Response to reviewers

1 Response to Editor's comments

2 Comments from Reviewer #1

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.

We carefully consider this question during our study. We deliberately did not rely on a "depth horizon" due to the facts that it is not consistently defined in the literature, and that recent work questions the validity of selecting a depth horizon as an "export depth" , arguing for a dynamics-based definition [Palevsky and Doney, 2018]. This is one of the reasons why the conclusions focus on export fluxes, rather than export *per se*, as the latter would require defining a possibly controversial export horizon.

Additionally, the base of the euphotic zone is preferred in our study for three main reasons:

1. A series of non-trivia important transformations occur in the euphotic zone [e.g., grazing, material packaging, and many others; see Denman and Pena, 1999, for example] that are not captured in our idealized setup, and are far beyond the scope of our study. By seeding particles at the base of the euphotic layer, we focus on the particulate material available to export, where remineralization and sinking are the dominant processes driving vertical fluxes (other than possibly advection. as demonstrated in this study).

2. The main results are expressed in terms of vertical fluxes. Vertical fluxes of particulate matter within the ML are not necessarily relevant to particle export, for the reasons detailed above. Including fluxes in the ML would introduce a bias into our results that is not relevant to our objectives.

3. Seeding over 80 m instead of 10 m would require 8 times as many particles, as it is important to preserve particle density to adequately capture the physical dynamics as small scale (i.e., if particle seeding is too sparse, the presence of submesoscale dynamics would have little impact as no particles would be moving through these features). This would bring the total amount of particles tracked from $\sim$6 millions to $\sim$48 millions, largely limiting the number of experiments we could conduct in a reasonable time frame.

Finally, the small particles released at the base of the euphotic layer do not aim to represent small particles at the time of particle production. It can also represent larger particles at production that went through remineralization in the ML.

<**NOTE**: *Based on this comment, the text has been modified to better justify the choices outlined above. Cite where and how text has been modified.*>

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.

<**NOTE**: *Hmm. this is a very good comment. This is true, for "very large" particles, the biomass flux will be larger than for small particles. 2 things can nuance this: (1) While the flux of biomass is larger for 100 m/day particles, it requires an infinite reservoir of those large particles to be able to relate the larger flux to larger export. Are 100 m/day particles present all year, everywhere? If not, the biomass flux would be larger until the source is exhausted – which is more likely to happen for 100 m/day particles than for smaller particles... (2) This is true, for ξ up to 5 (based on Equation 8). Above that, the biomass flux spectrum would have a negative slope. This is not known to occur in the ocean though. But this value could change to smaller slopes if other relationships are used to relate w_s and B to r. How to address this comment? Including a 100 m/day class will dilute the key conclusions of the study significantly, as the % obtained in Fig 7 and 8 will be much smaller for small particles. It could be addressed by arguing that we focus on the transitional range of sinking velocities?*
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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).

<**AUTHORS**: *This is correct*>

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.

<**NOTE**: *This is a good point. For Fig 7, the timestamp selected to display the results has little impact on the conclusions. A similar plot was produced for time-average biomass fluxes, and the results are only marginally different* ***(plots included in this document)****. The results are therefore displayed for day 25 for consistency, as this is the day used to display results in Fig 8. For fig 8.*>

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

<**AUTHORS**: *Text has been modified accordingly.*>

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.

<**AUTHORS**: *Text has been modified accordingly.*>

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

<**AUTHORS**: *Text has been modified accordingly.*>

3 Comments from Reviewer #2

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).

- 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.

- 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, Stukel 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.

- 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.

- 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.

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).

- Could you please present the case where the biomass spectrum slope is positive? Same as Figure 8 but with $= 2$ and contrast it with the case of $\xi = 4$?
- 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?
- 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.
- How do your results depend on the remineralization (0.13 d-1) length-scale?

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.

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)

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."

Section 2.2

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.

- Link to observational constraints on the slope of the size spectrum (see comment #2)?
- 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:

- Can you specify if and how equation 9 differs from the traditional Martin curve?
- L40: the last sentence of the first paragraph is vague and unnecessary. I would delete it.
- L 162: Problem with reference Laboratory, 2018?
- Figures 7 and 8. Please label the size classes on the insert or at least mention the colors-size relationship in the caption.

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

References. Baker, C.A., Henson, S.A., Cavan, E.L., Giering, S.L.C., Yool, A., Gehlen, M., Belcher, A., Riley, J.S., Smith, H.E.K., Sanders, R., 2017. Slow-sinking particulate organic carbon in the Atlantic Ocean: Magnitude, flux, and potential controls. Global Biogeochemical Cycles 31, 1051-1065. https://doi.org/10.1002/2017GB005638
Boyd, P.W., Claustre, H., Levy, M., Siegel, D.A., Weber, T., 2019. Multi-faceted particle pumps drive carbon sequestration in the ocean. Nature 568, 327-335. https://doi.org/10.1038/s41586-019-1098-2
Erickson, Z.K., Thompson, A.F., 2018. The Seasonality of Physically Driven Export at Submesoscales in the Northeast Atlantic Ocean. Global Biogeochemical Cycles 32. https://doi.org/10.1029/2018GB00592
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Stukel, M.R., Aluwihare, L.I., Barbeau, K.A., Chekalyuk, A.M., Goericke, R., Miller, A.J., Ohman, M.D., Ruacho, A., Song, H., Stephens, B.M., Landry, M.R., 2017a. Mesoscale ocean fronts enhance carbon export due to gravitational sinking and subduction. Proceedings of the National Academy of Sciences 114, 1252-1257. https://doi.org/10.1073/pnas.1609435114
Stukel, M.R., Ducklow, H.W., 2017. Stirring Up the Biological Pump: Vertical Mixing and Carbon Export in the Southern Ocean. Global Biogeochem. Cycles 31, 2017GB005652. https://doi.org/10.1002/2017GB00
Stukel, M.R., Song, H., Goericke, R., Miller, A.J., 2017b. The role of subduction and gravitational sinking in particle export, carbon sequestration, and the remineralization length scale in the California Current Ecosystem: Subduction and sinking particle export in the CCE. Limnology and Oceanography 63, 363-383. https://doi.org/10.1002/lno.10636

4 Comments from Andy Thompson

- Regarding submesoscales during the summer – it would be worth calculating the mixed layer deformation radius during summer. I suspect that it is at or less than your model resolution. This may mean that the real ocean could have a summertime advection of particles just at very small scales (and unlikely to penetrate too deep). Worth considering in your interpretation/discussion of results.

- John Taylor has a nice paper (JPO 2018) where we looked at a similar problem with his LES and in some cases, buoyant particles. It might be worth citing.

- I know computing time is always an issue, but it would be interesting to run these at higher (or lower) resolution to see how the fluxes change. I would be surprised if they were converging – in fact, I am not sure you would ever expect convergence depending on the wavenumber spectrum of w.

References

K.L. Denman and M.A. Pena. A coupled 1-d biological/physical model of the northeast subarctic pacific ocean with iron limitation. *Deep Sea Research Part II: Topical Studies in Oceanography*, 46(11):2877 – 2908, 1999. doi: https://doi.org/10.1016/S0967-0645(99)00087-9.

Hilary I. Palevsky and Scott C. Doney. How Choice of Depth Horizon Influences the Estimated Spatial Patterns and Global Magnitude of Ocean Carbon Export Flux. *Geophysical Research Letters*, 45(9):4171–4179, 2018. doi: 10.1029/2017GL076498.