1 General remark

The reply is structured as follows:

- Referee comment

  ⇒ Authors reply

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

2 Reply to Referee #2

2.1 General summary

- Review of Maerz et al: *Microstructure and composition of marine aggregates as co-determinants for vertical particulate organic carbon transfer in the global ocean*. The authors present a new scheme for the calculation of the mean sinking velocity of marine aggregates as a function of the aggregate composition and the fractal dimension. This scheme is reported to be cost-efficient and hence useful in large-scale ocean models. The model is described in detail and carefully evaluated. The authors report a substantial improvement in the simulation of the latitudinal pattern of POC transfer efficiency. This is an impressive effort and worth of publication. I have some specific comments that should be addressed before publication. Writing style: Sentences are very long and not always clear. This is particularly true for the introduction and model description.

  ⇒ We thank the reviewer for her/his constructive and positive review. We tried to respond to every single comment and hope to improved the clarity of the manuscript.

  → –

2.2 General comments

- I miss a comparison with the stochastic, Lagrangian model of sinking biogenic aggregates in the ocean (SLAMS) by Jokulsdottir and Archer. Jokulsdottir, T. and Archer, D.: A stochastic, Lagrangian model of

sinking biogenic aggregates in the ocean (SLAMS 1.0): model formulation, validation and sensitivity, Geosci. Model Dev., 9, 1455–1476, https://doi.org/10.5194/gmd-9-1455-2016, 2016.

⇒ Indeed, the model of Jokulsdottir and Archer (2016) is an interesting new model approach. Unfortunately, their Lagrangian model is currently limited to a conceptual 1-D application and is computationally likely too costly for the incorporation in an Earth System Model framework. However, it might provide valuable insights for further development and tuning of M^4AGO or similar models. We pick up on it and briefly discuss it in Sect. 3.10 Current limitations of M^4AGO.

→ Added on p. 40, l. 28 (after ... "subsequent aggregation (Martin et al., 2011)."): "More detailed models, such as e. g. the 1-D Lagrangian approach of Jokulsdottir and Archer (2016), can likely provide more insights into aggregate dynamics and can help to further improve the aggregate representation in ESM frameworks."

- Please make the model code publicly available. It is not in the repository that you mention.

⇒ The model code was, is and will be publicly available on request. However, we acknowledge that the provided information how and where to request it, was insufficient. We therefore now provide more information i) in the manuscript and ii) in the MPGPuRe repository, how to inquire it. It requires to agree to the MPI-ESM license agreement and registering at the MPI-ESM-Forum. Unfortunately, the license obliges users to this method for accessing the code due to third party rights on the code.

→ "Primary data and code for this study is stored and made available through the Max-Planck-Gesellschaft Publications Repository: https://pure.mpg.de." → "Primary data and code for this study is stored and made available through the Max-Planck-Gesellschaft Publications Repository: https://pure.mpg.de. The respective MPI-OM and HAMOCC model code (revision numbers: r4981 and r5003) is available on request after agreeing to the MPI-ESM license agreement and registering at the MPI-ESM-Forum (https://www.mpimet.mpg.de/en/science/models/licenses/)."

- explain ALL abbreviations and symbols used in the figures in each and every figure caption.

  ⇒ We carefully went through the manuscript and added the abbreviations and symbols, where we believe, it's necessary. If not otherwise stated in the modifications below, no modifications were performed for the caption.

  → Added in caption of Fig. 1:
  "$V_{p,i}$ is the primary particle volume, $\rho_{p,i}$ is the primary particle density, and $d_{p,i}$ is the primary particle diameter of primary particle type i. $m(d)$ is the mass of an aggregate of diameter d. $\langle d_p \rangle$ and $\langle \rho_p \rangle$ represent mean primary particle diameter and density, respectively."

  Added in caption of Fig. 2:
  "l denotes the thickness of the opal shell with volume V_{opal}, V_{aq} and V_{POM} are the encapsulated volumes of water and POM, respectively. $d_{p,\mathrm{frustule}}$ is the diameter of the diatom frustule."

  Added in caption of Fig. 3: "d_{max}"

  Refrained from adding the symbols explicitly in caption of Fig. 7, since they are given nearby in caption of Fig. 6 and we refer to them explicitly.

  Modifications caption of Fig. 9:
  "z^*_{POC}" → "the remineralization length scales, z^*_{POC},"
  "sinking velocity" → "sinking velocity, $w^*_{s,\mathrm{POC}}$,"
  "temperature-dependent remineralization rates" → "Q_{10} factor temperature-dependent remineralization rates with r_{POC} at reference temperature T_{ref}"
  "Relative contributions of sinking and remineralization" → "Relative contributions of sinking, $RC_{\langle w_s \rangle}$, and remineralization, RC_{remin},"

Added in caption of Fig. 10:
"Mathematical symbols are: $\langle \rho_p \rangle$: mean primary particle density; $\langle d_p \rangle$: mean primary particle diameter; d_f: fractal dimension; μ: dynamic molecular viscosity; $d_{\max}$: maximum aggregate diameter; b: aggregate number distribution slope."

Modified caption of Fig. 13:
"tracers $\rightarrow$ "tracers (oxygen, O_2, nitrate, NO_3^-, phosphate, PO_4^{3-}, silicate, Si)"

Modifications in caption of Fig. 15:
"POC transfer efficiency to $d_{p,\mathrm{frustule}}$" $\rightarrow$ "POC transfer efficiency to diatom frustule size, $d_{p,\mathrm{frustule}}$"
"and d_f" $\rightarrow$ "and fractal dimension, d_f"

- I am quite worried about the high buoyancy of diatom-dominated aggregates through the TEP formulation. This needs more justification. Do you here assume that all organic carbon has the same density as TEP? That would explain your low density of diatom-dominated aggregates. Is there sufficient evidence for such behavior?

  $\Rightarrow$ We acknowledge that our formulation is simplified. We will make the simplification more clear. However, we don't assume that all detritus has the same density as TEPs. We assume that TEPs lower the density of the diatom frustule and parametrize the effect with respect to the model-defined freshness of detritus. In our model framework, we thus focus on the qualitative effect of TEP that has been previously suggested (Mari et al., 2017) and, as we learned, also applied by Jokulsdottir et al. 2016. There are a number of observational and experimental studies that support the general behaviour of TEPs as buoyancy adding agent in marine aggregates. For example, the following experimental and observational studies point to low TEP density or show low buoyancy of aggregates, similar to our modeled diatom-dominated aggregates with mean excess densities of $\Delta \langle \rho_f \rangle_V \approx 2\,\mathrm{kg\,m^{-3}}$

  - Azetsu-Scott and Passow, 2004[2]: $\rho_{\mathrm{TEP}} \approx 700 - 840\,\mathrm{kg\,m^{-3}}$.

[2] Azetsu-Scott, K. and Passow, U.: Ascending marine particles: Significance of trans-

Thus TEPs are lighter than sea water and add buoyancy, if incorporated in aggregates.

- Alldredge & Gottschalk 1988[3]: excess density of median marine aggregates: $0.14\,\mathrm{kg\,m^{-3}}$, and for diatom aggregates (their Fig.2 d): $\approx 10^{-5} - 10^{-4}\,\mathrm{g\,cm^{-3}} = 10^{-2} - 10^{-1}\,\mathrm{kg\,m^{-3}}$ for large, porous diatom-dominated aggregates ($O(1\,\mathrm{cm})$).
- Laurenceau-Cornec et al. 2019[4]: excess density of diatom-dominated aggregates including TEPs (partly also including mineral components): $\approx 0.3 - 5.7\,\mathrm{kg\,m^{-3}}$

Some of the studies don't explicitly mention TEP (since they were not explicitly described at the time of the study), but nowadays TEPs are ubiquitously found in the global ocean and thus possess a likely explanation for the low aggregate excess densities. We believe that the confusion about our modeled diatom-dominated aggregates arises primarily due to the fact that most often diatom-dominated aggregates are reported in the form of marine snow, and hence large aggregates that have high sinking velocities of often more than $> 50\,\mathrm{m\,d^{-1}}$. Our model agrees well with sinking velocities of these individual aggregates, when considering only the respective sizes (and not the mean sinking velocity of the full size spectrum). For example, here using Stokes sinking velocity for simplicity, thus slightly overestimating w_s compared to the White drag parametrization:

$$
\begin{aligned}
w_s(d = 1\,\mathrm{mm} = 0.001\,\mathrm{m}) &= \frac{1}{18\,\mu}\Delta\rho_f\, g\, d^2 \\
&= \frac{1}{18\,\cdot 0.0015\,\frac{\mathrm{kg}}{\mathrm{m\,s}}} \cdot 2\,\frac{\mathrm{kg}}{\mathrm{m^3}} \cdot 9.81\,\frac{\mathrm{kg}}{\mathrm{m}} \cdot (0.001\,\mathrm{m})^2 \cdot 86400\,\frac{\mathrm{s}}{\mathrm{d}} \\
&\approx 62.8\,\mathrm{m\,d^{-1}}
\end{aligned}
$$

parent exopolymer particles (TEP) in the upper ocean, Limnol. Oceanogr., 49, 741–748, 2004

[3]Alldredge, A. L. and Gotschalk, C.: In situ settling behaviour of marine snow, Limnol. Oceanogr., 33, 339-351, 1988

[4]Laurenceau-Cornec, E.C, Moigne, F.A.C., Gallinari, M, Moriceau, B., Toullec, J., Iversen, M.I., Engel, A., De La Rocha, C.L.: New guidelines for the application of Stokes' models to the sinking velocity of marine aggregates, Limnol. Oceanogr., 2019, doi:10.1002/lno.11388

The due to Stokes slightly overestimated value of $\approx 62.8\,\mathrm{m\,d^{-1}}$ is of the same order as e. g. the measurements by Alldredge & Gottschalk (1988), who found the settling velocities can be best described by $50 \cdot (d[\text{in mm}])^{0.26}$ (their Fig. 3 a) which would be $\approx 50\,\mathrm{m\,d^{-1}}$ for our example diameter of 1 mm.

As a consequence of the reviewers comment, we now give the reference to Jokulsdottir and Archer (2016) and Mari et al. (2017) closer to the description of our diatom density description. Both author teams explicitly described the potential of TEPs to add buoyancy through the TEPs lower density than water ($\rho_{\mathrm{TEP}} \approx 700 - 840\,\mathrm{kg\,m^{-3}}$, Azetsu-Scott and Passow, 2004). In addition, we emphasize our simplification of TEP representation. We add the reference to Laurenceau-Cornec at the point, where we discuss the diatom-dominated aggregate excess density. To further clarify the fact that mean sinking velocity can differ significantly from reported large marine snow aggregates of sizes of typically > 1 mm, we add a note in the caption of Fig. 6. Further, we add a sentence on p. 22 l.2, where we present the sinking velocities.

→ "The density of diatoms becomes" → "To account for the additional buoyancy through TEPs (Jokulsdottir and Archer, 2016; Mari et al., 2017), we here simplify and assume that the frustule density is lowered by TEPs in dependency on the freshness of detritus. Eventually, the diatom density, ρ_{diatom}, becomes"

Added in caption of Fig. 6 (after concentration-weighted mean sinking velocity of aggregates):
"Note that $\langle w_s \rangle$ comprises the full range of many micrometer-sized to rare, large aggregates with low ($O(1\,\mathrm{m\,d^{-1}})$) and high ($O(> 100\,\mathrm{m\,d^{-1}})$) sinking velocities, respectively."

Added reference to: "Laurenceau-Cornec et al., 2019" on p. 22,l.20/21

"Particle properties and molecular dynamic viscosity determine the concentration-weighted mean sinking velocity of aggregates, $\langle w_s \rangle$ (Fig. 6 f,l), for particle sizes ranging from few micrometers to millimeters." → "Particle properties and molecular dynamic viscosity determine the concentration-weighted mean sinking velocity of aggregates, $\langle w_s \rangle$ (Fig. 6 f,l). For $\langle w_s \rangle$, M^{4}AGO considers particle sizes ranging from few micrometers to millimeters and thus the full size spectrum, where sinking velocities of $O(1\,\mathrm{m\,d^{-1}})$ to $O(>100\,\mathrm{m\,d^{-1}})$ are represented. $\langle w_s \rangle$ thus can significantly differ from reported sinking velocities for large individual aggregates."

2.3 Abstract

- Line 14: too much information given: delete rising CO2 and without CO2 climate feedback.

  ⇒ We rephrased the sentence and hope, that it is now better readable.

  → "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."

2.4 Introduction

- P.2 Please give more references for your statements, especially in the first paragraph. No reference given between line 5 and 11.

  ⇒ Given the general principles stated in the paragraph, we now provide a reference to the excellent book of Williams and Follows 2011: Ocean Dynamics and the Carbon Cycle: Principles and Mechanisms.

  → added reference: "(Williams and Follows, 2011)"

- P. 2, line 17: "The sinking velocity of aggregates is primarily determined by their size". This needs a reference. I would argue it is density, e.g. Iversen and Robert,http://dx.doi.org/10.1016/j.marchem.2015.04.009 . The next sentence also needs a reference (line 19)

⇒ A similar comment has been done by reviewer #1. We answered his/her comment and provide the answer here:
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:

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

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)[5] imply, when writing '2- to 3-fold higher **size-specific** sinking velocities'

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

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)"

- P. 2, line 32: primer → primary?

⇒ Changed.

→ "primer" → "primary"

2.5 Model description

- It would be very helpful to have a table with all symbols used in the equations at the beginning of section 2.1

⇒ We will provide the table in the appendix.

→ Adding table. At end of first paragraph of Sect. 2: Model description, p. 4, l.15, we add: "A table with the used mathematical symbols can be found in App. D, Tab. D1"

- P. 5, line 2, what is meant with "terminal sinking velocity"? I suggest to delete "terminal"

⇒ Terminal sinking velocity is the sinking velocity of any particle in steady state, when all involved forces balance. This is the classical assumption, when applying e. g. the Stokes formula for sinking velocity (or any other drag formulation in the provided Eq. (7)). By writing 'terminal sinking velocity', we make it clear that we are not attempting to solve the Maxey-Riley equation (M.R. Maxey & J.J. Riley 1983: Equation of motion for a small rigid sphere in a nonuniform flow. The Physics of Fluids 26, 883, https://doi.org/10.1063/1.864230). We thus remain with the present formulation.

→ Remained with the formulation.

- Eq. 3: I can guess what is meant with dd, but it is easily misunderstandable.

⇒ In agreement with reviewer #1 and aiming at clarity, we rephrased this part and now make clear that $\langle w_s \rangle$ is derived from integration over the size spectrum.

→ "The local concentration-weighted mean sinking velocity, $\langle w_s \rangle$, in M^4AGO is eventually 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, $w_s(d)$" → The local concentration-weighted mean sinking velocity, $\langle w_s \rangle$, in M^4AGO is eventually 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 $\langle w_s \rangle$,"

- P. 5, line 28: What is meant by a "primary particle". How does that differ from "a particle"?

⇒ Primary particles are the modelled (smallest) entities of which an aggregate is composed of. As we state in p.5, line 28, we consider e.g. phytoplankton cells or coccolithophore shells as primary particles. We render the definition of primary particles more precisely on p. 6, l.17-19, when we introduce the theory for heterogeneously composed aggregates. There, we state:
"With M^4AGO, we represent aggregates composed of poly-dense, poly-sized primary particles under the assumption of a singled value fractal dimension throughout the aggregate size spectrum. This allows for representing heterogeneous primary particles such as diatom frustules, coccoliths, dust particles, and detritus as principal components of marine aggregates. ".
'Particle' is a more general term and can refer, depending on context, to a primary particle or an aggregate. As a consequence of the reviewer's question, we carefully checked throughout the manuscript, if we always refer correctly to primary particles.

→ We remained with the text.

- P. 7, line 1: What is meant by “the total number of one primary particle type” ? The total number of one should be one. Do you mean “of particles of one particle type”?

  ⇒ Yes. We modified the sentence accordingly.

  → "between the total number of one primary particle type" → "between the total number of primary particles of one particle type"

- P. 8, line 5: “mean primary particle size, (. . .) which we apply as a lower integration bound”. Please give a justification for this choice.

  ⇒ The fractal scaling law breaks for aggregates smaller than the smallest composing entity, i. e. the primary particle size. For $\lim d \rightarrow \langle d_p \rangle$, the aggregate is composed of one single primary particle. Hence, the lower limit must be $\langle d_p \rangle$. Anyhow, we now provide an additional reference to Kriest and Evans (1999).

  → Added reference: "and hence, $d_{\min} = \langle d_p \rangle$" → "and hence, $d_{\min} = \langle d_p \rangle$ (following Kriest and Evans, 1999)"

- P. 10, line 5: no reference to Engel et al 2004? Engel, A. , Thoms, S., Riebesell,U. , Rochelle-Newall, E. and Zondervan, I. (2004) Polysaccharide aggregation as a potential sink of marine dissolved organic carbon. Nature, 428 . pp. 929-932. DOI 10.1038/nature02453.

  ⇒ Thanks for the reference. Added it.

  → Added: "Engel et al., 2004"

- Figure 2: explain abbreviations and symbols in each and every figure caption.

  ⇒ Where needed, we now do it. See the remark in the general comments section of the same reviewer.

  → Particularly for Fig. 2, we added to the caption:
  "l denotes the thickness of the opal shell with volume V_{opal}, V_{aq} and V_{POM} are the encapsulated volumes of water and POM, respectively. $d_{p,\text{frustule}}$ is the diameter of the diatom frustule."

- P. 11, line 18: how is m_e and $m_{\text{potential}}$ calculated? I can't follow whether the masses of opal and of TEP are taken into account correctly to calculate the density of the diatom-aggregate. Do you here assume that all organic carbon has the same density as TEP? That would explain your low density of diatom-dominated aggregates. Please clarify.

  ⇒ We do not assume that diatom frustule-associated detritus has the same density as TEP. We first calculate the frustule density according to Eq. (27), which is based on the density of opal, ρ_{opal}, and POM ρ_{POM} (now corrected to ρ_{det}). We admit that we should have written ρ_{det} instead of ρ_{POM}, which we change. For the eventual density of the diatom ρ_{diatom}, we assume that the amount of diatom frustule-associated detritus is proportionally linked to the presence of TEP, which makes the diatom frustule lighter. We apologize for not having provided the equations for m_e and $m_{\text{potential}}$, which we now provide. Our modeled primary particle densities in silicifier-dominated waters like the Southern Ocean are about $1060\,\text{kg}\,\text{m}^{-3}$ to $1100\,\text{kg}\,\text{m}^{-3}$ (values based on re-checked monthly mean model output) are in a normal range of detritus. We are certain that the calculated excess density of rather fresh, porous diatom-dominated aggregates in surface waters is with about $2\,\text{kg}\,\text{m}^{-3}$ in the range of previously measured excess densities of diatom aggregates. For example, a range of about 0 to $\sim 10\,\text{kg}\,\text{m}^{-3}$ is given by Alldredge and Gottschalk, 1988, Ploug et al. 2008 and Iversen and Ploug 2013 (as cited in the manuscript). Please see also the reply to the general comments of the same reviewer. Further, the modeled excess density of diatom-dominated aggregates increases with ongoing remineralization during the decent through the mesopelagic zone (Fig. 7 d).
  We clearly state in Sect. 3.10 (Current limitations of M^4AGO) that TEPs are only simplistically considered. We make it more clear now in Sect. 2.2.5, p. 11, l.16. Given the potential role of TEPs in aggregate formation, we currently consider to include TEPs explicitly.

  → "ρ_{POM}" → "ρ_{det}"

  "defined as the mass ratio between the actual amount of detritus m_e and the potential mass of detritus linked to opal production $m_{\text{potential}}$." $\longrightarrow$ "defined as the mass ratio between the actual amount of detritus, $m_e = n_{\text{frustule}} V_{\text{POM}} \rho_{\text{det}}$ and the potential mass of detritus linked to diatom frustules $m_{\text{potential}} = n_{\text{frustule}} (V_{\text{POM}} + V_{\text{aq}}) \rho_{\text{det}}$."

"The density of diatoms becomes" $\longrightarrow$ "To account for the additional buoyancy through TEPs (Jokulsdottir and Archer, 2016; Mari et al., 2017), we , we here simplify and assume that the frustule density is lowered by TEPs in dependency on the freshness of detritus. Eventually, the diatom density, ρ_{diatom}, becomes"

- P. 13, line 1: mention that this forcing is based on ERA reanalysis (be specific) and avoid the abbreviation OMIP which you don't explain (or explain it)

  ⇒ We now provide more information on the OMIP forcing and avoid the word OMIP.

  → "We run both, the standard and the M^{4}AGO run, in a GR15/L40-OMIP setup. This translates to a horizontal resolution of about 1.5°, 40 uneven vertical layers with highest resolution in the first few hundred meters of the ocean. OMIP is a climatological daily atmospheric forcing (Röske, 2005)." $\longrightarrow$ "We run both, the standard and the M^{4}AGO run, in a GR15/L40 setup with climatological forcing. This translates to a horizontal resolution of about 1.5°, 40 uneven vertical layers with highest resolution in the first few hundred meters of the ocean. The climatological atmospheric boundary conditions are derived from the second European Centre for Medium-Range Weather Forecasts (ECMWF) Re-Analysis project (ERA-40; Simmons and Gibson, 2000; Röske, 2005). The mean annual cycle of i.e. wind stress, heat and freshwater fluxes are resolved on a daily basis. The continental freshwater runoff is provided by means of a runoff model (Röske, 2005)."

- P. 13, line 3/4: are these global numbers? What are corresponding model parameters?

⇒ Yes, these are global number. We rephrased the sentence for clarity . As stated in the text, the values are the weathering rates for the substances: $CaCO_3$, dissolved organic phosphate, and silciate.

→ "The loss of POM, opal, and $CaCO_3$ due to sedimentation and subsequent burial was accounted for through homogeneously applied weathering rates which were adjusted accordingly, namely for the standard / M^{4}AGO run: $CaCO_3$ ≈17.2 / 26.5 T mol C yr^{-1}, dissolved organic phosphorus ≈99.6 / 101.5 G mol P yr^{-1}, and silicate ≈3.2 / 2.3 T mol Si yr^{-1}" → "The loss of POM, opal, and $CaCO_3$ due to sedimentation and subsequent burial was accounted for through homogeneously applied weathering rates which were adjusted for the standard run (and the M^{4}AGO run): Globally, we add ≈99.6 (101.5) G mol P yr^{-1} as dissolved organic phosphorus, and ≈3.2 (2.3) T mol Si yr^{-1}. To compensate for the loss of $CaCO_3$, we add ≈17.2 (26.5) T mol C yr^{-1} to surface dissolved inorganic carbon (DIC) and a corresponding amount to surface total alkalinity, A_T, as on DIC:2A_T."

- Table 1: caption: "The value for the Martin curve. . ." . Why is this single parameter given in the caption, please add it it to the list of parameters in the main body of the table.

⇒ Since the table is about the new M^{4}AGO parameters, we completely remove the sentence. The value is still provided and discussed in the appendix.

→ Removed.

- P. 16, line 4: "a minor role in biogenic fluxes". This statement needs a reference.

⇒ We provide now references.

→ added: "(described by e. g. Berelson et al., 2007; Fischer and Karakaş, 2009, Fischer et al., 2016)"[6].

[6]Berelson, W. M., Balch, W. M., Najjar, R., Feely, R. A., Sabine, C., and Lee, K.: Relating estimates of $CaCO_3$ production, export, and dissolution in the water col-

- P. 16, line 33/34: "adaptation .. within a few years." Is adaptation the right word here? Maybe "an equilibrium was established"? or its change after a few years was small..

  ⇒ We rephrased the whole sentence and split in in two sentences for clarity. We now use the word 'adjustment' instead of 'adaptation'.

  → "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."

2.6 Results

- P. 22, line 12-13: "diatom-dominated aggregates feature a high buoyancy through TEP." Is there any evidence for such behavior or is this a major model bug?

  ⇒ Yes, there is evidence for such behavior and we discuss the low excess densities of diatom-dominated aggregates in l. 19-21 on the same page. TEPs indeed feature high buoyancy (their density has

umn to measurements of $CaCO_3$ rain into sediment traps and dissolution on the sea floor: A revised global carbonate budget, Global Biogeochemical Cycles, 21, GB1024, https://doi.org/10.1029/2006GB0028

Fischer, G. and Karakaş G.: Sinking rates and ballast composition of particles in the Atlantic Ocean: implications for the organic carbon fluxes to the deep ocean , Biogeosciences, 6, 85–102, 2009

Fischer, G., Romero, O., Merkel, U., Donner, B., Iversen, M., Nowald, N., Ratmeyer, V., Ruhland, G., Klann, M., and Wefer, G.: Deep ocean mass fluxes in the coastal upwelling off Mauritania from 1988 to 2012: variability on seasonal to decadal timescales, Biogeosciences, 13, 3071 – 3090, 2016 been estimated to be about 700-840 kg m^{-3}, Azetsu-Scott and Passow, 2004 as cited in the mansucript). The excess density of modelled, fresh aggregates in diatom-dominated regions is with about 2 kg m^{-3} in the observed range of about 0 to $\sim$ 10 kg m^{-3} is given by Alldredge and Gottschalk, 1988, Ploug et al. 2008 and Iversen and Ploug 2013 (as cited in the manuscript). We want to emphasize here again that the sinking velocity provided in Fig. 6 and 7 are mean sinking velocity. Observed sinking velocity of individual, large aggregates cannot be compared directly to the mean sinking velocity. Modeled large aggregates feature substantially higher sinking velocities than reflected by the mean sinking velocity. Large aggregates in our model thus have similar sinking velocity as compared to observed marine snow. See also the reply in the general part.

→ Remained.

- P. 23, line 25: I assume z0 is 100m, please clarify.

  ⇒ Yes, it is. It was introduced z_0 on p. 4, l. 21. We added z_0.

  → "The transfer efficiency of POC from 100 m to" → "The transfer efficiency of POC from $z_0 = 100$ m to"

- Line 26: “to about 1000m” → at 1000 m.

  ⇒ "at" would be wrong for two reasons: first, it is the transfer efficiency from 100 m to (about) 1000 m, and second, we don’t interpolate the fluxes to exactly 1000 m, but use the fluxes across the model layer boundary at 960 m depth to calculate the transfer efficiency.

  → Remained.

- P. 28, line 29: this is not shown in Fig 7a, you only show mean density, not the effect of opal on density.

  ⇒ We disagree. Even at deep ocean regions, where detritus is almost remineralized (and thus POM and TEP play only a minor role for primary particle density), primary particle density of diatom frustules in silicifier-dominated regions (e. g. south of -40 °) is significantly lower than primary particle density in calcifier-dominated regions (e. g. at about -20 °). Hence, opal acts less than $CaCO_3$ as ballasting agent in our model.

→ Remained.

- Line 30: any indication in the literature and any scientific explanation why silicate frustule size affect the sinking speed if not by density?

  ⇒ This is indeed a good question. Unfortunately, observational studies have so far heavily focused on aggregate size-to-sinking velocity relationships. Minor focus has been put on the solid hydrated density. Hence, even less emphasis has been put on the relation of primary particle sizes, microstructure and sinking velocity. However, as we discuss below line 30, studies by e. g. Laurenceau-Cornec et al. 2015 point to a relevance of cell size and morphology, and dominant size of primary producers have been suggested to drive interannual changes in export fluxes (Boyd and Newton, 1995, see the manuscript). From theoretical considerations, the size of primary particles can affect the porosity, Eq. (6), and hence the aggregate excess density (irrespective of the density of the single primary particles). This is described in the manuscript by Eq. (5). We again searched for relevant literature on experimental studies that clearly disentangle these aspects for marine aggregates. A very recent study by Laurenceau-Cornec et al. (2019)[7] provides a summary, how porosity can vary and can have a decisive role for determining the settling velocity. A new study by C. Flintrop is in preparation that will provide further insights into settling dynamics of marine aggregates. We now explicitly refer to the relevant equations to address, why primary particle size can affect the packaging and thus sinking velocity. We add an explanatory sentence and provide the reference to Laurenceau-Cornec et al. (2019).

[7]Laurenceau-Cornec, E. C., Le Moigne, F. A. C., Gallinari, M., Moriceau, B., Toullec, J., Iversen, M. H., Engel, A., and De La Rocha, C. L.: New guidelines for the application of Stokes' models to the sinking velocity of marine aggregates, Limnol. Oceanogr., https://doi.org/10.1002/lno.11388, 2019

→ Adding on p.28, l. 32 (after: "...likely play a role."): "As indicated by Eq. (5) and (6), primary particle size affects the excess density and porosity of aggregates, which have decisive effects on sinking velocity (Laurenceau-Cornec et al., 2019)."

- Fig 10: colorbar label: conribution → contribution(add 't')

⇒ Thanks. Changed.

→ added the "t", see Fig. 1

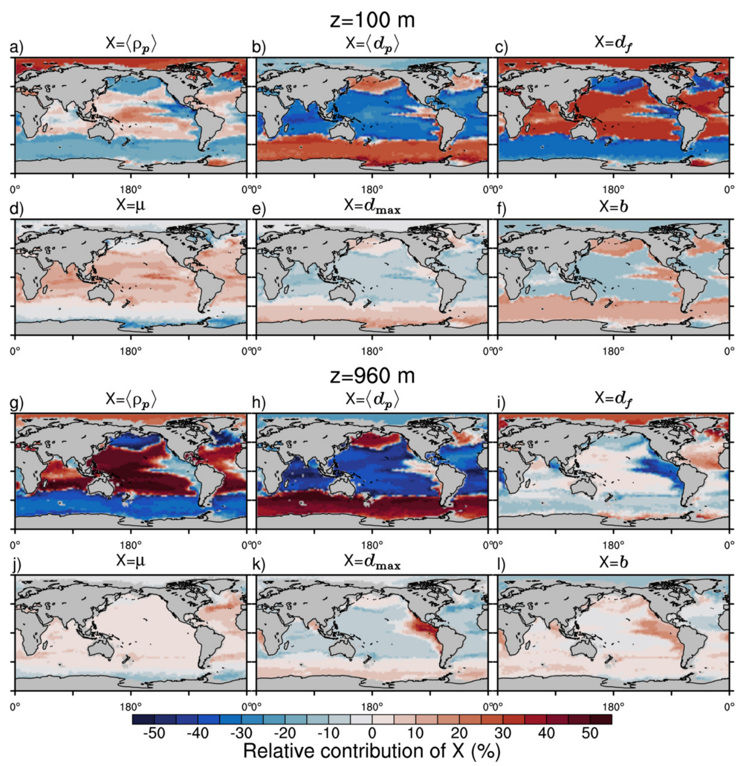

Figure 1: "conribution" → "contribution"

- P. 35 and Figure 14a: what is the reason of showing cumulative CO2 fluxes integrated over latitude? Please show just the zonal means, that's much easier to understand and compare to data. The units should not include per degree if it is cumulative.

⇒ We apologize for the wrong units. However, cumulative fluxes give directly the net-transport across the equator and also provide the information that both model runs are in well spun-up states. We therefore remained with the cumulative fluxes and only corrected the units.

→ Units changed.

- Figure 14c-k: cumulative fluxes make more sense here. I'd prefer actual fluxes/time and then the difference between the two could be cumulative. Then, one y-axis might also be enough.

⇒ We agree with the reviewer that cumulative fluxes make sense here since the subfigures provide a direct insight in how much CO_2 is taken up in the course of the years. We thus remain with the figure as it is.

→ Remained.

- P. 37, line 24: suggested → hypothesize (careful which tense you use). Also, please please back up this hypothesis with literature.

⇒ We here refer to the suggestion made earlier in the manuscript (in Sect. 3.5). We therefore keep 'suggested', and now explicitly refer to the section. In Sect. 3.5, we discuss and cite a number of experimental studies that link sinking velocity of marine aggregates to morphology and size structure of the phytoplankton community. Together with the theoretical derivation of the aggregate excess density (Eq. (5)) and porosity (Eq. (6)), being dependent on primary particle size, these and our study provide reasonable indications that primary particle size (or -diatom- cell size, respectively) potentially affects the sinking velocity of marine aggregates. This led us to suggest primary particle size as an additional factor for aggregate sinking. We are, however, aware that full experimental evidence is, to our best knowledge, lacking and will hopefully be part of future research, where the role of microstructure and composition will be deciphered and disentangled.

→ "We suggested" → "Previously in Sect. 3.5, we suggested"

- P. 38: you have not shown silicate distribution – is that reasonable? You refer to low transfer-efficiency in silicifier-dominate region, but this is not the case in the Southern Ocean, nor do you see much of an impact in Figs 15 a and d in the Southern Ocean, which is THE region dominated by silicifiers. This needs more explanation.

  ⇒ We show in Fig. 5a,c the opal-to-POC flux ratio. As described in the methods part, Sect. 2 (first paragraph), HAMOCCs opal production is directly coupled to silicate availability. The regions of silicifiers are therefore well defined and previously described in the manuscript (and silicate is included in our general Taylor diagram-based analysis of nutrient fields). With respect to the low transfer efficiency, Fig. 15 g clearly shows a drop in transfer efficiency for the AAZ and SAZ regions, once the frustule size of diatoms, $d_{p,\mathrm{frustule}}$ is decreased (cmp. M^{4}AGO run with $S(d_{p,\mathrm{frustule}})$, where $\langle T_{\mathrm{eff}} \rangle$ decreases from about 0.24 to about 0.14 for the Antarctic Zone (AAZ), and from about 0.19 to about 0.11 for the Subantarctic Zone (SAZ)). To make the difference more clear, we rephrase the sentence, where the sensitivity study is compared to the original M^{4}AGO run. We further add the specification, where the largest signal in silicate increase can be found.

    → "As a consequence, the transfer efficiency in silicifier-dominated regions is low (Fig. 15 g). Accordingly, opal dissolves closer to surface waters and the silicate concentration increases with respect to the M^{4}AGO run in silicifier-dominated regions (Fig. 15 d)."
    → "As a consequence, the transfer efficiency in silicifier-dominated regions is lower in $S(d_{p,\mathrm{frustule}})$ than in M^{4}AGO (Fig. 15 g). Accordingly, opal dissolves closer to surface waters and the silicate concentration increases compared to the M^{4}AGO run in silicifier-dominated regions, particularly in and downstream of coastal upwelling regions (Fig. 15 d)."