Biogeosciences Discussions

Biogeosciences Discuss., referee comment RC2
https://doi.org/10.5194/bg-2022-7-RC2, 2022


# Comment on bg-2022-7

Anonymous Referee #2

Referee comment on "Quantifying biological carbon pump pathways with a data-constrained mechanistic model ensemble approach" by Michael R. Stukel et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-7-RC2, 2022

Summary and recommendation: This study uses a 1-dimensional ecosystem model to assimilate data from Lagrangian experiments in the Costa Rica Dome, California Current, Southern Ocean (Chatham Rise) and Gulf of Mexico. The authors use a Monte Carlo approach to assess the uncertainty in model predictions, compare the model predictions to observations within each region, and assess the export mechanisms (gravitational, mixing, and migration) in each region within their model. They find that the gravitational pump is most important in most regions, followed by the mixing pump and then the migration pump. The manuscript is well written and the results are clearly presented for the most part, so I recommend publication subject to minor revisions to address the points below.

The main strength of this study is that it uses a wide variety of in-situ data (rates, biomass, chemical tracers etc.) from several different ecosystems, which allows the many (>100) parameters of their model to be reasonably constrained, and that they use a MCMC approach to quantify the uncertainty in their model predictions. One weakness of this study is that the model is 1-dimensional and neglects horizontal transport and connectivity, as well as only resolving the euphotic zone, but this weakness is thoroughly discussed by the authors. Another weakness that is not as well addressed is why the model was not used for predictions outside the assimilation regions. I was hoping that the authors could also provide results from their model for regions that were not assimilated into the model, i.e. to extrapolate to other regions so as to produce global maps of export by these different mechanisms, or at least maps of export ratio. Without such an extrapolation to larger time and space scales the study is interesting but lacks a prediction that can be compared to other export models (except in the 4 regions that provided data that was assimilated into the model, on short timescales). It is also odd that the authors fail to mention the data-assimilation model of DeVries and Weber (2017), given their relatively thorough review of other assimilation models in the introduction and elsewhere, as well as the recent study by Nowicki et al (2022) with a quite similar title. Some other minor issues are noted below.

- Lines 70-95: This discussion is missing the pioneering data assimilation models of Schlitzer (e.g. 2000; 2002) as well as the more recent work by DeVries and Weber (2017) and Nowicki et al. (2022).

- Lines 136-145: Here two different configurations of the model are mentioned, one that only resolves the euphotic zone and one that resolves deeper layers that the zooplankton can migrate towards. This makes it sound like the model is run in both of these configurations, but then later (line 197) they say that only the euphotic zone configuration was used. So, I recommend to remove discussion of the other configuration to avoid confusion.

- Figure 3: Some of the variables appear to have a peak probability that is at the limit of their allowable range. Does this represent a flaw in the model, or that the allowable range should be widened in order to better capture the values of these parameters in the model simulations?

- Discussion of the mixing pump in general: For the mixing pump especially (more so than the other export pathways) it is important on what timescale the material remains exported, and can therefore contribute to carbon sequestration. Since the authors are running short timescales experiments (30 days) they should clarify that their modeled export is over that time horizon, and would not necessarily be the same as export over the course of the year. It should also be mentioned that the large-scale physical mixing pump (e.g. Ekman pumping) is not captured. The authors should speculate as to whether their model would provide an over- or underestimate- of the mixing pump export on timescales relevant to carbon sequestration (> 1 year). This discussion could augment what the authors already have in lines 622-631.

- Figure 11: From this figure it is hard to assess how the model-predicted and observed export compare. It would be good to show a scatterplot of the correlation between modeled and observed export in one figure, in addition to what is shown here.

- Several times throughout the paper the acronym SalpPOOP is mentioned, but never defined. I assume this is the Southern Ocean experiment that is elsewhere referred to as Chatham Rise??

- Line 643 ff: The study of Nowicki et al (2022) assessed the sequestration times of the different export pathways and is highly relevant to this discussion.

- Section 4.2: Again this discussion is oddly missing reference to the data assimilation studies of DeVries and Weber (2017) and Nowicki et al (2022)

References:

Schlitzer, R. (2000). Applying the adjoint method for biogeochemical modeling: export of particulate organic matter in the world ocean. Geophysical Monograph-American Geophysical Union, 114, 107-124.
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DeVries, T., & Weber, T. (2017). The export and fate of organic matter in the ocean: New constraints from combining satellite and oceanographic tracer observations. Global Biogeochemical Cycles, 31(3), 535-555.
Nowicki, M., DeVries, T., & Siegel, D. A. (2022). Quantifying the carbon export and sequestration pathways of the ocean's biological carbon pump. Global Biogeochemical Cycles, 36(3), e2021GB007083.