Response to reviewer#1:

We thank the reviewer for their careful evaluation of our work and the helpful suggestions. Based on this feedback, combined with similar feedback from reviewer #2, we propose to substantially rewrite the manuscript to remove complexity and present a more focused text. Below we provide our point-by-point responses (in black) to the review comments (in blue).

Major comments:

> The manuscript presents an analysis of the effects of iron submodel, particle dynamics and climate responses, but does so in a faintly haphazard manner, with key simulations missing (possibly for good reasons) and with a poorly organised results section
> While doubtless reflecting the order, manner and limitations under which the work was performed, the manuscript does not present clean comparisons between the model improvements that it intends to showcase (particle dynamics), and instead convolutes these with changes to the iron submodel
> The description of the new particle dynamics scheme is relatively brief, is ambiguous on the relationships between POM, CaCO3 and opal, and consigns some details to the supplementary material despite this scheme being the focus of the work

My recommendation is Major Revisions as the manuscript would benefit from significant reorganisation to improve clarity and straighten out its narrative. Ideally, extra work would include simulations that gap-fill current omissions (e.g. the Martin simulation is really Martin_oldfe), but I appreciate this may be challenging.

We acknowledge that the manuscript contains various discussions and descriptions that may have obscured the main points we want to highlight. In the revised manuscript, we will focus much more strongly on the main topic, which is how dynamic particle fluxes vary with climate state. Thus, we will expand the description and discussion of the particle flux dynamics in each climate state to improve clarity. We note on this aspect that the new iron cycle implementation (after Nickelsen et al., 2015) was only tuned with respect to the new particle dynamics module. We also note that the governing equations of the particle model are described elsewhere, apart from the technical adjustment of using an implicit numerical scheme for computational efficiency in our study. Therefore, for the revised manuscript, we will put the main focus on an in-depth comparison of our simulations with the fully dynamic new model version, bringing relevant information for that simulation from the SI to the main text and in turn remove discussions of the other model versions from the main text.

Specific comments:

Ln. 2: “most rapidly exchanging” might not be entirely accurate; large volumes of the ocean are very slowly exchanging, and the terrestrial carbon cycle is arguably more interactive given it has larger gross fluxes

We will remove 'most rapidly exchanging' from this sentence.

Ln. 7: “long-term cooling” – it would be helpful if the abstract just stated glacial-interglacial cycles are of interest here rather than confuse readers with reference to the exact opposite from that which they might expect from an ESM study (i.e. global warming is the default expectation)

We will follow the suggestion and add a motivation for studying the cooling scenario.

Ln. 7: “almost doubles the sensitivity” – to what?

We will clarify that export production changes almost twice as much between the climate states with the dynamic scheme compared to the simplified static implementation.

Ln. 7-8: “amplifies the change in marine carbon storage by a factor of about 1.5” – again, this is missing context; change of what specifically in response to what specifically

We will clarify that we compare the difference between climate states in the dynamic and simple static schemes.

Ln. 8-9: “where carbon exclusively cycles between the atmosphere and ocean” – not the land?; but the model is described as an “Earth system model”; if land is excluded, this suggests that it’s not really an ESM; maybe an EMIC is a better description

We will clarify that we did not employ the land component of the model in these experiments for simplicity and efficiency and use the term EMIC instead of ESM.

Ln. 9-10: “approximately 20 ppm in response to a -9.1C cooling or +6.8C warming” – this is a bit clunky for an abstract; I guess what you mean is that for an air-sea exchange difference of 20 ppm, these are the temperature limits; but the “approximately 20 ppm” is somewhat jarring when set against more precise temperature changes; I suggest rewriting this to be simpler and clearer about the sensitivity of the model – I'm not sure what yet, but this is opaque

We will revise this sentence, remove the exact temperature changes and instead stress that for the tested differences in climate, the additional feedbacks in the dynamic scheme result in an atmospheric CO_2 difference of up to 20 ppm.

Ln. 23: "cations" – is this an oblique reference to Calcium?

Yes, it was meant to avoid unnecessary specificity by singling out calcium over other elements that can be incorporated (e.g. magnesium, strontium, barium). We agree that this is not relevant here so will replace 'cations' with 'calcium' for simplicity.

Ln. 29: something to consider here is a reference to the fact that the carbonate pump drives an alkalinity flux that decreases the buffering capacity of surface seawater; this puts the carbonate pump – in part – pointing in the opposite direction to the soft tissue pump

We will explicitly differentiate between the effects that export of organics and carbonate have on the surface ocean carbonate system (carbon removal in both cases but opposite impacts on alkalinity) and those deeper in the water column.

Ln. 39: "increasingly complex biogeochemical representations" – it would be helpful to point to some examples of these models here

We will add examples e.g. PISCES and HAMOCC6.

Ln. 55: "intermediate complexity Earth system model" – perhaps call it an EMIC, an "Earth system Model of Intermediate Complexity"?

We will use the suggested formulation.

Ln. 64: "Bern3D" – I would expect a model version number to be used at this point (I appreciate that it's mentioned a few lines later); it would certainly help if any comparisons between the original and revised model versions are made

We will follow the suggestion and add a model version number here.

Ln. 77: I might be inclined to break to a new subsection here to clearly separate out what's different in this version compared to default Bern3D (i.e. separate base and revised model subsections); I might also be inclined to give a model name / version number for the variant described; perhaps finishing subsection 2.1 with a statement about the best source(s) for a model description and validation would be helpful?

We will add a new paragraph after the generic introduction of the model to mention in detail the different model versions (with clear identifiers) that we discuss in the rest of the manuscript.

Ln. 79: “z0” is the flux at 0 metres?; or is this the flux at the base of the euphotic zone?; and if the base of the euphotic zone, what depth is this?

We will clarify that 'z0' is the flux at the base of the euphotic zone and specify that this is 81 m in our model.

Ln. 84: “columnar”?; do you just mean “water column”?

We will use the suggested expression 'water column model'.

Ln. 90: CaCO3 dissolution is introduced here but it is ambiguous whether it has any connection to the remineralisation of POM; as it’s introduced in-between descriptions of remineralisation and particle sinking speed, this seems implied, but there is no formal connection directly mentioned

We will clarify that POM remineralisation does not affect $CaCO_3$ dissolution rates directly in MSPACMAM but indirectly affects $CaCO_3$ dissolution via the sinking speed and changes in alkalinity.

Ln. 84-104: this is the focus of this manuscript so should be completely clear and unambiguous; currently, per my previous comment, I’m not certain of the relationship between POM and CaCO3 (and opal, for that matter); further the manuscript refers to large and small particles, but consigns details to the supplementary material; this is unsatisfactory given it’s the core of the novel work in this manuscript

We will be more explicit in the description of how different particle classes interact and how their remineralization/dissolution rates are related in a) the standard Bern3D model, b) MSPACMAM and c) our implementation of MSPACMAM in Bern3D.

Ln. 101-102: what might help is a diagram showing the vertical profiles of sinking POC, opal and CaCO3 for the original and revised models here (alongside, say, Martin et al., 1987); while there are plots almost like this in the Supplementary Material (SM), there’s nothing quite like this

We will add a figure that compares the vertical profiles as suggested.

Ln. 110-118: again, there’s something of a lack of detail here in the manuscript’s main body; I appreciate it’s in the SI, but it would be good to be clearer on what “oldfe” is and how it differs from the refined model

We will remove the experiments 'oldfe' and instead focus on simulations of different climate states with the new model version to reduce the complexity of the manuscript.

Ln. 120: while it's good to have all of this material in SM, in the case of specific model developments (e.g. the iron submodel; Ln. 105) it'd be helpful to point readers to specific figures in this so that they know they exist and where to find them; this generic reference to SM misses a trick in guiding the reader

We will move more information about the particle flux and Fe scheme implementations into the main text so that the reader has them immediately available. Where we keep referring to SI materials, we will be more specific, e.g. listing SI section numbers and figures or equations.

Ln. 131: "the model was spun up under preindustrial conditions" – it would be helpful to explain what's different between this spin-up and "normal" use under the EMBM atmosphere; I assumed that the model would always be using the EMBM and I'm not sure what it means not to

This is a technical simplification to speed up the equilibration of the whole system. It does not alter the equilibrium that is eventually achieved. We will clarify the technical steps of our spin-up procedure by rephrasing this section:

"The model was spun up under PI conditions, which includes greenhouse gas concentrations, orbital configuration, and land albedo. For this, the ocean component was first spun up under prescribed SST and SSS conditions from ERA5 for 15 kyr to reach full equilibrium. Afterwards the EBM was coupled to the ocean and run for an additional 10 kyr."

Ln. 134: first reference to 13C; I'd have expected to hear something about this before in the model description; even a passing reference would be helpful; is 13C to help with validation?

We will briefly summarise the implementation of carbon isotope dynamics in Bern3D, cite more detailed descriptions and clarify that it is a tuning target by adding:

"The implementation of carbon isotopes in Bern3D is described in detail in the SI of Jeltsch-Thömmes et al. 2019 and has been re-tuned by Pöppelmeier et al. 2023. The model explicitly traces ^{13}C and ^{12}C and includes parameterisations for isotopic fractionation during air-sea gas exchange, dissolved carbon speciation, primary production and calcification. The 3D $\delta^{13}C$ field is part of the tuning targets (see tuning section SI.3)"

Jeltsch-Thömmes, A., Battaglia, G., Cartapanis, O., Jaccard, S.L. and Joos, F., 2019. Low terrestrial carbon storage at the Last Glacial Maximum: constraints from multi-proxy data. *Climate of the Past*, *15*(2), pp.849-879.

Pöppelmeier, F., Jeltsch-Thömmes, A., Lippold, J., Joos, F. and Stocker, T.F., 2023. Multi-proxy constraints on Atlantic circulation dynamics since the last ice age. *Nature geoscience*, *16*(4), pp.349-356.

Ln. 138-139: "Atmospheric CO2 decreases from its PI value in response to the cooling but is in these sensitivity simulations not influencing radiative forcing and climate." – first, rewrite this to something like "Atmospheric CO2 decreases from its PI value in response to the cooling but does not influence radiative forcing or climate in these sensitivity simulations"; second, this feels like a strange choice given that such feedback seems important in this specific context; given the preceding sentence about the simulated cooling being at the observational limit, it feels like this is a choice to avoid the model being even cooler and beyond the observational range

We will use the suggested formulation and clarify that we made the choice to disable the carbon-climate feedback to have comparable climate states with all tested biogeochemical schemes. The different atmospheric CO_2 concentrations that emerge in simulations with different biogeochemical schemes would otherwise alter the respective climate states, preventing a clear comparison of the temperature sensitivity of these schemes.

Ln. 139-140: the choice to not allow the model's own atmospheric CO2 to influence climate seems even stranger when 4xCO2 experiments are mentioned; these experiments specifically allow this feedback so mentioning them is very odd; I think I understand what you're doing (applying a climate cooling / warming but avoiding feedback effects), but I think a clearly stated explanation would help readers

We compare the radiative forcing in our experiments to the effective radiative forcing in the '4xCO2' experiments to provide a sense of the strength of our applied forcing in terms of more familiar atmospheric CO_2 levels. We will clarify that we do not intent to suggest that our model would produce this radiative forcing if we performed the actual '4xCO2' experiment with carbon-climate feedbacks.

Table 1: can't say I'm a fan of long experiment names when EXP numbers or model version numbers are simpler and clearer to use on plots; but this is aesthetic

We will choose simpler experiment identifiers.

Table 1: it feels unsatisfactory to not use the updated Fe scheme in the main meat of the work here; many comparisons are between the Martin and Particle model versions

but the differences extend beyond the particle dynamics; as the model is low resolution it is presumably relatively inexpensive to run (though this it is not made clear in the main text), so the absence of clean comparison simulations is difficult to justify; is there an issue of model tuning that complicates simulation?; this is mentioned, but not fully articulated

Yes, the iron scheme is tuned with the dynamic particle flux scheme and produces unrealistic iron concentrations with the martin curve. Our revised text will put less weight on the different iron schemes and focus on the behaviour of the fully dynamic new model version, which will hopefully make the choice of presented experiments less confusing.

Table 1: if "oldfe" is to be included, its status as an intermediate step in the work here would be clearer if it was positioned as an intermediate step in this table; also, why not describe Martin as Martin_oldfe?

We will make sure to use consistent experiment identifiers. We will remove the oldfe experiments from the manuscript, which will alleviate this specific concern.

Table 1: why not put all of the simulations (piControl, cool climate, warm climate) on the same table?

We will produce one comprehensive table summarising all experiments, as suggested.

Ln. 145: provide a cite for the preformed / regenerated methodology

We will add a reference as suggested.

Ln. 146-148: this feels very much like a minor sensitivity experiment given the missing context; maybe it's important later on?; but it might be the sort of thing you ignore here only to introduce it at a relevant point in the discussion

As suggested by the reviewer, we will only present the core experiments in our Method section, namely the simulations with the new model version, and point to sensitivity experiments (delegated to the SI) where needed in the discussion.

Ln. 149: given the topic of the work the ordering and structure of the results section could perhaps be better; as the focus of the paper is the addition of particle dynamics, starting with the effect of this addition for the default climate (i.e. neither warmed nor cooled) would seem like a good section 3.1; the current separation of warming and cooling effects into separate subsections seems difficult to miss a trick by not contrasting their effects side-by-side; furthermore the separation of cooling effects from particle effects (subsection 3.1.1) seems very strange

We will follow the suggestion and change the structure of our results. We will show more of the differences of the tuned PI states in the SI to keep the main text streamlined, and in the main text focus on an in-depth discussion on how particle flux dynamics differ between climate states. Specifically, we will compare export production of each particle type, sinking speeds of small and large particles as well as remineralisation/dissolution profiles.

Figure 1: strange units (mol C / km2 / y); why not umol / m2 / y?; from my experience, observational scientists do not usually report things in per square kilometre

We will change the units as suggested.

Figure 1: the results of the Martin experiment are compared to those of the Particle experiment despite the latter including the new Fe scheme; this makes it challenging to separate the effects

Our focus will be on the model version with new Fe and particle scheme and to highlight the effects of dynamic particle fluxes under different climate states. For brevity and to keep the MS focused, we will not attribute in detail differences between the old model version and new model versions to changes in the iron versus changes in the particle scheme. We will state that changes in export production between the two model versions are due to changes in both the particle and iron scheme. We will revise the naming of the two model versions to avoid confusion.

Figure 2: does the model have about the right amount of sea-ice for the present-day?

We will add a panel depicting PI sea-ice extent for context.

Figure 2: these are sometimes referred to as “thermohaline transects” as they try to crudely capture the thermohaline circulation from younger waters in the North Atlantic through to the oldest waters in the North Pacific

We will use the suggested term 'thermohaline transect'.

Ln. 166: would one expect North Atlantic ideal age to decrease with warming?; possibly erroneously, I tend to expect warmer climates to have more stratified oceans and older ideal ages; in this vein, Li et al. (2024; see below) report younger ages in LGM simulations while Figure 2 would suggest older ages; I guess different models will simulate AABW and its ventilation differently, so perhaps this manuscript would benefit from discussing why its response might differ

[Li, L., Liu, Z., Du, J., Wan, L., and Lu, J.: Mechanisms of global ocean ventilation age change during the last deglaciation, Clim. Past, 20, 1161–1175, https://doi.org/10.5194/cp-20-1161-2024, 2024.]

We will add more detail to this discussion, including references (e.g. Yamamoto et al. 2015, Battaglia and Joos, 2018, Nobre et al. 2023). Models generally show more stratification in response to transient warming but a more vigorous ocean circulation in warm steady states.

Battaglia, G. and Joos, F., 2018. Hazards of decreasing marine oxygen: the near-term and millennial-scale benefits of meeting the Paris climate targets. *Earth system dynamics*, *9*(2), pp.797-816.

Nobre, P., Veiga, S.F., Giarolla, E., Marquez, A.L., da Silva Jr, M.B., Capistrano, V.B., Malagutti, M., Fernandez, J.P., Soares, H.C., Bottino, M.J. and Kubota, P.Y., 2023. AMOC decline and recovery in a warmer climate. *Scientific Reports*, *13*(1), p.15928.

Yamamoto, A., Abe-Ouchi, A., Shigemitsu, M., Oka, A., Takahashi, K., Ohgaito, R., and Yamanaka, Y.: Global deep ocean oxygenation by enhanced ventilation in the Southern Ocean under long-term global warming, Global Biogeochem. Cy., 29, 1801–1815, https://doi.org/10.1002/2015GB005181, 2015.

Ln. 257: “Response to warming” should be another “Results” subsection but it is numbered as a whole new section; please amend this

Thank you for spotting this. We will combine the paragraphs on warm and cold climate states as suggested in the next comment, which will resolve this issue.

Ln. 257: the brevity of this final section on warming favours merging it with the preceding section on cooling; discussing the difference in model behaviour due to opposite sign climate changes seems more sensible to me than presenting them almost as unrelated sensitivity experiments

We will combine the discussions of warm and cold climate states as suggested.

Ln. 267: would there be any value in reporting how cooling and warming simulations approach equilibrium and on what timescales?; I appreciate that the focus here is on the steady state, but I’d certainly be interested to know whether warming or cooling took longer to reach equilibrium

We will add information about the equilibration time scales for context.

Ln. 294-295: this study reports weakened AMOC at the LGM, but my (admittedly limited) suggests that there’s considerable uncertainty here; two recent papers I’m aware of on the subject are listed below ... (I’m sure there are others)

[Gu, S., Liu, Z., Oppo, D.W., Lynch-Stieglitz, J., Jahn, A., Zhang, J. and Wu, L., 2020. Assessing the potential capability of reconstructing glacial Atlantic water masses and AMOC using multiple proxies in CESM. *Earth and Planetary Science Letters*, 541, p.116294. https://doi.org/10.1016/j.epsl.2020.116294]

[Zhengyu Liu; Evolution of Atlantic Meridional Overturning Circulation since the last glaciation: model simulations and relevance to present and future. *Philos Trans A Math Phys Eng Sci* 11 December 2023; 381 (2262): 20220190. https://doi.org/10.1098/rsta.2022.0190]

We agree that, while a shallower overturning cell in the North Atlantic at the LGM has consensus, there is debate about the strength of AMOC. We will be more nuanced in our statements here and refer to the discussion in Muglia and Schmittner (2021).

Muglia, J. and Schmittner, A., 2021. Carbon isotope constraints on glacial Atlantic meridional overturning: Strength vs depth. *Quaternary Science Reviews*, *257*, p.106844.

Ln. 353: the code and data statement seems a bit remiss to me; why is this not available in a Zenodo archive or similar?; I can’t see it attached to the manuscript record

We will revise the data and code availability statement once the manuscript is accepted. The code development is fully accessible via Zenodo. A compressed version of that content was included in the initial manuscript submission.