This manuscript is a generally clear description of an extended ocean biogeochemical model in the GENIE EMIC. However, a number of equations are wrong, so I recom- mend correcting these, rerunning the model, updating the figures, and if needed the text and conclusions. While this amounts to major revisions, I am hopeful that a revised manuscript would be acceptable.

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

Major comments: Line 244: “Finally organic matter (D) is made up of K size classes of organic matter, each containing i_d organic nutrient element pools. (Note that strictly speaking, detrital organic matter is not explicitly resolved as a state variable in ECOGEM as we currently only resolve the production of organic matter, which is passed to BIOGEM and held there as a state variable. As a consequence, there is no grazing on detrital organic matter in the current configuration of EcoGENIE. We include a description of D and its relationships here for completeness and for convenience of notation.” This is in fact a really inconvenient notation, because it obscures what happens in the model. See Line 452 below.

Please see our response to the comment relating to lines 452-455.

Line 294: “The size of the quota increases with [. . .] the loss of carbon.” First, this statement is not true, because in Section 3.2.7 it is pointed out that grazing loss does not affect stoichiometry, which is the correct thing to do.

We have now removed the preferential loss of carbon (through respiration) in response to a comment below. As such, we have also removed this statement, which is no longer applies to the updated model.

Secondly, Ikeda et al. (2001, DOI 10.1007/s002270100608) show that the stoichiometry of respiration is undistinguishable from the stoichiometry of biomass as well. This incorrect representation would lead to quota that are sometimes in excess of Qmax, which would give unrealistic artefacts in the nutrient cycling and potentially a violation of mass balance, and should therefore be corrected.

We have removed the independent loss of carbon from the model, as it can indeed violate mass balance in some conditions.

Line 301: This incorrect equation also appears in Geider et al. (1998, Limnol. Oceanogr. 43:679), although it is given here without attribution. Please use the correct equation from Morel (1987, J. Phycol. 23:137) max = himax – (himax – lomax)*(Q- Qmin)/(Qmax-Qmin)

As is the case for all models, both the Geider and Morel formulations have their issues. It is misleading to refer to either one as “correct” or “incorrect”. The Morel model, for example predicts that Q=Qmax when μ=μmax. This prediction is clearly refuted for non-limiting nutrients by Elrifi & Turpin (1985, J. Phycol., 21, 592–602). For the sake of maintaining consistency with Ward et al. (2012), we have chosen to retain the Geider et al. (2007) formulation.

Line 303: The appearance of γFe in the denominator of this equation is incorrect. It would make Chl synthesis increase as cells run out of iron, when in fact Chl:C decreases at low iron (Sunda and Huntsman 1997, Nature 390:389). A photosynthesis model that reproduces this iron limitation effect is given in Buitenhuis and Geider (2010, Limnol. Oceanogr. 55:714)

Assuming the reviewer means equation 13, The appearance of γFe in the denominator does not imply that Chl synthesis increases as cells become Fe limited, because, when Fe is limiting, γFe also appears the numerator (via P_C and P^{sat}; Equations 9 and 11).

Section 3.2.6 uses several words that have physiological meanings (limitation term, half saturation, inhibition) in a section describing light attenuation. If these sentences in fact deal with α (it would help to rename this to αChl), then it should be moved to Section 3.4.3. If it deals with light attenuation, it should be made clear how kChl is derived.

We have rearranged the text accordingly.

Line 343 : “length scale of 20 m” Is this used to calculate kw or the average value of ktot?

BIOGEM doesn’t represent Chl, so water attenuates light with a constant optical depth. We have modified the text to clarify this.

“*In both BIOGEM and ECOGEM, the incoming shortwave solar radiation intensity is taken from the climate component in cGEnIE and varies seasonally (Edwards and Marsh, 2005b; Marsh et al., 2011). However, ECOGEM uses a slightly more complex light attenuation scheme than BIOGEM, which simply calculates a mean solar (shortwave) irradiance averaged over the depth of the surface layer, assuming a clear-water light attenuation scale of 20 m (Doney et al., 2006).*”

Line 345: “At the ocean surface” This would be a logical sentence to start the section.

We have rearranged the text to provide a more logical order to the sentence.

Between Line 452 and 455 D changes from 6 state variables in ECOGEM to 2 (C contents) in BIOGEM. Please explain what happens to the organic nutrient concentrations.

The ambiguity probably arose through our use of the singular in reference to the POM and DOM state-variable/flux vectors (each corresponding to three ECOGEM state-variables). We have changed the text to make it clear that there are 3 DOM state-variables and three POM fluxes in BIOGEM.

“*The dissolved organic matter vector (D1) includes three explicit tracers that are transported by the ocean circulation model and are degraded back to their constituent nutrients with a fixed turnover time of λ (= 0.5 years). Particulate organic matter (POM) is not represented with explicit state vari- ables in either ECOGEM or BIOGEM. Instead, its implicit production in the surface layer (and the corresponding export below the surface layer) is given by…*”

Line 533: It would make more sense to change e.g. the range between Qmin and Qmax, the partitioning between POM and DOM and the decay of POM with depth, which have much more uncertainty than the unrealistic choice noted in Line 294.

We have increased QminP (i.e. decreased max biomass C:P ratio) to compensate for the removal of C respiration.

Section 3.2.9: See comment on Line 294.

We have addressed the choice at line 294 above.

Figure 5 and Line 595: It is confusing to speak of POC production when there is no state variable for POC, and it leads to confusion with primary production. It would be easier to understand to speak of POC flux. Given the central importance of POC flux for air-sea CO2 flux and nutrient distributions, I suggest comparing it to observations (Schlitzer (2004), J. of Oceanography 60:53-62, https://lred.uea.ac.uk/web/green-ocean/data) and including these in Figures 3 and 19.

We have changed the text to describe POC flux rather than production.

“*The relative proportions in which these elements and compounds are exported from the surface ocean are regulated by the stoichiometry of biological production. In cGEnIE (BIOGEM), carbon 595 and phosphorus production are rigidly coupled through a fixed ratio of 106:1, while POFe:POC and CaCO3:POC production ratios are regulated as a function of environmental conditions. In ecoGEnIE (ECOGEM), phosphorus, iron and carbon production are all decoupled through the flexible quota physiology, which depends on both environmental conditions, and the status of the food-web. Only CaCO3:POC production ratios are regulated via the same mechanism in the two models (although 600 we decreased the average CaCO3:POC ratio in ECOGEM to compensate for the elevated POC production relative to POP).*”

We prefer not to use the Schlitzer POC flux dataset. It is based on data assimilation exercise in the North Pacific, and it is not clear how well it extrapolates to the global scale. Indeed, global estimates for vertical POC are still highly uncertain and even contradictory (see for example the discrepancy between Henson et al. doi:10.1029/2011GL046735, 2011 and Marsay et al. 10.1073/pnas.1415311112 2015), so we would prefer not to use these data as a benchmark of model performance.

Line 605: Rather than change ECOGEM to reproduce an arbitrary result in BIOGEM, it would be much more helpful to compare the CaCO3 export to observations (Lee (2001) Limnol. Oceanogr. 46: 1287–1297) and adjust the model to reproduce that.

The BIOGEM result was not arbitrary. It was from a model systematically calibrated to global phosphate and alkalinity measurements. An important aspect of the work here is traceably distinguishing the performance of ECOGEM and BIOGEM, so it is important to consistently evaluate the former against the latter.

Line 617: “total oceanic DIC inventory increased by just under 2% from 0.299 mol C” This makes no sense. The total oceanic DIC inventory is ~3.3 Examol.

Thanks for pointing this out. The ‘Exa’ prefix was omitted in error.

Line 652 and Figure 17: “The model predicts higher chlorophyll concentrations in the Southern Ocean” Figure 17 is inadequate to decide whether this is a reasonable comment to make.

This comment was in reference to Figure 14, which shows higher model chlorophyll concentrations in the Southern Ocean, relative to the SeaWiFS data.

Please have the y-axis range from 10-2 to 10 (values between 10-5 and 10-2 are insignificant), put the station names inside the panels, so that the panels can be made higher, and include the satellite chl in the figure.

The y-axis range was chosen to show the dynamic range of the model. Values < 1e-2 are indeed insignificant in the observations, but low winter values are an important component of the model dynamics. As such, we feel it is important to retain the y-axis range of 1e-5 to 1e1.

We have added SeaWiFs chlorophyll to the figure.

If that shows the in situ measurements span the satellite estimates, delete the Dierssen reference and rewrite this to reflect the findings of Le Quere et al. (2016, doi:10.5194/bg-13-4111-2016), that models underestimate SO chl because they underrepresent macrozooplankton. Also, after correcting the error on line 303, this may improve/decrease SO chl.

We have plotted SeaWiFS data at the three Southern Ocean sites. The satellite data do show a tendency to underestimate in situ observations on the Southern Ocean. We have therefore retained the Dierssen reference.

Figure 18 needs to be described.

We have added a description.

"*The seasonal cycles of primary production in the surface layer are compared to time-series observations in Figure 18. As also indicated in Figure 14, the spatial variance in modelled primary production is too low, with primary production overestimated at the most oligotrophic site (HOT) and typically underestimated at the most productive sites (esp. the equatorial Pacific, NABE and the Ross Sea). In contrast to the lack of spatial variability, the model exhibits significant seasonal variation, often in excess of the observed variability (at those sites where the seasonal cycle is well resolved).*"

Line 737: "the ecological community conforms to expectations in terms of standing stocks" This has not been shown. Comparison to Buitenhuis et al. (2013, doi:10.5194/essd-5-227-2013) would test this statement. Given the different definition of plankton groups, comparison could be made to Fig. 5a.

We have corrected this statement to read "*the ecological community conforms to expectations in terms of standing stocks and fluxes, both in terms of large-scale spatial distributions, and the seasonal cycles at specific locations (Figures 14 and 17)*".

Minor comments: Line 85: for the how -> for how Line 143: in terms its -> in terms of its Line 162: modularised -> modular Line 176: a greater intention to explore long timescale -> the intention to explore longer timescale Line 262: the the -> the Line 363: level the -> level of the Line 421: The O2:C ratio is in fact >1. Anderson and Sarmiento (1994) find it's ~170/117=1.45, so even 138/106 would be quite low, and it would be helpful to justify it. Line 439: used equations -> used in equations Line 592: in tropical -> in the tropical Line 636: Figures 12 and 13 we -> In Figures 12 and 13 we Line 687: Figure 18 -> Figure 17 Line 690: is probably too low -> is too low

We will correct all these errors in the resubmitted manuscript. Note that the O2:CO2 ratio was inverted in error. This has been corrected.