Biogeosciences Discussions

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Comment on bg-2022-134

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Referee comment on "Observation-constrained estimates of the global ocean carbon sink from Earth system models" by Jens Terhaar et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-134-RC2, 2022

In this manuscript, Terhars et al. investigate how Earth system models estimates of the global ocean carbon sink can be constrained by a combination of physical parameters (the sea-surface salinity and the strength of the Atlantic Meridional Overturning Circulation) and a biogeochemical parameter (the Revelle factor).

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The manuscript is timely, clearly written and proposes a sound methodology. The results are well explained and discussed through the manuscript. This work presents an important basis for the research community studying the ocean carbon cycle as this work proposes a first approach to bring together estimates of ocean carbon sink based on observational data with those based on Earth system models' simulations. I liked very much the fact that the authors explain step by step the use of a suite of emergent constraints and then perform several validations to test the robustness of their approach.

I only have one major comment and a set of minor comments/suggestions that aims to clarify some point of the paper.

Major comments:

Although the authors did a great job in defining and applying observational constraints to improve Earth system models' simulations/projections, they miss to thoroughly discuss how each physical or biological parameters are correlated between each other. For instance, pattern of sea-surface salinity is linked to water mass properties, which is in turn, tightly linked to large-scale circulation (deacon cells and the strength of the AMOC). Same caveat could hold for the buffer factor (globally average) which result from biological but also from chemical properties of the models.

If constraining fields are correlated between each other in the observations and/or in the ESMs, this might bring light on a more mechanistic explanation of the "cascade of errors" = hydrodynamics => large-scale circulation => buffer factor rather than a "sum of errors" = hydrodynamics + large-scale circulation + buffer factor. This might be needed as a justification of applying this set of observational constraints (avoid cherry picking).

In addition, for this 'biological' parameter, I think further discuss should be needed in the light of Figure A.1.2 which shows the correlation between surface buffer factor and the difference between alkalinity (AT) and total dissolved inorganic carbon (CT) at surface. It highly biases in either CT or AT that might result from the calibration of model alkalinity (as highlighted in several model reference papers or in Table 3 of Seferian et al. 2020 (https://link.springer.com/article/10.1007/s40641-020-00160-0/tables/3).

Finally, in the light of deficiency/weakness of observational-based estimates of the ocean carbon sink, it might be interesting to decompose your approach on regional/basin scale uptake. Driving mechanisms, long-term trends and variability of the North Atlantic carbon sink is better understood than those of the Southern Ocean (which suffer from incomplete observational mapping across seasons). As such, does the model (and your observational constraints) help to improve the agreement between model and observation-based estimates. Besides, does the ratio in carbon uptake in the North Atlantic and the Southern Ocean is well captured between models. In the context of this paper, I wonder how far this ratio might be an additional constraint to test or a verification measure to assess the robustness of your approach.

Regarding the conclusions of the paper, I think the authors could make a stronger point resulting from this work.

First, I might be relevant to discuss the consequence of this work on the carbon budget (Friedlingstein et al. 2022), especially in the context of the budget imbalance term. Revised (contrained) estimates appears to be about 10% higher than the unconstrained estimates. The magnitude of the revision is thus greater than the budget imbalance. What would be the consequence then? a weaker land-surface carbon sink?

On the other hand, in a context of improving estimates of the carbon feedbacks, what would be the revision of the Beta and Gamma as inferred from your approach. In might be interesting to include in your work ssp585-bgc (which has been conducted by most of the modelling center) and see how your approach works on ocean Beta and Gamma.

Minor comments:

L12: explain the buffer factor in the abstract

Figure 1: please use the same temporal baseline for panel a) and b). from 1950 onwards ?

L106 Improve syntax,"so-estimated"

L157: Many other papers have used emergent/observational constraints (Boé et al., Bourgeois et al., Cox et al., Douville et al., Plazzotta et al., Schlund et al., etc....) — They can also be listed here.

Figure 2: please add 'the strength of' before "the Atlantic meridional…"

Figure 3: Please add R-square for each panels c, e and g as an indication of the quality of the fit

On this figure, it is unclear if model estimate are based on multiple realisation or just one single member

L237: one can also consider the CO2 mole fraction that is *really seen* by the ocean

carbon module because of various treatment of the air-sea CO2 exchange (Hauck et al. 2020, already mentioned in this work)

L341: Conclusion — see above comments

Appendix: Biogeosciences allows more materials than short/letter paper, I would recommend to move some of the material of the appendix into the heart of the paper. Some of them are central to your work.

Table A.1.1 please consider adding data citation doi (where relevant) for improving the reproducibility of the work.

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