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Geoscientific Model Development Discussions

Interactive comment on "A High-resolution Biogeochemical Model (ROMS 3.4 + bio_Fennel) of the East Australian Current System" *by* Carlos Rocha et al.

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Summary:

I enjoyed reading this paper and it has the potential of adding to our understanding of BGC dynamics in the EAC separation region. My major criticism at this point relates to the interpretation of the results. I'd like to encourage the authors to consider adding additional interpretation and analysis on a number of fronts that are suggested below. Whilst the comments may appear critical, I think they would strengthen the study.

General Comments:

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The initial slope of the P-I curve and half-saturation coefficients have been tuned to recreate the observed Chl-a concentrations. In section 3.1 the model is assessed against observed Chl-a using modelled surface concentrations of Chl-a. The Remotely sensed Chl-a could be considered a some form of "depth weighted" averaged concentration over the optical depth (which can be quite deep in this region). Therefore by taking into account "difference in kind" error between modelled Chl-a and the CMEMS GlobColor Chl-a products, combined with the comparison of a modelled surface Chl-a being compared with a "depth weighted" averaged, there is scope for the "tuning" to be biased. Would it not be better to average the modelled Chl-a over an optical depth?

A majority of the results and discussion focus on Chl-a and Nitrate, yet there are 4 other non-observed state variables that influence the dynamics. What do these distributions look like? Are they sensible? Do they qualitatively behave as one would expect?

There is little discussion of the interaction of physical processes with BGC? For example, the vertical supply of nutrients into the photic zone. What is the typical flushing time of water in the mixed layer? In areas of the domain where the flushing time is short (through horizontal advection), the BGC dynamics will be dominated by the prescribed boundary conditions. Whereas in areas where the flushing time is comparatively long, BGC dynamics will be dominated by internal model processes. Such an analysis would help explain the discrepancies in PCA mode 1 as mentioned below.

The colorbars on many of the figures are such that it is really hard to look quantitatively at the results. It is really difficult to pick discernible differences in color between 0.2 and 0.5 mg Chla m-3. More attention needs to be given to the colormaps used to generate the figures. The addition of a shelf contour to the plots will allow the reader to discriminate the deep ocean, from the shelf and shelf-break.

As it stands the paper is descriptive of observed phenomena, but the power of a model is that it allows you to explore unobservable quantities. There is little if any discussion about the dynamics of the unobserved state variables nor derived quantities like primary production etc.

If the model is to be used to quantify and interpret the 3D time evolving state of the EAC, then the authors must assess the model in a way that presents evidence to the reader that the model is fit for purpose. Broad statistics are used to show that there is reasonably good correlation between the model and remotely sense observations, but in many cases a detailed interpretation of the results is not presented. Furthermore, the model has only been assessed against remote sensing and in-situ climatology (nitrate), there exists a rich set of BGC observations from gliders and research cruises for the area. I would strongly encourage the authors to undertake an assessment of the BGC model against in-situ data. Why not try a comparison of the model fields against Schaeffer et al., (2016). Whilst it is close to the shelf break, it may assist with providing an additional in-situ dataset for which to assess the model against.

Specific Comments

Page 1, Lines 24-30: There are varying complexities of BGC models ranging from highly parameterised through to extremely complex. The parameter identifiability problem associated with additional complexity is discussed in Friedrichs et al., (2007) with further suggestions on how to adequately represent uncertainty in Parslow et al., (2013). As for using Chl-a as a variable to assess model skill, Baird et al., (2016) show that observed OC3M Chl-a from satellites can at times be very different to simulated Chl-a from a model. This is confirmed in Jones et al., (2016). These “difference in kind errors” are important in the interpretation of the results later in the manuscript.

Page 2, Lines 14 - 21: References needed.

Page 3, Lines 16 - 25: Can you comment as to the suitability of this N2PZD2 model for this particular area? There are other choices available, both more complex and simpler. Is a single P group suitable for this region?

Page 6, Lines 1-8: You mention here that the model is initialised with Nitrate from

CARS. How are the other model variables initialised, especially those that are unobserved?

Page 8, Line 12: What is the likely cause for the bias? It appears that the model is overestimating the Chl-a by a factor of 2 for substantial periods of time. This relates to my question posed above given that you are comparing a surface value with an observed value calculated over an optical depth.

Page 9, Line 3: If you use a 200m depth contour on the plots, it will help denote the region you are discussing.

Page 9, Line 4: Is the model parameterised to simulate large or small phytoplankton?

Figure 7: Suggest adding column titles to denote modes 1-4. Top row - y-axis needs explaining in caption

Page 10, lines 21-26: This section is very light on the analysis and interpretation of the PCA analysis shown in Fig. 7. Whilst the correlation coefficient might be high, the are very obvious differences in the spatial structures of the model and obs. Interpretation is needed to explain these differences beyond just that relating to correlation. e.g. is the model over or under predicting the spring bloom, and in what areas? This may assist in determining why there are discrepancies in the northern section of the domain.

Page 11, lines 1-2: This transect lies so close to the eastern boundary of the model domain that there is a risk that what is being seen in Figure 8 is influenced by the climatological boundary conditions prescribed at the boundaries. What does a transect from the central domain look like?

Figure 9 would benefit from an additional row showing the difference between the model and CARS, such a plot would assist in the interpretation of subtle differences including showing the differences in the supply of nitrate to the surface waters which is important for primary production.

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