1 Responses to Reviewer I

Dear Reviewer,

many thanks for you valuable comments on our manuscript. In the document below, please find

⇒ in blue your initial review comment

⇒ in darkcyan our reply, with potential indicated ~~to be changed~~ → modifications/additions in the manuscript text

Kind regards,

Joeran Maerz on behalf of the co-authors

1.1 General comment

The study by Maerz et al. provide an extensive analysis of the M4AGO parametrisation in a context of climate change. This parametrization includes temperature-dependant remineralization, oxygen limitation of remineralization, sea water viscosity, ballasting (composition) and a microstructure (fractal dimension / porosity) representation with aggregation/desagregation processes including particle density, size and stickiness. Sinking velocity is ultimately considering sea water viscositiy, particle composition (of ballast material), particle density, porosity and size. The study is very well written (although very verbose and some times convoluted), and provide a dense, comprehensive, well referenced, honest (especially about the limited impacts on global air-sea CO2 fluxes and limitations in general) and transparent analysis of this ambitious parametrization. The authors demonstrate a very high level of mastery in their disciplines.

⇒ Many thanks. We very much value the appreciative tone of the review.

They found little influence and global scale but highlighted regional important differences such as in the Arctic Ocean.

The review of this article was challenging. About 23 pages that must also include the lengthy study of Maerz et al. 2020 (another very lengthy and technical paper introducing the M4AGO parametrization). The writing is sometimes lengthy and technical. The paper in general would deserve a more synthetic and accessible bite. The problem with that is I really wonder who is able to read and actually digest this article beyond the small BGC modeling community.

⇒ We acknowledge that reviewing such manuscript is demanding, in parts due to its reliance on the previous publication of Maerz et al. (2020) which introduces M^{4}AGO. When revising the manuscript, we will sharpen the presentation to improve accessibility while retaining the technical detail required for the targeted biogeochemical modeling and observational communities.

The other problem is related to the microstructure parametrization which the authors claim is an important factor elucidating regional patterns of the BCP. If all other parametrizations are relatively simple and are developed similarly in other models, the microstructure parametrization increase complexity substantially with a lot of under- (or non-) -constrained parameters (see Maerz et al. 2020). I am aware that the authors already acknowledge this, guaranty computational efficiency and provide quantitative effects. Still, this is very hard to proof what is done here especially noting that the code is not open. How can this parametrization could be evaluated with observations? (The comparison with CO2 fluxes does not necessarily show an improvement to be honest). How could we constrain more the numerous parameters? (although if not achievable now, what could be used in the future?) I know they acknowledge there is little information available so far, but how could we proceed then? Are we sure the regional patterns are more realistic?

⇒ Maerz et al. (2020) aimed at very thoroughly documenting M^{4}AGO including its underlying assumptions and potential weaknesses to make it accessible to other researchers. This is supported by the code availability being open as part of the current MPIOM master branch (the ocean component of MPI-ESM) as well as ICON-O, the successor model, both licensed officially under the BSD-3-C License. In addition, the first author is the maintainer of a publicly available M^{4}AGO standalone code basis, which is under active further development, also licensed under BSD-3-C License. We consider to provide this information on the repository explicitly as well.
As already partially demonstrated by Maerz et al. (2020), M^{4}AGO can be regarded and serve as an extendable framework that aims to bring observations and models closer to each other. We acknowledge that some parameter uncertainties in M^{4}AGO are high. However, in contrast to simpler parametrizations, all parameters of M^{4}AGO are technically measurable and are consistently and mechanistically linked to each other. Thus far, limited observational throughput and thus

statistics certainly limit the capability for comparison. For example, M^4AGO rather simulates mean particle than individual particle properties. However, (subsets of) the methods underlying M^4AGO are applicable even for individual particles and thus could build a bridge from individual particles to M^4AGO which would allow to evaluate the model approach. In terms of regional patterns, our comparison of e.g. the transfer efficiency through climatological fluxes as derived by Weber et al. (2016) likely provides a reasonable constrain at least for present day climate which we better capture with M^4AGO (as previously discussed in Maerz et al., 2020). We are, however, fully aware that M^4AGO has its limitations, which we discussed in Maerz et al. (2020) and the present manuscript. Nevertheless, we are optimistic that M^4AGO captures general particle features (see also comment on line 410ff below).
Since CO_2 fluxes, as discussed in the manuscript, are strongly governed by circulation, CO_2 not necessarily provides the best measure to judge advancements in ocean biogeochemistry. Given the deficiencies in representing ocean circulation and ocean mixing, we believe that ocean biogeochemistry modelling is a bit stuck in a 'performance-limbo' - trying to build -easily- tunable OBGC models that tend to cover ocean circulation deficiencies (e.g. limited -sub-mesoscale- eddy representations, sluggish circulations responsible for extended oxygen deficit zones as discussed in the manuscript, etc.). Circulation deficiencies tend to be become amplified in OBGC model responses. As a consequence, OBGC models tend to lack fidelity in process representation (e.g. in our case the Martin-curve like representation, where e.g. transfer efficiency is almost fixed entirely) and response to climate change in future scenarios. The latter underpins the need to explore a variety of model representations, i.e. here via M^4AGO for the biological carbon pump.
Bringing datasets together or building new, more complete datasets would thus be beneficial - i.e. featuring particle composition, size, density and sinking velocity, ideally including further environmental variables like temperature, salinity, turbulent shear rate and (vertical) turbulent mixing rates. We further believe that with increasingly available optical data, even the link between particle composition (based on particle grey scale values and/or differences in RGB channels), particle density and size could be established in a statistically meaningful sense, which would enable to verify or falsify M^4AGO's underlying assumptions and to expand M^4AGO further.

On the regional aspect, the authors put a lot of emphasis on the Arctic

Ocean (and OMZ). If conclusions rather make sense for most regions to me, I was still puzzled by the conclusions drawn for the Arctic mostly because of the lack of synthesis capacities of the authors. Explanations are scattered around which makes a lot of work for the reader to re-assemble the results and conclusions. They show that M4AGO allows higher transfer efficiency compared with CMIP6 (Appendix D show maximum sinking velocity in the Arctic? Why?). But climate induce change towards:

- More NPP, more export, less compact & larger particles, more buoyant (decrease sinking velocity)
- Warming temperatures decrease viscosity (increase sinking velocity)
- Less Opal / more calcite compared to POC in the future (total effect?)

→ If I understand well, this overall has the effect of decreasing the sinking velocity in the Arctic (Appendix D)

However, this is combined with:

Warming temperatures increase remineralization

I finally got the sense of the overall message: The total effect is RLS & transfer efficiency decrease despite increase in NPP (positive feedback loop). The authors should wrap this up somewhere better, it's not an intuitive result. Same for other regions eventually.

⇒ Many thanks. We will provide an aggregated view on the underlying responses by showing a radar plot (Fig. 1). For the revision, we will likely further split the current graphics up (and consider aggregating it further) and focus on a subset of the ocean regions (e.g. the Arctic Ocean region), while showing the remaining ocean regions in the appendix.
For further explanation on this matter, in the Martin-curve approach in the CMIP6 version, the annual mean transfer efficiency remains fairly fixed for both periods that is smaller than in M^4AGO in the high latitude regions (see Fig. 4 of the manuscript; the fixed transfer efficiency also becoming immediately clear from Eq. (34) in Maerz et al., 2020, when considering globally fixed remineralization rates and linearly increasing sinking velocity with depth that determines the Martin curve slope - note that this is only for climatological POC fluxes, while on

sub-annual time scales, the transfer efficiency can still vary a lot due to time lags in vertical POC fluxes with depth). This in itself already has an effect on the different response particularly in the Arctic Ocean (i.e. higher transfer efficiency can be associated with deeper nutrient loss and thus less NPP). Additionally in M^4AGO, the increased NPP in the Arctic Ocean leads to more buoyant particles (decreasing fractal dimension and mean primary particle density, while increasing mean primary particle size and maximum particle size). As an overall effect, this reduces the RLS (see also Fig. 3 below in this reply) and thus reduces transfer efficiency in the future (while it still remains higher than the transfer efficiency modeled via the Martin-curve approach).

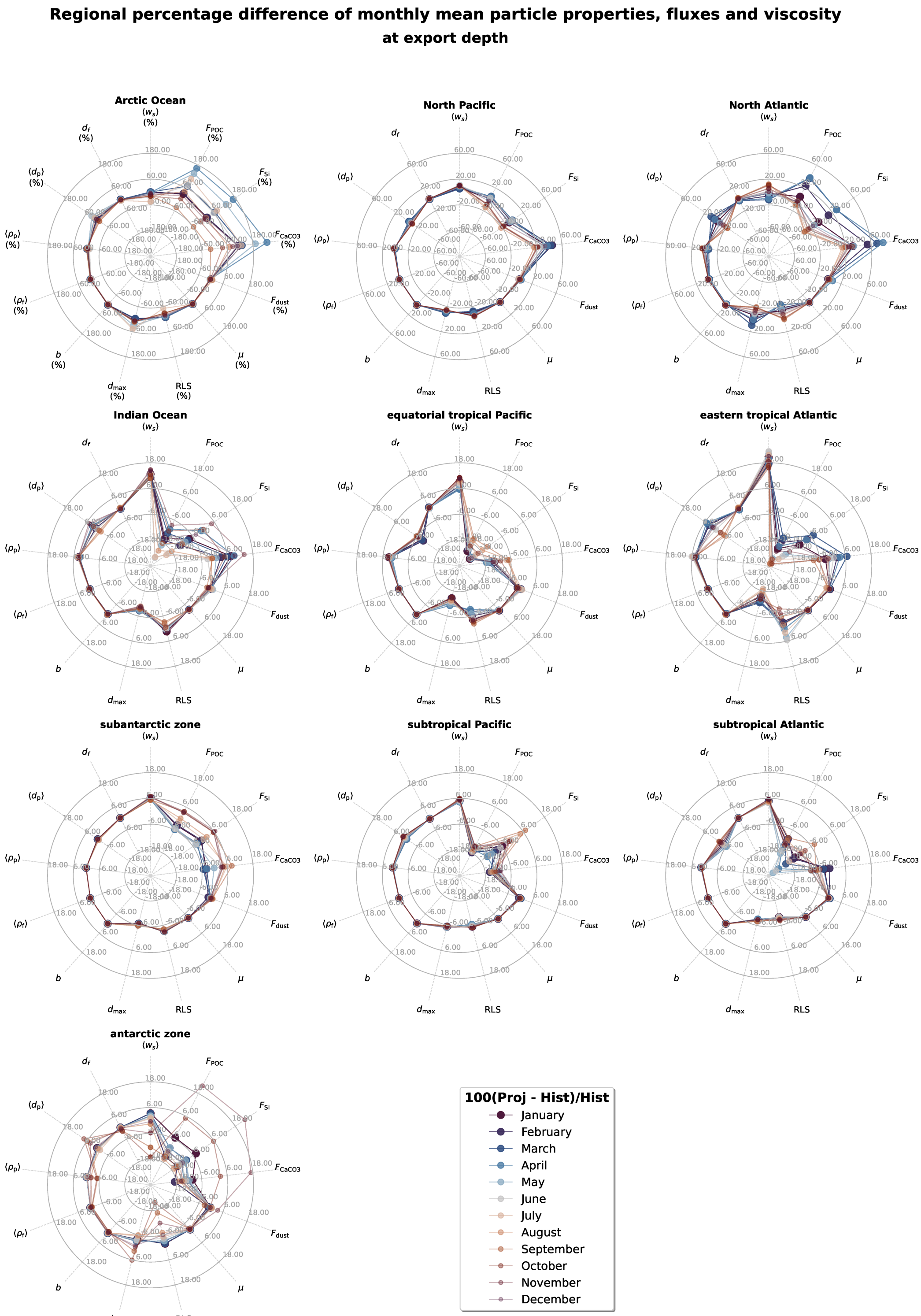

Figure 1: Radar plot for climatological percentage difference between the projection and the historical time period of monthly, area-weighted mean particle properties, particulate export fluxes and molecular dynamic viscosity at export depth for major ocean regions. Note that first row represent different ±-percentage changes than the rest of the subplots.

This article is certainly worth publishing, but I would recommend a few changes and clearer explanations before doing so.

1.2 Specific comments

I have noted point-by-point comments below:

Line 107: What are the limitations of such hypothesis? In general this does not stand in case of strong lateral advection.

⇒ We agree with the general limitations through a fixed power law form of a size distribution representation. However, the power law size distribution is widely used and accepted as reasonable approximation in literature on open ocean particle size spectra. We discuss the limitations of the fixed size distribution form in the general discussion section (and discussed it also previously in Maerz et al., 2020) particularly for high resolution applications (l. 538ff of the initial submission). Note, however, that these limitations cannot be overcome by more simplified model representations, either. To mention it, we were positively surprised to see i.e. the spatial structure of sinking velocity and other particle properties in a cross-shore transect in the ICON coast setup that is well in line with observationally based findings (cmp. Fig. 12 in Mathis et al. (2022) to Fig. 4 and 5 in Maerz et al. (2016)). This is particularly possible, since M^{4}AGO captures both, size distribution and particle density, in a physically reasonable manner which is a step forward in bringing these key particle properties explicitly together. We are, however, aware that it ideally deserves a thorough testing where the parametrization breaks or produces too large biases and that it deserves future investigations on how to incorporate the represented particle properties more adequately in models, when also better capturing size distribution dynamics. Both, further testing and incorporating size dynamics while remaining computationally feasible, remain challenging. We address the comment by more explicitly mentioning the fixed functional form issue in the general discussion section: "These features make an application in high-resolution, sub-mesoscale resolving ocean models possible and promising (Jungclaus et al., 2022; Hohenegger et al., 2023; Nielsen et al., 2025), while the thus far limited representation of size distribution dynamics → , namely through a fixed functional form and lower represented variability than measured, and internal homogeneous particle composition poses challenges to represent particle dynamics in high resolution models adequately."

Section 2.2: Why not adjusting calcite?

⇒ As a sinking tracer with a water residence time of about a few month, e.g. assuming an average sinking velocity of about $30\,\mathrm{m\,d^{-1}}$, there might be an initial mismatch between adjusted alkalinity and DIC, but this does not affect the long term behavior of the model and leaves minimal trace in the sediment and thus bottom waters. For $CaCO_3$ production, the amount of deposited $CaCO_3$ aligns well with literature values. Hence, we were neglecting performing further adjustments to the M^4AGO model.

Line 161: physical internal variability is not assessed, is there any differences in the physical fields?

⇒ The physical model MPIOM is identical in both simulations, M^4AGO and the CMIP6 version. As described, we branched off the physical restart file from the pre-industrial control run of the CMIP6 version. To allow for the adjustment of the biogeochemistry, the spin-up of M^4AGO-MPIOM HAMOCC was extended by approximately 700 years under pre-industrial conditions. Please note, that the changes in the biogeochemistry do not feedback to the physical ocean. Therefore, the physical ocean in M^4AGO shows the same internal variability as the CMIP6 version. With respect to the physical fields, the M^4AGO simulation could be regarded as an additional realisation to the 10 existing ensemble members of MPI-ESM1.2-LR CMIP6 simulations available on the ESGF repository.

Line 165: Not true. Stratification only decrease in the Atlantic sector of the Arctic. Fix also statement line 227.

⇒ To provide evidence for MPI-ESM showing less stratification during summer in the future, we here provide the monthly evolution and changes of the vertical maximum stratification of the topmost 500 m in the Arctic Ocean. Changes show clearly a mean weakening particularly towards end of summer in vast areas of the Arctic Ocean for the future in MPI-ESM (true for both simulations, showing very similar response, here shown for the M^4AGO run, see Fig. 2). For the manuscript revision, we consider aggregating the plot further to only show the mean summer season changes.
CMIP6 models represent the Arctic Ocean atlantification and its general response to climate change in a diverging manner (Muilwijk et al., 2023) and annual mean values cannot capture such seasonality-governed

monthly climatological mean in the Arctic Ocean. We will clarify this further in the manuscript text.

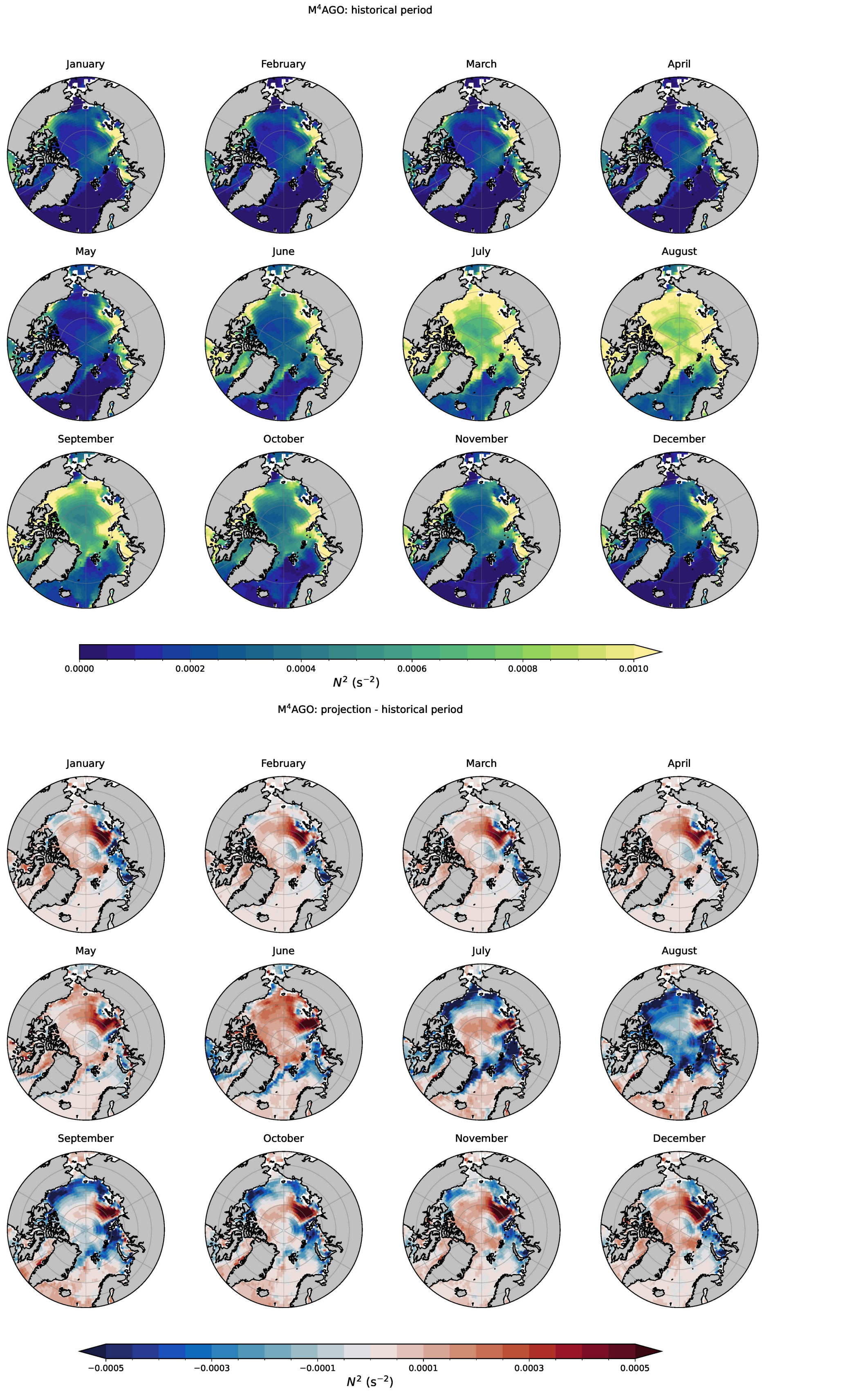

Figure 2: Monthly climatological vertical maximum squared buoyancy frequency, N^2, in the topmost 500 m and its future changes in the Arctic Ocean - here for the M^{4}AGO simulation, the CMIP6 simulation shows very similar pattern and response.

Line 174: With all due respect, this sentence is too complicated. There is sea ice now and there always will be ... in winter. You are talking about summer sea ice. Seasonality, I guess you refer to the winter polar night (absence of light → no NPP). And yes the Arctic is a small ocean but what the point if you discuss relative changes in?

⇒ We will rephrase and shorten the sentence as follows: → In the northern latitudes (>66°N), NPP is generally low due to the ice cover at present day, and general seasonality. With continuing warming of the Artic Ocean, NPP increases by 20.3 % in the M^4AGO run and with 26.9 % even more in the CMIP 6 run in the future period.

Line 176: 100m is not the euphotic depth. It is a simplified threshold depth considered as the euphotic depth. Of course, much less accurate that an actual calculation of the euphotic depth (variable in time and space) to derive the export production. It's fine! But reformulate.

⇒ We fully agree that the terminology of euphotic depth is debatable in this context. We will redefine it in the model description and will consider 100 m as model-defined export depth in the revised manuscript throughout the text.

Line 179: While still using the SSP585 while we know this is not the way to go? Hausfather, Z. & Peters, G. P. Emissions – the 'business as usual' story is misleading. Nature 577, 618–620 (2020).

⇒ We acknowledge that SSP5-8.5 might overestimate CO_2 emissions (both, short and longterm). Our aim with the manuscript was to describe potential responses, which makes the SSP5-8.5 scenario as extreme scenario well suitable. We address the reviewers comment by explicitly mentioning that the SSP5-8.5 scenario is likely overestimating CO_2 emissions in Sec. 2.2 line 147: → ... to showcase responses under an extreme scenario, while acknowledging that it likely overestimates CO_2 emissions (Hausfather and Peters, 2020).

Line 204: Did I miss the obvious or the remineralization is not shown?

⇒ We initially neglected to show remineralization rates explicitly in this manuscript, since it was discussed and presented in Maerz et al. (2020), for example, see their Fig. 9. With the revision of the present manuscript, we will provide used sinking velocity and remineralization rate for the Martin case explicitly in Sec. 2.1: "Brief model description" (in the

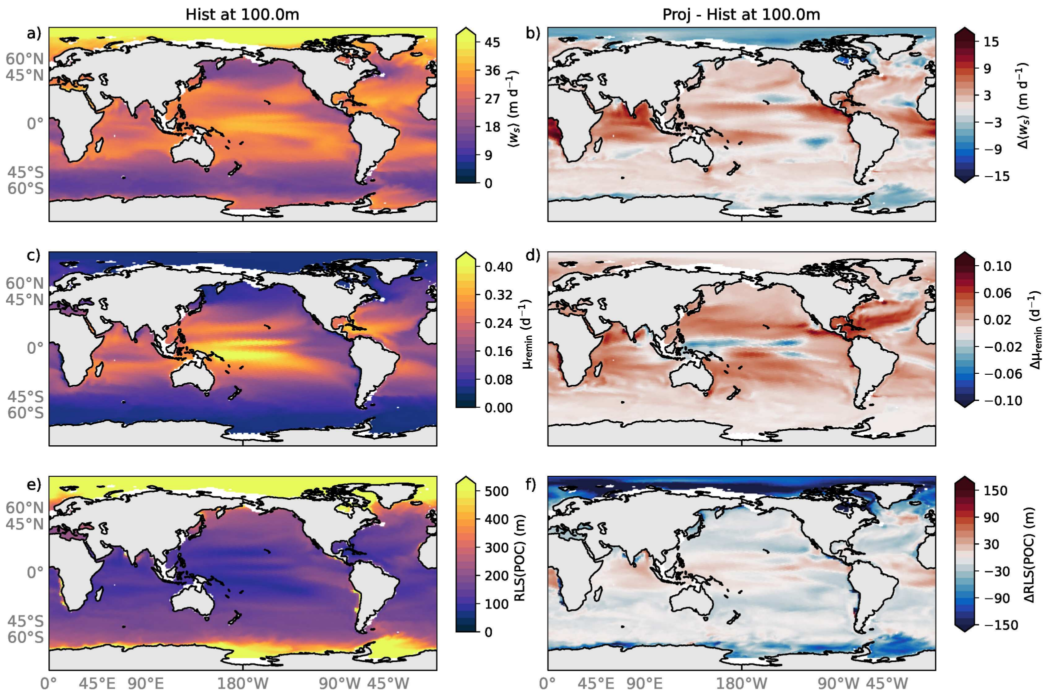

Figure 3: Climatological mean for the historical period and their changes in the future period in M^{4}AGO for a,b) sinking velocity; c,d) Q_{10}-dependent remineralization rate; e,f) remineralization length scales of POM. For comparison, the CMIP6 version features a globally constant sinking velocity of $3.5\,\mathrm{m\,d^{-1}}$ between 0 m to 100 m depth, a remineralization rate of $0.026\,\mathrm{d^{-1}}$ (times oxygen limitation) and thus a RLS(POC)≈135 m at export depth (assuming no oxygen limitation here for simplicity).

text and in a table, see also comments for reviewer II). Further, we will additionally to the sinking velocities and their changes in M^{4}AGO provide the remineralization rates and their changes and the remineralization length scales - all for the 100 m export depth - in the appendix Fig. D1. See here Fig. 3

Line 233: Sequestration. I have also used this word wrongly for while, I am not blaming, but could we fix that? You can refer to the nice Visser 2025 which clarifies: "carbon sequestration is synonymous with an offset of carbon emissions" https://doi.org/10.1002/lol2.70053 replace by storage at greater depth or similar.

⇒ As far as we understand, Visser (2025) aims at advocating for applying the term sequestration only for the offset of anthropogenic emissions. We agree that clarity in the terminology - also congruent with the definition in the IPCC report - is desirable and we will review our manuscript and follow the advice of the reviewer, where applicable. In line 233, we perform the modification: ~~sequestration~~ → storage

Line 255: Arctic Ocean amplification, ref: Shu, Q. et al. Arctic Ocean amplification in a warming climate in CMIP6 models. Sci. Adv. 8, eabn9755 (2022).

⇒ We will add: In combination with temperature-enhanced remineralization → ... due to Arctic Ocean amplification (Shu et al., 2022).

I agree but this is counter-intuitive for most reader and non-experts. Can you clarify here quickly what is meant? You mean that there is more POM and therefore, relatively, less ballast material in the composition of particles if I refer to Appendix C. Why seasonal average?

⇒ Seasonal was misleading (trying to refer to seasonally varing POC-to-mineral ballast, on average in favor of more POM, which rather complicated the message to convey). We rephrase the sentence to: The additional POM → compared to ballasting minerals increases the buoyancy of marine particles on ~~seasonal~~ → average and thus decreases settling velocity.

Line 260: If the inter-annual variability is represented by the STD, say it.

⇒ We will rephrase the sentence to: The largest interannual variability of transfer efficiency → , expressed as interannual standard deviation, is associated to ...

Line 270-272: needed?

⇒ Yes, since we would highly advocate for aiming for 1 yr (or more) measurements of the transfer efficiency in ocean regions, which would enable linking observations closer to models and also constrain models (as laid out by Kriest et al., 2023, for POC fluxes). Short term transfer efficiency calculations suffer from too large dependency on potentially vertically time-lagged fluxes (e. g. Giering et al., 2017, see also Maerz et al. 2020, Fig. C1), which could be overcome by year-long integrated measurements (under the assumption of relatively small inter-annual variability, see Fig. 4 d for regions more/less suitable for such an approach). Ideally this should be accompanied by measurements on processes affecting the RLS over the water column to enable bridging between observations and models. However, we are aware of the high logistical and costly requirements for such observational endeavor, which likely renders it challenging to achieve this. We will consider to move this part in a more aggregated form into the conclusion section.

Line 274: Even a flux cannot! Only change in storage.. See article by Frenger, I. et al. Misconceptions of the marine biological carbon pump in a changing climate: thinking outside the 'export' box. Glob. Change Biol. 30, e17124 (2024).

⇒ We here disagree with the reviewer, since net carbon fluxes (including biological carbon pump and the circulation- and mixing-driven DIC counter pump) ultimately set the storage. Both, Wilson et al. (2022) and Frenger et al. (2024, see their supplementary Figure S2-c) show that the carbon storage is affected by deep ocean carbon fluxes.

Line 309: you mean vertical DIC gradient right? fix through the text.

⇒ Yes. We will precision the respective text.

Line 316: I can understand why (simulations from data product or your simulation) internal variability is a problem, but why the mean of the observational product is?

⇒ Local, time point-wise means of the data product still feature an internal variability over the 30 year time period. This internal variability is not necessarily in phase with the internal variability of the simulations, which complicates the comparison. We will clarify this in the revision.

Line 355: time-cumulative?? you mean yearly integrated?

⇒ Yes, it is time-cumulative (as in time-integrated by summing up). To enable the reader to follow more easily, we will refer to the figure subplot (i.e. current Fig. 8 e,f).

Line 365: It is appreciated that the authors acknowledge that physico-chemical process dominate air-sea CO2 fluxes dynamics. Although this is repeated several times in the manuscript.

⇒ While this fact cannot be overemphasized, we agree that the statement is made a few times across the manuscript. We will thus shorten the text and delete the statement in applicable places.

Line 370: Yes the BCP if responsible for the most part of the vertical DIC gradient. Rephrase.

⇒ We will rephrase the sentence.

Line 410: I don't understand how more detritus production necessarily leads to less compact & bigger particles.

⇒ In M^4AGO, detritus features a slightly higher stickiness, leading to a lower fractal dimension and is generally less dense than mineral primary particles. This leads to lower sinking velocities at the same size as e.g. for highly compact, mineral-rich particles. Hence, POM-rich particles can grow larger until the critical particle Reynolds number for fragmentation is reached. From the observational perspective, the authors draw on e.g. coastal turbulent water studies, where the seasonality in particle/floc characteristics is often associated to the availability of fresh organic matter in addition to sediment (mineral) particles. Studies of e.g. Chen et al. (2005) and more recent studies of e.g. Fettweis et al. (2014) and others show frequently that organic-rich particles grow larger and are resistant to fragmentation beyond the Kolmogrorov microscale (a feature that also has been shown for open ocean regions by Takeuchi et al., 2019). In their laboratory study, Hamm (2002) also showed that (sedimentary) mineral ballasting acts as a size-reducing ingredient in a shear-free (apart from potential chamber wall-particle interactions) rolling chamber experiment. For the open ocean, slower sinking and more sticky organic-rich particles have a longer residence time in the upper water column, which also enables them to grow larger due to aggregation processes, before escaping via sinking to deeper regions where no or only little new organic matter for further aggregation is formed.
We will extend this part by providing more information, since it was also recommended by reviewer II to provide more explanation on particle properties in Sec.3.4 to understand particularly the Arctic Ocean

change in transfer efficiency. We extend/modify the text in the following: ~~In the high latitude regions,~~ → In the Arctic Ocean, where a transition from frequently ice-covered or at least ice-influenced to seasonally ice-free happens in future, increased NPP (see Sec. 3.1), and thus detritus production, leads to, on average, looser, less compact, larger particles. → As modeled by M^4AGO, detritus tends to be more sticky than mineral particles, which loosens the internal microstructure of particles due to less intrusion into each other upon collision. Further, the low density of POM compared to minerals lets particles grow larger until reaching the critical particle Reynolds number for fragmentation. While the overall contribution to sinking velocity was ambiguous in laboratory rolling chamber experiments of Hamm (2002), they also found a decrease in particle density and increase in size with decreasing abiotic mineral particle concentration similarly as represented by M^4AGO. In M^4AGO, this leads to a decrease in sinking velocity in the Arctic Ocean.~~The~~ → By contrast, the increase of water temperature in most parts of the euphotic zone and upper mesopelagic of the oceans leads to lower dynamic sea water viscosity and thus contributes to increasing sinking velocities.

Line 420: Explain me how temperature dependant remineralization has a direct effect on particles density and porosity? You mean temperature in general? I don't understand this sentence.

⇒ We will rephrase the sentence to: ~~Temperature-dependent~~ → Enhanced remineralization → in future due to temperature-dependency has thus a two fold effect on particles in M^4AGO → in these regions: i) an increase in primary particle density and ii) decreasing porosity, which together lead to denser, faster sinking particles.

As brief explanation here: Increasing temperature increases the remineralization rate (through the Q_{10}-approach). Hence, POC is remineralized faster in warmer regions and under future warming. This reduces the mean stickiness of primary particles making the particles more prone for compaction (i.e. stronger intrusion into each other during aggregation, but also less resistant against particle restructuring). This is mimicked by a higher fractal dimension. The process of POC remineralization is faster than the dissolution of opal and $CaCO_3$. As a consequence, particles become less buoyant and more compacted, both of which increases sinking velocity. However, increasing sinking velocity increases the particle Reynolds number and hence, the maximum diameter at which particles become vulnerable to fragmentation according to our critical particle Reynolds number approach is becoming smaller.

Line 338: variable distribution slope? you mean the size distribution? Not clear to me.

⇒ Yes, we meant the size distribution slope - we will clarify it accordingly, when revising the manuscript by adding → size distribution slope.

Line 500: Likely true. Positive feedback loop maybe see Oziel et al. 2025. Not represented in CMIP6 models... not so sure, prove it.

⇒ We here referred to our CMIP6 model version. We try to make this clearer and refrain to prove it for any other CMIP6-partaking model, which is beyond the scope of the manuscript. For our CMIP6 model version, the feedback loop is not represented.

Line 516: between

⇒ Thanks - we will fix the typo.

Line 545: "more realistic" in terms of process maybe, but in terms of model performance? Not sure.

⇒ Thanks. Yes, we meant process realism, which we will precision when revising the manuscript.

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