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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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Leung et al. present an interesting study in which they thoroughly analyse the "particle size remineralization" (PSR) feedback in a simple 3D biogeochemical model. The PSR feedback is described by decreased circulation leading to increased nutrient limitation at surface, which in turn leads to plankton communities with smaller sizes producing smaller sinking particles, leading to a slower sinking speed and hence remineralization at shallower depths. This chain of mechanisms increases nutrient concentration at these shallower depths, dampening the decreased nutrient supply to the surface. Their results show that the PSR feedback dampens projected decrease in export production by 14% globally and also details the dampening in different regions. In particular the detailed regional analysis is very informative, suggesting that changes in particle sinking speed in the Southern Ocean have only a small impact on carbon export. Overall I find this an important and interesting study, well written and clearly presented and I recommend a speedy publication after the following comments have been addressed.

We thank the reviewer for their constructive comments and appreciate the positive response to our manuscript.

Main comments:

- I like that the PSR feedback has been thoroughly analyzed, but it should be discussed or mentioned earlier that there are several other processes potentially affecting the nutrient supply to the surface (varying stoichiometry, temperature dependence of remineralization, ballasting, aggregation/fragmentation etc) that are ignored in this study. There is a good discussion of the caveats at the very end in the conclusions, but I think it would help to mention early on that this paper analyses the PSR feedback in isolation of the other feedbacks.

The reviewer makes a good point about structure. We will move this discussion to the introduction and then mention it once more succinctly in the conclusion.

- The proposed PSR feedback has been analysed for carbon export through 100m depth, however I would expect that there is a decrease in POC flux at deeper layers in

the ocean (as the PSR feedback mainly acts in the first few 100m? My interpretation of course). For instance for the question how much carbon is sequestered from the atmosphere or how much food supply is available for mesopelagic organisms, the carbon flux below 100m depth might be of higher relevance. It would be very interesting to see how much the carbon flux at deeper layers is dampened by the PSR feedback.

This is a great point, and something we discussed at length when we were writing this manuscript. We elected to constrain the scope of this manuscript to exploring export, rather than carbon sequestration. An analysis of sequestration is a clear direction for future work, either by our group or by another. We will expand our discussion of what this PSR feedback might mean for carbon sequestration in the conclusion.

The reason why we have focused on export rather than carbon storage/sequestration is because carbon storage responses to changes in biological pump strength will manifest on longer timescales than changes in export, and will likely be small compared to changes driven by anthropogenic CO_2 uptake or solubility. Furthermore, export is important in its own right, and is more directly related to the changes in nutrient cycling explored here.

The reason why we focus on export out of the bottom of the euphotic zone and not carbon flux at some deeper depth is because export, by definition, is a measure of the *total* food supply to subsurface heterotrophic communities, and so the PSR feedback does buffer the productivity of mesopelagic communities as a whole, by damping changes in export. It's true that the feedback has the opposite effect on deeper fluxes (say at 500m), and this results in a vertical re-organization of the food supply, with more food available in the upper mesopelagic and less in the lower mesopelagic. But the net effect is still a buffering of the *total* food supply from the surface ocean.

We will add substantial explanation of this vertical reorganization in our revised manuscript with the aid of a new figure. This figure will contain a repeat of Fig. 7e-f, but show change in 500 m flux (Fig. 1 shown below), rather than export, with the feed-

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back on/off. One noteworthy finding is that it is at about 150 m below the bottom of the euphotic zone that the PSR feedback effect on POC flux changes sign across most ocean regions, so we will also include a global map showing the exact depths at which the PSR feedback turns from negative to positive across all of the ocean regions.

Though we want to keep our main focus on export, which determines the *total* food source to the mesopelagic, we thank the reviewer for pointing out this very cool vertical reorganization! We are excited to add this discussion into the paper and think it will make it quite a bit more interesting.

- There are models participating in CMIP5/CMIP6 that parameterize two different particle size classes with different sinking speeds (I have written a bit more in the line-by-line comments). Is the full particle size spectrum really needed for the PSR feedback, or is it also possible to use just two particle size classes?

This is a great question, but not one we can definitively answer in the current study. We believe that a model like ours that resolves a continuous size spectrum is most likely to accurately capture the feedback, especially given that the export/particle-size relationship can be directly constrained with remotely-sensed spectral observations. Clearly, models with 2 different particle size classes, with different sinking speeds, could qualitatively reproduce the same feedback, but it is not clear whether the magnitude of the feedback would be accurately captured. Furthermore, in the CMIP5 models, the relative proportion of large and small particles is determined by internal model dynamics, and not prescribed empirically as in our model. Their ability to reproduce the feedback would therefore depend on the degree to which the observed relationship between export and particle-size emerges from the ecosystem model. We hope that in future work, our study can provide a baseline against which ESMs can assess their ability to reproduce particle size feedbacks, and we will add a note on this to the discussion section of the revised manuscript.

- I think some aspects of the model should be described in more detail, in particular

the PRiSM component used to estimate particle flux (see line-by-line comments)

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 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.

Line-by-line comments:

L 9: Please write something like "This decline is mainly caused by. . .", as there are other mechanisms projected as well, for instances stronger grazing pressure due to higher temperatures

We will change the statement from:

"This decline is caused by increased stratification..."

To:

"This decline **is mainly** caused by increased stratification..."

L 11: But there are some Earth System Models that simulate changes in remineralization depth, due to e.g. changes in phytoplankton community composition? For instance CESM-BEC, IPSL-PISCES and GFDL models all simulate changes in formation and sinking of particles under climate change. It is true that most models don't have a dynamic particle size spectrum though. (I am also wondering, do the Earth System Models that already have particles with different size classes also predict lower export decrease? But this is probably impossible to disentangle from other effects such as a temperature-dependent remineralization or varying stoichiometry etc.)

Good point. We will revise our language to better specify which ESMs include which

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processes and feedbacks.

Regarding export changes in ESMs that already have particles with different size classes: Based on Fig. 9 in Bopp et al. (2013), we see that having different particle size classes does not seem to be a major determinant of the magnitude of projected export declines. Looking at Fig. 9b in particular, IPSL, CESM, and GFDL model projections span the entire range. As noted in a previous response, simply representing different sized particles does not ensure that a model will adequately represent the particle size feedback described here. They would also need to contain some mechanism that gives rise to the strong relationship between carbon export and particle size that we specify empirically in our model. We will mention this in our Discussion section.

L 45/46 Maybe cite Laufkotter et al. 2015 for drivers of future declines in primary production as well

Good point. An important reference! We will add this citation.

Introduction: I miss a discussion or mentioning of other drivers of sinking speed/particle remineralization, such as particle density/porosity and ballasting, also fragmentation and aggregation which potentially change the particle size over time

Good point. As stated above, some of this information is currently included in the conclusions section, but would be better placed in the introduction. In the revised manuscript, we will move this discussion to the introduction (and specifically mention the roles of density, ballasting, and fragmentation) and then reiterate it more succinctly in the conclusion.

L 67/68 "parameters and processes in most previous models are not constrained by observations of particle size distributions" - Yes! Nice!

Thanks!

L74/75 Are there more recent references for the particle size distribution?

Yes. We will add citations to the following 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.

White, A.E., Letelier, R.M., Whitmire, A.L., Barone, B., Bidigare, R.R., Church, M.J. and Karl, D.M., 2015. Phenology of particle size distributions and primary productivity in the North Pacific subtropical gyre (Station ALOHA). Journal of Geophysical Research: Oceans, 120(11), pp.7381-7399.

Buonassissi, C.J. and Dierssen, H.M., 2010. A regional comparison of particle size distributions and the power law approximation in oceanic and estuarine surface waters. Journal of Geophysical Research: Oceans, 115(C10).

L 99 Just a small comment: I think the "shallowest" can be removed?

We will change the statement from:

"Here we define remineralization depth as the shallowest depth at which POC flux..."

To:

"Here we define remineralization depth as **the depth** at which POC flux..."

L 117 I think the decreased nutrient supply causes the decrease in phytoplankton/particle size, not the decrease in export?

In our model, everything is linked by the empirical function/relationship between phytoplankton/particle size and export. Thus, the causal chain in our model is that the reduced nutrient supply results in reduced export, which (empirically) drives a shift towards smaller particles. In reality, this shift to smaller particles is likely driven by increased dominance of smaller phytoplankton, but this is not explicitly represented in our model, only the export/particle-size relationship is.

The introduction reads slightly redundant to me and could be streamlined a bit, particularly part 1.2 (PSR feedback). However, I acknowledge that other reviewers seem to particularly like it and find it "very clear", so maybe for readers with little background in particle export it is better the way it is.

We appreciate this observation. We will tighten this section, while maintaining the clarity that the other reviewers appreciated.

L 164 Does the plankton not become small plankton, or does the small plankton not produce small particles? Oh I see, plankton community isn't explicitly represented, correct?

Correct, the phytoplankton community isn't explicitly resolved - our model simulates phytoplankton growth (NPP) and organic matter export as a function of temperature, light and nutrients. The size spectrum of sinking particles (β) is then computed based on the empirically derived export/particle-size relationships.

I don't understand where Prism has the microbial respiration rates from, and whether they increase due to temperature in a warmer ocean? Is there an oxygen dependence? Is there aggregation/fragmentation? Can the sinking speed change over depth? Given that these questions are essential to this study, please explain Prism in more detail.

See our response above to the comment that reads "I think some aspects of the model should be described in more detail, in particular the PRiSM component used to estimate particle flux (see line-by-line comments)."

L173/174 wouldn't it make sense to also increase the production of DOC when changing the beta of the particle size spectrum?

In general, the cutoff between DOC and POC is arbitrary (i.e., based on filter sizes). Perhaps there should be more DOC production when particles are smaller (because there would be a relatively greater number of very small particles beneath the minimum filtration size), but we do not have sufficient information that we can use to parameterize this in an empirical way.

Fig 3: That's probably only my personal preference, but I generally prefer mapping data using a log scale, instead of plotting log(data) and using a linear scale.

This preference is likely shared by many. We will change the scale to log rather than plotting log(data) and using a linear scale.

L 233/234 The NPP estimates you are using are significantly biased in the Southern Ocean. This should probably be discussed here, or you could use an NPP estimate that has been specifically created for the Southern Ocean (for instance Johnsson et al. 2013).

We thank the reviewer for pointing this out. At this stage, we cannot repeat our entire suite of analyses using a new NPP estimate, but we will certainly note this limitation of the estimates that we do use.

L254 How sensitive are your results to this simplified representation of future ocean circulation?

This is a great question, and not one that we can definitively answer. Because our model represents circulation using a mass-conserving transport matrix, we can only uniformly scale circulation rates up or down, rather than manipulating the patterns of circulation in more realistic ways. This limitation is already noted in our paper.

However, it is important to point to note that although we use a simplified representation of future changes in ocean circulation, the exact same simplified representation is used in both the PSR feedback-on and feedback-off cases.We are therefore isolating the effects of the particle size feedback from the effects of the circulation change itself. It is therefore not clear that our results would be significantly different if we used a model with a more complex representation of future circulation changes, as long as that model also applied the exact same circulation changes in feedback-on and feedback-off scenarios.

L 282 In light of these "counterintuitive" relationships I think it would be really interesting to use an NPP algorithm that's been developed for the Southern Ocean, as mentioned above (the result might stay the same of course, and I like how it is discussed/explained with the LamBishop findings)

Good point - we will mention this in the text as a suggested direction for future work.

L 346: Is this a conclusion from the correlation between P200m and E or something that is actively diagnosed in the model?

We answer this question and the following L355 question together below.

L 355 ff The considerations up to this point hold for the nutrient-limited low and mid latitudes. But here you discuss a global uniform decrease of circulation rates. I found the jump from regional to global a bit confusing. Wait, I see now that in line 349, eq 3 is meant to hold at any given location, including regions in which eq. 2 does not hold, yes? Is that mathematically sound??

This answer is in response to the above 2 comments.

Equation 2 is intended to provide a simplified interpretation of results in terms of changes in subsurface nutrient concentrations. It reflects the assumption that at steady-state, the export flux out of the euphotic zone must approximately balance the supply of nutrients into the euphotic zone by upwelling. Of course, because this is a simplification of reality, the relationship will never be 100% accurate, if this is what the reviewer means by "mathematically sound." However, it is a reasonable assumption for much of the ocean and is widely used in the biogeochemical literature (e.g. Ducklow et al., 2001; Passow and Carlson, 2012). We note that equations 3-4 are directly derived from Equation 2 via perturbation analysis. They therefore hold precisely to the same degree that Equation 2 holds.

In the revised manuscript, we will note more clearly that Equations 2-4 are only intended to help simplify our interpretations, and are not mathematically equivalent to the full model solution. However, Fig 7 demonstrates that Eqs 2-4 do explain much of the full model behavior, and in doing so allow us to deconvolve and better understand the different mechanisms leading to export changes. Where these equations hold, they allow us to demonstrate how the PSR feedback operates to buffer export under slower circulation (i.e., by increasing nutrient concentrations in the shallow subsurface). We will clarify wording in the text to explain this.

Circulation rates are indeed decreased uniformly across the global ocean ($\Delta w/w_{baseline}$ = -10% in all regions in Equation 4), but the strength of the PSR feedback varies regionally depending on how much P200 changes in that region between the feedback on and off runs (see Equation 4).

Added reference:

Passow, U. and Carlson, C.A., 2012. The biological pump in a high CO_2 world. Marine Ecology Progress Series, 470, pp.249-271.

L 358 "This decrease in P200m is .." - Is this your interpretation or has this been diagnosed in the model?

This is our interpretation, backed up by an understanding of how Southern Ocean nutrient utilization drives low latitude nutrient supply from other model studies (e.g., Sarmiento et al., 2004; Marinov et al., 2006).

We will cite the above 2 studies and change the wording from:

"This decrease in P200m is largely driven by enhanced biological nutrient utilization. . ."

To:

"This decrease in P200m **is likely** largely driven by enhanced biological nutrient utilization. . ."

Added references:

Sarmiento, J.L., Gruber, N., Brzezinski, M.A. and Dunne, J.P., 2004. High-latitude controls of thermocline nutrients and low latitude biological productivity. Nature, 427(6969), pp.56-60.

Marinov, I., Gnanadesikan, A., Toggweiler, J.R. and Sarmiento, J.L., 2006. The southern ocean biogeochemical divide. Nature, 441(7096), pp.964-967.

L 404 - Is eq. 2 a close approximation in the low latitudes or globally? Is it possible to show on a map how well Eq 2 holds regionally?

Fig. 7g-h show how well this approximation holds both zonally and regionally. The orange lines/bars show feedback strength as approximated from the right hand side of Equation 4, which is directly derived from Equation 2. The blue lines/bars show actual feedback strength calculated from changes in export (left hand side of Equation 4). The closer the orange lines/bars are to the blue lines/bars, the better the approximation in Equation 2. We will add more explanation of this in the text.

L 484 is this a typo “1.16 times”? Otherwise, I don’t understand, why 1.16 times? 1 - 16%? But I wouldn’t understand that, either. Sorry if I am missing something obvious.

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.**"

L 485 I believe the Pisces models used in the Bopp et al. study implements two different particle sizes, so it's possible that the PSR feedback is at least partly included? Please check

Yes, this is true. This is mentioned in Lines 64-65, but we will clarify this and add more details.

L 507/508 "PSR feedback strength remains relatively constant whether circulation rates are increased/decreased by 10% or 50%" - That is very surprising to me. Why would that be? Are you reaching a maximum/minimum particle size distribution such that a further decrease in nutrient supply does not affect the size distribution anymore? Please discuss this a bit more. I also think this sensitivity test should be mentioned in the results already.

Remember that the PSR feedback strength is calculated as the relative difference in projected export decrease between the feedback on and off cases, which are both run with exactly the same circulation rate change. It is this PSR feedback strength, NOT the amount of PSD slope (β) change, that stays the same when you amplify circulation rate changes. Thus, even though β changes more in the 50% decreased circulation case when the feedback is on, the PSR feedback strength is relatively unchanged from the 10% decreased circulation case. In other words, the percentage difference in projected export change between the PSR feedback on and off cases is constant even as circulation rates slow more. To be sure, however, absolute export decreases are indeed greater in both the feedback on and off cases under the 50% reduced circulation rates. We will add this explanation into the text.

L 528 I think the study by Briggs et al. 2020 is good recent reference for fragmentation

Good point. We will add this reference.

L 545 The effect of temperature increases and oxygen decreases on future carbon export has been analyzed in Laufkotter et al 2017, maybe good to cite here

Good point. We will add this reference.

Conclusion: If I understand everything correctly, your results suggest that changes in particle size/sinking speed in the Southern Ocean have only a small impact on carbon export regionally and globally. This could be interpreted as a justification for modelers to give implementation of particle size a low priority in this region - would you agree?

Very interesting point! I suppose this would be true if modelers only really cared about better resolving the effects of this particular feedback in the Southern Ocean. However, correctly modeling particle size in this region may be important for accurate estimates of absolute export. Indeed some of our earlier work shows that large particles in cold temperatures in the Southern Ocean contribute to particularly high carbon sequestration in this region (Cram et al., 2018). Thus, I think modelers/observationalists should keep working on improving particle size representations in the Southern Ocean.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-156, 2020.

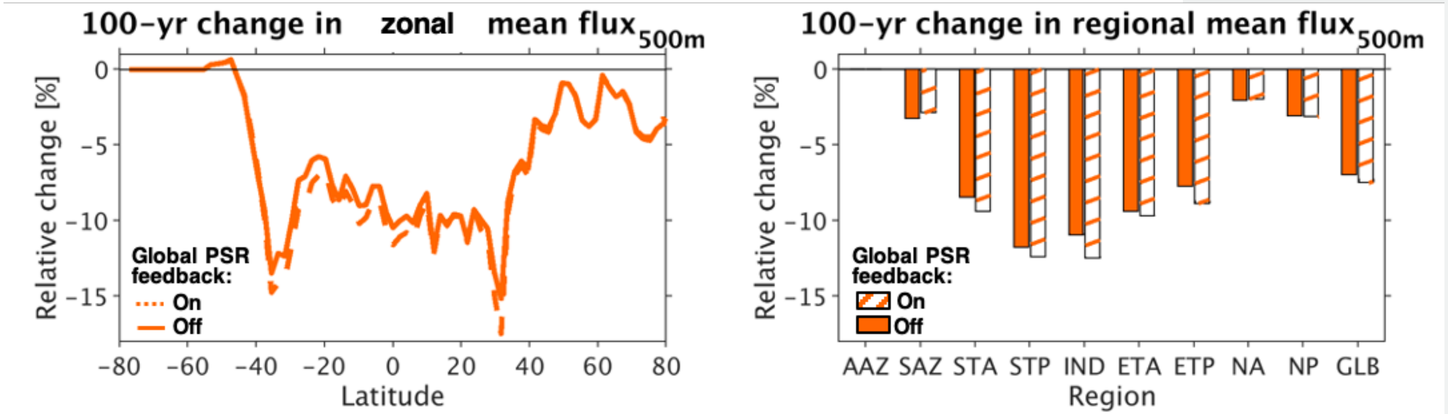

Fig. 1. Particulate organic carbon fluxes at 500 m with and without the particle size remineralization feedback

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