

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.

Anonymous Referee #2

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General comments

The manuscript "Challenges in modelling spatio-temporally varying phytoplankton blooms in the Northwestern Arabian Sea and Gulf of Oman" by Sedigh Marvasti et al. is a fundamentally solid model-data intercomparison. Having said that, I do find the manuscript somewhat unbalanced. Most importantly, 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. 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. Overall, I think that this

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manuscript is publishable in Biogeosciences after a major revision.

Specific comments

1) In my view, when analysing observations, you should always try to understand the most general patterns first, before focusing on more specific features. Figure 2 shows a clear general seasonal pattern that is consistent between the large and the small region and between different measures of phytoplankton biomass. Every year, there are two blooms: a large one around September and a small one around February. The models used by Sedigh Marvasti et al. reproduce a September bloom due to the Southwest monsoon and a February bloom due to the winter mixed-layer deepening. However, the two seasonal blooms have approximately the same amplitudes in the Core-TOPAZ and Coupled-TOPAZ models, whereas the 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 result 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.

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

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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.

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

Technical corrections

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

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

References

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