Biogeosciences Discussions

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Reply on RC2

Michael R. Stukel et al.

Author 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-AC2, 2022

Our responses are in bold below. For ease of reading, we have also included the comments as a supplementary pdf in which are responses are in red. Our many thanks for the constructive comments from our reviewers.

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.

The main reason that we do not extrapolate the model to other regions in its

current form is that the simulations that we conducted require incredibly detailed and robust ecosystem analyses spanning physics, biogeochemistry, and ecology in order to prescribe accurate initial conditions, boundary conditions, and forcing. There are very few programs that have sufficiently measured all of these processes simultaneously. We thus believe that the appropriate way to extrapolate these results to other regions is to conduct fully-coupled four-dimensional ensemble simulations based on the parameter sets determined in this study. That will require re-coding the model into a global circulation model (e.g., HYCOM or MITgcm) and conducting hundreds of global simulations. We thus consider it beyond the scope of this manuscript, but hope to address it in a future study.

Thank you for reminding us of the DeVries and Weber (2017) study. We had focused on data assimilation with fully mechanistic models, but we agree that the DeVries and Weber (2017) study, which uses satellite remote sensing to force a more simplified model of the biological pump, utilizes an interesting data assimilation approach that is certainly worth discussing in the context of our manuscript. We did not cite the Nowicki et al. (2022) study for the simple reason that it had not been published (and we hence were completely unaware of it) when we submitted this manuscript.

- 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).

We thank you for pointing these out and will include them in the revised manuscript.

- 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.

This manuscript functions, in part, as a description of a new model system. We thus believe it is appropriate to describe both configurations, even though we only actually use one. However, in the revised draft, we will make it more clear that we only used the euphotic zone-only configuration.

- 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?

We do not consider this a flaw. Instead, it demonstrates that the data is successfully constraining the possible solutions. For example, consider the respiration term for small phytoplankton (res_{SP} shown in Fig. 3). This term represents the proportion of small phytoplankton biomass that is lost each day due to basal metabolism. This term is uncertain, because it is not trivial to

separate out phytoplankton respiration from respiration of other microbes in field measurements. Hence, we assumed a priori that it could be anywhere between 0 and 0.1 d^{-1} (with an initial guess of 0.002 d^{-1}). Model results showed that values of res_{SP} greater than ~0.01 are inconsistent with the observations. This thus puts a strong constraint on that parameter. Similarly, consider the excretion parameter (exc_{SP} also in Fig. 3), which quantifies the proportion of the gross primary production of small phytoplankton that is excreted (i.e., active metabolic processes). Based on prior results, we assumed that this parameter could take a value between 0 and 0.3 (with an initial guess of 0.135). However, model results showed that values of exc_{SP} on the lower end of that range were inconsistent with the observations and that most likely exc_{SP} is > 0.25). It is certainly possible to question whether or not our prior estimated ranges are the best choices (as with priors in Bayesian statistics there can always be critiques of what ranges should be allowable). However, we chose ranges that we believed were ecological and biologically realistic based on a combination of field and laboratory measurements and previous model results and parameterizations. We believe that the posterior distributions of parameters derived from our ensemble approach provides important information for more objective priors in future work (i.e., in future projects incorporating new datasets, we will likely model the prior of exc_{SP} as a normal distribution with a mean of 0.27 and a standard deviation of 0.027 based on the results of the current manuscript's analysis.

- 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.

The reviewer is certainly correct that our model gives little information about the long-term fate of carbon leaving the euphotic zone via the mixing pump. In fact, it gives little information about the long-term fate of carbon leaving the euphotic zone via any mechanism; the model as currently formulated specifically asks the question of how much carbon is leaving the euphotic zone and by which process without quantifying the depth at which any of that carbon is sequestered and hence its long-term carbon storage potential. We certainly expect that carbon sequestration temporal horizons will vary for each of the different export mechanisms (as an aside, we are looking at this in detail with datasets from the California Current Ecosystem using different approaches). We believe that answering questions about the length of time that carbon is sequestered will require three-dimensional coupled runs (which we plan to do in the future). In the updated version of the manuscript, we will certainly mention this important topic.

With respect to which aspects of the mixing pump are included, it is a little bit complex, because while we model a one-dimensional water column using only diffusive processes, the eddy diffusivity coefficient is based on Thorpe-scale analysis, which utilizes the magnitude and frequency of density instabilities to estimate shear-generated mixing. Thorpe-scale analyses thus do not explicitly

map onto any of the different mechanisms of the mixing pump (as defined by Body et al. 2019 or Levy et al. 2013) but can be impacted by all of them. As with quantification of carbon sequestration timelines, we believe that three-dimensional coupled modeling is necessary to explicitly look at the different aspects of the mixing pump. We are not comfortable speculating as to whether the current approach over or underestimates the magnitude of the mixing pump, although we do note that the results derived for the CCE in this manuscript were not too dissimilar from results of an entirely independent approach (three-dimensional Lagrangian particle tracking, Stukel et al. 2018). We will, however, try to make these distinctions about the different mechanisms of the mixing pump (and the uncertainty associated with estimating the mixing pump in a one-dimensional framework) clearer in the revised manuscript.

- 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.

We note that we already include an explicit comparison between modeled and observed export in Fig. 6. However, we will happily add an extra figure showing a scatterplot of this data if the reviewer believes it will be more helpful to readers.

- 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??

Yes, you are correct. Thank you for noting that we forgot to define it. We will make sure it is defined in the revised draft. The cruise acronym is Salp Particle expOrt and Oceanic Production

- 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.

As mentioned before, the Nowicki et al. (2022) study was not available when we submitted the manuscript. It is, of course, highly relevant and we will incorporate its insights throughout the introduction and 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)

Agreed. Our failure to include the DeVries and Weber study was a definite oversight. The Nowicki study was simply not included because it had not been published when we submitted the manuscript.

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2022-7/bg-2022-7-AC2-supplement.pdf