3 Response to reviewer 3

General Comments

The present study of J. Terhaar proposes an innovative approach to evaluate the ocean carbon sink, by combining modelled results of forced global ocean-biogeochemical models (GOBMs) and fully coupled Earth System Models (ESMs). This approach uses the respective strength of the two modelling frameworks, as atmospheric forcings constrain GOBMs to reproduce the historical observed climate variability while the ESMs full coupling produces long-term trends more consistent with the exchanges existing between compartment (land, ocean, atmosphere). The author shows that his new "hybrid" estimate stays in accordance with that of the Global Carbon Budget (GCB) initiative over the period 1959-2022, and allows to reduce the uncertainty of the ocean carbon sink by 70%.

Response:

I thank the reviewer for their evaluation and comments, which have substantially clarified and improved the manuscript. I have taken each comment carefully into account, provide responses to each point below, and adapted the manuscript accordingly.

Comment 3.1

Mathematical formulation that would support this approach is lacking. How can we add up anomalies from GOBMs with ESMs when we know that there is a strong state dependency of the ocean carbon uptake on the ocean surface alkalinity (at least) ? How biases in coupled models can be combined with GOBMs (much less biased) ? For this question, I would be tempted to focus on models contributions that provide a consistent framework in terms of resolution and model version as a tested bed before extrapolating to the multi-model mean.

Response:

Following the reviewers comment, I have added the following section to the manuscript:

"**4.3 Combining high-frequency variability and long-term trends to form the composite estimate**

As described in the results, the composite estimate combines two components of the ocean carbon sink timeseries. One component is the short-term variability, and one component is the long-term trend. Mathematically, the ocean carbon sink can be separated into these two components:

$S_{OCEAN} = S_{OCEAN}{}^{STV} + S_{OCEAN}{}^{LTT}$, (1)

where S_{OCEAN} is the total carbon sink and $S_{OCEAN}{}^{STV}$ is the short-term variability component and $S_{OCEAN}{}^{LTT}$ is the long-term trend component. The ocean carbon sink of each GOBM ($S_{OCEAN}{}^{GOBM}$) and each ESM ($S_{OCEAN}{}^{ESM}$) (after adjustment for biases) was then separated into their short-term variability components ($S_{OCEAN}{}^{GOBM-STV}$ and $S_{OCEAN}{}^{ESM-STV}$) and their long-term trend components ($S_{OCEAN}{}^{GOBM-LTT}$ and $S_{OCEAN}{}^{ESM-LTT}$):

$S_{OCEAN}{}^{GOBM} = S_{OCEAN}{}^{GOBM-STV} + S_{OCEAN}{}^{GOBM-LTT}$, (2)

$S_{OCEAN}{}^{ESM} = S_{OCEAN}{}^{ESM-STV} + S_{OCEAN}{}^{ESM-LTT}$. (3)

Here I performed the separation using an Enting spline with a 15 year cut-off period as described in section 4.2 and also tested the sensitivity of the results to a change in the cut-off period and to the use of a different method to extract the long-term trend, i.e., a running mean. The separation was performed for each model individually. If the same method is used to separate the short-term variability and the long-term trend ESMs and GOBMs, there is no double counting of forcing that is present in both ensembles, such as volcanoes (Fay et al., 2023).

As described in the Introduction and Results, the historic short-term variability is more accurately simulated in GOBMs as they are forced with atmospheric reanalysis data and the long-term trend is more accurately simulated in ESMs due different ways of setting up these GOBMs, e.g., the lengths of the spin-up, different atmospheric CO2 during the pre-industrial spin-up (Terhaar et al., 2024), and a too warm pre-industrial ocean and hence too weak transient warming in GOBMs (Huguenin et al., 2022). Thus, the composite model estimate ($S_{OCEAN}{}^{COMPOSITE}$) is calculated as follows:

$S_{OCEAN}{}^{COMPOSITE} = S_{OCEAN}{}^{GOBM-STV} + S_{OCEAN}{}^{ESM-LTT}$. (4)"

However, it remains unclear to me why the composite estimates cannot be combined in this way. Of course, the ocean carbon sink depends on surface alkalinity, but also on the transport of surface waters into the ocean interior. These biases were adjusted for in the ESMs by Terhaar et al. (2022). Unfortunately such an adjustment is not possible in GOBMs, likely due to different starting dates and pre-industrial pCO_2 (Terhaar et al., 2024).

I am also not sure how this could be tested. If one used any model in a coupled mode, as ESMs, and in hindcast mode, as GOBMs, one could use that one model to calculate a composite estimate based on this model. But there would still be no ground truth against which the approach could be tested. If one used only one model and seperated the long-term trend and short-term variability, the sum would again lead to the exact overall carbon sink that was simulated as there is no residual in the separation.

Hence, I would be grateful for further clarification by the reviewer about the suggested changes and tests if needed. Until then, I keep the approach as it is, especially as reviewers #1 and #2 found that the "manuscript is clear", "well-reasoned", "represents a dose of constructive reflection", "would represent a valuable contribution to the effort of improving our estimates of the ocean carbon sink", and "deserves inclusion in GCP-type analysis of marine carbon uptake".

Terhaar, J., Frölicher, T. L., and Joos, F.: Observation-constrained estimates of the global ocean carbon sink from Earth System Models, 19, 4431–4457, https://doi.org/10.5194/bg-19-4431-2022, 2022.

Terhaar, J., Goris, N., Müller, J. D., DeVries, T., Gruber, N., Hauck, J., Perez, F. F., and Séférian, R.: Assessment of Global Ocean Biogeochemistry Models for Ocean Carbon Sink Estimates in RECCAP2 and Recommendations for Future Studies, J. Adv. Model. Earth Syst., 16, e2023MS003840, https://doi.org/https://doi.org/10.1029/2023MS003840, 2024.

Comment 3.2

The present text and figures do not allow to evaluate individual ESMs trends and spline fits, in order to compare them with the mean spline fit shown in Fig. 2a.

Response:

For a comparison between individual spline fits and the mean of all spline fits, Figure A2 for GOBMs and Figure A3 for ESMs were added to the revised manuscript as suggested by the reviewer.

Comment 3.3

The effect of the correction you applied on ESMs circulation and carbonate chemistry following Terhaar et al. (2022) is not properly discussed. I would at least expect a comparison of the results you obtained with your hybrid model in case with and in case without applying this ESM correction. As is, this post-processing appears to me as a "hidden tuning" of your hybrid model. You did not explain what is done on each ESM, and how it affects their trends, so it is rather opaque to the reader and do not allow to isolate (and evaluate) the good results of your hybrid model from that of your correction. Finally you conclude the study by a spectacular reduction of 70% of the uncertainty, but I wonder what part of this reduction may be due to your ESM correction ?

Response:

As suggested, I added a Figure that allows to compare the composite model estimate with and without the adjustment to the ESMs. Furthermore, I also added a much longer explanation about the adjustment to the existing text in the manuscript following the reviewers comment:

"The carbon sink estimates from ESMs were adjusted for biases in each model's circulation and carbonate chemistry as described by Terhaar et al (2022). Each model's annually averaged and globally integrated ocean carbon sink was adjusted based on three predictors that were previously identified to determine the strength of the ocean carbon sink. The rate by which surface water with high anthropogenic carbon is transported to the ocean interior and replaced by newly upwelled water was shown to be largely driven in ESMs by the surface ocean salinity in the Southern Ocean that largely determines the rate of formation of mode and intermediate waters in that region (Terhaar et al., 2021c) and the strength of the Atlantic Meridional Overturning Circulation that determines the strength of the subsurface water formation in the subpolar North Atlantic in ESMs (Terhaar et al., 2022). In addition, the globally averaged Revelle factor determines the amount of carbon that the surface ocean can take up for a given increase of CO2 in the atmosphere in ESMs (Terhaar et al., 2022). These three predictors, the same as in Terhaar et al (2022), were used here. For the target variable, however, I here did not use the ocean carbon sink estimates from 1997 to 2014 but the ocean carbon sink from 1959 to 2022, the period for which this new composite model estimate provides annual ocean carbon sink estimates. To calculate the adjustment factors, I here used SSP5-8.5 after 2014 for which 3 more ESMs provide output (Table A4) as the higher number of ESMs makes the fit between predictors and target variables more robust (r2 decreases from 0.82 to 0.63 when only using the 14 ESMs that provide output for SSP1-2.6). The adjustment increases the mean carbon sink in the ESMs and hence the estimate of the composite estimate by 11% and reduces the uncertainty by 12% (Fig. A2). Hence, the main part of the reduction in uncertainty stems indeed from the combination of the two model classes and not from the adjustment to the ESMs by Terhaar et al. (2022). However, the adjustment is necessary to adjust for a known bias in the strength of the ocean carbon sink in ESMs (Terhaar et al., 2022)."

The text and the figure show that the largest part of the uncertainty reduction does not come from the adjustment to the ESMs. I hope that this extended explanation as well as the published manuscript that explains the adjustment in detail shows that this adjustment is no 'hidden tuning' but a well-reasoned and substantially tested approach that accounts for existing biases in the circulation and surface ocean carbonate chemistry in ESMs.

Comment 3.4

The capacity of the model to fit with recent GCB estimates of the recent years is not discussed.

Response:

I am not sure about this comment. In the previously submitted manuscript, I had already added a comparison between the composite estimate and the best estimate of the GCB:

"The composite estimate is almost identical to the best estimate of the ocean carbon sink provided by the Global Carbon Budget 2023 (Fig. 2d), which represents the average of the estimate by GOBMs and the pCO2 products. The composite estimate thus corroborates this best estimate by the Global Carbon Budget 2023. However, instead of simply averaging two independent estimates, the composite model estimate uses the best available knowledge to remove the weaknesses of each model estimate and to only keep the strengths. As the composite model estimate explains the differences, its uncertainty is around 70% smaller than that of the best estimate of the Global Carbon Budget 2023."

I have now changed the the first sentence in this paragraph following the reviewers comment:

"The composite estimate is almost identical to the best estimate of the ocean carbon sink provided by the Global Carbon Budget 2023 (Fig. 2d), which represents the average of the estimate by GOBMs and the pCO2 products, until the atmospheric CO2 differs in the ESMs after 2015."

In addition, I had already discussed the mismatch after 2015 in the previously submitted manuscript in the Discussion:

"One shortcoming is the forcing of ESMs with SSPs after 2014 that leads to slightly too high carbon sink estimates from 2015 to 2022. This shortcoming could be overcome if ESMs were run with observed atmospheric CO_2 after the historical forcing from CMIP6 ends in 2014."

and in the Results:

"After 2014, the increasing difference may either be explained by slightly different trajectories of atmospheric CO_2 in ESMs under SSP1-2.6 and the historical atmospheric CO_2. Based on a previously identified relationship between changes in the trend of atmospheric CO_2 and trends in the ocean carbon sink (Terhaar, 2024), the trend in the ocean carbon sink in the ESMs from 2013 to 2022 is likely around 0.2 Pg C yr^{-1} dec^{-1} too large."

Comment 3.5

Finally, the denomination "hybrid" seems not adequate, as nowodays it is a terminology that people apply to models integrating both AI and process-based model. I would suggest to replace it by "composite" model as it takes benefit of the fully coupled ESMs for the trends and GOBMs for the simulated variability.

Response:

Changed as suggested.

Comment 3.6

L.56-59 In Takano et al. (2023) no specific corrections or adjustments have been done on the ESMs analyzed on their Fig. 4d, showing that the simulated ocean warming aligns with the observed ones. So, I wonder how the bias adjustments you made for the ESMs carbon sink will not indirectly affect these good OHC transient warming trend. Correcting the circulation will change the OHC too: did you try to apply the same correction that you applied on carbon to the OHC in order to evaluate how your correction supposes a different physical mean state ? It sounds contradictory to me to emphasize good properties resulting from circulation (as the OHC trend), but to correct the circulation to get a better carbon sink.

Response:

The same approach that was used to constrain the ocean carbon sink unfortunately does not work for the ocean heat uptake as the boundary conditions are different. ESMs in CMIP6 were mostly simulated in concentration-driven mode, which means that the atmospheric CO_2 concentration trajectories were prescribed in each scenario. Hence, differences in the ocean carbon sink resulted from differences in the simulated ocean circulation and surface ocean carbonate chemistry. The boundary for the ocean heat content, i.e., the change in atmospheric warming strongly varies across ESMs (Nijsse et al., 2020; Tokarska et al., 2020). As model differences in the ocean heat uptake are hence driven both by model differences in the ocean circulation and by differences in the atmospheric warming rate, the relationship between proxies of the ocean circulation and the ocean heat uptake weakens substantially. Moreover, CMIP6 models overestimate the atmospheric warming rate on average by 16-22% (Nijsse et al., 2020; Tokarska et al., 2020) but underestimate the ventilation of the ocean interior by around 6%. (Terhaar et al., 2020). It is thus no surprise that the simulated ocean heat uptake in CMIP6 models is even a bit larger than the observation-based estimates over the last decades (Fig. 3b in Takano et al., 2023)

Nijsse, F. J. M. M., Cox, P. M., and Williamson, M. S.: Emergent constraints on transient climate response (TCR) and equilibrium climate sensitivity (ECS) from historical warming in CMIP5 and CMIP6 models, Earth Syst. Dynam., 11, 737–750, https://doi.org/10.5194/esd-11-737-2020, 2020.

Takano, Y., Ilyina, T., Tjiputra, J., Eddebbar, Y. A., Berthet, S., Bopp, L., Buitenhuis, E., Butenschön, M., Christian, J. R., Dunne, J. P., Gröger, M., Hayashida, H., Hieronymus, J., Koenigk, T., Krasting, J. P., Long, M. C., Lovato, T., Nakano, H., Palmieri, J., Schwinger, J., Séférian, R., Suntharalingam, P., Tatebe, H., Tsujino, H., Urakawa, S., Watanabe, M., and Yool, A.: Simulations of ocean deoxygenation in the historical era: insights from forced and coupled models, Front. Mar. Sci., 10, 2023.

Tokarska, K. B., Stolpe, M. B., Sippel, S., Fischer, E. M., Smith, C. J., Lehner, F., & Knutti, R. (2020). Past warming trend constrains future warming in CMIP6 models. Science Advance, 6, eaaz9549.

Comment 3.7

L.83 "As there small inter-model differences [...]": A verb is missing: As there is small... ?

Response:

The sentence was corrected and now reads as follows:

"As there are small inter-model differences in the simulated high-frequency variability..."

Comment 3.8

L.90 Please, give more details on this bias adjustment. I understand that you already explained it in a paper (Terhaar et al., 2022), but as your hybrid method depends on this ESM pre-processing, the reader needs to really understand the implications of using it or not. The paragraph in Appendix (L. 199-205) in itself is not sufficient to understand the correction method you used.

Response:

As suggested by the reviewer, more details on the bias adjustments and an additional figure that visualizes the effect of the adjustment have been added. Please read the response to Comment 3.3 for a detailed answer.

Comment 3.9

L.131-132 "The difference before 2014 is likely due to a bias in the GOBMs [...] that also exists in ESMs but was corrected for". As nothing shows your correction, the reader must believe you: please illustrate this correction by adding individual trends of ESMs members you used with and without your corrections on Figure 2a, as well as the GOBMs mean trend (extracted from Figure 1c). Moreover you may compute your hybrid estimate without correcting the ESMs and superimpose this result on Figure A1a. These analyses may help to demonstrate that, before 2014, the difference (hybrid – GOBMs) is of the same order that (ESM – corrected ESM), and so attributable to the bias of circulation and carbonate chemistry.

Response:

The ESMs with and without bias adjustment are now shown as an additional figure in the Appendix as suggested by the reviewer in comment 3.3. In addition, I also added the difference between the composite estimate without correcting the ESMs and the GOBMs to Figure A1a as a dashed line, as suggested by the reviewer. The new line in Figure A1a shows that the difference before 2014 between the hybrid estimate is indeed due to the bias in the GOBMs that also exists in ESMs but was corrected for. I thank the reviewer for the suggestion and hence remove the word "likely" in the revised manuscript.

Comment 3.10

L.135-137 You state that the trend in the ocean carbon sink in the ESMs is too large after 2014 and that half of the difference is explained by the atmospheric CO2 trajectory: please show and explain how did you reach this conclusion. Why a half ? I see only one possibility to assess and quantify it, that would be to force GOBMs with atmospheric forcings extracted from the ESMs atmosphere with the prescribed atmospheric CO2 of SSP1-2.6 (as that would force GOBMs to follow the ESMs CO2 trajectory, and will allow a direct comparison between the two trajectories).

Response:

An exact answer can indeed only be given by the simulations proposed by the reviewer. Therefore, I have carefully paid attention that the wording in this paragraph is not definite and highlights the speculative character of this paragraph. The numbers presented here are based a finding that the trend in the ocean carbon sink is strongly related to changes in the trend in atmospheric CO_2 and on the quantification of this relationship across an ESM ensemble from CMIP6 (Terhaar, 2024). Here I have used this recently quantified relationship to estimate the expected difference in the trend in the ocean carbon sink that is caused by the different trends in atmospheric CO_2 in SSP1-2.6 and the real world. I have changed the sentence and provided further explanation as suggested by the reviewer:

"Recently it has been shown that the trend in the ocean carbon sink is sensitively related to changes in the trend of atmospheric CO_2. Based on a previously identified relationship between changes in the trend of atmospheric CO_2 and trends in the ocean carbon sink across ESMs (Terhaar, 2024) and the evolution of atmospheric CO_2 in the real world and in SSP1-2.6 after 2014, the trend of the ocean carbon sink in ESMs should only have been 0.2 Pg C yr^{-1} dec^{-1} larger than in GOBMs and not 0.4 Pg C yr^{-1} dec^{-1} larger. The difference in the atmospheric CO2 trajectory thus explains around half of the difference of both estimates after 2014."

However, as the manuscript has been submitted in the letter format and as I have already exceeded the word limit during the revisions, I will have to refer the reader to Terhaar et al. (2024) for more detailed information.

Terhaar, J.: Drivers of decadal trends in the ocean carbon sink in the past, present, and future in Earth system models, Biogeosciences, 21, 3903–3926, https://doi.org/10.5194/bg-21-3903-2024, 2024.

Comment 3.11

L.137-140 "The other half might be due ..." As is, it seems speculative, please clarify why the spin-up strategy of Huguenin et al. (2022) would help reduce the biased transient warming.

Response:

As suggested by the reviewer, the sentence was adjusted to better explain how a change in the spin-up strategy might affect the ocean carbon sink trend:

"The difference in the atmospheric CO2 trajectory thus explains around half of the difference of both estimates after 2014. The other half might be the spin-up strategy by GOBMs with too warm atmospheric during pre-industrial times that result in too weak transient ocean warming (Takano et al., 2023; Huguenin et al., 2022) and hence too little loss of natural carbon to the atmosphere as a consequence of this warming."

Comment 3.12

L.193 "Estimates... was used": were used ?

Response:

Changed as suggested.

Comment 3.13

L.210 "is the defined as": is defined as ?

Response:

Changed as suggested

Comment 3.14

L.213 "so garantee": to garantee ?

Response:

Changed as suggested.

Comment 3.15

L.217 "at the start of the end of the timeseries": at the starts and the end ?

Response:

Changed as suggested.