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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.

Anonymous Referee #2

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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.

(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 complications 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.

(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.

(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.

(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.

(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.

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.

- 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/).

- 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.

- 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.

- 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 dˆ0.26, 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.

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

- 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".

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

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

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