1 Responses to Reviewer II

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

Review of "Marine particles and their remineralization buffer future ocean biogeochemistry response to climate warming" by Maerz et al.

The manuscript by Maerz et al. provides an important synthesis of information related to particle formation, sinking, and remineralization processes in ocean biogeochemical models. The authors systematically compare a simple model version -referred to as "CMIP6"- with one that includes more complex particle-sinking and remineralization processes, "M4AGO", for both historical and future periods. They offer a detailed analysis of projected changes and assess the impact of using a more complex representation of particle sinking and remineralization.

The authors document the effect of representing marine particles and their remineralization in a more complex way (e.g., temperature- and oxygen-dependent remineralization, the effect of particle microstructure on sinking speed, such as the influence of ballast minerals) on climate projections. They clearly show that two regions most affected in terms of transfer efficiency are oxygen-deficient zones and the Arctic Ocean. These findings are highlighted in the results from the more complex model, "M4AGO" simulations. Authors also compare future changes (2070–2099) with historical periods (1985–2014) using two model versions: the simpler 'CMIP6' and the more complex 'M4AGO.' They demonstrate that marine particles play a role in buffering the future ocean biogeochemistry response to climate warming, especially in the tropical and subtropical regions. They detail how a simple representation of marine particles in a climate model could alter projections of future p-ratios in these regions.

Overall, the manuscript is well written, well organized, and highly informative for the biogeochemical modeling community. It makes a significant contribution to ongoing research on the biological carbon pump using Earth system models. However, given the frequent references to Maerz et al. (2020) and Mauritsen et al. (2019) and the length of the manuscript, the Methods/Conclusion sections could benefit from adjustments to clarify the comparisons for readers.

⇒ Many thanks for the generally positive review of our manuscript. We will address the comments in the following.

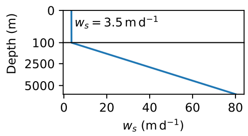
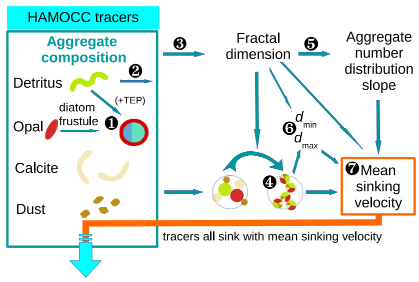
1.2 Specific comments

My specific suggestions and comments are listed below:

Line 117: I suggest adding a small table or a simple illustration highlighting the differences between the CMIP6 version and M4AGO. As it is, the reader needs to refer back to Mauritsen et al. (2019) and Maerz et al. (2020) to fully understand the setup. A summary of the key differences would make the comparison easier to follow in the subsequent sections. A similar addition could be made for Section 2.2.

⇒ We will provide a brief table (see Tab. 1) with key-different parameterizations in the revised manuscript. However, we will try keeping Section 2 as short as possible, since we believe that Maerz et al. (2020) documented M^4AGO and differences to the Martin curve approach extensively.

Table 1: Brief model differences between the CMIP6 and the M^{4}AGO version. $[O_2]$ represents the oxygen concentration, K_{O2} =10 µmol L^{-1} the half-saturation constant for oxygen limitation of aerobic remineralization and T the local water temperature. M^{4}AGO schematics taken from Maerz et al. (2020).

	CMIP6	M^{4}AGO
Sinking velocity		
POM (m d^{-1})		
Opal (m d^{-1})	30	
$CaCO_3$ (m d^{-1})	30	
Dust (m d^{-1})	≈0.05	
POM remineralization rate (d^{-1})	$0.026 \cdot \frac{[O_2]}{[O_2+K_{O2}]}$	$0.120 \cdot \frac{[O_2]}{[O_2+K_{O2}]} \cdot 2.1^{\frac{T-10}{10}}$
Opal dissolution rate (d^{-1})	$0.01 \cdot (0.1(T+3))$	$0.023 \cdot 2.6^{\frac{T-10}{10}}$

Line 185: These kinds of metrics are difficult to standardize. In the biogeochemical modeling community, different metrics are used for similar analyses, but they represent different concepts, such as the f-ratio, e-ratio, p-ratio, and s-ratio. In this manuscript, the p-ratio is chosen to represent export efficiency, defined as the ratio of export flux to NPP. Since you frequently cite Laufkötter et al. (2016), it might be less confusing for readers referencing the same literature if you adopt consistent notation and explicitly cite that paper when introducing the metric. In Laufkötter et al. (2016), the p-ratio refers to the ratio of total POC to NPP, while the e-ratio is defined as export efficiency, the ratio of export flux to NPP. I am aware that p-ratio is also used in Maerz et al. (2020); I just wanted to raise this point for clarity, in case authors wish to change.

⇒ The reviewers comment let us to careful review available literature. We very much appreciate the aim for clarity and using common definitions among the OBGC community. However, we believe that the nomenclature in Laufkötter et al. (2016) is rather the exception to the norm in terms of nomenclature - re-defining some ratios defined/precisioned earlier in the literature. I.e. the *p*-ratio and *pe*-ratio was earlier defined by Brix et al. (2004), Dunne et al. (2005) and Brix et al. (2006) (while admittedly, we haven't cited these sources in Maerz et al. (2020) and should have used *pe*-ratio as nomenclature instead, which we will do when revising the manuscript). In the present manuscript, we thus refer to the original literature (Dunne et al., 2005; Brix et al., 2006) and briefly annotate the equivalent in Laufkötter et al. (2016) - their *e*-ratio that by definition of Laws et al. (2000) also encompasses dissolved organic matter fluxes and non-gravitational POM fluxes. We apologize to Dunne, Brix and co-authors to not having citet them properly initially in Maerz et al. (2020). We will now do so in the present manuscript.

Line 204: 'Higher remineralization':

- I noticed that the comparison of remineralization rates between the two models is not shown in any of the figures presented in the manuscript. Could this be added as a supplementary figure? Adding a figure would help confirm whether the observed differences are indeed due to higher remineralization.

⇒ We initially neglected to show the remineralization, since it was discussed in Maerz et al. 2020 (see their Fig. 9). Acknowledging the wish for a more comprehensive manuscript also by reviewer I, we will explicitly mention values now in Sec. 2.1 (Brief model description), provide the values in the table (see above) and will provide a figure in the appendix. See Fig. 1 that will replace former figure D1.

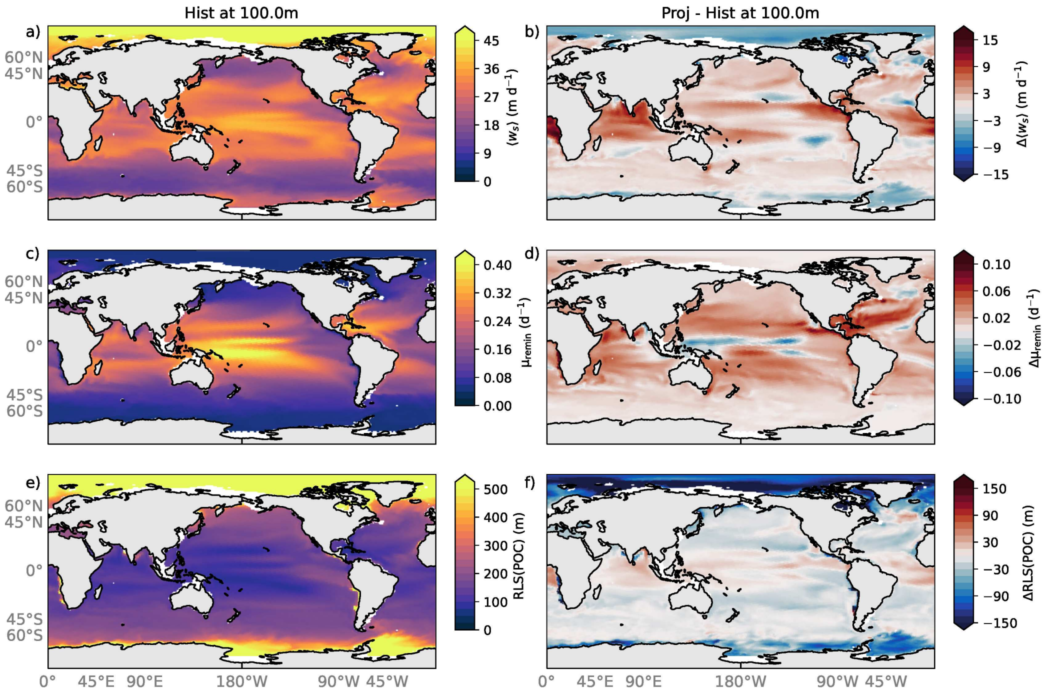


Figure 1: Climatological year mean for the historical period and their changes in the future period in M^{4}AGO for a,b) sinking velocity; c,d) Q_{10}-dependent remineralization rate; e,f) remineralization length scales of POM. For comparison, the CMIP6 version features a globally constant sinking velocity 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).

- When I checked the sinking speed from the standard model, it appears to be 3.5 m d^{-1} at the top 100 m. In contrast, in M4AGO, the concentration-weighted mean sinking velocity seems to be higher in the subtropics (Figure D1). Typically, one would expect higher nutrient export from the euphotic zone in a shorter time under such conditions. However, as stated, remineralization in the M4AGO case is significantly higher. Could you clarify how this balance between sinking speed and remineralization impacts nutrient export in models?

⇒ Indeed, instead of focusing only on sinking velocity, the remineralization length scales need to be considered when comparing two different sinking schemes in the same/similar circulation field to better understand loss of nutrients due to sinking and remineralization of POM to the mesopelagic. As Fig. 10 b shows only the zonal average of RLS, we will provide an additional subplot in Fig. D1 for the remineralization length scales for M^4AGO at export depth (and provide the globally constant value for the CMIP6 case, ≈ 136 m - here under the assumption of well oxygenated waters), see Fig.1. Eventually, this translates into the pe-ratio as a measure of nutrient loss to the mesopelagic (shown in Fig. 3 of the manuscript). Thanks for hinting at that point to make it more explicit in the manuscript.

Regarding your decision to adopt temperature-dependent remineralization with Q10 factors, what was the motivation behind this choice? Would you expect that the results would change a lot depending on your Q10 choice?

⇒ During the development of M^4AGO, we reviewed the available literature and found a suggested range for an optimal POM remineralization Q_{10} factor for the ocean spanning between 1.5 to 2.01 (Laufkötter et al., 2017) and 2.5 ± 0.2 (DeVries and Weber, 2017). In a detailed study on microbial remineralization dynamics of marine particles, Mislan et al. (2014) applied a $Q_{10} = 2.0$ based on an extensive physiological meta study by Dell et al. (2011). In the development/tuning process for M^4AGO published in Maerz et al. (2020), we saw some effects on the regional transfer efficiency values, when varying the Q_{10} factor, but not on the overall global pattern of transfer efficiency. Further, the equatorial and subtropical gyre phosphate concentrations posed another constrain on varying the remineralization rate and Q_{10} factor. For some sensitivity of the phosphate concentration and transfer efficiency on changing remineralization length scales, see Maerz et al. (2020), Fig. 15 - particularly by fixing d_f, which affects the sinking velocity as counterpart of remineralization, but also changing the frustule size, which also affects sinking velocity in silicifier-dominated regions. Eventually, we settled on the compromise between the three studies and chose $Q_{10} = 2.1$. While varying the Q_{10} relatively easily let's investigate the effect on the remineralization rate (just graphically), its feedback on sinking velocity in M^4AGO is more difficult to assess. It would thus deserve an in-depth study which is outside the scope of manuscript, while it certainly could be a valuable study on its own.

Line 208: The statement about "increasing stratification, weaker mixing, and less recovery of exported nutrients" is compelling. However, it would be helpful to back this up with evidence showing the relationship between increased temperature or increased stratification in your model results. Including figures in the appendix would strengthen the argument and make the reasoning easier to follow.

⇒ Thanks for this recommendation. We will include a figure showing the climatological annual maximum mixed layer depth (MLD) of daily maximum values as measure for the winter mixed layer and the potential to recover nutrients from deeper ocean layers. Further, we will show the mean maximum vertical stratification (represented by the squared buoyancy frequency N^2) as a measure on how strong the potentially nutrient-deprived mixed layer is separated from the below, typically more nutrient-rich ocean layers. For both measures, we also show the future changes (see Fig.2, which we will include in the manuscript). For completeness for the discussion, we below also show the monthly changes of these measures which we do not intend to populate further into a revised manuscript version (see Fig. 3 and 4, CMIP6 simulation shows similar pattern, not shown).

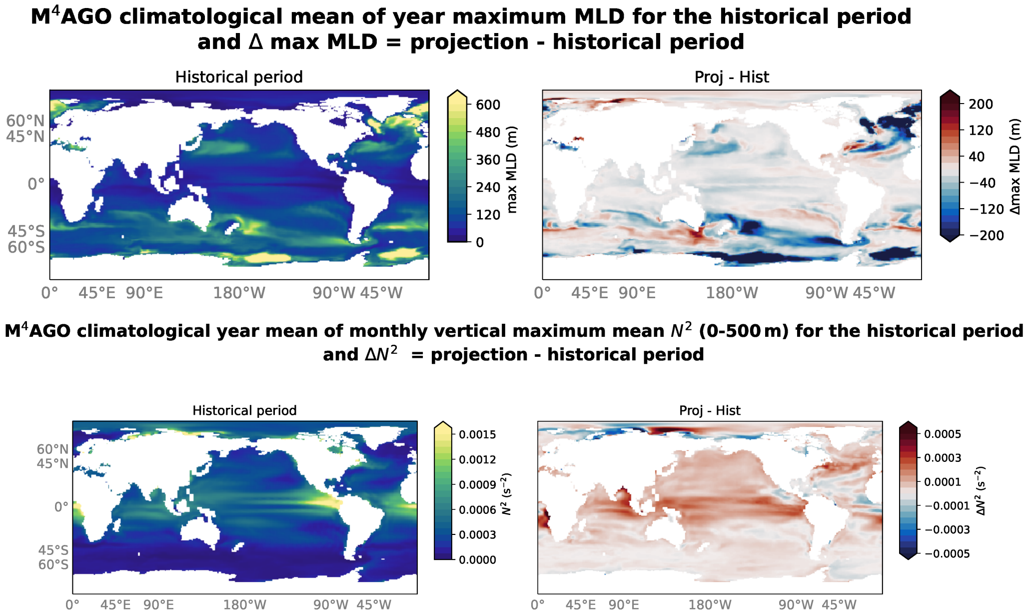


Figure 2: M^{4}AGO climatological maximum mixed layer depth in the historical period and its changes in the future projection. Below: Climatological mean of vertical maximum of monthly mean squared buoyancy frequency, N^2, of the upper 500 m in the historical period and changes in the future period.

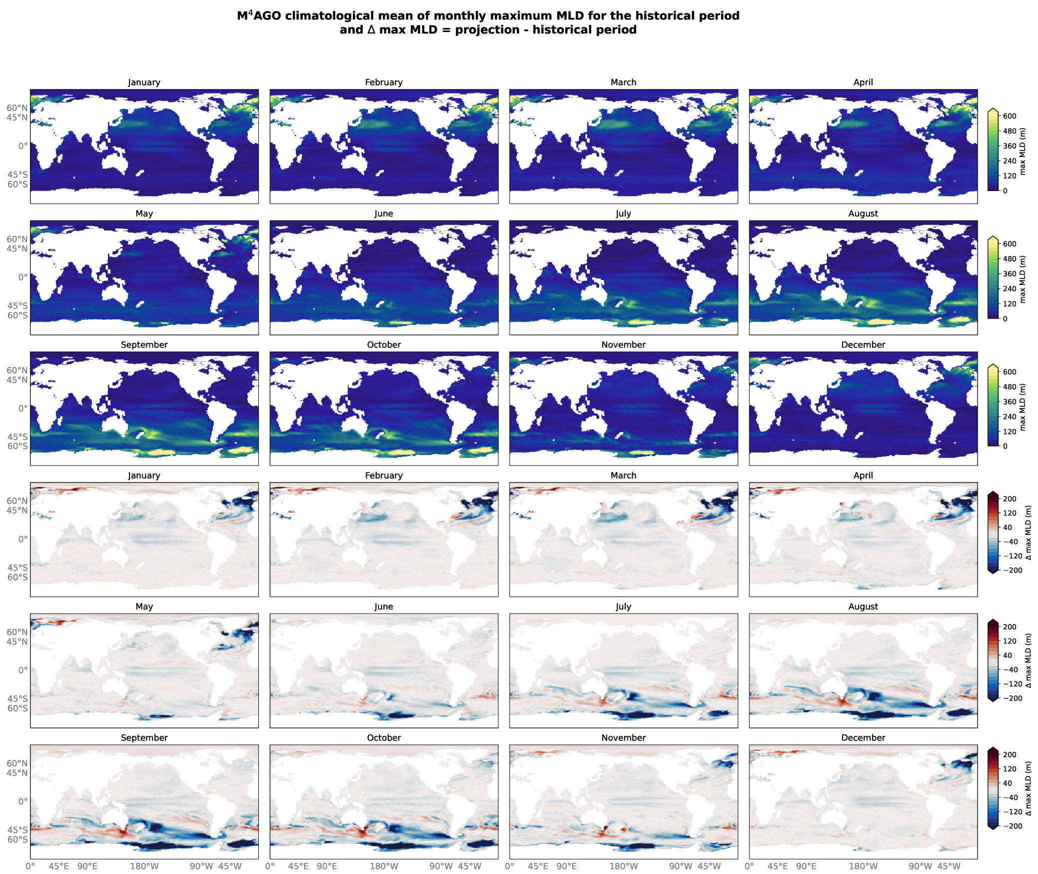


Figure 3: Monthly resolved maximum mixed layer depth for the M^{4}AGO simulation.

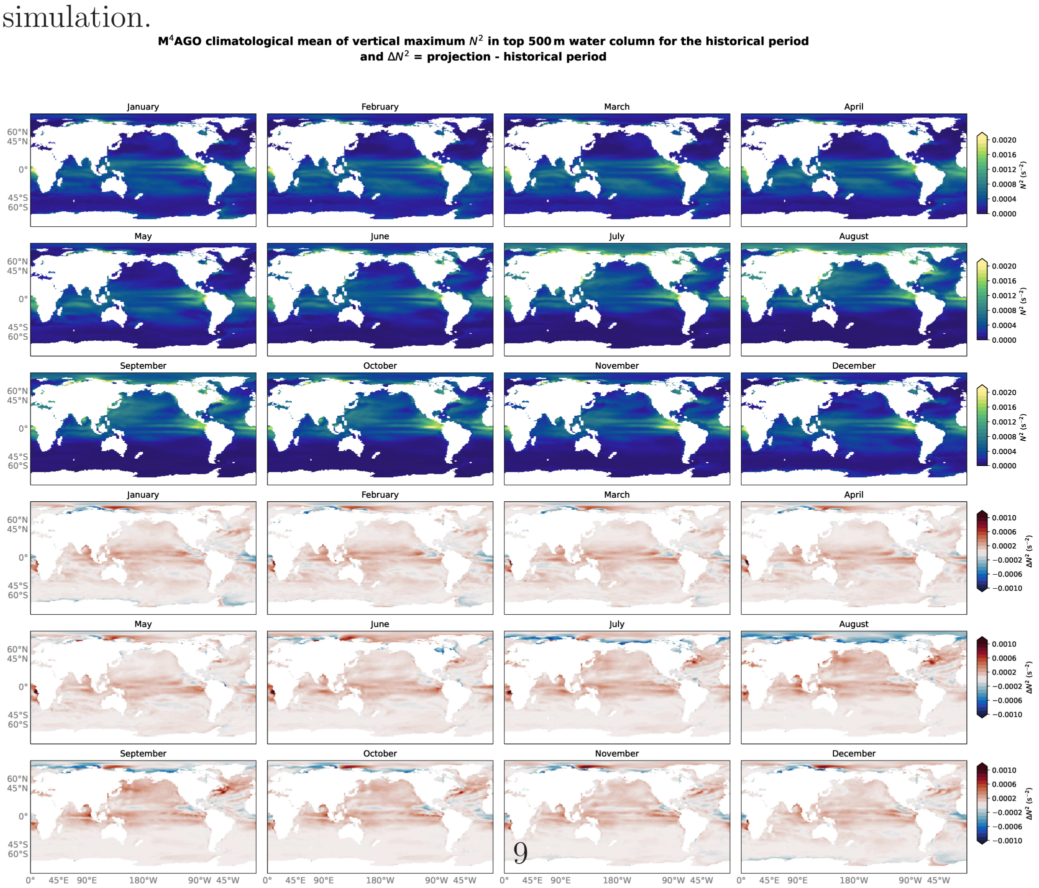


Figure 4: Climatological mean of vertical maximum of N^2 in the upper 500 m and future changes globally - here for the M^{4}AGO simulation.

Line 251: When I read Equation 2, the transfer efficiency appears to be independent of NPP. The primary driver of its change is the balance between sinking speed and remineralization. The manuscript states that adding POM increases the buoyancy of marine particles, thereby decreasing sinking velocity. Would it be more effective to integrate this explanation with the discussion on changes in particle properties in Section 3.4? While Appendix C also conveys this message, readers must carefully analyze the notations and navigate a rather crowded figure to understand it fully. Simplifying or consolidating these points in the main text could improve clarity.

⇒ We will consolidate these points in Sec. 3.4, when revising the manuscript. In Sec. 3.4, we will extend and clarify the part, where we address the high latitude changes in particle properties. In the context of line 251, we will thus reference to Sec. 3.4, instead of formerly Appendix C, while we believe that the short reference to particle buoyancy aids in grasping the reason for changes in transfer efficiency in the future Arctic Ocean. In addition, by providing a summary graphics, we belive to further summarize and clarify this behavior.

Line 260: Can you clarify how a reader can see that the Weddell and Ross seas from Figure 4c?

⇒ Any parts of the Weddell and Ross sea ≥1000 m are encompassed in the transfer efficiency value for the antarctic zone. For detailed values of these regions, refer to Fig. 4a. While addressing this point, we noticed, though, that we should clarify that regions with depths smaller than 1000 m render the transfer efficiency metric as non-applicable, which is why it is set to NaN. We clarify this in the figures caption text. We add: → Ocean regions like shelf seas with water depths represented smaller than 1000 m are neglected and are displayed in white.

Line 505-560: The manuscript could benefit from a summary figure that highlights all key changes documented across the results sections (e.g., responses of Arctic, subtropical, and tropical regions). Adding such a figure would be beneficial, given the large amount of information presented in the paper, as it could help a broader audience beyond just biogeochemical modelers. A concise visual summary would make it easier for readers to understand and engage with the key findings.

⇒ Thanks for this consideration. It was also suggested by reviewer I and we aim at following the advice by including a summary figure - a radar plot. See comments to reviewer I.

2 Typos:

Figure 4 caption: Typo ”standrad” - should be ”standard”.

⇒ Thanks. Typo will be corrected when revising the manuscript.

Line 497: Typo in ”mesopelgic” - should be ”mesopelagic”.

⇒ Thanks. Typo will be corrected when revising the manuscript.

References

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