

Biogeosciences Discuss., community comment CC1
<https://doi.org/10.5194/bg-2022-134-CC1>, 2022
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Comment on bg-2022-134

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Community 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-CC1>, 2022

Assessment:

Terhaar et al. use an emergent constraint approach to make essentially two arguments: Current ocean CO₂ uptake estimates are 9-11% too low, and that their constraints permit them to reduce the present and past CO₂ uptake by 42-59%. The topic is relevant, the method is sound, the paper is overall well written (with some exceptions), and the results are important. Thus, this study clearly deserves to be published.

But I have two important concerns that need to be addressed, in my opinion, before I can endorse the publication of this manuscript.

- **Robustness:** In my opinion, the major conclusions, particularly the latter regarding the substantial uncertainty reduction, are not robust as presented. By using a class of non eddy-resolving models, which disregard a set of critical processes in the ocean that are known to be relevant for controlling the uptake of transient tracers through their impact on deep water formation, the results are potentially seriously biased. Thus while the results appear precise, they may not represent an accurate estimate of the global uptake.
- **Observational constraints:** The study is entirely based on rather indirect constraints, i.e., the salinity of parts of the Southern Ocean, the surface buffer factor, and the AMOC (in decreasing order of relevance), while there are many direct constraints that the authors have decided to disregard. This may be a valid approach to provide an independent estimate, but it then behooves the authors to demonstrate that the constrained models are actually doing better against the unused observational constraints. Particularly relevant here is the three-dimensional distribution of anthropogenic CO₂ in the ocean interior. Are the models that are within the best constrained range also those models that reproduce the reconstructed distribution the best?

In summary, I have serious concerns about the conclusion drawn here. Given the structural biases that are inherent in the models and the rather indirect nature of the constraints, the proposal of a strongly reduced uncertainty for the oceanic uptake of CO₂ seems far-fetched. To me this seems like a classical case for overconfidence stemming from a limited perspective of all the issues at stake.

Recommendation:

I recommend a major revision that revisits the uncertainties of the approach taken and the conclusions that the authors draw from their work. The power of the emergent constraint rests primarily with the future, while the relevance (and novelty) for the past and present is much less clear. I thus strongly encourage the authors to de-emphasize the discussion of the relevance for the present (which is anyway less evident since the coupled models produce their own climate variability) and instead focus the study on what the constrained ensemble can say about the future.

Detailed arguments:

Regarding Robustness:

Emergent constraints essentially rely on the relationship between biases in the models and the biases that result from them with regard to a particular outcome – here the ocean uptake of CO₂. While this is a well-tested method, its limits always need to be carefully evaluated. This is especially the case when an attempt is made to improve knowledge about a process for which a lot of information is already available, such as the past and present uptake of CO₂ by the ocean.

A fundamental underlying assumption in the method is that while individual models can be (and should be) biased, there is no common bias across all models that would lead to an overall bias set of models. This assumption is violated here. None of the employed ocean models is eddy-resolving – meaning that they all share similar biases with regard to a number of critically important processes. The role of eddies for determining global ocean circulation is well established, particularly with regard to the processes in the Southern Ocean, where the interplay between Ekman drift induced overturning circulation and eddy-driven circulation is particularly important (see Marshall and Speer (2012) and Rintoul (2018)) for determining the structure and magnitude of the subduction of mode and intermediate waters, i.e., the important conduits for how anthropogenic CO₂ is entering the thermocline of the Southern Ocean. This process is not well captured by most coarse-resolution models, as evidenced, e.g., by their poorly modeled distribution of salinity. Lachkar et al. (2007) showed the impact of resolution on the uptake of anthropogenic CO₂, CFCs and $\Delta^{14}\text{C}$ quite impressively, highlighting how it not only alters the global uptake, but also the processes and the locations of the uptake. Given this evidence, I have substantial concerns that the relationship established here is as robust as the authors make us believe. (note on the side: this would not be the first time an emergent constraint falls apart once additional processes are taken into account).

I think also a bit more critical thinking would do this study well. One needs to recall that in the end, emergent constraints can only emerge from a model suite if at least some of the models are flawed. In addition, emergent constraints study often just emphasize the variables that work. They rarely state (also not in the case of this study) of all the variables that did not work. For example, it turns out that interfrontal salinity in the Southern Ocean ends up to be the most important constraint. But why not interfrontal density, which is actually dynamically the more important variable? And why not winds, and why not winter mixed-layer depths and why not many other variables that are clearly relevant for the determining the anthropogenic CO₂ uptake in the Southern Ocean? The lack of consideration of the fact that these emergent constraints emerge from a substantial amount of trial and error approach also tends to lead to overconfidence.

Regarding data constraints.

The authors compare their emergent constraints only with regard to the global uptake numbers with other data based constraints. But the proof of the pudding is the eating.

Unless the authors can demonstrate that the constrained models are indeed doing better with regards to the observational constraints for the oceanic uptake of anthropogenic CO₂, I have little confidence in their results. Of course, the observational constraints come with their own uncertainties, but there are a number of well established features in terms of basin and depth distributions that can be exploited (note e.g., that the Sabine et al. 2004 and the Gruber et al. 2019 estimates are statistically fully independent since they use a fundamentally different methodology). I also think that the ocean models should demonstrate their ability to represent the air-sea CO₂ fluxes, since these are increasingly dominated by the anthropogenic CO₂ flux components.

Detailed comments:

P5, **line 116** "However, ... significantly smaller than the previously assumed flux of -5 Pg C (Gruber et al., 2019a),": Given that the ESMs employed here have their own climate variability, this comparison is fundamentally not tenable. The 5 Pg C could be related to anthropogenic climate change, but it could also be related to naturally occurring interannual to decadal climate variability. Thus the authors are comparing two different things here.

P13, line 247ff Along similar lines, I think the discussion of the budget imbalance stands on weak grounds here. This may or may not reflect anthropogenically forced trends, but with the ESMs not simulating the weather and climatic events over the past 20 years correctly, the power of these statements is very limited. This is the reason why I recommend that the authors focus their paper more strongly on the future, where the ESMs have their strengths. The past 20 years is not their forté.

References:

Lachkar, Z., Orr, J. C., Dutay, J.-C., & Delecluse, P. (2007). Effects of mesoscale eddies on global ocean distributions of CFC-11, CO₂, and $\Delta^{14}\text{C}$. *Ocean Science*, 3(4), 461–482. <https://doi.org/10.5194/os-3-461-2007>

Marshall, J., & Speer, K. (2012). Closure of the meridional overturning circulation through Southern Ocean upwelling. *Nature Geoscience*, 5(3), 171–180. <https://doi.org/10.1038/ngeo1391>

Rintoul, S. R. (2018). The global influence of localized dynamics in the Southern Ocean. *Nature*, 558(7709), 209–218. <https://doi.org/10.1038/s41586-018-0182-3>

