1 General remark

The reply is structured as follows:

- Referee comment

  ⇒ Authors reply

  → Modification(s) in the manuscript. "old" → "new"

2 Reply to Referee #1

2.1 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 CO2 -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 CO2 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.

⇒ We thank the reviewer for her/his comprehensive, constructive and positive review. With respect to the section on atmosphere-ocean carbon dioxide fluxes (Sect. 3.8 Regional CO_2 uptake), we admit having used an erroneous y-label for Fig. 14 a. Indeed, it is $\mathrm{Gt\,C\,yr^{-1}}$ for the cumulative zonal CO_2 fluxes. We apologize for the confusion. Apart from that, we are confident that the results are in agreement with present knowledge, which we comment on below. Since the CO_2 fluxes are of clear interest in an ESM framework and a benchmark for the development of such a comprehensive aggregate-representing model component, we decided to keep this present section. For the sake of clarity, we changed the section title.

→ Changed the unit (see Fig.1).
Section Title: "Regional CO_2 uptake" → "Regional CO_2 fluxes"

2.2 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).

⇒ Thanks for your suggestions. We did the replacement accordingly. In addition, we now mention the opposing pattern on page 3, line 30, see also below.

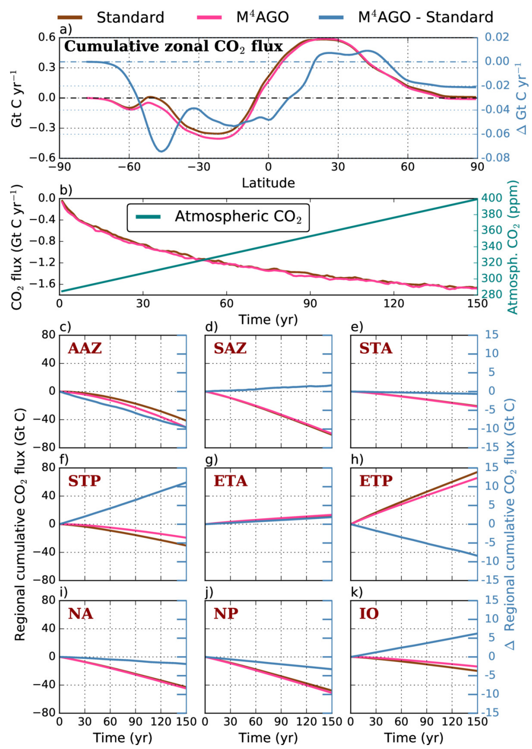

Figure 1: Units from manuscript Fig. 14 a corrected ("Gt C yr^{-1} deg^{-1}" → "Gt C yr^{-1}").

→ "which has been recently constrained by" → "as recently constrained by",
"...more reliable than previous estimates (e.g. Henson et al., 2012; Marsay et al., 2015)" → "...more reliable than previous estimates with partly opposing latitudinal pattern (e.g. Henson et al., 2012; Marsay et al., 2015)"

- Abstract, lines 14-16: Please rephrase. In standalone runs with rising carbon dioxide... M^{4}AGO only alters the simulated fluxes. Sentences could maybe also be shortened, e.g.: Using M^{4}AGO in standalone runs with prescribed rising CO2 concentrations (with-out climate feedback) leads to higher CO2 uptake in the Southern Ocean, and to lower CO2 uptake in the subtropical gyres compared to the standard run, while the global oceanic CO2 uptake remains the same.

⇒ Rephrased.

→ "In ocean standalone runs and rising carbon dioxide (CO_2) without CO_2 climate feedback, M^{4}AGO alters the regional ocean-atmosphere CO_2 fluxes compared to the standard model." → "Prescribing rising carbon dioxide (CO_2) concentrations in standalone runs (without climate feedback), M^{4}AGO alters the regional ocean atmosphere CO_2 fluxes compared to the standard model."

- 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)?

⇒ Thanks. We referred here to the temperature effect on remineralization. We clarified it.

→ "a driving factor" → "a driving factor for remineralization"

- 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).

⇒ This is an interesting comment and we realize, also by the same comment of reviewer #2, that there seems to be much confusion about the controlling factors for sinking velocity, which deserves a publication on its own (being in progress). We want to emphasize here that we clearly state in the follow-up sentence that structure and composition of aggregates regulate the excess density and can thus have a high impact on sinking velocity (we now provide a reference for it). Nevertheless, we would here argue from the mathematical perspective. For simplicity and neglecting the changing drag coefficient for particles with higher Reynolds particle numbers, let's consider the Stokes sinking velocity for low particle Reynolds numbers:

$$w_s(d, \rho_f, \ldots) = \frac{1}{18\,\mu}\,(\rho_f - \rho)\,g\,d^2 \qquad (1)$$

where d is the diameter, μ the molecular dynamic viscosity, ρ_f the aggregate density, ρ is the density of the ambient fluid, and g is the gravitational acceleration constant. It is obvious that $w_s \propto (\rho_f - \rho)$ and $w_s \propto d^2$. Hence, sinking velocity only linearly increases with aggregate density, while it increases with a power law relationship of the diameter. This suggests that size is indeed the primary factor controlling sinking velocity. If we consider the fractal scaling relationship for excess density (Eq. (5) and (8) in our manuscript), this clarity becomes blurred, because the aggregate excess density is itself size-dependent. However, if we further consider that natural aggregate size ranges over more than an order of magnitude (from sizes of about $0.45 \cdot 10^{-6}$ m, which is operationally defined by typical filter pore sizes for POM filtration, to size of $O(10^{-2}\,\mathrm{m})$), while aggregate excess density $(\rho_f - \rho)$ typically ranges only between zero (neutrally buoyant) and $O(100\,\mathrm{kg\,m^{-3}})$, it is obvious that size is the dominant factor (for non-neutrally buoyant aggregates), while, as we clearly state, excess density can entail high variability of sinking velocity. This is also, what e. g. Iversen & Robert (2015)[1] imply, when writing '2- to 3-fold higher **size-specific** sinking velocities' for mineral ballasted aggregates.

→ We add the reference Iversen & Robert 2015 to the follow-up sentence: "…entail high variability of excess density and thus sinking speed of aggregates (Iversen & Robert, 2015)"

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

⇒ Thanks.

→ Changed.

[1] Iversen & Robert 2015: *Ballasting effects of smectite on aggregate formation and export from a natural plankton community.* Marine Chemistry 175, 18 - 27.

- 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^{4}AGO? 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?

  ⇒ As stated above, we now mention the opposing latitudinal pattern explicitly. From our present knowledge about the model responses, the pattern proposed by Henson et al. 2012 could be likely reproduced by applying unreasonable parameter values. However, an investigation of this question would require multiple model simulations, which is a computationally costly task and out of the scope of our manuscript. We therefore only provide reasons to relate our model results to Weber et al. 2016 (an order of magnitude more phosphate than direct flux observations, which makes the transfer efficiency calculations of Weber et al. more reliable), and don't discuss the pattern proposed by Henson et al. intensively, which may be a future work.

  → Changed as described above. We now mention the opposing pattern.

- 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?

  ⇒ The reviewer is right, and we agree that this information is useful. We hence provide the equation. However, we believe, it is better placed on p. 12, l18, where the new Q_{10}-dependent remineralization has been introduced. While doing so, we realized that we haven't introduced the symbols r_{opal}, T, T_{ref} and r_{POC} which we additionally added.

  → Added after Eq. (30):
  "where r_{opal} is the opal dissolution rate at the reference water temperature $T_{\mathrm{ref,opal}}$ and T is the ambient water temperature."

"In the standard version, we remain with the former linearly temperature-dependent opal dissolution (Ragueneau et al., 2000, Segschneider and Bendtsen, 2013)" $\rightarrow$ "In the standard version, we remain with the former linearly temperature-dependent opal dissolution ($\partial_t[\text{opal}] = -r_{\text{opal}}\,(0.1\,(T+3))\,[\text{opal}]$) (Ragueneau et al., 2000, Segschneider and Bendtsen, 2013)"

"... where K_{O_2} is the half saturation constant in Michaelis-Menten kinetics" $\rightarrow$ "... where K_{O_2} is the half saturation constant in Michaelis-Menten kinetics, and r_{POC} is the remineralization rate at reference temperature $T_{\text{ref,POC}}$

- 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:

  $\Rightarrow$ Thanks. Rephrased accordingly and accounted for the reviewers #2 comment on integration differential.

  $\rightarrow$ "... determined by a truncated number distribution, Eq. (2), through the minimum and maximum aggregates sizes, d_{min} and d_{max}, respectively, the aggregate mass, $m(d)$, and the sinking velocity of single aggregates, $w_s(d)$" $\rightarrow$ "... computed from the number distribution, Eq. (2), that is truncated at the minimum and maximum aggregate sizes, d_{min} and d_{max}, respectively, and expressions for the aggregate mass, $m(d)$, and the sinking velocity of aggregates, $w_s(d)$, of a particular diameter, d. Integration over the aggregate size spectrum yields $\langle w_s \rangle$,"

- 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 df =1 (i.e., a chain) just given by its length?

  $\Rightarrow$ Yes. Adding a sub-clause.

  $\rightarrow$ A $d_f = 1$ would depict a chain of aggregate constituents, where the length equals the aggregate diameter, ... "

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

  $\Rightarrow$ Ok. Done.

  $\rightarrow$ Moved the reference.

- 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 $n \neq n_p$?

  $\Rightarrow$ We here derive the mean primary particle size, based on the encapsulated solid volumes of individual, poly-sized primary particles inside the fractal aggregate, to conserve the total solid volume and thus the porosity. This comes at the cost that the theoretical number of primary particles of mean primary particle diameter $\langle d_\mathrm{p} \rangle$ is not necessarily the same as the number of individual primary particles (which would only be the case for mono-sized primary particles). This, however, is negligible for the calculations that follow. We now give some more explanation that clarifies the issue.

  $\rightarrow$ "while $n \cdot \langle d_p \rangle^3 = \sum_i n_i \, d_{p,i}^3$ with $n \neq \sum_i n_i$, and thus the porosity of the aggregate is unimpaired." $\rightarrow$ "and thus the porosity of the aggregate is unimpaired, while the calculation does not presume equal number of mean, n, and individual primary particles, $\sum_i n_i$, (hence, $n \cdot \langle d_p \rangle^3 = \sum_i n_i \, d_{p,i}^3$ with $n \neq \sum_i n_i$ for poly-sized primary particles), which is negligible in the following as we don't consider n any further."

- 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/dp (for all aggregate diameters d)?

  $\Rightarrow$ Yes.

  $\rightarrow$ Seems, as there is no change needed. Remained.

- Page 7, line 9: Please define Vp,i more accurately; volume of the primary particles per unit volume of sea water?

⇒ $V_{p,i} = \frac{1}{6}\,\pi\,d_{p,i}^3$ is the individual primary particle volume. We clarified it.

→ "$V_{p,i}$" → "$V_{p,i} = \frac{1}{6}\,\pi\,d_{p,i}^3$"

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

⇒ Thanks, but we remained with the extra sentence, since we believe it is an important information that shouldn't be given in a sub-clause.

→ Remained.

- 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.

⇒ Changed.

→ "the mass of a mean primary particle can be written as" → "multiplication by the volume of the mean primary particle then yields the mass of a mean primary particle"

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

⇒ Added the previously given references.

→ added: (Logan and Wilkinson, 1990; Kranenburg, 1994)

- Page 9, line 1: For consistency, if j=0..3 here, also add definitions of a/bj=0 in last paragraph of page 8.

⇒ We added a sub-clause at the end of the whole paragraph, p.9, l.7, since the application of $a_{j=0}$ and $b_{j=0}$ is only useful in the context of how the lower integration boundary of the mean sinking velocity is defined. Further, we introduce $a_{j=0} = b_{j=0} = 1$, since dividing by zero in case of $a_{j=0} = b_{j=0} = 0$ is not defined.

→ is the maximum diameter of aggregate, and by applying $a_{j=0} = b_{j=0} = 1$, the lower integration boundary equals the mean primary particle diameter

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

  ⇒ We extended the sentence to specify that the dynamic steady state is between aggregation and fragmentation

  → "Instead of modeling the processes of aggregation and fragmentation explicitly or prescribing b, we assume dynamic steady state for the slope of the number distribution" → "Instead of modeling the processes of aggregation and fragmentation explicitly or prescribing b, we assume dynamic steady state between aggregation and fragmentation to describe the slope of the number distribution."

- 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?

  ⇒ Thanks for pointing out the lacking information. Since we are aware of and discuss the limitations of the dynamic steady state size distribution, we here avoided extra complexity where little benefit as compared to an explicit representation of the dynamic size distribution is expected (see Sect. 3.10: Current limitations of M^{4}AGO, where we discuss the limitations of our current approach versus an explicit representation of a dynamic size distribution). We therefore simplify at this point and remain with text.
  With regards to the drag formulation: in a previous model version of M^{4}AGO, we applied the simple Stokes sinking velocity ($c_D = 24/Re_p$) and disregarded the restriction to $Re_p < 0.1$, with similar well results for the transfer efficiency, but clearly underestimated the maximum diameter of aggregates and thus the represented size range of aggregates. For future applications of M^{4}AGO, this can be of relevance. In order to point out this advantage, we add a sentence in the previous paragraph (p.8,l.24) and remove sub-clause on p.9, l.4

→ "We approximate this representation by" → "This drag representation leads to smaller settling velocities for large aggregates than the classical Stokes drag ($c_D = 24/Re_p$). Hence, aggregates can grow larger, until they reach the globally fixed critical Re_p for fragmentation, Re_{crit}, which leads to a more realistic representation of the size range of aggregates. We approximate the White drag representation by"
Removed: "where Re_{crit} is the globally fixed critical Re_{p} for fragmentation."

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

⇒ Modified.

→ "as one value across a particle size spectra" → 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...?

⇒ No, since there can be more detritus available than needed to fill the void.

→ Remained.

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

⇒ Done.

→ "thus fractal dimension of aggregates is small" → "thus decrease the fractal dimension of aggregates"

- 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?

⇒ The reviewer is right that the dissolution rate is larger in M^4AGO than in the standard model setup. However, the RLSs for opal (the ratio between sinking velocity and dissolution rate) eventually determine the silicate retention in the water column. As briefly discussed in Sect. 3.6: Regional fluxes & rain ratios, the opal RLSs are indeed shorter in surface waters, but longer in regions of the mesopelagic zone and below. In total, the attenuation of opal fluxes with depth remain similar in most regions (see Fig. 11). Hence, HAMOCC with M^4AGO doesn't show much difference in the global opal to $CaCO_3$ production ratio, which can also be seen in the flux ratios, shown in Fig. 5.

→ Remained with the present state of description.

- Page 16, line 7: I am not sure if I understand correctly: Is dp,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 dp,calc ..., to account for ....

⇒ Modified accordingly.

→ ", and hence, we set $d_{p,\mathrm{calc}} = 3\,\mu\mathrm{m}$, which is thus at the lower bound of the observed range while accounting for the volumetric density effect of non-spherical plate-like coccoliths." → "We set $d_{p,\mathrm{calc}} = 3\,\mu\mathrm{m}$, which is thus at the lower bound of the observed range, to account for the volumetric density effect of non-spherical plate-like coccoliths."

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

⇒ We aimed at clarification and rephrased the sentence.

→ "The close connection between the parametrized processes of sinking and remineralization, the transfer efficiency and the climatological nutrient field hampers the clear distinction between tuning and evaluation data when comparing the model results to literature values for transfer efficiency" → "The newly parametrized processes of sinking and remineralization directly affect the transfer efficiency, and thus the climatological nutrient fields. This close connectedness hampers the clear distinction between data employed for model tuning or for model evaluation, when comparing the model results to literature values for transfer efficiency."

- 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. (?)

  ⇒ For clarity, we split the sentence.

  → "Since the adaptation of the sinking velocity versus the remineralization and dissolution rates, and thus the transfer efficiency, was within a few years, parameter variations aiming at a quantitative agreement with the transfer efficiency of Weber et al. (2016) enabled a useful strategy to select for promising parameter sets." → "We performed parameter variations aiming at a quantitative agreement with the transfer efficiency of Weber et al. (2016). Since the adjustment of the sinking velocity versus the remineralization and dissolution rates, and thus the transfer efficiency, occurs within a few years, this strategy was useful to select for promising parameter sets."

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

  ⇒ Since this part includes methodological aspects, we follow your suggestion to start a new paragraph in the methods section.

  → New paragraph started.

- 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?

⇒ We are using a climatological forcing, which, by definition, does not resolve interannual events such as ENSO. However, internal variability of ocean circulation still happens and we here therefore provide the climatological 100 a mean of the transfer efficiency and its respective standard deviation (Fig. 2). As can be seen, the standard deviation is small for most ocean regions and exhibits higher values in the Antarctic polar regions. These are likely linked to the shifting of polar fronts through the internal variability of ocean circulation that imprints on the ocean biogeochemistry. However, the overall latitudinal mean pattern of the transfer efficiency resembles the one of Fig. 8 in the manuscript. In our manuscript, we are not concerned with the internal variability and focused on the general potential effects of aggregate composition and microstructure on POC fluxes as explanatory factors for the global pattern of transfer efficiency. We may revisit the aspect of internal variability in a follow up work and don't want to lengthen the manuscript further at this stage. We therefore remain with the present status.

→ Remained.

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

⇒ Thanks. Rephrased accordingly.

→ "In the M^{4}AGO run, the equatorial Pacific exhibits the lowest export efficiencies, features maximum values of about 0.14-0.16 in the subtropical gyres and about 0.20 in the Arctic region (Fig. 4 d)" → "In the M^{4}AGO run, the equatorial Pacific exhibits the lowest export efficiencies, the subtropical gyres feature maximum values of about 0.14-0.16, and the Arctic region about 0.20 (Fig. 4 d)"

- 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?

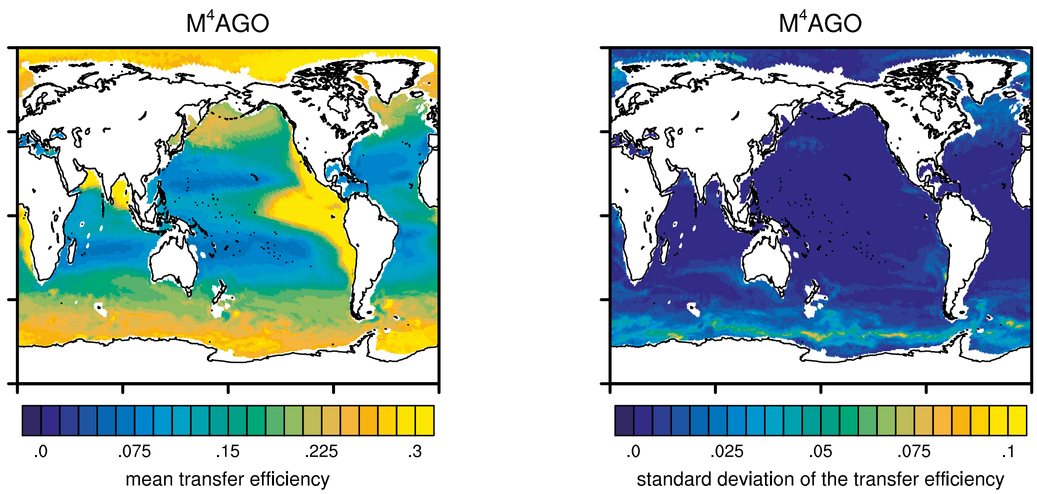

Figure 2: 100 a climatology of the transfer efficiency (left) and the standard deviation (right). Shifting of polar fronts due to internal variability of ocean circulation and the affected biogeochmistry are likely the cause for higher standard deviation of the transfer efficiency in the Antarctic polar region than in the rest of the ocean. The overall mean transfer efficiency pattern resembles the one shown in Fig. 8 in our manuscript.

⇒ We double-checked. The standard has even smaller minimum p-ratios than the M^{4}AGO run. We therefore remain with the text.

→ Remained.

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

⇒ Thanks.

→ Modified: "7 a,d" → "7 a"

- 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.

⇒ We now describe the location in the caption. Caption changed:

→ "Modeled marine aggregate properties on WOA transect P16" → "Modeled marine aggregate properties on the Pacific WOA transect P16, which is located at about 150 °W"

- 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?

  ⇒ We aimed at clarification and rephrased the sentence.

  → "While compaction likely enhances the internal number of binding links in aggregates, we neglect this effect versus the lower primary particle binding forces on the overall susceptibility to shear stress and thus kept Re_{crit} globally constant." ⟶ "Compaction can coincide with an increasing number of binding links in aggregates, which can lower the overall susceptibility of aggregates to shear stress. In M^4AGO, we disregard this effect and keep Re_{crit} globally constant.

- 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.

  ⇒ We rephrased the sentence and now stick to present tense. See above.

  → As reformulated in the previous comment.

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

  ⇒ The reviewer is right. Thank you very much! We modified it accordingly

  → "half" ⟶ "$1/e$ ($\approx 37\,\%$)"

- 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.

  ⇒ For clarity, we add the 30 % contour label, see Fig. 3.

  → Contour label added.

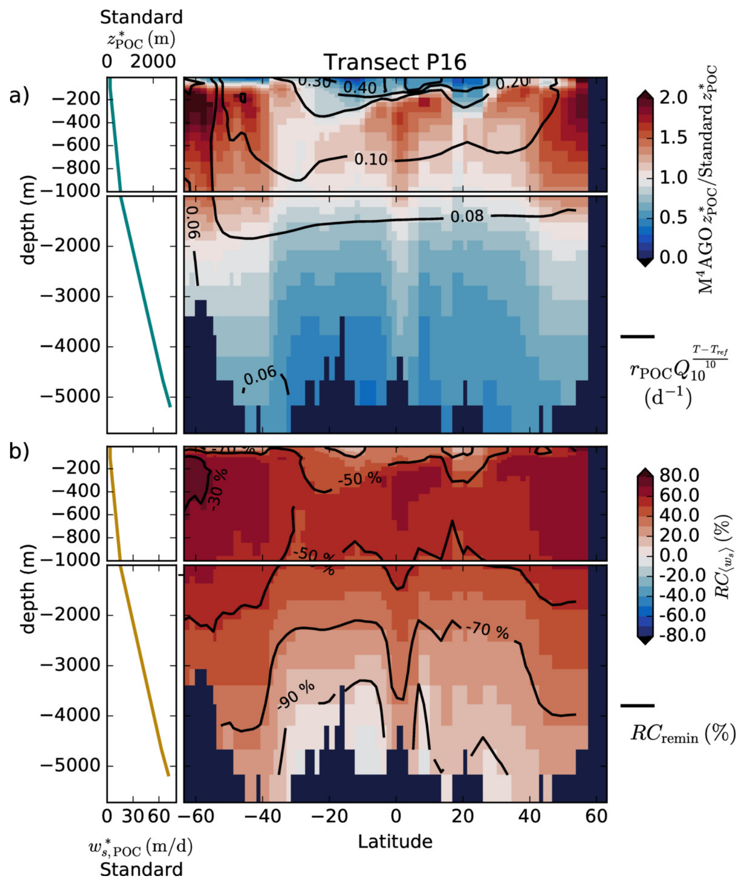

Figure 3: Added "-30 %" contour level label in b)

- 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.

  ⇒ We modified the sentence.

  → "The relative contributions provide an information on the main driving factors, expressed as percentage, for the local $\langle w_s \rangle$ as compared to global average aggregates." → "The relative contributions provide information about the main driving factors for the local $\langle w_s \rangle$ as compared to $\langle w_s \rangle$ of global average aggregates."

- Page 28, lines 23-24: Please rephrase / clarify sentence; e.g.: M^{4}AGO thus likely underestimates the spatial variability and relative contribution of b to ws.

⇒ Thanks. Done.

→ "likely underestimates the spatial variability of the relative contribution to $\langle w_s \rangle$" → "likely underestimates the spatial variability and relative contribution of b to $\langle w_s \rangle$"

- 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 ...

⇒ Modified accordingly.

→ "Similar to the POC remineralization length scales, opal fluxes exhibit shorter opal RLS in the surface waters, while they exceed the standard RLS in the mesopelagic zone and below (not shown)." → "Similar to POC fluxes, opal fluxes exhibit shorter opal RLSs in the surface waters, while they exceed the standard RLSs in the mesopelagic zone and below (not shown)."

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

⇒ It's a result from low opal production in the subtropical gyres, visible in Fig. 5 a,b. We therefore give a reference to Fig. 5a,b

→ added: "(see Fig. 5a,b)

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

⇒ We now provide the global Si flux to sediment for the standard run as well.

→ "$\sim$ 1.03 Gt Si per year" → "$\sim$ 1.03 Gt Si per year ($\sim$ 1.04 Gt Si per year in the standard run)"

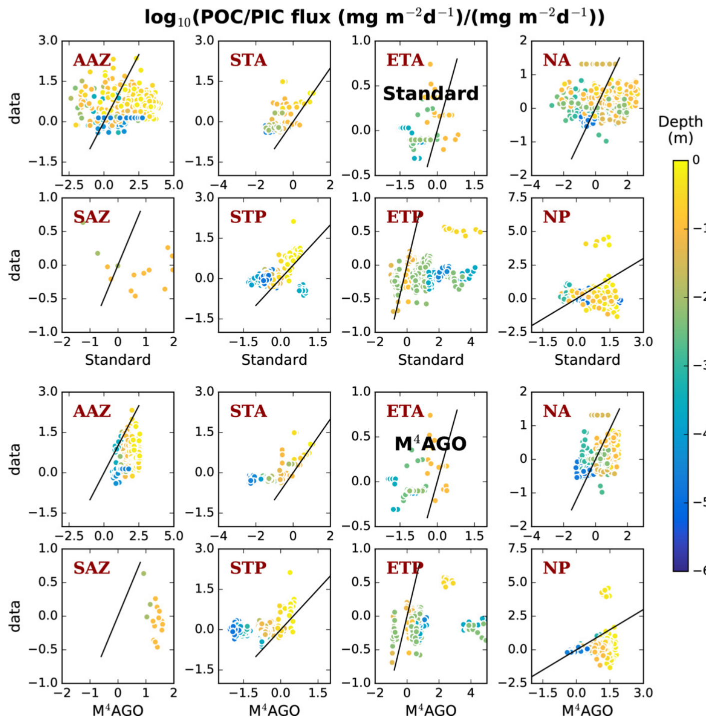

Figure 4: As Fig.12 in the manuscript, but for POC/PIC ratio. Comparison of the standard run and the M^{4}AGO run to the Mouw et al (2016a,b) data set. Standard refers to upper two rows, M^{4}AGO to the lower two rows.

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

  ⇒ We here provide the PIC/POC fluxes in the reply, see Fig. 4

  → Nothing to be changed.

- 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^{4}AGO 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, variability is also introduced due to differences between Si- and POC-sinking speeds?

⇒ We write at the end of the first paragraph of Sect. 3.6 that M^4AGO couples the timing of mineral and POC fluxes and took that also as motivation for the comparison to data, as lined out on p. 31, l.1 ff. We therefore agree that the compression likely stems from the joint sinking of mineral and POC components, which we also discuss on p. 31, l. 11 ff. We therefore remain with the present text.

→ Remained.

- 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?

⇒ We modified the caption for clarity.

→ "POC/Si rain ratios in the standard run and M^4AGO compared to the Mouw et al. (2016a,b) data set." → "Monthly POC/Si rain ratios in the standard run and in M^4AGO compared to the monthly climatological mean derived from the Mouw et al. (2016a,b) data set."

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

⇒ Yes. Thanks for improving clarity.

→ "North America coast" → "North American West Coast"

- Page 35, lines 20-22 / page 36, Figure 14a: If negative fluxes really do represent a net-CO2 uptake by the ocean in Fig. 14a, as stated in the caption, the southern hemisphere ocean acts as a net sink for atmospheric CO2 (and not a source), and the northern hemisphere ocean acts as a net source (not a sink). Consequently, the oceanic CO2 transport would be from south to north.

⇒ We are generally very sorry for having caused confusion about the units and thus the interpretation of Fig. 14a. It should be $\mathrm{Gt\,C\,yr^{-1}}$. For easier interpretation, we add the starting point of the cumulative flux calculation (from south to north). Given that the cumulative fluxes show a positive sign at the equator, the southern hemisphere is a net-source of CO_2. We remain with the general statement and provide additional information on the cumulation procedure in the caption of Fig. 14.

→ "Climatological cumulative zonal CO_2 flux in the standard and the M^4AGO run" → "Climatological cumulative zonal CO_2 flux in the standard and the M^4AGO run (from south to north)"

- Irrespective of the sign / flux direction, these results are in stark contrast to CO2 flux observations of net zonal mean outgassing at the Equator and net ocean CO2 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?

⇒ The plot is in agreement with this general pattern. The cumulative sum shows a positive trend towards the equatorial region (and beyond until about 15 °N), which indicates outgassing in the tropics. We, however, have to admit, that the text gave a different impression, which we change. Many thanks for pointing this out!

→ p.35, l. 24,26: deleted "sub" of "subtropical" → "tropical"

- I also am not sure if I understand the units in Figure 14a. Does the left axis show the net sea–air CO2 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 CO2 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.

⇒ We are again sorry for the wrong unit, which we correct for. Generally, the uptake in the southern hemisphere is lower than 1.1 GtC/yr, since Fig. 14a corresponds to pre-industrial conditions, as stated at the beginning of Sec. 3.8. We add a sentence to provide references for the qualitative and quantitative agreement.

→ Added the sentence: "In general, the latitudinal zonal CO_2 fluxes and the cross-equatorial southward oceanic CO_2 transport agree qualitatively and quantitatively well with former forward-integrated models for pre-industrial conditions (e. g Sarmiento et al., 2000; Gloor et al., 2003; Mikaloff Fletcher et al., 2007)."

- Page 35, lines 22-24: "In the simulation with M4AGO, a stronger CO2 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 CO2 fluxes do *not* differ more, despite the very different transfer efficiencies. Why do the CO2 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?

⇒ We are again sorry for the misleading, wrong axis label. At first glance, the strong differences in transfer efficiency contradicts the little changes in atmosphere-ocean CO_2 fluxes in some regions. However, transfer efficiency is not equal to actual POC fluxes. Transfer efficiency only describes, which fraction of exported POC is transferred to certain depth (in our manuscript calculated for about 1000 m). For example, in the Arctic ocean, we find a high transfer efficiency of the exported material, but actual POC fluxes are small. Thus the high transfer efficiency in the Arctic has hardly any impact on the atmosphere-ocean CO_2 fluxes. We now provide a brief explanation for it. With regards to the region south of 55 °S, we hope that with clarification of the units in Fig. 14 a it becomes clear that indeed, a clear effect of the increased transfer efficiency in M^4AGO compared to the standard run is visible. There is a stronger uptake in the AAZ region in M^4AGO (see the difference between M^4AGO and the standard run), as also visible in the rising CO_2 experiments (cmp. Fig. 14 c). This is also described in the text (see p. 37, l.7ff: "Quantitatively, differences of regional cumulative CO_2 fluxes larger than 5 Gt C appear in the Antarctic Zone (AAZ),") We rephrase the sentence and link the transfer efficiency and the primary production pattern.

→ "Qualitatively, this coincides well with the higher transfer efficiencies in these regions (cmp. to Fig. 8)." → "Qualitatively, this coincides well with the primary production, respective export and the higher transfer efficiencies in these regions (cmp. to Fig. 4 and 8). In regions of higher transfer efficiency, but similar CO_2 fluxes as compared to the standard run, either POC export fluxes are small (e. g. in the Arctic Ocean) or physical processes such as mixing or upwelling dominate over biologically induced CO_2 fluxes (e. g. in the SAZ)."

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

  ⇒ Modified.

  → "…which is suggested…" → ". The increasing water temperature has been suggested…"

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

  ⇒ Yes, such eco-physiological responses refer to the change of primary particle size.

  → Remained with the sentence.

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

  ⇒ No, it's more than 150 %. The sentence is fine.

  → Remained.

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

  ⇒ Modified.

→ "Phosphate previously utilized by diazotrophs in the Panama basin now partially populates the downstream equatorial current and increases the subtropical gyres phosphate concentrations." → "Phosphate previously utilized by diazotrophs in the Panama basin now partially populates the downstream equatorial current 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?

  ⇒ We rephrased the sentences accordingly.

  → "First, size of aggregate constituents, particularly of diatom frustules, as potential factor for high sinking velocity suggest to widen the perspective of mineral ballast studies towards a size-and-ballast hypothesis." → "First, the finding that the size of aggregate constituents, particularly of diatom frustules, act as potential factor for high sinking velocities, suggests to widen the perspective of mineral ballast studies towards a size-and-ballast hypothesis."
  "it requires" → "such extended size-and-ballast hypothesis requires"

- 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?

  ⇒ In the referred standard run, remineralization is not temperature-dependent, but oxygen-dependent. Thus, β is implicitly prescribed by the gradient of sinking velocity ($\partial_z \bar{w}_s$) and the remineralization rate R_{POC} in oxygenated waters. Following Kriest and Oschlies (2008), the vertical mass concentration exponent is defined by $R_{\mathrm{POC}}/\partial_z\,\bar{w}_s + 1 = 2$, (their r/a+1 in Eq. (5)), which is prescribed in HAMOCC. Hence, it translates to a preset $\beta = 1$, which, by internal processes such as reduced R_{POC}, upwelling, but also numerical diffusion, then results in an effective Martin slope $\langle\beta'\rangle$ that is smaller than one. For clarity, we now provide information about the assumption on the oxygenation state.

→ "that is smaller than the prescribed value of $\beta = 1.00$" → "that is smaller than the prescribed value of $\beta = 1.00$ in oxygen-saturated waters"

2.3 Typos

- Page 2, line 32: Primary / fundamental(?) determining factors?

⇒ Thanks. Changed.

→ "primer" → "primary"

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

⇒ Thanks. Added.

→ added: "an"

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

⇒ It's also the mass concentration-weighted mean sinking velocity, but different from the spatio-temporally variable one in M^{4}AGO. So, we added the information.

→ "Below z_0, we assume a linearly increasing mean sinking velocity with depth." → "Below z_0, we assume a linearly increasing mass concentration-weighted mean sinking velocity with depth."

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

⇒ Modified.

→ "Opal" → "The opal"

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

  ⇒ Modified.

  → added: "the"

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

  ⇒ Thanks. Changed.

  → "lead"

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

  ⇒ Changed.

  → added "data" and plural "s" in pattern"s"

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

  ⇒ Changed.

  → "Both model simulations show similar pattern of opal to detritus ratio fluxes" → "Both model simulations show a similar pattern of opal to detritus flux ratios"

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

  ⇒ Changed.

  → "In turn" → "By contrast"

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

  ⇒ Modified.

  → "aggregates" → "their"

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

⇒ Changed.

→ removed ","

- Page 23, line 17: linearly increasing

⇒ Changed.

→ "linear" → "linearly"

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

⇒ Changed.

→ "from relationship between the Martin curve slope parameter and transfer efficiency" → "from the relationship between the Martin curve slope parameter and the transfer efficiency"

- Page 24, line 5: M^4AGO posseses...

⇒ Changed.

→ "possess" → "possesses"

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

⇒ Allows for is correct.

→ Remained.

- Page 25, line 5: extent

⇒ Changed.

→ "extend" → "extent"

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

⇒ Changed.

→ added: "the"

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

  ⇒ Modified in parts.

  → "In the mesopelagic zone, the RLSs in M^4AGO is similar or pronounced longer and decreases" ⟶ "In the mesopelagic zone, the RLSs in M^4AGO are similar or pronounced longer and decrease" "The longer RLS" ⟶ "The longer RLSs"

- Page 27, line 16: In summary, the temperature-dependence...induces...

  ⇒ Changed.

  → "temperature-dependence of remineralization in M^4AGO induce" ⟶ "the temperature-dependence of remineralization in M^4AGO induces"

- Page 27, line 28: ...$X_{i,z}$ is (not as)

  ⇒ Changed.

  → "as" ⟶ "is"

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

  ⇒ Changed.

  → "given the microstructure general importance" ⟶ "given the general importance of the microstructure"

- Page 35, line 29: ... in M^4AGO on *the* CO2 uptake ...

  ⇒ Changed.

  → added: "the"

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

⇒ Thanks. Changed.

→ "primary production in the in the equatorial upwelling regions (ETA and ETP) thus lead" → "primary production in the equatorial upwelling regions (ETA and ETP) thus leads"

- Page 40, line 13: grazing through zooplankton

⇒ Changed.

→ "though" → "through"

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

⇒ Changed.

→ added: "the"

- Page 50, line 33: initials for Núñez-Riboni

⇒ Bibliography updated. Thanks.

→ Done.

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

⇒ Yes. Thanks.

→ Done.

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

⇒ Done.

→ added: "PANGAEA"

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

⇒ Yes. It's the official abbreviation for 'Comptes Rendus Geoscience'

→ modified to: "Comptes Rendus Geoscience"