Ward et al. present a new, size-based, marine ecosystem module for the EMIC “GENIE”, called “ECOGEM”, that is intended to replace the simpler module “BIOGEM”. They compare the results of two long-term simulations with these different ecosystem modules.

We would like to thank the reviewer for a very thoughtful and constructive review.

General Comments

The manuscript is generally well written; the ECOGEM equations are presented in a comprehensible way. Since this module has been used in a previous study (Ward et al. 2012), I will only comment on the specific use of ECOGEM in GENIE. Specifically, I am missing a critical discussion concerning ecosystem complexity versus simplifications in GENIE and possible problems related to light attenuation, export production (no prognostic variable for POC) and the neglect of physical transport of the ecosystem variables.

In addition, the results section must be improved as there are several shortcomings (see below); some figures are poorly explained.

Specific Comments

- title, line 158: please use a consistent terminology: either EcoGE**n**IE or EcoGE**N**IE

  We have changed to GEnIE and EcoGEnIE throughout.

- line 21: please rephrase: fisheries is not “life in the ocean”

  Changed to “*support almost all life in the ocean, including the fish stocks that provide essential nutrition to more than half the human population*”.

- line 25: since the reference is the latest but not the most common or original work, please use at least **e.g.** Hülse et al., 2017

  Changed.

- Figure 1: a similar figure for cGENIE and not only EcoGENIE would be helpful to immediately see the differences in complexity

  The figure currently includes the BIOGEM module (cGEnIE & EcoGEnIE) and the ECOGEM module (EcoGEnIE only). This think this should be sufficient for understanding the relationship between the two models.

- sections 3.2.5 Photoacclimation/3.2.6 Light attenuation: Please explicitly state that “photoacclimation” will not be relevant in this current ESM setup. The light attenuation in “GENIE” is overly simplified by assuming an *average* irradiance for the entire surface mixed layer and zero below. The idea to introduce a variable C:Chl ratio is mainly to allow for the development of subsurface chlorophyll maxima that do not correspond to phytoplankton biomass (carbon) maxima. Since the model resolution is too coarse and mean light levels are assumed, the C:Chl- ratio will not vary with depth.

  The C:Chl ratio also varies horizontally, as a function of PAR and nutrient availability. We included it to make comparisons to satellite data more meaningful.

lines 406/407: In my experience a minimum concentration of 1 x10^{-6} mmol C m^{-3} is high and will affect the results significantly; the variability and signals (like extinction) that might become relevant on longer time scales will be smeared out. A smaller value should be used at least for future studies.

Thanks for pointing this out.

- section 3.3.3 Dissolved organic matter: please explicitly state that export production within and below the mixed layer is the same (otherwise the figure caption in Figure 5 is confusing).

  Changed to "*implicit production in the surface layer (and the corresponding export below the surface layer) is given by…*".

- section 4.2 Observations: the references for all observations must be properly provided (e.g. WOA09 is not sufficient)

  We now cite references for the GLODAPv2 and WOA data used. We also acknowledge the source of the SeaWiFs Chl data in the acknowledgements.

- section 5 Results: the entire section is presented in a very sloppy way. A few more explanations about why differences occur between the results of both model configurations or between model and observations are necessary. Please also provide the units for *all* quantities in *all* figures!

  Units are now included for all figures. We also provide a more complete description of the results (e.g. description of Figure 18).

- section 5.1.1 Global surface values: there is general agreement that primary production in the Southern Ocean (the largest HNLC area) is limited by iron, which explains the high macronutrient (e.g. phosphate) concentrations. ESMs generally overestimate the iron concentrations and thus nutrient uptake in the SO. Although the phosphate concentrations in the SO (Fig. 3) are difficult to identify, it seems to me that this is the case here, too. Is it true?

  The high surface PO4 concentrations in the SO are likely a consequence of low Fe, low irradiance (deep mixing) and cold temperatures.

- line 683: please be more specific. The general statement "Iron limitation in high latitude regions" is wrong. As far as I can deduce from Figure 16, iron limitation occurs mainly in the Southern Ocean and the western part of the North Subarctic Pacific Ocean.

  Fe limitation is clearly seen in all high latitude regions (especially among the larger phytoplankton size classes). We have adjusted the text to highlight this in Figure 16.

  "*Iron limitation dominates in high latitude regions, especially among larger size classes. Among these larger groups, the upwelling zones appear to be characterised by iron-phosphorus co-limitation.*"

- line 711: "costs" should be used here instead of "overheads".

  Changed.

- all Figures showing spatial maps: what does the number 10000 on the North American continent refer to?

  This was the model integration year. It has been removed from all figures.