

Interactive comment on “Projected decreases in future marine export production: the role of the carbon flux through the upper ocean ecosystem” by C. Laufkötter et al.

Anonymous Referee #2

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This manuscript looks at the drivers of export production and its predicted changes with future warming within 4 different global biogeochemical models. The authors find that the models differ greatly in their representation of processes affecting export and its predicted changes. The manuscript is thorough in its explanation of the distinct processes driving export within each model and is thus especially helpful in illuminating the reasons behind differences in predictions between the models. It provides a nice, comprehensive framework for thinking about the drivers of export and its efficiency in terms of well-defined components (NPP, e-ratio, s-ratio, p-ratio), which could be readily systematically applied to future model and observational studies for straightforward comparisons. It also highlights the types of future modeling work and observations

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needed to improve our models. It is therefore an important work and should be published after some minor corrections and clarifications.

Specific Comments:

Pg 19944, Line 23: Leung et al. (2015) show that there is some agreement on the mechanisms driving changes in NPP among the CMIP5 models within the Southern Ocean, so perhaps saying that there is no agreement on the mechanisms among the models is a bit too strong.

Pg 19947, Line 21: Could you explain with one more sentence or so how/why aggregation and mortality are wrapped up in the same APOC term? What processes are meant to be simulated within this term (aggregation, viral lysis, other specific mortality-inducing processes)?

Pg 19948, Line 21: Although defining export production as the amount of POC sinking out of 100 m depth is a common practice, it is not always the right way to think about export if we actually care about the amount of carbon making it down to the deep ocean. See Palevsky et al. (2016). It would be nice to mention this briefly.

Section 2.2, in general: I think that it would be a lot clearer if you presented all equations separately for each model with clear labels as to which model is being represented. Example 1: Equation 8 could be written out separately for BEC and PISCES, so that you can represent what happens in BEC with biomass concentrations greater than 22 mmolC/m² and can also add in the linear mortality term. It's a bit cumbersome to do the mental gymnastics of altering equation 8 for each different model as one reads the text. Example 2: Equation 11 could be written out separately for each model, so that there isn't as much confusion about which parameters depend on which variables differently within each model. This would make the textual explanation a lot easier to follow. f_{grazPOC} could have a subscript for BEC as one example. You could also denote that f_{grazPOC} is a function of temperature for TOPAZ as another example. I do like the way that the equations are done in Section 2.2.3.

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Pg 19950, Lines 11-14: Here you say that aggregation grows linearly with biomass after a certain biomass concentration, but then say that there is also an additional linear mortality of phytoplankton that you ignore. I'm a bit confused by this; are you referring to the same term? Also, do you justify that leaving this term out really doesn't matter? I don't see the harm in adding this linear term to the quadratic term and calling that the total mortality/aggregation, assuming that they are representing the same processes.

Pg 19950, Line 16: You say that aggregation increases under nutrient limitation for diatoms in PISCES, but I'm unsure of what equation to look at to see how this really works. Could you point out which equations represent this phenomenon?

Pg 19950, Line 24: Could you help me more easily see how "in both BEC and PISCES high and low diatom fractions allow higher aggregation than intermediate diatom fractions"? I'm again unsure of what equation to look at to see how this works (not equation 8, right?).

Pg 19952, Line 11-12: What does it mean mechanistically/physically for zooplankton grazing to be modeled implicitly and to be independent of zooplankton biomass? Could you add one more sentence or so to explain this more?

Pg 19952, Line 12-13: This is confusing when read with Pg 19951, Line 12. The way Pg 19951, Line 12 was worded made it seem like BEC was the exception, but now we find out that TOPAZ was the exception. To clarify, I suggest listing in parentheses the name of the three models with constant $f_{grazPOC}$ and taking out the word "however" on Pg 19951, Line 12.

Pg 19952, Line 17: You say finally to denote the last way in which TOPAZ is different on Line 12, but now you add another difference. Please tweak this wording.

Pg 19952, Line 19: Why do you choose to compare the fractions routed to POC at 0 degrees C? Is this the average temperature at some meaningful depth?

Pg 19952, Line 21-23: Could you add a sentence or two on how particles would mech-

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anistically be formed via zooplankton consumption by higher trophic levels? Is this by production of fecal matter by higher trophic level biota after consuming zooplankton?

Model evaluation section: I agree with Reviewer 1 on the need for some Taylor diagrams. It would be great to see these diagrams for grazing, e-ratio, and export.

Pg 19954, Line 26 and Pg 19955, Line 4: I would caution against calling the satellite-based maps of export "observational estimates" or "observations," but would rather them be called "observationally-based", "empirically-based", or "satellite-based" estimates due to the fact that they are based on observed empirical relationships rather than direct observations themselves.

Pg 19955, Line 2: It would also be nice to see global maps of e-ratio within each of the 4 models compared to observationally-based maps of e-ratio.

Pg 19955, Line 16: I would again caution against using the word "observations."

Pg 19958, Line 23-24: I appreciate the separation into high and low latitudes as a starting point for understanding different mechanistic changes throughout the ocean. However, I'm unsure if this is the most meaningful way of understanding how processes differ in different regions as it obscures and mashes together potentially very different trophic regimes. Have you tried looking at different ways of breaking up the ocean (namely, recreations of Figures 5 and 7) to see if it makes much of a difference? One quick starting point could be just looking at productive equatorial upwelling regions separated from the subtropics and the high latitudes. Another method would be to use biomes as in Sarmiento et al. (2004) and Cabre^lA et al. (2014).

Pg 19959, Line 21-22: Add increase and decrease percentages for the Southern Ocean and Arctic as well.

Pg 19960, Section 4.4: It might be nicer for this section to be written more like the previous sections; that is, by describing one model at a time, rather than by comparing between all models within the same paragraph. It is still easy to see the differences

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between the models this way, but makes it much easier to follow and read.

Pg 19960, Line 23: Need to add “and” between the two grazing flux terms. I think that these terms should’ve been introduced and used earlier, namely when equation 11 was being explained and possibly even earlier.

Pg 19964, Line 13-17: Could you include a figure to show this?

Pg 19968, Line 13-14: Could you provide some examples of non-diatom large phytoplankton in the real ocean?

Pg 19968, last paragraph: This paragraph’s wording and sentence order is a little confusing. I suggest the following: - Take out “but see.” Also cite Guidi et al. (2016). - I’m unsure why our previous assumption of negligible small phytoplankton aggregation consequentially leads to these fluxes constituting at most a few percent of total EP in the models. Are you saying that the model creators used this idea to purposely make aggregation for the smaller phytoplankton less? If instead you are suggesting that these fluxes constitute too little of total EP in the models based on the new information from Richardson and Jackson (2007), then make that clear. - I would move sentence 3 (“However, contributions of pico- and nanoplankton. . .”) earlier. After citing Richardson and Jackson (2007), you could then say “For example, contributions of pico- and nanoplankton to total export of up to ...” and THEN talk about how the models get these fluxes too low.

Pg 19971, Line 20: Could you give some more specific examples of what you mean by idealized simulations?

Pg 19971, Line 25: It would be nice/helpful if you could provide a couple of specific examples of what exactly you would like these studies to measure. This could be done earlier in the paper as well. This would help observationalists and laboraticians reading this paper figure out exactly what they need to measure/what experiments they need to conduct.

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Table 3, Remin. length scale: Add in the range of remin length scales for PISCES (small POC at slowest remin rate to large POC with fastest remin rate).

Table 4: Add in units for NPP and EP. What does the +21% mean in the grazed % of NPP column?

Figure 1: Make the arrowheads going from POC to Zoo, Zoo to POC (brown), Zoo to POC (gray, right), “Aggregation of DOC” bigger. They are hard to see right now.

Figure 4: A vertical line at 0% change in each subplot would help. From this, it looks like NPP changes are driving export changes a lot more within PISCES and REcoM2. In all of the models, however, it looks like the zonal variations in export change match (i.e., are correlated with) those in NPP change. In PISCES, it looks like NPP and e-ratio changes are very well anti-correlated, while in TOPAZ they are somewhat well-correlated. What might be some reasons behind this?

Figure 5: I think the images of the phytoplankton and zooplankton in a) are not necessary here and are instead kind of distracting.

Figure 7 caption: Zooplanktonmortality should be two words. Change comma to a period after “fluxes are given in percent of total NPP.”

Figure 7: I found it very confusing for the largest fluxes to have red arrows. I think that the size of the flux should just be represented by the width of the arrow, so that you can immediately tell which fluxes are largest by that alone. The fact that the arrows are red and then the numbers denoting the changes are red, too, is another cause for confusion. Thus, just leaving those numbers red, while making all of the flux arrows black will be clearer. Make arrowheads larger, as they are sometimes invisible. Why are there dashed arrows for REcoM2 high latitudes? As Reviewer 1 noted, TOPAZ high latitude grazing arrows are on top of the zooplankton box unlike the other diagrams, and it would be nice to have titles at the top of each column saying “low latitudes” and “high latitudes.”

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Conclusions: It would be nice to mention the role of physics (both model-resolved and those occurring on scales smaller than the models can resolve) in driving EP and caveats of leaving physical effects out of this analysis. See Omand et al. (2015), for example. It would also be nice to see where the largest differences between models lie and to thus speculate on what may be the biggest source of uncertainty for projecting future EP changes (at least among the models analyzed here). Is it in p-ratios, s-ratios, or NPP? Mechanistically, is it grazing efficiency, aggregation efficiency, aggregation/mortality functional form, temperature dependence, etc.?

Technical Corrections:

Pg 19947, Line 24: Change "(DOC), moreover" to "(DOC). Moreover,"

Pg 19948, Line 19: length misspelled

Pg 19949, Line 17-18: Change to: We use the terms "grazing efficiency" and "aggregation efficiency" to describe the fraction of NPP that is transformed into particles via grazing and aggregation processes, respectively.

Pg 19951, Line 12: "four models, however in" should be "four models. However, in" or with a semi-colon rather than a period. Or change to "four models, while in BEC fPOCgraz is larger for grazed diatoms compared to grazed nanophytoplankton."

Pg 19951, Line 21: It might be clearer to say Pj dependence here, instead of just P-dependence.

Pg 19952, Line 16: "particle formation, however ..." should be "particle formation. However, ..."

Pg 19953, Line 24: Suffers should be suffer.

Pg 19959, Line 19: Change "," to ";

Pg 19970, Line 8: Change "if they capture the processes how particles are formed" to "if they capture the processes dictating how particles are formed" or something like

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this.

Table 1: GFDL-ESM2M should be under the CMIP5 project.

Table 3 caption: Change from a comma to a period after "'Agg' is short for aggregation."

References:

Leung, S., A. Cabré, and I. Marinov. "A latitudinally banded phytoplankton response to 21st century climate change in the Southern Ocean across the CMIP5 model suite." *Biogeosciences* 12.19 (2015): 5715-5734.

Palevsky, Hilary I., et al. "The annual cycle of gross primary production, net community production and export efficiency across the North Pacific Ocean." *Global Biogeochemical Cycles* (2016).

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Omand, Melissa M., et al. "Eddy-driven subduction exports particulate organic carbon from the spring bloom." *Science* 348.6231 (2015): 222-225.

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