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Earth System Dynamics
Discussions



# *Interactive comment on* "Resolving ecological feedbacks on the ocean carbon sink in Earth system models" *by* David I. Armstrong McKay et al.

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Armstrong McKay et al., present a set of Earth system model experiments to demonstrate the importance of resolving plankton ecosystems for the ocean carbon sink when considering biogeochemical feedbacks. In particular they focus on the temperature dependent remineralisation of particulate organic carbon (POC) as a key feedback. They achieve this by simulating future climate scenarios in an Earth system model with both a parameterised export production scheme and trait-based ecosystem model. They find that the strength of the biological pump, i.e., export production of POC, generally increases when adding temperature dependence of POC. In contrast, export production



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decreases significantly when using the plankton ecosystem model. The net change in the carbon sink associated with these changes is further modulated by concurrent changes in the inorganic carbon pump.

In general, the concept of the manuscript is interesting given that few studies have approached the interactions between ecological and biogeochemical complexity due to computational limitations. The use of EcoGEnIE here facilitates this novel idea in a straightforward and logical way. The key results seem generally sound but there are significant parts of the results that are not backed up with figures and many explanations about the role of different processes that are not quantified. As such, the manuscript makes a good case for resolving ecological complexity but not necessarily which components of this complexity are important and why. The manuscript would benefit from major revisions including new figures and additional experiments to quantitatively show how the various components of the ecological complexity lead to the main results.

General Comments

1) Calibration of global biological pump strength

The spin-ups are all calibrated to have the same global POC export (~7.5 Gt C year-1) but there are no details about how this has been achieved in the model, i.e., what parameters were modified. However the calibration has been achieved it needs to be described as the BIO+TDR and ECO+FPR set-ups now differ from their published versions (John et al., 2014. P3; Ward et al., 2018. GMD).

The supplementary plots comparing each set-up after calibration also need to be more comprehensive. It is not totally surprising that surface fields are similar across set-ups as POC export has been calibrated to the same global value, particularly for those fields that are strongly influenced by export such as PO4. These fields may then differ more in the ocean interior. The authors should add difference plots showing depth slices for the various fields and/or a Taylor diagram to show how the calibration affects





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global fit statistics like correlation and standard deviation.

The main concern I have is that the authors may have achieved the same POC export by altering parameters associated with POC remineralisation - because the BIO+FPR and ECO+FPR output in Table S1 should have the same POC sedimentation:export ratio if the fixed remineralisation profile is the same. Apologies if this is not the case, but if it is it may have implications for the results in the manuscript. Firstly, the differences in POC export in Figure 4 would be a combination of adding the various TDR and ECO components but also the calibration adjustments that presumably vary across set-ups to achieve the same POC export. (This is significant for any calibration). Secondly, if the ECO experiments have deeper remineralisation to offset the higher POC export in the Ward et al., (2018) set-up, this could potentially bias the results if the deep ocean takes longer to experience changes in temperature, i.e., the transient ECO response may be slower due to the calibration. The relative change in carbon/nutrient feedbacks may also differ because the residence time of carbon/nutrients in the ocean interior is different and because carbon/nutrients may be redistributed spatially via different circulation pathways (e.g., Pasquier and Holzer 2016, JGR Oceans). While I don't think this changes the general findings of the manuscript, it does make me question the relative magnitude of changes between each set-up.

I do appreciate that the baseline states will aways differ in some way because of the use of different parameterisations! The authors need to acknowledge the reason for choosing to constrain POC export across runs and what issues this may introduce, e.g., are there differences in spatial export patterns; are you compensating for any errors in the circulation and biogeochemical model? Alternatively, the original BIO+FPR, BIO+TDR and ECO+FPR set-ups have all been (somewhat) calibrated to achieve similar global distributions of dissolved tracers compared to observations. The authors could repeat their experiments with these published set-ups and recalibrate just the ECO+TDR set-up to achieve similar global tracer distributions. This can be defined using various fit statistics like root mean square error. This alternative set of results

would help demonstrate that the POC export calibration is not biasing the results.

2) Background and Model Description

The description of processes in the Background section and the model description is too brief to support the main results. The biological pump is described mainly in terms of export production but has little description of the role of POC remineralisation and circulation. A few statements describing that the POC flux rapidly decreases with depth to a small asymptotic flux by $\sim$1000m and that the ventilation age of the ocean increases with depth would really help clarify a lot of later statements in the results. Similarly, there is no basic description of the allometric relationships for plankton and how they relate to metrics like primary production.

The model description is also very sparse in specific details that would aid the reader in understanding the results in more detail. For example, important details such as the saturation-state dependent PIC:POC rainratio (Ridgwell et al., 2007: Biogeosciences) and the nature of allometric trends like size-dependent DOC:POC export production (Ward et al., 2018: GMD) are not described. Whilst these are described fully elsewhere, it would help to describe these briefly as they are directly relevant to the results and discussion.

3) Results from the plankton ecosystem modelling

The description of how plankton ecosystem structure impacts the biological pump is difficult to follow (mainly lines 231 - 246 and other related sentences throughout). There are a lot of discussion of changes in plankton size but this is never visualised despite being a standard output of the model.

I am not totally convinced by the explanation of why the ecosystem model leads to a greater decrease in export production. Size structure, variable stoichiometry and DOC:POC export ratio are all alluded to throughout the manuscript but there are additional components that haven't been considered. In steady-state warm-climate ex-

periments using the same model there is a net decrease in plankton biomass due to increased grazing pressure because grazing rates are temperature dependent in Eco-GEnIE (Wilson et al., 2018, Paleoceanography and Paleoclimatology). This grazing effect also co-varies with nutrient availability leading to distinct latitudinal trends in size, biomass and export. This needs to be factored into the explanation here. This is a novel application application of a model of this type so it would be really helpful and informative to know what aspects of the ecological complexity are crucial to this result!

4) Results from the Ocean Carbon Sink capacity

I found it hard to follow the logic in this section because the factors involved are not quantified and/or illustrated in figures. A figure illustrating the changes described would really help to clarify the text in this section.

The increase in export production but decrease in carbon sequestration has been noted before (Kwon et al. 2009, Nature Geoscience; Gnanadesikan & Marinov 2008, Marine Ecology Progress Series). The impact on carbon sequestration is in part due to a change in organic carbon cycling and in inorganic carbon cycling but It is not clear in the manuscript what the relative impact of these processes are. This could be separated by running additional experiments with a uniform PIC:POC rain-ratio to remove the impact of any spatial differences in POC export between se-ups and a prescribed spatially variable ratio from the associated spinup to isolate the impact of changing saturation state.

Specific Comments

Lines 18 - 20: the manuscript does not actually show plankton size or deal significantly with ocean acidification

Introduction/Background: generally I found the structure of these sections difficult to follow. Particularly there are a number of concepts and abbreviations in the Introduction, such as Fixed Profile Remineralisation, that are not described sufficiently until the

Background section.

Line 51: cGEnIE does not have a NPZD model. It parameterises the export of production by plankton as a function of nutrient availability using Michalies-Menten kinectics and other limiting factors. This needs to be made clearer throughout the manuscript.

Line 54: “a weakening carbon sink” - w.r.t. anthropogenic climate change?

Line 56: the biological pump is described too briefly here and focused very much on the export of organic matter from the surface. It would help readers to expand here on the additional role of depth variation in remineralisation rates and ocean ventilation ages, particularly as this is a key concept needed to understand the model results.

Lines 60 - 61: this statement surprised me! There have been significant model developments that try to resolve the ecological drivers of the soft-tissue pump such as cell-size and aggregation (e.g., Jokulsdottir & Archer 2016. GMD; Omand et al., 2020. Scientific Reports). I am not sure we are at a stage where we fully understand the interactions yet or are able to couple these models into global biogeochemical models though.

Lines 90 - 91: Strictly speaking it is the metabolic rates that increase between 100% and 200% whereas gross primary production and community remineralisation are additionally limited by other factors.

Line 94: “raising the remineralisation depth . . . higher up in the water column” - this is repetition. Either the remineralisation depth moves higher up or it is raised.

Line 94: “(the point at which most POC is remineralised)” - an e-folding depth is often used to define this as the depth at which 63% of the exported flux has been remineralised.

Lines 106 - 108: please briefly outline why this happens

Line 161: “better representation of biodiversity” - relative to what? If relative to cGE-

nIE then this is really just resolving diversity (and biomass!) compared to the export production parameterisation.

Line 175 & 179: NPZD here is misleading as the export production scheme in cGENIE does not resolve plankton biomass, phytoplankton or zooplankton.

Line 204: though the biological pump strength does increase for the BIO+TDR experiments by 2100

Line 207: How does the 6.1% decrease (and generally across all experiments) compare with CMIP model simulations?

Lines 210 - 212: this is not an explanation of what is happening in cGEnIE as it does not resolve plankton and productivity is restricted to a single surface layer.

Line 218: “more POC is being remineralised with warming” - I struggled to follow the logic of this. Does this mean more POC is remineralised in the surface ocean so lowering export production? If so, this should be checked that POC remineralisation occurs in the surface grid-boxes in cGEnIE and is not exported from the base of the surface layer.

Lines 220 - 221: “warming-induced shoaling of the remineralisation depth has been modelled to reduce POC export (Kwon et al., 2009)” - this may be a typo or the wrong reference? The Kwon paper perturbs the remineralisation depth directly for a fixed climate, and it shows POC export increasing, not decreasing, with increasingly shallower remineralisation depths (e.g., Fig. 2a in Kwon for values of b>0.9).

Line 237: “rapidity of carbon cycling within the surface ocean” - what does this refer to? A shift from POC to DOC export production? If so, I would expect the increase in the rate of nutrient cycling associated with more semi-labile DOC production to rather increase production and biomass because it will be remineralised near the surface due its short lifetime.

Lines 231 - 246: plankton size outputs are available in EcoGENIE but are not plotted

to support any of these statements. This would be an interesting thing to see!

Line 277: “adding ECOGEM reduces total ecosystem POC/PIC production” - i cannot see this in a figure and it is not described or demonstrated why this happens as a result of having ECOGEM

Lines 294 - 304: I wonder if resolving plankton biomass also plays an important role as part of this? Galbraith et al., (2015) in JAMES showed nicely that seasonal/transient behaviour varies between a model with parameterised export of POC and one that explicitly resolves plankton biomass. A parameterised model, like cGEnIE, responds much more rapidly to environmental changes because growth rates are not buffered by a biomass pool. There are a few entries in Tables 1 that this parameterised export model. Following on from this, it would be interesting to speculate what the representation of ecological complexity needs to be to reliably simulate the biological pumps response to environmental change.

Lines 313 - 314: “does not feature trait-based size classes or flexible stoichiometry, which we have shown is critical for determining the soft-tissue biological pump response” - the role of flexible stoichiometry has not been explored here.

Lines 325: “flexible nutrient usage” - the influence/impact of this has not really been quantified or discussed in the manuscript.

Figure 5: it would help to combine these panels with Figure 3 so they can be compared side by side.

Figure 4: I spent most of the time thinking these differences were 2100 vs. baseline because that is the format of the other figures. Expanding the labelling might help clarify this.

Table 1: this is very valuable, thank you!

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