although it is consistent the finding of advective source of nutrients in summer. We have added the following paragraph to the introduction (Lines 114-120).

Resplandy et al. (2011) indicted that the spatial variability associated with mesoscale eddies in the Arabian Sea produces spatial variability in the bloom and that another source of variability is found to be restratification at these structures. Advection from coastal region is identified as the mechanism providing nutrients in summer, while vertical velocities associated with mesoscale structure are found to increase the overall nutrient supply. However, this work does not make clear how the spatial distribution of the eddy supply is related to the eddies.

Additional discussion has been added to the conclusions as well (lines 630-637).

It is worth noting that regional models, (such as Resplandy et al. (2011)) do have the potential to better simulate the hydrography of the Northern Arabian Sea. Because such models are very tightly constrained through "sponges" that restore hydrography at the boundaries, they may not have the problems that global models do at representing the effects of overflows that they do not properly simulate. However, such models cannot by themselves simulate the effects of changing climate, which in turn changes the boundary conditions. For this reason, global models must still be used for projection, making it important to identify the reasons that they are not going to work.

3a) Regarding why the model fails are reproducing the observations is not particularly interesting and not convincing either. In particular, I was not at all convinced that interannual variability of the bloom was driven by eddy activity.

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. The reviewer considers Fig. 5 and 6 convincing evidence of this in a statistical sense. We have added an additional figure (now Fig. 4) that clearly shows that whether a bloom is seen in the vicinity of Ras el Hadd during the winter is completely determined by the location of eddies.

Additionally we have added a new Fig. 10 showing that in the low-resolution models (Core and coupled Topaz) the interannual coefficient of variation is far too low relative to the satellite. This further motivates understanding the high-resolution models, but only if they can also simulate the eddy-bloom relationships.

- The conclusions regarding the inability of the model to reproduce interannual variability are not convincing and do not bring sufficient insight

- Fig 7 does show interannual variability in the data and also in all models - but the bloom amplitudes are so different that it is difficult to conclude anything on the ability of the model to reproduce the interannual variations since they do a bad job at reproducing the seasonal variations already.

Response : There is increasing interest in using global models to project ecological changes and to understand interannual variability, and it is so important to identify regions where the models are not going to work and the reasons why they might break down. Accordingly, as indicated in the title of the current paper, we are trying to highlight the challenges in modeling phytoplankton blooms using cutting-edge earth system models in the northwestern Arabian Sea and Gulf of Oman. That these models are less sophisticated than regional models (such as Resplandy et al.) is to be expected as they have not been tuned for this specific region. This point is now made in the introduction (Lines 56-67) as well in the concluding paragraph.

In order to get a better sense of whether the models break down because of physics or biogeochemical formulation, we now present a more detailed comparison across a wider range of models, including a low-resolution model with both the BLING and miniBLING formulations, following Galbraith et al. (2015) (manuscript revised for JAMES). We find that some of the bias in the winter bloom occurs simply from transitioning from TOPAZ to BLING. This is now discussed on lines 400-427.

3b) Another important contribution comes from the variability of the mixed-layer depth, which is not addressed here (Keerthi et al., 2015,Climate Dynamics).- It is not because the main source is vertical mixing that vertical mixing is too strong during winter. The question is how the mixed-layer depth compared with observations.

Response : This is a good point and one which we have added to the revision. Comparison of the high resolution model to the measured values in WOA09 and the Argo mixed layer dataset, verifies that in the winter time the modeled mixed layer depth is considerably deeper than that of summer. As is now shown in Fig. 1d, the wintertime mixed layer depth in our region is around 65m. However, when the same mixed layer criterion is used in the model, values of 130-150m are frequently found. This excessively deep mixed layer is likely related to the failure of the model to reproduce the sharp nutricline in the Northern Arabian Sea.

(Note: The figure in the published discussion was found to contain an error in that both the modeled and observed mixed layers were too shallow.)

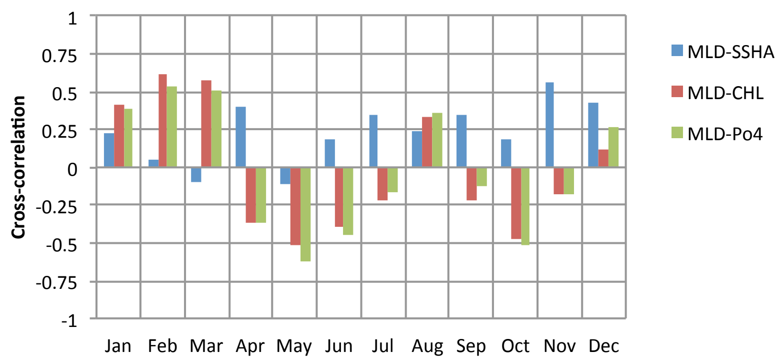

Figure R1. Mixed layer depth (MLD) cross-correlation with sea surface height anomaly (SSHA), surface chlorophyll a, and surface phosphate (Po_4) for year 197 of the CM2.6 (miniBLING) models (60–64E, 17–24N).

There are two possible ways for the eddies to modulate mixing of nutrient from below. The first is that they could modulate the near surface stratification, so that in warm anticyclones we would expect the mixed layer to be shallower. We would then expect a negative correlation between MLD and SSH, and a positive correlation between MLD and nutrients. The second is that the eddies could modulate the depth of the pycnocline, bringing nutrients closer to the surface and more accessible to mixing. It is not clear that this would necessarily produce strong correlations between mixed layer depth and SSH, because the pycnocline is closer to the surface in cyclones, but in anticyclones the density constrast between warm surface waters and the colder pycnocline is larger.

The model does not clearly show either of these possible mechanisms. Mixed layer depth shows a weak positive correlation with SSHA for almost all months during year 197 (Fig R1), consistent with the pycnocline being a bit shallower. However while the mixed layer depth cross-correlation with chlorophyll a and nutrients (PO_4) is positive in JFM, it is negative during SON.

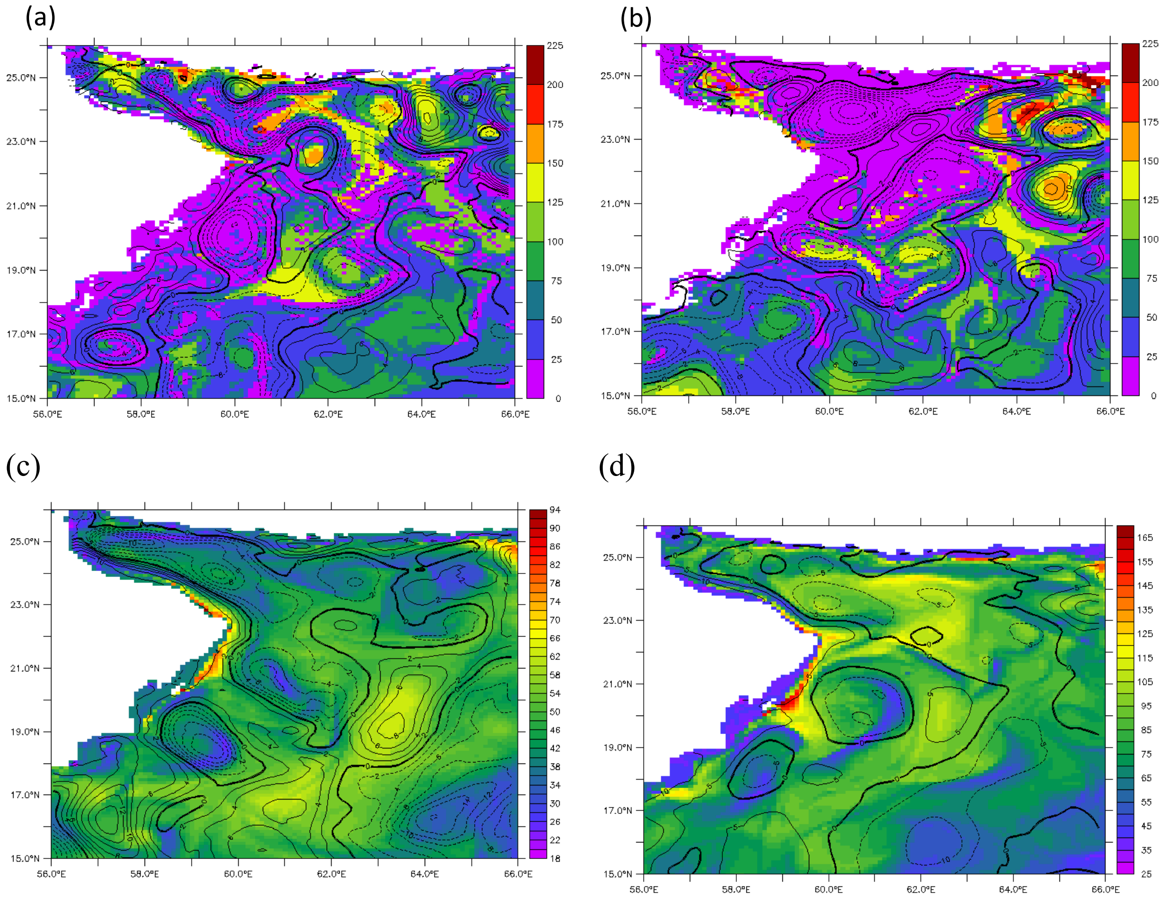

Figure R2: CM2.6 (miniBLING) mixed layer depth (MLD) and sea surface height anomaly (SSHA) in: (a) January, (b) February; and i (c) November, (d) December of year 197.

As shown in Fig. R2, comparison of mixed layer depth (MLD) and sea surface height anomaly (SSHA) during the winter shows very deep mixed layers, relative to the measured values from WOA09 (see Fig. 1 in the paper). However, in the northern regions the MLD seems to be too deep in winter. There is no consistent relationship between the mixed layer depth and the eddies, with deep mixed layers sometimes appearing in the centers of both cyclonic and anticyclonic eddies (see for example the upper right hand corner of Fig 6b). This observation again justifies the hypothesis of having too deep MLD in the northern part of the region to let mesoscale eddies modulate chlorophyll blooms. If we look during the time period where we have eddies E1 and E2 (Nov-Dec. year 197, Fig 6c,d) we see shallower mixed layers associated with both eddies.

3c) Moreover, the authors do not clearly show that eddies are supplying nutrients to the euphoric layer but this was shown with another eddy resolving model of the Arabian Sea by Resplandy et al (2011, JGR).

Response: The mechanism in Resplandy et al., (2011) is actually different than ours. The argue that the advective term dominates nutrient supply in the winter and that eddy driven advection a dominant term in that supply. However, the clearest signatures of vertical advection are found at the boundaries of eddies, not in the interior as appears to be seen in the observations. (As Resplandy et al., do not correlate their Chl with SSH it is not clear whether they also miss this phenomenon). This point will be made in the revision.

3d) It should also be noted that the NEM bloom is very likely driven by convective supply of nutrients - but possibly also by reduced grazing during convection (Marra et al. 1995).

Response: Our model does of course parameterize grazing, and does so based on a global synthesis of data (see discussion in new manuscript 250-262). However, in reading Marra et al. (DSR, 1998), which talks about the Arabian Sea, he actually argues that it is the dilution of production, rather than the reduction of grazing, that is responsible for the bloom. Instead we reference Behrenfeld (2010) on lines 286-288.

Additionally, our model will not capture cases where dilution of grazers (as proposed by Behrenfeld (2010)) acts differently from having lower grazer biomass as mixing increases and phytoplankton become more light-limited.

4) Regarding the analysis: - the scale at which the study is performed (a rather small box in the WAS) is not suited to address the question of interannual variability (Fig. 2).

Response: To examine the effect of region size on the interannual variability of the model, eight different regions with different sizes are used to calculate coefficient of variation of the chlorophyll results in the satellite data. The region bigger than the study region in this study (56^{o}–66^{o}E, 15^{o}–26^{o}N: thick line in Fig. R3) are plotted with solid lines and the regions smaller than the study region are dashed lines. Fig. R3 shows that for smaller regions (i.e. south, north, eddy E1, and E2), the interannual variability is considerably higher due to comparable size of the regions to the mesoscale structure. For bigger regions, however the interannual variations asymptote and the size effect odes not significantly change the results and diagnostics.

In addition, many studies have used regions of similar size to ours, in Resplandy et al. (2011) the region is (40^{o}–80^{o}E, 3^{o}–27^{o}N), in Al-Azri et al. (2010 and 2013): (53^{o}–63^{o}E, 18^{o}–28^{o}N), in Gomes et al. (2008): (55^{o}–75^{o}E, 5^{o}–28^{o}N), in Sarma et al. (2013): (50^{o}–65^{o}E, 15^{o}–30^{o}N), in Piontkovski et al. (2012): (50^{o}–63^{o}E, 15^{o}–27^{o}N).

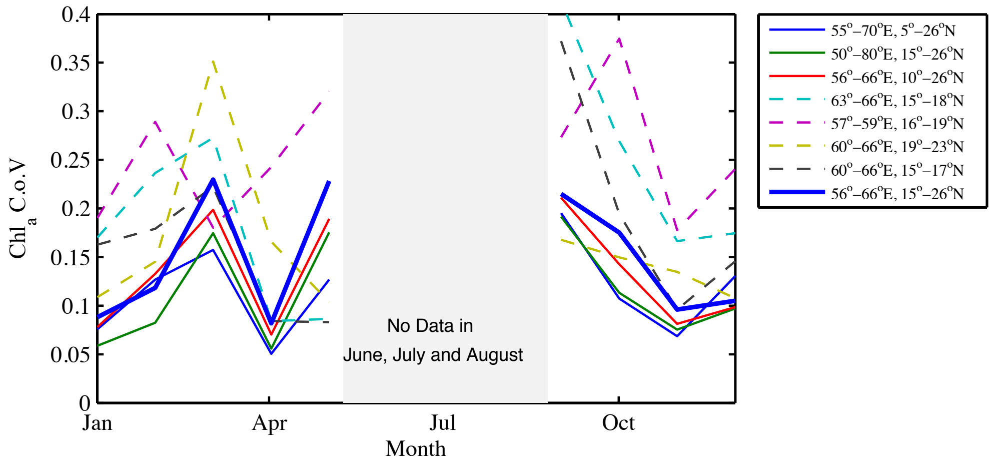

Figure R3. Average monthly coefficient of variation of Chlorophyll a in satellite data between 1998 and 2005 within different regions.

5) The correlations between Chl_a and SSHA are convincing (Fig 5 and 6) but not Fig 4

Response: We have replaced Fig. 4 with a figure 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 above, there is a very clear relationship during the month of November, particularly in years 1998, 2001, 2002, 2004 and 2005.

6) Fig. 8 is clearly not sufficient to explain what drives the bloom in the model.

Response: The two terms in Fig. 8 are the two terms in the model that affect growth rate. (See response to 7 below). Because biomass in the miniBLING model is a function of growth rate also, it is in fact sufficient to understand what drives the growth in the model. We have tried to make this clearer in this version of the paper.

7) What about grazing?

Response: The model utilizes an allometric The fact that such a model is so different than Resplandy et al. is another reason to evaluate it in the open literature.

8) Fig 9. over what level ? for what nutrient ?

Response : Thank you very much for this comment. Figure 9 is averaged flux values over the upper 50 meter calculated for phosphate (PO_4)

For more clarity the caption of Figure 9 is revised as follows:

Figure 9: PO_4 Advection, diffusion and tendency flux from the CM2.6 model over the whole region averaged over top 50 m (56°–66°E, 15°–26°N).

9) The authors seem to have missed recent literature that have examined their hypothesis in much more details than what they are doing:

- Gaube et al, 2014, JGR, looked at Regional variations in the influence of mesoscale eddies on near-surface chlorophyll and the cross-correlation between SLA and Chl at the global scale, including the Arabian Sea where they find a negative correlation

Resplandy et al. , 2011, JGR, Looked at the contribution of eddies to the nutrient budget in the AS using a 1/12◦ model are highlighted the important role of eddies in supplying nutrients to the euphoric layer during both the NEM and SWM blooms .

- Levy et al. , 2014, GRL, examined how mesoscale variability could affect the interan- nual variability of the bloom in the NA and their conclusion suggests that the variability is shared between internal (eddy) and external (atmosphere) forcings.

Response: Thank you again for these references. It was our mistake to miss these three papers, which do indeed bring up some of the key physics. We have revised the manuscript to include these references and to better address summer and winter blooms, mesoscale eddy activity and mechanisms.

Gaube et al (2014) has been discussed in item 2.

On Resplandy et al (2011) and Levy et al. (2014):

These papers are a relevant paper to our work. Both papers indicted that the spatial variability associated with mesoscale eddies produces spatial variability in the bloom and that another source of variability is found to be restratification at these structures. Advection from coastal region is identified as the mechanism providing nutrients in summer, while vertical velocities associated with mesoscale structure are increasing nutrient supply. However, we believe to some extent our paper contradicts this paper due to the following reasons:

First, Resplandy et al. (2011) do not really focus on structures at the eddy scale, they are more concerned with the net impact of eddies. One could easily read this paper as an observational oceanographer and miss the tight coupling we see in Fig. 5 above. We make this point in the revised manuscript in the introduction.

Second, because regional models are very tightly constrained, they do not have the problems that global models do. Regional models sponge the boundaries, hiding key physics not represented in global models. It should be noted that the global models are being used for projection, and it is so important to identify the reasons that models are not going to work. This point is also made in the revised manuscript in the conclusions.

Third, the papers argue that the advection of nutrients by eddies is most important. We point out that in our model, the only two eddies that actually look like what we see in the satellite observations involve enhanced mixing from below. This is a different result from Levy et al. and Resplandy et al. Moreover it is not clear whether these papers get the seasonal correlation with SSH or not. We note this in the revised manuscript.

10) The introduction discusses red tides with no relation with the content of the paper.

Response: Fair point. The red tides in the introduction are to emphasizing that this phenomenon is important in the Gulf of Oman and nearby regions such as Persian Gulf. We recognize that just chlorophyll is not equivalent to red tide, but the first step in understanding that is: Are the nutrients flux going to change? Where do we expect the nutrient flux to change and what are the bloom dynamics? A significant point of this paper is that we can't use the global climate models to answer this question. 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. 2005)

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.

11) The diagnostics are performed over a region, which is too small

Response: The region size effect on the results is studied in the response to comment 4. Actually, for management purposes the region is not too small. We just used the results of a global mode in a smaller region that includes Northwestern Arabian Sea and Gulf of Oman. Please see answer of the comment 4 for examples with relatively similar region sizes, and for our quantification of the stability of the coefficient of variation.

Response to reviewer 2

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.

Response: Although others have studied this topic, we have expanded our discussion as the reviewer suggested. We have added a fourth panel to Fig. 1 showing the mixed layer depth in the region, which peaks at about 65m in the data and discuss the fact that the mixed layers are much deeper in the model in the latter part of the paper. We have also added discussion of a coarse-resolution version of the model with the BLING and miniBLING biogeochemical codes, enabling us to get a better sense of what accounts for getting the asymmetry. The fact that we find it to be much worse in the BLING and miniBLING models at coarse resolution suggests that actually carrying a biomass tracer may be helpful- though problems with making light limitation comparable confound this analysis.

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 tried to respond to this criticism in multiple ways, by rewriting the introduction, by adding a new Figure 4 that better illustrates for us the striking association of eddies and blooms in certain parts of the year, and by revamping our discussion of the model figures. From a management point of view, the eddies are a big part of understanding who has to deal with wintertime phytoplankton blooms. We think this point needs to be made much more clearly.

A) ***Specific comments:***

2) 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 a 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.

The interesting thing here is that TOPAZ actually doesn't have as much of an asymmetry as BLING and miniBLING, suggesting that it is less likely that the problem is grazing per se, and more likely that the problem is either a lack of explicit biomass or too little light limitation in the model. We've added more discussion of how these models behave in the coarse resolution model, with a discussion on lines 400-427 of what might account for the difference.

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

Response: While we have tried to move somewhat in the direction suggested by the reviewer, we have not gone as far as suggested. 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 are the evidence of this in a statistical sense but we have added an additional figure for more clarity. Fig. 4 now shows sea surface chlorophyll and height during the months of November across the eight years. 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. Again, the seasonality of this relationship has not been well documented, nor has it been validated against anything more than chlorophyll. For regions of coastal management alone, we feel this result is worth emphasizing.

4) 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. 2005).

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.

We then discuss the potential role of monsoons and eddies. Hopefully this meets the reviewer's objections.

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

Response: We have replaced Fig. 4 and the discussion of qualitative correlation with a new Fig. 4 that we believe clearly illustrates the relationship between eddies and chlorophyll (and hopefully helps the reviewer to understand why we seek to retain the discussion of eddies in our paper).

A) Technical corrections:
p. 9660, l. 1: are simulated with of five -> are simulated with five

Thank you for this comment. The sentence is changed to address the grammatical problem.

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

Thanks a lot for the catch. The typo is fixed in the manuscript.

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