**From:** gbc@agu.org
**Subject:** 2019GB006412: Decision Letter
**Date:** November 6, 2019 at 10:38 PM
**To:** mdever@whoi.edu

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Dear Dr. Dever:

Thank you for submitting your manuscript to Global Biogeochemical Cycles. I have received 2 reviews of your manuscript.

I am sorry to inform you that based on the reviews of your manuscript, I have decided to decline it for publication in Global Biogeochemical Cycles. While the manuscript is not acceptable now, with additional work, as outlined in the comments, I believe that you will be able to turn this into a more suitable paper. I am rejecting it now so as not to put a time constraint on your revision and allow you ample time to complete the additional work. Once the revisions are complete, I would like to encourage you to resubmit the paper.

If you resubmit this paper, please follow our Checklist and use our Templates for the main file and any supplements. Please provide the following:

1. A response to reviewer file that lists each major comment and describes how the manuscript has/has not been modified.

2. A copy of the manuscript with the changes noted (e.g., highlighted, "track changes," italics or bold changes).

3. The final revised manuscript and separate final figure and supporting information files with the changes incorporated, which will be used for publication if the manuscript is accepted.

AGU requires that data needed to understand and build upon the published research be available in public repositories following best practices. This includes an explicit statement in the Acknowledgments section on where users can access or find the data for this paper. Citations to archived data should be included in your reference list and all references, including those cited in the supplement, should be included in the main reference list. All listed references must be available to the general reader by the time of acceptance. AGU requires the corresponding author, and encourages all authors, to register for an ORCID.

When you are ready to submit your revision, please login to your account (https://gbc-submit.agu.org/cgi-bin/main.plex) and click "Submit Resubmission for 2019GB006412."
I look forward to receiving your revised manuscript. If you have any questions, please contact us at gbc@agu.org.

Yours sincerely,

Sara Mikaloff Fletcher
Editor
Global Biogeochemical Cycles

-----------IMPORTANT INFORMATION-----------------------
Additional information on text preparation, formatting, acceptable file formats, supporting information, graphics preparation, and AGU style, is here. AGU has recently updated its styles.

Sharing your work is an important part of the research process, and AGU leverages and shares published research to promote the https://eos.org/editors-vox/earth-and-space-science-for-the-benefit-of-humanity" target="_blank">broader importance of Earth and space science. Learn how you can promote your paper, including how your paper can be considered for additional publicity or for the issue cover.

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Associate Editor Evaluations:

Associate Editor (Remarks to Author):

This is an novel idealized modeling study that explores the role of mesoscale dynamics in shaping the contribution of a range of particle size classes to carbon export at the Ocean Station Papa site in the North Pacific Ocean in different dynamical regimes. The two reviewers have raised a significant number of serious concerns about how the experiments were set up and analyzed which calls in question the conclusions of the study. I am strongly encouraging resubmitting the manuscript after having taken the time to conduct the new simulations and analyses that have been suggested, and/or to reshape the manuscript objectives and conclusions that can be derived from the present simulations

Reviewer #1 Evaluations:
Significant: There are major errors or gaps in the paper but it could still become significant with major changes, revisions, and/or additional data.
Supported: No
Referencing: Yes
Quality: Yes, it is well written, logically organized, and the figures and tables are appropriate

Quality: Yes, it is well-written, logically organized, and the figures and tables are appropriate.
Data: Please Select

Reviewer #1 (Formal Review for Authors (shown to authors)):

Review of "Size-differentiated export in different dynamical regimes of the ocean"

The submission by Dever et al. explores the role of mesoscale dynamics in shaping the contribution of large versus small particles to carbon export. Advective export of particles by eddies and frontal zones has received a great deal of attention recently, but the focus on particle size in the current study is novel. The authors configure a high resolution, idealized dynamical model to reflect the Ocean Station Papa site in the North Pacific Ocean, and conduct simulations for the summer and winter. Small scale dynamics are greatly enhanced in the winter, and therefore these simulations represent two different dynamical regimes. The authors seed each model with particles of varying sinking velocity (reflecting different sizes), and quantify the amount of biomass exported by each size class through a combination of gravitational settling and vertical advection.

By comparing summer and winter simulations, and cases with and without remineralization, the authors arrive at two main: First, export by small particles can dominate in dynamically active regimes; second, remineralization enhances the contribution of small particles to carbon export. These conclusions are novel and interesting, and would warrant publication in Global Biogeochemical Cycles. However, I am concerned that both conclusions are simply artefacts of how the experiments have been designed, and how the analysis has been conducted, and are not representative of the real ocean. Therefore, I cannot endorse publication of this manuscript until the authors have addressed the three major points below, and either conduct the new simulations suggested or convince me that they are not necessary to support the conclusions.

First, it is not clear why the authors have chosen to release particles only at the base of the euphotic zone between 75-85, which is within 20m of the horizon defined as the export depth (100m). In fact, particles are produce throughout the entire euphotic zone, and the production rate (i.e. NPP) is generally highest within the first 10-20m where light is ample. Distributing the particles through the euphotic zone in this way would almost certainly diminish the contribution of small particles to export, as they would spend significantly longer remineralizing within the mixed layer before passing the export horizon. I suggest a new simulation with particles initially distributed between 0-80m, or a clearer reasoning for selecting 75-85m as the release depth.

Second, it is unclear why 5m/day is selected for the largest particle class. Studies using Underwater Vision Profilers reveal that a large portion of the particle flux is contributed by large aggregates of 0.1-5mm, which can have sinking speeds above 100m/day (see Guidi et al. 2008, Kiko et al. 2017). Comparing the summer and winder simulations, it seems that small particles only dominate the export flux when vertical advective velocities exceed the large particle sinking velocity. This condition would not be met if an aggregate particle class with sinking velocity of 100m/day were included. I suggest the inclusion of an additional "large aggregate" size class with appropriate sinking speed.

Finally, the analysis demonstrated in Figures 7 and 8 does not seem appropriate for gauging the contribution of small and large particles to export, and the role of remineralization. As far as I understand, the insets in these figures show the flux associated with each particle class at 25 days after the particle release, computed based on their abundance and velocity (advective plus sinking). But why is the flux at day 25 the important quantity? By this time, the large particles have had time to decay to a size where their sinking velocity is negligible, and therefore their contribution to the flux will be small. But, what about all of the large particles that settled across the export horizon (100m) earlier in the simulation, while their sinking velocity is still high? Really, we should be interested in how much of the initial biomass in each size category has "escaped" through the export horizon by the end of the simulation, i.e. their time-integrated contribution to export. For particles released at 80m, sinking at 5m/day and remineralizing at a rate of 0.13/day, it seems that at least 50% of the inital biomass in the large size category must be exported through the 100m horizon, before remineralizing. In contrast, Figure 6 shows that only a very small fraction of the small particles reach 100m in winter, even when they are not remineralizing. It therefore seems that even in winter, large particles must dominate the integrated export flux in the simulation with remineralization, contradicting the second major conclusion of the study. I suggest the authors repeat their analysis, comparing time-integrated export through 100m in order to assess the contribution of large vs. small particles.

Minor points:

Line 306: Is it three weeks, or 28 days (four weeks)?

Figure 7+8: It would be useful to point out either in the caption or axis label that the x axes are the vertical velocity combining both sinking and advective components, and that the velocity in the legends is the initial sinking velocity. This confused me for a few minutes.

Acknowledgements: Will the model output be archived for public access?

Reviewer #2 Evaluations:
Significant: The paper has some unclear or incomplete reasoning but will likely be a significant contribution with revision and clarification.
Supported: Yes
Referencing: No
Quality: The organization of the manuscript and presentation of the data and results need some improvement.
Data: No

Reviewer #2 (Formal Review for Authors (shown to authors)):

Major comments:

1) Range of sinking speeds.
The sinking speed examined by the authors are 0.025, 0.05, 1 and 5 m/d. All these values would be considered as "slow sinking rate" by the community. Yet, the manuscript concludes on the relative contribution of slow and fast sinking particles, which is misleading. It also means that the authors cannot really conclude about the relative role of particles that have sinking rates similar in magnitude or faster than submesoscale motions (i.e. 50-300 m/d).

a) Throughout the text, the authors should be very clear about what they call fast and slow sinking particles. They should also compare the values they use to existing observations of rates. I recommend to see Baker et al 2017 or Riley et al 2016 for example who present in-situ observations of sinking speed and define slow < 20 m/d and fast > 20 m/d. Also note that this nomenclature is consistent with the rates used in ocean biogeochemical models, which usually have a fast sinking rate of 50-200 m/d, and sometimes an additional pool with slow sinking rate of 1-5 m/d.

b) The authors need to justify their choice, discuss the implications of this choice and explain how it informs the current view of the community on POC export. The authors should justify the narrow range of vertical velocity they explore. Prior observation-based studies emphasize the importance of particles sinking at slow rates (< 10 m/d) but also those sinking at very fast rates similar in magnitude to submesoscale vertical motions (200-300 m/d) (e.g. Baker et al 2017, Riley et al 2016, Stuckel et al. 2017b). What are the reasons to look at only very slow sinking particles? Is it because of limitations related to the Stokes Law? Is it because the model is not adapted to look at faster sinking rates.

c) By limiting their study to slow sinking particles, the authors target by design the particles that will be most sensitive to submesoscale dynamics (see Stukel et al) and exclude particles that sink with rates similar or faster than submesoscale motions and that can efficiently export at depth and participate to carbon sequestration. Indeed, submesoscale is largely trapped in the upper ocean and only have a limited impact on export at greater depth (see previous discussions of these effects in Stukel et al, Erikson et al and Resplandy et al). The authors should acknowledge these limitations and discuss their implications.

d) The author could introduce the study by acknowledging up front that slow sinking particles are the particles most impacted by submesoscale (as shown in previous papers) and this is why they are the focus of this paper that explores the sensitivity to size spectrum etc.

2) Remineralization and slower sinking particle contribution.
The choice of some parameter appears arbitrary. I like the fact that the authors sweep the parameter space of the size spectrum ($\xi$ = 2, 3, 4). The choice of this parameteris however key in the conclusion that are drawn (contribution of slower-sinking particles , L557-558).

a) Could you please present the case where the biomass spectrum slope is positive? Same as Figure 8 but with $\xi = 2$ and contrast it with the case of $\xi = 4$?

b) The authors briefly mention that slopes greater than 3 have been observed (L640). Could this discussion be augmented? What observational constraints do we have on the spectrum slope? Do we know if the biomass spectrum slope is positive or negative? Is it likely to change sign with season, biomes, looming conditions etc.? (not necessarily around station PAPA). Bridging your modeling results with available observations would greatly benefit the paper and facilitate the use of your conclusions by the community (please look into prior work to make these links). I strongly encourage you to discuss the implications of this result, what they mean for people measuring particles and export, what they should be looking for in the field?

c) The effect of negative vs positive biomass spectrum slope is translated in your abstract by the rather vague "under specific conditions ..." (L27). Please try to clarify what this means in Layman terms in the abstract.

d) How do your results depend on the remineralization (0.13 d-1) length-scale?

3) Literature Survey and Discussion
It is very concerning that the authors are missing key recent papers published on the subject, including observation-based papers that should be discussed with the author's modeling results. Please find below a list of relevant papers that should be included in introduction and/or discussion. This list is absolutely not exhaustive.

4) The introduction lacks some coherence. The different paragraphs are not clearly connected and the flow is rather tedious. It needs streamlining.

- The second paragraph presents detailed theoretical arguments about particle size spectrum and sinking velocities.
- The third paragraph list some previous results suggesting a role of submesoscale vertical velocities in exporting carbon. Note that numerous recent papers, including observation-based studies, are missing from this list (see below).
- The fourth paragraph describe submesoscale frontogenesis.

- The fifth paragraph repeat the idea that submesoscale can export POC.
- The sixth paragraph add to the first paragraph on our knowledge about particle size and sinking speed. I would merge paragraphs 6 and 2. I would also strongly suggest to mention observations (paragraph 6) and what observed sinking speeds are (please add references for this. E.g. Baker et al 2017, Riley et al 2016 etc.). Then I would present the theoretical arguments (paragraph 2).
- Paragraphs 8 and 7 should be merged with paragraph 7. I suggest to present what the aim of the study is (some of paragraph 8) before mentioning the processes that your model does not resolve (e.g. surface wave in paragraph 7)

5) Abstract
The abstract is long and technical but at the same time vague in presenting the key results to the reader. For example, the following sentences list raw modeling results without hinting at the mechanistic drivers of this response in the model.
"a steeper particle size spectrum increases the relative contribution of smaller slow-sinking particles." "Implementing a remineralization scheme generally decreases the total amount of biomass exported[...]", "Under specific conditions, remineralization processes counter-intuitively enhance the role of slower-sinking particles."

6) The method section is detailed and relatively clear except for section 2.2.3. To clarify this section, the author should give some contextual information about the different metrics (N, B etc.) and why they are presented to the reader.
a) Link to observational constraints on the slope of the size spectrum (see comment #2)?

b) Explain why you examine the dependence between these different metrics, e.g. something like "we explore the sensitivity of X and Y to the particle size spectrum ..... We consider three distributions with slopes of Z, ZZ.." etc.

Minor comments:

7) Can you specify if and how equation 9 differs from the traditional Martin curve?

8) L40: the last sentence of the first paragraph is vague and unnecessary. I would delete it.
9) L 162: Problem with reference Laboratory, 2018?

10) Figures 7 and 8. Please label the size classes on the insert or at least mention the colors-size relationship in the caption.

11) The model data used in this study should be made available as requested by AGU standards.

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