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Discussions

# *Interactive comment on* "Biogeochemical versus ecological consequences of modeled ocean physics" *by* Sophie Clayton et al.

**Anonymous Referee #1**

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Note: please refer to the pdf attachment, as equations/formatting did not all paste correctly here.

Summary Statement: Clayton and colleagues explore the importance of model resolution on ocean ecology and biogeochemistry in two versions of ECCO. Both models were coupled to the same version of the Darwin ecosystem/biogeochemistry model with the primary difference between the two being 1deg verus 1/6 deg resolution, with the later considered 'eddy permitting.' The primary finding is that the phytoplankton biogeography in terms of functional type was much more stable than rates and stocks, such as primary productivity and total phytoplankton and zooplankton abundance. These conclusions are somewhat unexpected and a valuable contribution to understanding how mesoscale physics may interact with ecosystem and biogeochemical dynamics.



The manuscript addresses an important topic relevant to Biogeosciences and reaches novel conclusions. It is well written and structured. With some revision, I feel the manuscript will be a valuable contribution to Biogeosciences.

I do have some concerns regarding how the authors interpret differences between HR and CR simulations at the regional scale, particularly for the in the northern high latitudes. These concerns as well as minor comments are listed below.

General Comments: P8-L12-32: Interpretation is primarily through a steady-state framework (i.e., R*, P*,Z*). This holds when the timescale of physics is » timescale of biology, supply is constant, etc. My largest criticism is the attempt of the authors to explain the "Dilution-Recoupling Hypothesis" using a steady-state framework (Page 7, line 31 – Page 8 line 1). From Fig 7, it is clear that dP/dt and dZ/dt are almost never close to zero, and certainly not simultaneously zero. The hypothesis is fundamentally driven by perturbed systems where steady-state is not valid. I suggest either further justification why the authors think the Z*/P* framework is applicable here, or to use non-steady-state arguments.

âĂć Would not the deeper spring ML depth in HR simulations result in a greater annual SN? The authors argue that higher Z* in CR is due to higher SN, but present no evidence that SN is higher in CR. If anything, it seems SN should be higher for HR?

âĂć Comparing Fig 7a and 7b along with the relationship PProd = mu*P, it seems that P growth rate is higher year-round in the CR simulation. Light limitation could explain the difference in spring, as the authors point out, but what about the rest of the year?

Equatorial upwelling is minimally addressed. In Fig 2 there is a dramatic increase in phytoplankton stock and primary productivity in the Equatorial Pacific. It would be interesting to diagnose if there is a change in net nutrient supply from equatorial upwelling and if there is a change in subsequent meridional transport of nutrients, or if they are effectively locally trapped. The authors mention a change in equatorial productivity related to a change in poleward Ekman nutrient transport, but there is no discussion if there is any change in the supply rate from upwelling (rather than mld changes). Could the authors compare over an equatorial band between the two model resolutions?

Another general comment is that the model includes two Zooplankton that I believe have size specific grazing preference. It would be interesting to diagnose if the change in resolution causes any systematic changes in the efficiency of predator-prey coupling. i.e., is there any change in the average 'g' term? One might expect that higher resolution physics could disrupt predator-prey coupling. Have the authors looked into this? (This is just my curiosity, and I am fine if the authors feel the topic is beyond the scope of this manuscript).

P8-L2-3: In equations (5) and (6) it is unclear to me why there is an 'R' term in the right side of each equation. The description of an idealized light-limited model system implies replete nutrients. Under nutrient replete conditions (R » k), the R/(R+k) term in Eq (1) approaches 1, and eq 5 should simplify to:

I do not see the need or justification for . In fact, there is no change in solutions for N* and Z* (Eqs. (9) and (10)). Equation (8) would then be superfluous and should be removed.

Minor comments: P8 L30-32: 1/6deg definitely doesn't represent the Mahadevan (2012) restratification mechanism which is fundamentally submesoscale. It might capture the McGillicuddy (2003) mechanism.

Figure captions: General comment: Indicate in each figure caption if results are annual averages for 1999, or some other time period. Figure 3: The 'green' in (a) and (b) looks quite blue/teal to me Figure 5: I would suggest that the order of (b) and (c) be switched. Also, in panel (b), it would be useful to color code by what the There are also some distracting red dots, such as south of New Zealand, that appear to be islands.

Figure 6: Does 'higher' mean deeper? Suggest that you say Positive values indicate deeper. . .



Figure 7: It would help interpretation to also include the seasonality of mixed layer depth for HR and CR as a separate panel (or overlay on the existing panels). Consider including seasonality of surface nitrate also.

Editorial Comments: P5-L15: tus thus P6-L31: m would be preferable, as alone typically refers to realized growth rate, not maximum growth rate. Also, SN SR P8-L19 "drives in an.." "drives an.." P9-L4 Northern and Southern Pacific North and South Pacific Fig 2 caption: Although 'annual' is in the text, units for primary production of g C m-2 y-1 would be preferable such that the units are consistent with a rate.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-337/bg-2016-337-RC1-supplement.pdf

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