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Interactive comment on "One size fits all? – Calibrating an ocean biogeochemistry model for different circulations" *by* Iris Kriest et al.

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We thank referee 1 very much for his/her encouraging and helpful comments! Please find below our reply and answer to the referee's suggestions and comments (in blue).

General Comments: Some of the figures were a little hard to interpret due to the large number of model set-ups. Figures 6, 8, 9, 10 and 12 would really benefit from a legend or key to more quickly pick out which model set-up is which (a legend on the first relevant plot that is referenced for subsequent plots for example). Interpreting the plots with reference to the text was difficult because of this.

We have now added a symbol legend to Figure 6 and will refer to this in plots 8, 9, 10 and 12. Note that we will also increase the number of digits in the regression equations (formerly truncated to 2-4).

Specific Comments: Section 2.7: I found this section a little unclear due to the discussion about the previous optimisations. It would help to focus on the parameters varied in this study based on the second 2017 optimisation and mention more briefly that the plankton parameters were the same as the first 2017 optimisation.

We found it somehow difficult to rephrase this by just mentioning the first optimisation by Kriest et al. (2017) in passing (and secondly), but suggest to rewrite this subsection to hopefully make it easier to understand the sequence of optimisations: *"The optimisations presented here are based upon two successive optimisations presented by Kriest et al. (2017) and Kriest (2017). Both studies applied model MOPS coupled to TMs derived from MIT28. The cost function, as presented in Equation1 was calculated after a spin up of 3000 years. In the first optimisation, Kriest et al. (2017) optimised four parameters related to plankton growth and loss terms, together with* b *and* $R_{-\mathrm{O2:P}}$*. The optimal parameters of this first calibration led to a better agreement of simulated global biogeochemical fluxes to observations of primary and export production, zooplankton grazing, particle flux at 2000m, and organic matter burial at the sea floor (Kriest et al., 2017). In a subsequent optimisation Kriest (2017) kept the four optimal plankton parameters fixed, and calibrated four parameters related to remineralisation and nitrogen fixation (namely* K_{O2}, K_{DIN}, DIN_{min} *and* μ_{NFix} *described in subsection 2.4), together with* b *and* $R_{-\mathrm{O2:P}}$ *(see Table 1). This second optimisation by Kriest (2017) led to a good match to independent estimates of pelagic denitrification, and is hereafter referred to as MIT28*. It serves as the starting point for the four additional optimisations presented in this paper."*

Line 347: "MOPS coupled to UVic circulation is more robust with respect to changes in parameters." : the sentence meaning is unclear, does this mean that the calibrated parameters are similar across the three UVic circulation used? (In comparison to ECCO or MIT28).

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We will replace this by *"The misfit function changes less when the optimal parameters are swapped among the different UVic circulations."*

Line 349: ". . .the large impact of oxygen on the misfit function. . .": could you elaborate briefly why this is the case here.

We will rephrase this by *"In the model the global oxygen inventory adjusts dynamically to the combined effects of circulation and biogeochemical parameters, causing a large impact of this tracer on the misfit function (Kriest et al., 2017). Therefore, optimisation attempts to reduce the global oxygen bias, which is low for each optimal model configuration, indicated by the low values along the main diagonal of Figure 7, panel (B)."*

Lines 399-413: this analysis assumes that the interactions between circulation, biogeochemistry parameters and the misfit are linear and additive? Figure 11 suggests that this might not be the case as the delta_par and delta_circ bars do not sum to the delta_all bar. The analysis in this form is fine (and considering non-linear interactions would not be easy!) but I think this assumption should be mentioned.

Indeed, the fact that the individual contributions of delta_par and delta_circ do not add up to delta_all indicates that the effects are not linear and additive. Thank you for pointing this out. We will add a sentence on this at the end of this section: *"We note that the individual contributions of ΔPar and ΔCirc for both diagnostics do not add up to ΔAll (Table 3), indicating that the effects of biogeochemical parameters and circulation are not linear and additive."*

Lines 453-454: ". . .it prevents fast settling of organic particles out the euphotic zone" is a little unclear. Does this mean there is effectively an increased residence time of particles in the euphotic zone which equates to a larger fraction of particles being remineralised before reaching the ocean interior? Is there also an impact of the plankton model in this instance, e.g., changes in zooplankton grazing?

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Because the plankton parameters have not been changed during optimisation, global grazing follows almost linearly primary production (r=0.95), and all the statements and conclusions made with respect to the latter flux apply. In particular, a larger b (slower settling; longer particle retention time at the surface) leads to an enhanced nutrient turnover in the euphotic zone; but as for primary production, this also depends on the circulation. We suggest to change this paragraph to make this clearer: *"(...) and thus primary production as the ultimate source of export production; on the other hand, it prevents fast settling of organic particles out of the euphotic zone. Because the plankton parameters were not changed during optimisation, global grazing follows almost linearly primary production (r=0.95), and the statements and conclusions made with respect to the former flux largely apply to grazing (no figure). Therefore, the combined antagonistic effects of b on surface (and subsurface) nutrient turnover, subsurface nutrient concentrations (as a source of nutrient entrainment and mixing) and direct organic particle flux in the upper few hundred meters explain the relatively small variation caused by biogeochemical parameters (...)"*

Lines 459-461: "long term storage of nutrients and carbon will, to a large extent, depend on the prescribed particle flux profile"-the air-sea balance of CO2 might depend on circulation more than nutrients to the gas exchange component, similarly to the arguments made about O2 previously.

We agree, and suggest to rephrase this as *"Therefore, simulated organic matter supply to the deep ocean and deep nutrient concentrations will, to a large extent, depend on the prescribed particle flux profile, with potential effects on the long-term storage of carbon dioxide."*

Line 469-474: There should be a caveat that these findings are for MOPS specifically.

We agree, and will add: *"Therefore, at least for this particular biogeochemical model,..."*

Figure 4D is very hard to interpret due to the colour contrasts and placement/combination of lines. The panel is not explicitly mentioned in the text so I would suggest to move the figure to supplementary or separate into more panels to make it clearer.

Because the percentage deviation especially in the deep ocean is just complementary, yet for some people important information, we now moved this panel to the supplement (as an additional plot), and have added a legend for the line colours and thicknesses, to make the plot more easily accessible.

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