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Geoscientific
Model Development
Discussions

Interactive comment on "Evaluation of a near-global eddy-resolving ocean model" *by* P. R. Oke et al.

P. R. Oke et al.

peter.oke@csiro.au

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Response to reviewer's comments on " "Evaluation of a near-global eddy-resolving ocean model", by Oke et al. (gmd-2012-113)

Reviewer 1:

General comments: The manuscript describes setup and performance of a near-global eddy–resolving ocean model developed for analysing ocean dynamics and variability. This latest version of OFAM has been extended to all longitudes between 75S–75N and includes a simplified biogeochemistry module.

The manuscript is easy to read and understandable. Model development and results of model evaluation are presented in a comprehensive manner. But figures (fig. 11)

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and labelling of figures have to be improved (see specific comments). I have only a few comments and some questions which address primarily the newly included biogeochemistry module. I recommend this manuscript for publication if the following points will be clarified.

RESPONSE: We thank the reviewer for this thorough review. We have addressed the issues relating to figures and the figure labelling – see below.

Specific comments: My major questions are related to the set up of the biogeochemistry module.

1. To me it is not obvious, why biogeochemistry has been included in this modeling effort. The discussion on chlorophyll is very limited. Other tracer e.g. DIC, Alk or nutrients are not presented/discussed at all. Furthermore, Fig.11 should display model results on chlorophyll, but it shows satellite observations (identical to Fig.12). The modelled RMS of chlorophyll is quite different to the observed one, but the discussion on potential reasons is rather poor (p 4319, line 9-11)

RESPONSE: The development of OFAM is the first step in the development of a new operational ocean forecast system that will replace the current Australian short-range operational ocean forecast system (www.bom.gov.au/oceanography/forecasts/; OceanMAPS). We intend to include forecasts of plankton in the new forecast system, which is why we have included it here. Further, we plan to constrain/initialise the model phytoplankton with satellite-derived Chlorophyll a. This is one of the reasons we focus on Chlorophyll in this paper. But, we also note that Chlorophyll is the only variable that we can readily compare to observations on global scales (using satellite-derived ocean colour data).

RESPONSE: Our motivation, and plans for future developments are described more clearly in the revised paper.

RESPONSE: There was an error in the originally submitted Figure 11. It should have

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Interactive Discussion
Discussion Paper

shown the simulated field not a repeat of the remotely sensed observations. This figure shows the simulated fields have similar spatial structure with the remotely sensed observations. An example of the intended comparisons – and the comparisons in the revised paper is presented in Figure 1.

The model-data comparison for Chlorophyll a and the subsequent assessment is a complex task because the model's performance reflects both the physical realism of the simulation and the biological parameterisation. While a more through assessment of the BGC model is needed, it is beyond the scope of this first paper. This paper is intended to serve to present the model, which does now include a BGC component - but the more detailed analysis requires a second paper that will focus on the BGC behavior.

For the short integration time (18 years), the difference in the DIC, Alk and nutrient fields from the initial state (the observations) is small and does not make this a very useful indicator of the model behaviour. In contrast, the phytoplankton and zooplankton fields rapidly adjust to the physical circulation and initial nutrient fields in a time-scale of a few years, which makes this field more appropriate for comparing to observations.

2. The formulation of detritus remineralisation seems to be only temperature dependent. Given that oxygen is a prognostic tracer I would expect that oxygen limitation on remineralisation is included in a near-global model. There are large areas where remineralisation of organic material is restrained due to the lack of oxygen. Furthermore, eq. B12 directly relates changes in N to changes in O2, which consequently leads to negative oxygen concentrations if remineralisation is not limited by O2. This has to be discussed in greater detail.

RESPONSE: The remineralisation of detritus can have a temperature dependent decay rate in the ocean interior to reflect enhance bacterial activity in warmer water [e.g. Rivkin RB. Biogenic Carbon Cycling in the Upper Ocean: Effects of Microbial Respiration. Science 2001: 291 (5512): 2398–400], but under oxygen-replete conditions detrital remineralisation can also occur when oxygen levels are zero through the process of denitrification. The processes of denitrification would reflect a loss of nitrate when Detritus is remineralised without consuming oxygen or losing phosphate. For this reason the N (nutrient) in our BGC model represents Phosphate, rather than Nitrate. We agree that these points were not clear in our BGC model description and we have modified BGC model description to clearly state that Phosphate is our nutrient tracer, and that we allow denitrification to remineralise Detritus with no oxygen consumption, and that the oxygen levels are not allowed to go below zero.

3. The BGC module includes the formation of calcite. The dissolution of this mineral is primarily dependent on the saturation state of calcium carbonate in the water column. According to eq. B11 and Table 2 the authors apply a globally constant, temperature dependent dissolution rate and neglect the saturation state. Moreover, this dissolution rate increases with increasing temperature which is completely wrong for calcite. This mineral dissolves faster in cold water. For what reason is calcite production considered in this context?

RESPONSE: The reviewer is correct that our parametrisation of calcium carbonate remineralisation as a function of temperature is not justifiable, and we will change it to remove the temperature dependency on remineralisation. However, we note that for the results presented here alkalinity and DIC do not impact the simulated nutrient and plankton evolution. Therefore, the figures presented are not altered by a change in calcium carbonate remineralisation. Further, for the short integrations presented here (18 years) the impact of remineralisation has little impact on the interior distribution of alkalinity and DIC (less than 10 mmol Eq/mˆ3 Alkalinity). As stated early, the motivation to include calcium carbonate production/remineralisation in this model was to enable a more complete simulation of pCO2 in the upper ocean and in an assimilation mode, we envisage the simulated pCO2 will become a key independent field to assess the BGC simulation.

4. Subsection 3.5: It is not an appropriate tool for model evaluation to compare ob-

served and simulated NINO indices when the model is relaxed to monthly-averaged Reynolds SST (Reynolds et al., 2007) with a restoring time-scale of 10 days. I would expect nothing less than an excellent agreement for the correlation. Therefore, I recommend removing this whole subsection from the manuscript.

RESPONSE: We regard the comparison with NINO indices as a necessary step in evaluating the model. We agree that “excellent agreement” is expected here. However, we do not expect all readers to accept that this expectation is necessarily met without showing it explicitly. For this reason we have retained this comparison in the revised paper.

We think there may be some confusion about the “strength” of the heat flux restoring. We have added some discussion of this to the revised paper that should clear this up. The new text follows:

“The surface restoring term for temperature scales like rho cp Δz_surf /ΔtâĹij 23 W m−2 K−1 (rho cp âĹij4×106 J K m-3; Δz_surf =5 m is vertical grid spacing at the surface; and Δt = 10×86400 s, is the restoring time-scale). Each time-step the impact of this restoring term is quickly spread over the surface mixed layer, “diluting” the impact of the restoring term by the ratio of Δz_surf /MLD (where MLD is the mixed layer depth). So, if the MLD is 50 m (10 times Δz_surf) then the effective restoring time-scale is ten times greater than the prescribed time-scale.” Further, to quantify the degree to which the surface restoring term influences the model SST, in the revised paper we present the mean plus/minus standard deviation of the area-averaged heat flux components (Table 1, below), including the restoring term, for the entire model domain and for each NINO region. This analysis demonstrates that the surface restoring term for temperature is small compared to most components of the heat flux.

Technical comments:

- Introduce all abbreviations: ACC, EAC, BMC, WBC

RESPONSE: Done

- page 4311, line 26, wrong fig. number (4c)

RESPONSE: Done

- page 4312 , line 11 and 14, fig. numbers wrong

RESPONSE: Done

- page 4314, line 7, typo INSTANT

RESPONSE: Done

- page 4315, subsection 3.7.1. should be restricted to data description. All information of model-data comparison should be given in the following subsections

RESPONSE: Section 3.7 has been rearranged as the reviewer suggests. Now, the descriptions of each observational data set are in the subsections that describe their comparison with the model.

- page 4316 line 1 ff, explain in greater detail which data have been excluded from the AMSR-E SST data set (what is e.g. near coast?)

RESPONSE: We simply excluded observations if they are over depths shallower than 50 m. This is now stated explicitly in the revised paper.

- page 4320 line 2 SSTA instead of SST

RESPONSE: Done

- page 4322 line 13 typo in relationship.s

RESPONSE: Done

- page 4325 line 12 give reference for SeaWIFSKd-490 and explain the concept briefly

RESPONSE: A reference has been added to Lee et al. (2005; JGR), along with a short explanation.

- page 4330 line 13 why is it here explicitly mentioned that nutrients are controlled by upwelling? No term in eq. B4 is referring to physical ocean dynamics, but, of course, ocean physics affect all tracer fields.

RESPONSE: This point was motivated by the fact that the nutrients in the BGC model respond rapidly to the circulation (unlike some BGC variables, like Alkalinity and DIC).

- page 4330 line 15 "Redfield" ratio for O2 is commonly given as negative number, see eq. B12

RESPONSE: Done

- page 4330 line 25 replace “particulate organic carbon” by “detritus” according to eq. B3

RESPONSE: Done

- page 4345 correct figure label

RESPONSE: done

- page 4349 to 4351 replace top/bottom with a/b

RESPONSE: done

- page 4352 wrong figure is displayed

RESPONSE: the correct figure has been added.

- page 4353 label and color bars are difficult to see - please improve figure 11 and 12

RESPONSE: the font sizes for the labels in Figure 11 and 12 (now Figures 15 and 16) have been increased.

Interactive comment on Geosci. Model Dev. Discuss., 5, 4305, 2012.

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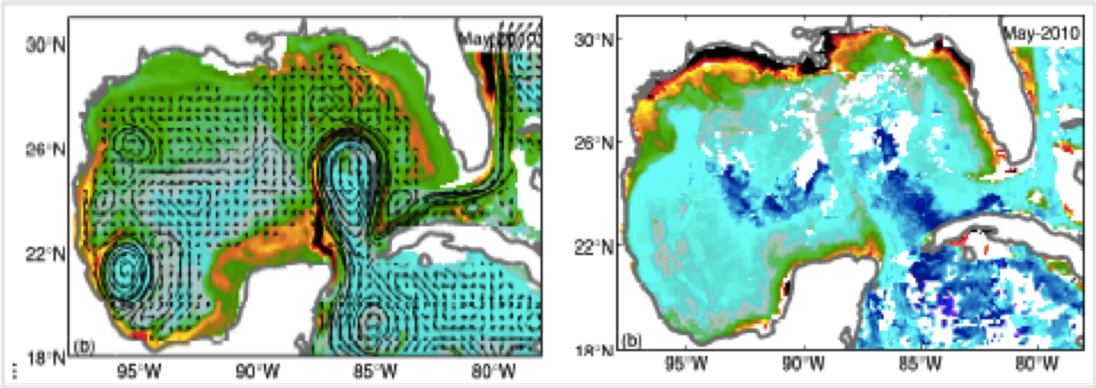

Fig. 1. Example comparison of the modelld phytoplankton (left) and the observed Chlorophyll a (right) for the Gulf of Mexico. These panels are Figure 16b and 17b in the revised paper.

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Interactive Discussion

Discussion Paper

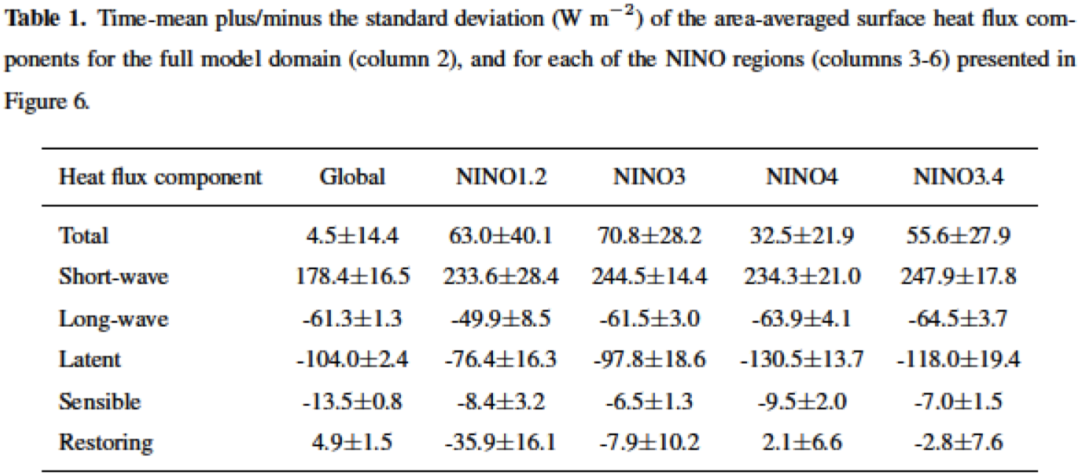

Table 1. Time-mean plus/minus the standard deviation (W m^{-2}) of the area-averaged surface heat flux components for the full model domain (column 2), and for each of the NINO regions (columns 3-6) presented in Figure 6.

Heat flux component	Global	NINO1.2	NINO3	NINO4	NINO3.4
Total	4.5±14.4	63.0±40.1	70.8±28.2	32.5±21.9	55.6±27.9
Short-wave	178.4±16.5	233.6±28.4	244.5±14.4	234.3±21.0	247.9±17.8
Long-wave	-61.3±1.3	-49.9±8.5	-61.5±3.0	-63.9±4.1	-64.5±3.7
Latent	-104.0±2.4	-76.4±16.3	-97.8±18.6	-130.5±13.7	-118.0±19.4
Sensible	-13.5±0.8	-8.4±3.2	-6.5±1.3	-9.5±2.0	-7.0±1.5
Restoring	4.9±1.5	-35.9±16.1	-7.9±10.2	2.1±6.6	-2.8±7.6

Fig. 2.