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Interactive comment on "Evaluation of an operational ocean model configuration at $1/12$ spatial resolution for the Indonesian seas – Part 2: Biogeochemistry" *by* E. Gutknecht et al.

Anonymous Referee #1

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The manuscript 'Evaluation of an operational ocean model configuration at 1/12° spatial resolution for the Indonesian seas – Part 2: Biogeochemistry' by Gutknecht et al. presents the marine biogeochemistry component of an high-resolution operational ocean model over the Indonesian seas. The authors conduct a thorough skill assessment of simulated biogeochemical fields from nutrients to the mesozooplankton against available satellite-derived product, climatologies and field measurements. The manuscript is overall well-written and demonstrates the good accuracy of this model configuration to replicate regional distribution of observed biogeochemical tracers. Nevertheless, I think this paper could be structured more efficiently and needs some clarification that have to be addressed first, and which prevent me of accepting

this paper in its present form. Therefore, I recommend acceptance of this manuscript after some minor revisions.

Major comments:

(1) While I acknowledge that several dataset employed in this study precludes to investigate interannual variability of some biogeochemical tracers, some of them can be used still (e.g., satellite-derived observations). Regarding the target of this operational configuration (up to monitor fisheries), assessing the interannual variability of the low trophic levels seems to be relevant.

(2) It is unclear to me how the authors can evaluate the water masses transformation without showing any formation-transformation diagnostics. This is maybe a direct outcomes of the companion paper. If not, I recommend to use the terms 'water mass hydrodynamics characteristics' or equivalent which are presently shown and evaluated.

(3) The authors point the influence of external inputs but they do not assess how accurate are the product employed to force PISCES. While I acknowledge that river inputs are difficult to assess, atmospheric inputs might be evaluate against air-borne measurements or available model outputs.

Specific comments:

P 6670

L1 'D'evelopment

L10 coupled and 'on-line' are redonant.

L10 degradation is mentioned several times in the ms. I recommend the authors to explain a bit these terms if they are useful for the study or to remove them if they do not.

L14 please reword this sentence because the papers focuses on the last 4 years of the simulation.

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L25 ‘water mass hydrodynamics and biogeochemical properties’ ?

L26 vertical distribution of what ?

P6671

L25 change ‘but propagate’ into ‘but also propagate’

P6672 L7 Change ‘capacity’ into ‘capability’

L9 Madec et al. 2008 shall be preferred regarding the NEMO model version

P6674

L26 please confirms that NEMO3.2 is used.

P6675

L17 Do you refer to the standard PISCES set of parameters ?

P6676

L6 please expand the TOP acronym and explain its function. Is this another module coupled to PISCES ?

I think that merging the PISCES description with the 3.1 section could make the model setup clearer.

Few words on the Redfieldian assumption of PISCES provided in the ms can be also detailed here. Please check the references of external inputs ? (It seems to me that atmospheric dust deposition were derived from Tegen and Fung, (1995))

P6677

Section 4: I suggest to re-order subsection with (1) INDOXMIX measurements which presents hydrodynamics properties, (2) nutrients and finally (3) ecosystems parameters.

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L14 please indicate the exact period of the simulation

L20 please provide the evolution of tracers' concentrations at various depth because 3D average generally masks drift. Please give few metrics to quantify the steady state of the tracers' concentrations.

P6679

Section 4.2 : please refer to Henson et al. (2010) which used several satellite-derived algorithm to compute phytoplankton production. This paper is of interest for this study since it discuss the uncertainty of the CbPM algorithm compared to the others.

L14 please give a reference for MAREDAT.

P6680

L4 Please explain. In PISCES, phytoplankton growth is limited by the various nutrients concentration (e.g., Laufkötter et al., 2015). Therefore, nitrate and phosphate should be decoupled in the model.

Section 5: Here also, I suggest to re-order the subsection by presenting (1) annual mean state nutrients and ecosystem parameters and (2) seasonal to interannual variability of some biogeochemical fields (if Chl and NPP have reached a steady steate before 2010). According to Gierach et al. (2012), the 2009-2010 ENSO event has implications on the biological fields.

P6686

L17 please detail what water mass transformation means.

References: Gierach, M. M., Lee, T., Turk, D. and McPhaden, M. J.: Biological response to the 1997-98 and 2009-10 El Niño events in the equatorial Pacific Ocean, Geophys. Res. Lett., 39(10), L10602, doi:10.1029/2012GL051103, 2012.

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Tegen, I. and Fung, I.: Contribution to the Atmospheric Mineral Aerosol Load From Land-Surface Modification, J Geophys Res-Atmos, 100, 18707–18726, 1995.

Interactive comment on Geosci. Model Dev. Discuss., 8, 6669, 2015.