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Interactive comment on "Microstructure and composition of marine aggregates as co-determinants for vertical particulate organic carbon transfer in the global ocean" *by* Joeran Maerz et al.

Anonymous Referee #1

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1 General comments

The authors present a new sinking scheme for marine aggregates that takes into account selected important effects of aggregate microstructure (such as estimates of porosity, TEP content, and density based on the aggregate composition, which is derived from HAMOCC tracer concentrations) and of the resulting estimated aggregate size distribution. The authors achieve this without the use of an explicit aggregation model, and without introducing different particle size classes, thereby keeping the

scheme very affordable, affordable enough for long-term global carbon cycle modelling. Because several of the incorporated mechanisms that affect the sinking of particulate carbon in the ocean were previously neglected in global carbon cycle models, the presented work is a welcome contribution to the field and should be published.

While the presented sensitivity experiments with respect to selected parameters of the sinking scheme seem well-placed in the manuscript, I would suggest to reconsider wether the CO_2-sensitivity experiments would be better-placed in a separate manuscript, 1) given the length of the manuscript, 2) given that the title does at least not explicitly reflect those results, and 3) given some inconsistencies compared to atmosphere–ocean CO_2 flux observations described below that may be better addressed in more detail in a separate manuscript, specifically aiming at the role of aggregate and sinking speed changes in response to greenhouse gas emissions and climate change.

The manuscript provides a large amount of sinking-relevant background information that is interesting on its own, and necessary to understand the (incorporated or neglected) processes in the new sinking scheme. The description of the new sinking scheme itself is also very detailed, making the results reproducible – also with the help of the very well-documented supplementary material. This, combined with the presented extensive analysis and selected parameter sensitivity experiments, understandably leads to a rather long manuscript. However, I do believe that the manuscript can still be shortened and readability can be improved by clarifying / simplifying some formulations (see comments on selected sentences below).

Some additional minor comments to improve/clarify the manuscript prior to publication, as well as some typos are listed below.

2 Minor comments

Abstract, line 10: I would suggest to replace *"which has been recently constrained by"* by *"as recently constrained by"*, to clarify that this particular latitudinal pattern of POC transfer efficiency is reproduced. I think it would be appropriate to mention (here or at least later on page 3 around lines 6-12 or 29-31) that previous estimates of transfer efficiency showed an opposing latitudinal pattern (Henson et al. 2012).

Abstract, lines 14-16: Please rephrase. In standalone runs *with* rising carbon dioxide... M^4AGO only alters the *simulated* fluxes. Sentences could maybe also be shortened, e.g.: *Using M^4AGO in standalone runs with prescribed rising CO_2 concentrations (without climate feedback) leads to higher CO_2 uptake in the Southern Ocean, and to lower CO_2 uptake in the subtropical gyres compared to the standard run, while the global oceanic CO_2 uptake remains the same.*

Abstract, lines 12-13: Please rephrase / clarify. Are temperature effects contributing ("driving factor") to the simulated transfer efficiency pattern? Wouldn't at least the temperature effect on viscosity counteract the simulated pattern? Or does this refer to the newly introduced temperature-dependent remineralization of POC, which, if I understand correctly, least counteracts the high sinking speeds in the high latitudes (countours in Fig. 9b)?

Page 2, line 17, "The sinking velocity of aggregates is primarily determined by their size." I understand that aggregate size does matter, but is it really the main factor? Reference? Even very large aggregates can be rather buoyant (e.g., Riebesell 1992).

Page 3, line 15: Please replace "while ignoring" with "while neglecting" (the effects are still discussed).

Page 3, lines 29-31: As mentioned above, I would point out that Henson et al. (2012) suggested an opposing pattern. Would it be possible to reproduce also this opposing pattern with M^4AGO? I think a brief discussion of this issue would be interesting –

potentially regarding the presented sensitivity experiment with smaller diatom frustules showing much lower transfer efficiencies in high latitudes?

Page 4, line 28: Please consider including the equation for opal dissolution explicitly, also to better understand the given dissolution rates in Table 1. As far as I understand / looking at the HAMOCC code, the opal dissolution rate given in Table 1 corresponds to 7°C?

Page 5, line 9-11: Please rephrase; e.g., *... is eventually computed from a number distribution that is truncated at the minimum and maximum aggregate diameters ..., and expressions for the mass and sinking velocity of aggregates of a particular diameter:*

Page 5, line 26: It would be helpful to define the diameter *d* of an aggregate more accurately here. For example, is the diameter of an aggregate with d_f=1 (i.e., a chain) just given by its length?

Page 6, line 7: Please move reference to "well known" Stokes (1851) here.

Page 6, line 26: I am a little lost here. What is the motivation for this paragraph? What is *n*? And why is that equation only true for $n \neq n_p$?

Page 6, line 27: Do I understand correctly that *"same heterogeneity in a size spectrum"* means that all aggregates of a particular composition / in a particular grid cell are assumed to have the same heterogeneity/microstructure/d_p (for all aggregate diameters d)?

Page 7, line 9: Please define $V_{p,i}$ more accurately; volume of the primary particles per unit volume of sea water?

Page 7, line 6-8: Maybe shorten to *"... tracer C_i, namely detritus, opal, calcite and dust."*

Page 7, line 15: Add *"Multiplication by the volume of the mean primary particle then yields..."*; helps the reader / I didn't see this at first.

Page 8, line 3: Please add reference to traditional scaling relationship.

Page 9, line 1: For consistency, if j=0..3 here, also add definitions of a/b$_{j=0}$ in last paragraph of page 8.

Page 9, line 13: It is not clear to me what *"dynamic steady state"* means.

Page 9, line 20-21: I am wondering why the assumption of Reynolds numbers between 0.1 and 10 here is okay, while the authors go through the extra trouble of deriving expressions for the sinking speed for even smaller and even larger Reynolds numbers in Section 2.2.2. Is there a reason for this?

Page 10, line 6: Please clarify / see above comment on "in a size spectrum": *... as one value across all aggregate sizes.*?

Page 11, line 10: Shouldn't it read: *When detritus from the frustules is remineralized, it is replaced...*?

Page 12, line 2: Please rephrase: *...thus decrease the fractal dimension of aggregates, and ii) ...*

Page 15, Table 1: Is it correct that the applied opal dissolution rate in the setup with M^{4}AGO is larger than that in the standard model setup? What is the resulting effect of the temperature-dependency here? Wouldn't the larger remineralization rate combined with the slower opal sinking speeds in the euphotic zone lead to very high opal production, and consequently to very low calcite production?

Page 16, line 7: I am not sure if I understand correctly: Is d$_{p,calc}$ chosen particularly small to avoid an overestimate of the volumetric density effect? If so, for clarity, *while accounting for* could be rephrased: *We set d$_{p,calc}$ ..., to account for ....*

Page 16, line 24: Sentence unclear to me; *...distinction between parameter tuning and model evaluation, when...* (?)

Page 16, line 33: Please rephrase: Since the adaptation of the sinking velocity *and thus*

of the transfer efficiency to the remineralization and dissolution rates occurs within a few years, parameter variations aiming at a quantitative agreement with the transfer efficiency of Weber et al. were feasible. (?)

Page 17, lines 8-13: Move to results section / next paragraph?

Page 17, lines 12-13: The annual mean of only one year seems rather short. Have you checked how sensitive your results are with respect to interannual variability due to, e.g., ENSO or deepwater formation variability?

Page 18, lines 1-3: Please rephrase / correct sentence structure (*"features"* can not refer to *"In the M^{4}AGO run"*).

Page 18, lines 3-4: It is difficult to say from Fig. 4 wether the use of M^{4}AGO really leads to smaller latitudinal variability, since the minima and the global mean p-ratio also seem lower than in the standard run. Maybe remove this statement or double-check?

Page 20, line 8: This is only shown in Fig. 7a, not in Fig. 7d.

Page 21, Figure 7 (and page 26, Fig. 9): It would be helpful to show (or at least describe) the location of WOA transect P16.

Page 23, line 7: Sentence unclear to me: ... we neglect this effect *versus*? Maybe delete *versus the lower primary particle binding forces*? What are those forces?

Page 23, line 8: "we neglect... and kept" For clarity, I would suggest to consistently stick to present-tense for the work performed for this study, and to past tense for previous results.

Page 25, line 26: Shouldn't it read *"...decay to 1/e of its initial value, ..."*?

Page 27, line 9 / Figure 9: The lower remineralization rates described here are hard to see in Figure 9b, also due to the missing label on the -30% (?) contour; maybe smaller contour intervals would help.

Page 27, lines 29-30: Maybe clearer: *The relative contributions provide information about the main driving factors for local sinking speed deviations from the global mean.*

Page 28, lines 23-24: Please rephrase / clarify sentence; e.g.: *M⁴AGO* thus likely underestimates the spatial variability *and* relative contribution *of b* to w_s.

Page 30, lines 22-23: Unclear sentence structure. Maybe: *Similar to POC fluxes, opal fluxes exhibit shorter RLSs in ..., while they exceed the standard RLSs in ...*

Page 30, line 27: "... fluxes are generally small." Is this true for both model versions? Not shown here, or is it?

Page 30, lines 28-29: Please add sedimentation flux in standard model for comparison.

Page 31, line 7: "The M^4AGO run represents the PIC/POC fluxes equally well as the standard run." It would be interesting to know how well that is.

Page 31, lines 8 and 11: "... the scatter around the 1:1 line is reduced ..." (line 8) At least for some regions, e.g. for the Sub Antarctic Zone, the points are not really scattered around the 1:1 line. But I agree with the view that M^4AGO reduces the variability in the POC/Si ratio (line 11). Isnt't this reduced variability / compression in the POC/Si fluxes expected, because the variability of the fluxes is only due to the variability of the POC/Si concentrations in M^4AGO (POC and opal sink at the same speed), while in the standard model, varibility is also introduced due to differences between Si- and POC-sinking speeds?

Page 32, Figure 12: Do I understand correctly that each dot in the figure is a generated monthly mean data point, compared to the respective location and monthly mean of the last year in the model run?

Page 33, line 34: ... and the North American Westcoast (?)

Page 35, lines 20-22 / page 36, Figure 14a: If negative fluxes really do represent a net-CO_2 uptake by the ocean in Fig. 14a, as stated in the caption, the southern hemisphere

ocean acts as a net sink for atmospheric CO_2 (and not a source), and the northern hemisphere ocean acts as a net source (not a sink). Consequently, the oceanic CO_2 transport would be from south to north.

Irrespective of the sign / flux direction, these results are in stark contrast to CO_2 flux observations of net zonal mean outgassing at the Equator and net ocean CO_2 uptake in mid-latitudes (e.g., Figure 14 in Takahashi et al. 2019). Maybe this is just due to a plotting error in Figure 14?

I also am not sure if I understand the units in Figure 14a. Does the left axis show the net sea–air CO_2 flux accumulated over the respective 1° latitude band? If that is true, the values seem very large. I am guessing from Fig. 14a that the ocean CO_2 uptake accumulated in the southern hemisphere would then amount to around 0.2 GtC/yr/deg*60deg≈12 GtC/yr, which is an order of magnitude larger than the observed net uptake by the southern hemisphere ocean (south of 14°S) of about 1.1 GtC/yr.

Page 35, lines 22-24: "In the simulation with M⁴AGO, a stronger CO_2 uptake in the region ... coincides with ... increased transfer efficiency" This is a very interesting point; does it still hold despite the (to my understanding) erroneous Figure 14a? To me it is surprising that the CO_2 fluxes do *not* differ more, despite the very different transfer efficiencies. Why do the CO_2 fluxes hardly differ south of, say, 55°S, where the transfer efficiency difference is largest? Why is there hardly an effect in the Arctic Ocean?

Page 39, line 7: Sentence structure. *... body size decreases with increasing water temperature. And increasing water temperature has been suggested to ...* (?)

Page 39, line 10: Does *"... such eco-physiological responses ..."* refer to the primary particle size change, or to other effects?

Page 39, line 29: ... increases the phosphate concentrations in the subtropical gyres *by up to* 50% (?)

Page 40, line 1-2: Please rephrase (phosphate increases phosphate concentrations...), e.g. by: *...phosphate ... populates ... and reaches the subtropical gyres.*

Page 41, lines 21-29: This paragraph, describing the main implications of this study, is not formulated very clearly. *"Our findings ... suggest a number of implications."* Number=2, according to later "first" and "second"? Please rephrase second sentence. E.g., *First, the finding that the size ... is a potential contributor to high sinking speeds suggests that the ballast hypothesis needs to be extended to a size-and-ballast hypothesis.* What does *"it requires"* in line 23 refer to?

Page 43, line 9: As far as I understand β is not prescribed in the standard run, but only the sinking speed, i.e., β still depends on the remineralization (which varies with temperature and oxygen concentrations). How do you get to the value of β=1?

3 Typos

Page 2, line 32: Prim*ary* / fundamental(?) determining factor*s*?

Page 3, line 29: ...benefits from *an* order of...

Page 4, line 23: Bar over w_s meaning global mean / annual mean?

Page 4, line 27: *The* opal dissolution rate...

Page 10, line 27: ... enhance *the* sinking velocity...

Page 18, line 7: ... which also lead (plural)

Page 18, line 8: ... from either satellite *data*, in situ observations, or models lead to partly contrasting pattern*s* (add "data" and plural "s")

Page 19, line 5: Both model simulations show *a* similar pattern (add "a")

Page 22, line 11: Use *"By contrast"* rather than *"In turn"*?

Page 22, line 13: ... during *the* aggregates*'s* descent. (?)

Page 23, line 3: It is likely that (no comma)

Page 23, line 17: linear*ly* increasing

Page 23, line 30: ...from *the* relationship... and *the* transfer efficiency.

Page 24, line 5: M^{4}AGO posses*es*...

Page 24, line 11: ...allows *to more reliably constrain POC transfer efficiency*

Page 25, line 5: exten*t*

Page 25, line 30: ... in surface waters and *the* upper mesopelagic zone...

Page 25, lines 32-33: ... the RLSs... *are* similar or *slightly* (?) longer, or *smaller* again in ... The longer RLS*s* in the mesopelagic zone... (plural)

Page 27, line 16: In summary, *the* temperature-dependence...induce*s*...

Page 27, line 28: ...X$_{i,z}$ *is* (not as)

Page 28, line 20: ... given the general importance of the microstructure....

Page 35, line 29: ... in M^{4}AGO on *the* CO_2 uptake ...

Page 39, line 34: one *in the* too much; thus lead*s* to (singular)

Page 40, line 13: grazing th*r*ough zooplankton

Page 43, line 7: underpins *the* previously...

Page 50, line 33: initials for Núñez-Riboni

Page 53, line 4: Aiko Voigt (not Vogt?)

Page 53, line 19: please check reference / entry missing?

Page 54, line 27: Ocean-Atmosphere

Page 55, line 11: please check / C. R. Geoscience?

References

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Riebesell, U.:The formation of large marine snow and its sustained residence in surface waters, Limnology and Oceanography, 37(1), pp. 63–76, http://doi.org/10.4319/lo.1992.37.1.0063, 1992.

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