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Interactive comment on "Variable phytoplankton size distributions reduce the sensitivity of global export flux to climate change" *by* Shirley W. Leung et al.

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For this journal's review process, authors are expected to post a response to all reviewer comments before revising the actual manuscript. Based on these author responses, the editor either invites the authors to submit a revised manuscript or directly rejects the manuscript. We therefore do not yet include a revised manuscript along with answers to the following comments. See more details on the process here: https://www.biogeosciences.net/peer_review/interactive_review_process.html.

In the paragraphs below, all reviewer comments will be italicized, while author responses will be in normal font.

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The authors apply a global biogeochemical model to examine the effect of variable particle (phytoplankton) size distribution on surface and subsurface nutrients, and their mutual feedbacks when nutrients are supplied under different physical forcings. The feedback effect of the (nutrient-dependent) size distribution and subsequent particle sinking and remineralisation dampens the model response to changes in physics. I find this manuscript generally well written. The authors do a great job in explaining the mechanisms involved. In general, the experimental design to disentangle the effects of circulation, ecology and sinking is clear and well justified. Thus, the manuscript provides valuable new insights into a potential negative feedback mechanism in global biogeochemical models. However, I have a few points that I think could be improved with regard to model description and its critical discussion.

We thank the reviewer for their positive comments about our experimental design, and for their constructive criticism below, which we believe will help greatly improve the manuscript.

(1) Model description: I recommend to describe the biogeochemical model, particle sinking and remineralisation in detail (including equations), and also explain its basic assumptions. As far as I understand, the model assumes a power law size distribution of particles at the surface; particles then sink depending on their size, and remineralise with a size independent rate. Therefore, the particle size distribution changes with depth, favouring large particles as depth increases, similar to the 1D approach presented by Kriest and Oschlies (2008). (In fact, there seem to be only small differences between both models, in terms of formulation and results.) Both approaches make quite strong implicit assumptions about constant individual particle properties, which do not change with time or depth. In particular, the models neglect any processes besides sinking and remineralisation that might affect the particle size distribution below the euphotic zone, such as particle breakup, reworking by zooplankton (e.g., flux feeding, formation of fecal pellets), particles becoming more or less porous because of bacterial degradation, etc.. Of course, one cannot address all details and compli-

cations at once especially in global models; but describing the current implicit model assumptions in detail would help the reader to understand how the model works, and what its limitations and merits might be.

We thank the reviewer for pointing out that the model description falls short. The mathematical description of the PRiSM model has been fully laid out in previous references and cannot be repeated in full here. However, we agree that enough information needs to be provided to allow the reader to understand how the model "works", without extensive cross-referencing to previous papers. We will revise the methods section to expand the description of the PRiSM model. Specifically, at line 157 we will insert the following:

"In the PRiSM model (DeVries et al., 2014), particles are produced in the surface euphotic zone (<75m) following a power-law size spectrum, in which log10(particle number density) declines linearly with log10(diameter), and the relative abundance of large and small particles is controlled by the slope of the spectrum on a log-log scale (β). The simulated particle size spectrum then evolves through the water column due to remineralization and size-dependent sinking, which are each parameterized based on empirically derived relationships and observed particle properties. Remineralization is represented by first-order mass loss from each particle, such that particles shrink and sink more slowly with depth, resulting in attenuation of the particle flux. Because smaller, slower-sinking particles reside for longer within any given depth interval, and therefore have more time to remineralize, they are preferentially lost from the particle population over depth, resulting in a flattening of the size spectrum (reduced β) and thus increased average sinking speeds at deeper depths. A constant rate of microbial respiration is used, optimized to fit global in situ phosphate distributions (DeVries et al., 2014). There are therefore no temporal changes in bacterial respiration due to warming, for example, which allows us to isolate changes in export that stem from the PSR feedback alone. While PRiSM has recently been expanded to include temperature and oxygen effects on bacterial respiration and remineralization (Cram et al., 2018), as well

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as to represent particle disaggregation (Bianchi Weber et al., 2018), here we use the original version described in DeVries et al. (2014), which can be solved analytically and has previously been optimized to best fit the global phosphate distribution. The recent expanded versions of PRiSM must be solved numerically, which is less computationally efficient and therefore not suitable for incorporation into our global three dimensional simulations."

Added reference:

Bianchi, D., Weber, T.S., Kiko, R. and Deutsch, C., 2018. Global niche of marine anaerobic metabolisms expanded by particle microenvironments. Nature Geoscience, 11(4), pp.263-268.

(2) Model description: The description of the model and its general setup is somehow unclear about how the phytoplankton size distribution might be related to larger particles, which likely contribute most to mesopelagic and deep particle flux. For example, the work by Kostadinov et al (2009), from which the observed size distribution at the surface is taken, is based on phytoplankton, i.e. extends only to a size of ca. 50 um. However, the present model applies a size range of 20-2000 um (Table S1). Moreover, the model parameters sometimes seem to relate to phytoplankton properties (e.g., the exponent of eta=1.17 relating cell diameter to sinking speed is based on phytoplankton data by Smayda, 1971), whereas other relate more to porous aggregates (e.g., the exponent relating particle mass to size of zeta=1.62; see also Kriest, 2002 http://dx.doi.org/10.1016/S0967-0637(02)00127-9, and citations therein). Again, here it would be useful to present and discuss these basic model assumptions. This is done partly on page 3, yet I think this subsection could be improved (see below, my comments Lines 80ff, 82ff, 95). In summary, I would suggest to more clearly distinguish between phytoplankton and particle size distribution, and to address potential connections between these more comprehensively.

We assume that the particle size distribution slope computed by Kostadinov et al.

(2009) continues to hold for particles larger than those they explicitly compute the slope for. Prior research backs up this assumption (e.g., Durkin et al., 2015). We will add a note about this in addition to the just mentioned citations in the text.

We assume that phytoplankton simply behave as smaller particles. We will make a note of this in the text.

The driving mechanism behind the particle size feedback is the relationship between export production and particle size, which we determine empirically from remote sensing data in our study. Our model setup simply computes particle size based on this empirical relationship to export, and makes no explicit assumption about the root cause of the relationship. We hypothesize that the export/particle size relationship arises from plankton community structure simply because this seems like an intuitive mechanism, and is supported by correlative evidence: large particles and large phytoplankton taxa are both generally more dominant in regions of high productivity and export (Cram et al., 2018; Hirata et al., 2011), and we therefore find it reasonable to assume that large phytoplankton aggregate (either directly or by grazing) into large particles. However, this needn't be true for the particle size feedback to hold. Any other mechanism that gives rise to the observed export/particle-size relationship would give rise to the same feedback. In the revised manuscript, we will be more careful about distinguishing between the explicit assumptions and relationships "baked in" to our model, and the mechanisms that we are hypothesizing give rise to those relationships.

Added reference:

Durkin, C.A., Estapa, M.L. and Buesseler, K.O., 2015. Observations of carbon export by small sinking particles in the upper mesopelagic. Marine Chemistry, 175, pp.72-81.

(3) Experimental setup: To me it is not clear how the circulation was reduced (e.g., Lines 253-254 "To simulate increased water column stratification and reduced vertical exchange due to warming, we uniformly and instantaneously reduce circulation and diffusion rates by 10% throughout the ocean.") - I would appreciate a more in depth

explanation.

Our model uses the Transport Matrix Method, in which all physical fluxes (advection and diffusion) of element X are represented by the matrix-vector product A*X, in which A is the mass-conserving transport matrix that quantifies the mass exchanges between every gridcell in the model. We change circulation rates in an idealized way, simply by multiplying A by a factor of 0.9 (a 10% reduction in circulation rates) or 1.1 (a 10% increase in circulation rates). Therefore, the patterns of circulation remain unchanged, but the absolute exchange rates are scaled up or down. This explanation will be added into the revised manuscript.

(5) Discussion: The model shows a large response and differences between the two setups (with or without PSR) in the equatorial upwelling regions. However, especially models of coarser resolution tend to suffer from an insufficient representation of the equatorial current system, with possible consequences for the representation of nutrients and/or oxygen (e.g., Dietze and Loeptien, 2013, https://doi.org/10.1002/gbc.20029; Duteil et al., 2014, https://doi.org/10.1029/2011GL046877). I would suggest to add some discussion on these potential effects.

Great point. Coarse resolution dynamical models do tend to represent equatorial regions poorly. However, our transport matrix is derived from the observationally-constrained Ocean Circulation Inverse Model, which assimilates passive and transient water mass and ventilation tracers. Thus, even though the resolution does not allow accurate simulation of equatorial currents from a dynamical perspective, the data-assimilation ensures that the net effect of these currents on tracer transport is realistic. The model has been used successfully for simulation of nutrients (DeVries, 2014) and oxygen (DeVries and Weber, 2017), and does not suffer from the equatorial biases often evident in coarse resolution models. Nevertheless, we will point out the potential shortcomings of using a coarse resolution model in the discussion section of the revised manuscript.

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(4) Discussion: Section 3 is named "Results and discussion", yet it almost entirely presents the results. In contrast, Section 4 is named "Conclusions", but partly discusses the results before the background of other works, is quite long and partly repetitive. I would suggest to rename section 3 to "Results", add a "Discussion" section, that extends a bit on the comparison of results obtained here with other model studies and also includes a critical discussion of model processes and properties. The "Conclusions" section could then be shortened and more concise.

We thank the reviewer for this helpful suggestion. We will take this suggestion seriously as we re-write the paper and will make sure that the final version is structured clearly.

Specific comments:

- Lines 35-37: "Where sinking POC fluxes are particularly high, enhanced bacterial breakdown of particles can deplete available oxygen and create hypoxic or even suboxic conditions [...] " - there are many places in the ocean where sinking POC fluxes are high; another necessary condition for the development of OMZs is that supply of oxygen by physical transport is low.

We will change the statement from:

"Where sinking POC fluxes are particularly high, enhanced bacterial breakdown of particles can deplete available oxygen and create hypoxic or even suboxic conditions. . ."

To:

"Where sinking POC fluxes are particularly high **and supply of oxygen via physical transport is low,** enhanced bacterial breakdown of particles can deplete available oxygen and create hypoxic or even suboxic conditions. . ."

- Line 66: Note that there are further global ocean models that address spatial and temporal variation of the size distribution (of marine aggregates) and sinking speed, e.g., Schwinger et al. (2016, www.geosci-model-dev.net/9/2589/2016/) and Niemeyer et al. (2019, https://doi.org/10.5194/bg-16-3095-2019). On a local (1D) scale, even

very complex models of particle transformations have been developed (Jokulsdottir and Archer, 2016, www.geosci-model-dev.net/9/1455/2016/).

We thank the reviewer for bringing these highly relevant and useful studies to our attention. We will add discussion and citation of these studies to our text.

- Lines 80ff: "Large particles tend to exist in the ocean where larger microphytoplankton (>20 um in diameter) are dominant, while relatively small particles tend to exist where smaller picophytoplankton (<2 um in diameter) are dominant (Guidi et al., 2007; Guidi et al., 2008; Guidi et al., 2009). [...]" - The observations by Guidi et al. (2007, 2008), are based on UVP data of large particles (aggregates, fecal pellets, ...), of a size of at least 250 um. Therefore, I don't think that these observations can be used to justify the assumptions about the phytoplankton size distributions made in this paper.

Yes, it is true that UVP data is measuring larger particles. However, our model does include some particles of these sizes, as the reviewer noted above. Furthermore, again, we assume that the particle size distribution slope holds throughout the entire range of particle sizes in the ocean. We will make a note of this in our text. See also our response to the "(2) Model description" comment above.

- Lines 82ff: "The presence of large phytoplankton leads to the generation of larger particles perhaps because large phytoplankton are more likely to form aggregates and be transformed into large fecal pellets by large zooplankton, whereas small phytoplankton are more likely to be degraded by bacteria and consumed by smaller zooplankton (Bopp et al., 2005; Guidi et al., 2007; Guidi et al., 2009; Michaels and Silver, 1988). The exact mechanisms governing the processes by which smaller and larger phytoplankton become smaller and larger particles are not clearly known, however, and is an active area of research." - The global model study by Bopp et al. does not address aggregates; moreover, as a model study it is based on a priori assumptions, and does not provide insight into real in situ mechanisms. As noted above, the study by Guidi et al. (2007) addresses the UVP size range and the study by Michaels et al. is also a

(food web) model. While I tend to agree with the idea that large phytoplankton triggers large sinking particles, I would appreciate a more convincing reasoning why one can extend the phytoplankton size range up to particles 2 millimeters in diameter.

As stated in response to the "Model Description (2)" comment above and repeated here:

"We assume that the particle size distribution slope computed by Kostadinov et al. (2009) continues to hold for particles larger than those they explicitly compute the slope for. Prior research backs up this assumption (e.g., Durkin et al., 2015). We will add a note about this in addition to the just mentioned citations in the text."

Furthermore, Kostadinov et al. (2009)'s particle size distribution slope is available globally and is also temporally and spatially resolved. Thus, this was really the best PSD slope dataset that we could find. In essence, it was what was available. Our method really requires this kind of global data available over long enough timescales and with enough spatiotemporal resolution to compute the necessary correlations.

Additionally, to reiterate our points above, though we do implicitly assume that small phytoplankton = small particles to explain/understand the results of our empirical analyses, our model setup and study in general do not require this relationship to be true. Our model setup simply computes particle size as an empirical function of export. The empirical positive relationship between export and particle size illuminated by our satellite data analysis showed that increasing productivity and export are associated with larger particle sizes. Increasing productivity is in turn associated with larger phytoplankton; thus, based on our empirical analysis, it seems that larger phytoplankton are associated with larger particle sizes. We bring up other studies that find or assume this merely to better explain the mechanistic underpinnings of this result and NOT to say that this must be the case for our model setup to hold. Our empirical analyses could have shown that export and particle size were negatively correlated, for example, in which case, our model would have demonstrated a positive particle size remineralization feedback, rather than a negative one. Thus, our results/findings of a negative particle size remineralization feedback effect hinge entirely on the direction of the empirical relationship between export and particle size, rather than on our own assumptions. We will alter wording/discussion in the paper to clarify this point.

- Line 95: "Past work has also firmly established a strong positive relationship between particle size and sinking speed in the ocean (Alldredge and Gotschalk, 1988; Smayda, 1971) [...]" - The relationship between diameter and sinking speed in Alldredge and Gotschalk (1988) is w=50 d ^0.26, and shows considerable scatter. I would not call this a strong relationship. This weak relationship is possibly because of the fractal and variable nature of aggregates - indeed, single cells show a higher exponent (Smayda, 1971). Again, here I would suggest to more clearly distinguish between aggregates and single phytoplankton cells.

We will change the sentence from:

"Past work has also firmly established a strong positive relationship between particle size and sinking speed in the ocean (Alldredge and Gotschalk, 1988; Smayda, 1971) (although there are exceptions to this rule, particularly in the Southern Ocean – see McDonnell and Buesseler (2010))."

To:

"Past work has also **suggested a positive relationship** between particle size and sinking speed in the ocean (Alldredge and Gotschalk, 1988; Smayda, 1971), although **there appear to be complications and exceptions to these rules (Cael and White, 2020; Laurenceau-Cornec et al., 2019),** particularly in the Southern Ocean (McDonnell and Buesseler, 2010)."

Added references:

Cael, B.B. and White, A.E., 2020. Sinking versus suspended particle size distributions in the North Pacific Subtropical Gyre. Geophysical Research Letters, p.e2020GL087825.

Laurenceau-Cornec, E.C., Le Moigne, F.A., Gallinari, M., Moriceau, B., Toullec, J., Iversen, M.H., Engel, A. and De La Rocha, C.L., 2020. New guidelines for the application of Stokes' models to the sinking velocity of marine aggregates. Limnology and Oceanography, 65(6), pp.1264-1285.

- Line 100: "by a factor of e" - e is 2.718, do you really mean a factor of e?

Yes, this is the e-folding length scale.

- Line 157: "physical relationships between particle size, mass, and sinking velocity" - I don't think that the relationship between particle size, mass and sinking velocity of organic particles is a purely physical one; at least the relationships by Smayda (1971) and Alldredge and Gotschalk (1988) are empirical. I suggest to skip "physical".

We will change the sentence from:

"PRiSM computes particle flux profiles as a function of particle size distribution (β) at the surface, microbial remineralization rates, and physical relationships between particle size, mass, and sinking velocity."

To:

"PRiSM computes particle flux profiles as a function of particle size distribution (β) at the surface, microbial remineralization rates, **and relationships** between particle size, mass, and sinking velocity."

- Line 184-185: "time-mean export" - mean over what time? A year?

We will change the statement from:

"...time-mean normalized export (En,obs) (i.e., absolute export divided by time-mean export at a given grid point)."

To:

“**...**time-mean normalized export (En,obs) (i.e., absolute export divided by time-mean export **calculated over the SeaWiFS period** at a given grid point).”

- Line 483-484: "This implies that global models without the PSR feedback may be overestimating 100-year climate-driven export decreases by ~1.16 times." - What is meant with 1.16 times?

We will change the sentence from:

"Within our model, including these effects reduces the magnitude of predicted 100-year changes in global export production by ~14% when circulation rates are decreased by a conservative 10% (Fig. 5). This implies that global models without the PSR feedback may be overestimating 100-year climate-driven export decreases by ~1.16 times."

To:

"Within our model, including these effects reduces the magnitude of predicted 100-year changes in global export production by ~14% when circulation rates are decreased by a conservative 10% (Fig. 5). This implies that global models without the PSR feedback may be projecting 100-year climate-driven export decreases that are ~1.16 times too large."

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