I am deeply thankful to Prof. Galen McKinley for her constructive and very helpful evaluation of the manuscript, which has strongly improved its quality. Below, I have addressed the points that she has raised point by point. Prof. McKinley's text is shown in black and my responses in blue. Text that will be part of the revised manuscript is shown in italic and blue color.

Dr. Terhaar studies CMIP6 historical and future projections to assess the drivers of trends in the global ocean carbon sink. The author compares decadal trends in atmospheric pCO2 to decadal trends in the ocean sink, and proposes a 1 decade lag of the ocean behind the atmosphere. A mechanistic explanation for this lag is not offered. The author also proposes that this analysis of ESMs demonstrates that the decadal trends in pCO2 products are too large.

Major comments

- The a 1 decade delayed response of the ocean sink to trends in the atmospheric growth rate in CMIP6 is intriguing. But why? A proposed mechanism for this effect is missing in the manuscript. In McKinley et al. 2020, we show that change in the atmospheric growth rate impacts the ocean sink with no lag, due to the impact on the delta pCO2. Lovenduski et al. (2021) demonstrate this mechanism with CANESM5 for changes in the atmospheric growth rate consistent with COVID19. What is different mechanistically, or about the analysis performed here, that leads to a very different conclusion here? The author needs to address this directly.

**Response:** I believe this is a misunderstanding. What I proposed here is not that the decadal trend in the ocean carbon sink is driven by the trend in the atmospheric $CO_2$ (= atmospheric $CO_2$ growth rate) in the preceding decade, i.e., a 1-decade delayed response. Instead, I propose that it is the trend in atmospheric $CO_2$ in this decade compared to the trend in atmospheric $CO_2$ in the previous decade that drives the trend in the ocean carbon sink. As such, it is not a 1-decade delayed response but includes information from both decades.

The here proposed mechanism is in line with Lovenduski et al. (2021) and McKinley et al. (2020). McKinley et al. (2020) write in their abstract: "First, the global-scale reduction in the decadal-average ocean carbon sink in the 1990s is attributable to the slowed growth rate of atmospheric $p\text{CO}_2$. The acceleration of atmospheric $p\text{CO}_2$ growth after 2001 drove recovery of the sink." In their study, they define a slowed down trend with respect to a prescribed linear trend. However, computing the difference to a linear trend works not for the entire historical period where $p\text{CO}_2$ growth is exponential so that a linear trend, even over 30 years, would always lead to a too small $p\text{CO}_2$ growth first and a faster $p\text{CO}_2$ growth faster, and would also not work when atmospheric $p\text{CO}_2$ growth peaks and changes from an increase to a decline.

Here, I use the mechanism of the slowing and acceleration of define the slowing or acceleration of atmospheric $p\text{CO}_2$ growth as the driver of changes in the ocean sink on decadal trends but define slowing and acceleration not with respect to a theoretical linear trend but compared the atmospheric $p\text{CO}_2$ growth in the preceding decade. Changes in decadal trends, as opposed to shorter trends, are used because inter-annual variability in the ocean carbon sink and the atmospheric $p\text{CO}_2$ growth disguise the trends on shorter timescales as shown by Lovenduski et al. (2021). Throughout the manuscript, I show that this definition of slowing and acceleration of the atmospheric $p\mathrm{CO_2}$ growth with respect to the preceding decade works well for low- to medium $CO_2$ emission scenarios.

In the revised manuscript, I will clarify this as follows to avoid any further misunderstanding:

*"Although neither the atmospheric $CO_2$ nor its growth rate can quantify the strength of the ocean carbon sink various time period and different trajectories of atmospheric $CO_2$, the atmospheric CO2 growth rate can nevertheless be used to understand changes in the ocean carbon sink on decadal timescales, i.e., decadal trends of the ocean carbon sink. For the period from 1980 to 2018, it has been shown that a slowing of the growth rate in comparison to a linear trend has led to a stagnation of the increase of the ocean carbon sink and that an accelerated increase of the growth rate has led to a strongly increasing carbon sink (McKinley et al., 2020)."*

*[…]*

*As a slowing or acceleration of the growth rate in comparison to a theoretical linear trend as in McKinley et al. (2020) is not anymore possible over longer time periods of exponential growth or when atmospheric $CO_2$ peaks, I here generalize the idea of McKinley et al. (2020) that a slowing or acceleration of the atmospheric $CO_2$ growth rate drives the trends of the ocean carbon sink by defining such slowing or acceleration as the difference in the growth rate in a given decade with respect to the preceding decade. When defining slowing or acceleration of the atmospheric $CO_2$ growth rate that way, a clear relationship ($r^2$=0.91) emerges indeed over the entire historical period and all four future scenarios over the 21st century (excluding years where the ocean carbon sink exceeds 4.5 Pg C $yr^{-1}$) between changes in the atmospheric $CO_2$ growth rate and the decadal trend of the multi-model average of the ocean carbon sink (Fig. 3)."*

- What is the impact on the findings of the adjustments to model output following Terhaar et al. (2022)? Dr. Terhaar and colleagues' previous findings are interesting, but not conclusive. Others, such as Goris et al. (2018) propose alternative metrics for such a constraint. It is important to understand the impact of this adjustment on these results.

**Response:** The alternative metrics presented by Goris et al. (2018) are not opposite to those found by Terhaar et al. (2022b). Instead, they are complimentary and indicate similar biases in the models, as also shown for ocean-biogeochemical models in hindcast mode by Terhaar, Goris, et al. (2024). Below, I have re-made the main figure from the manuscript without the adjustment. Without the bias-adjustment for each Earth System Model, the strength of the relationship slightly reduces from $r^2$=0.90 to $r^2$=0.83 but remains strong and significant. Hence, the findings are not sensitive to the bias-adjustment but the bias-adjustment still yields more reliable results as the studies from Goris et al. (2018), Terhaar et al. (2022b) and Terhaar, Goris, et al. (2024) suggest. For clarification, I added the following sentence to the Methods:

*"The adjustment corrects for known biases in the models' circulations and surface ocean carbonate chemistry and hence reduced differences in the overall magnitude of the simulated carbon sink between ESMs (Terhaar et al., 2022b). This reduction in the difference in the magnitude of the carbon sink also reduces differences between the magnitude of trends and*

*slightly improves the relationships found here as it ($r^2$ in Figure 3 would have been 0.83 without adjustment instead of 0.91 with adjustment). Nevertheless, the results would quantitatively and qualitatively almost identical with and without that adjustment. "*

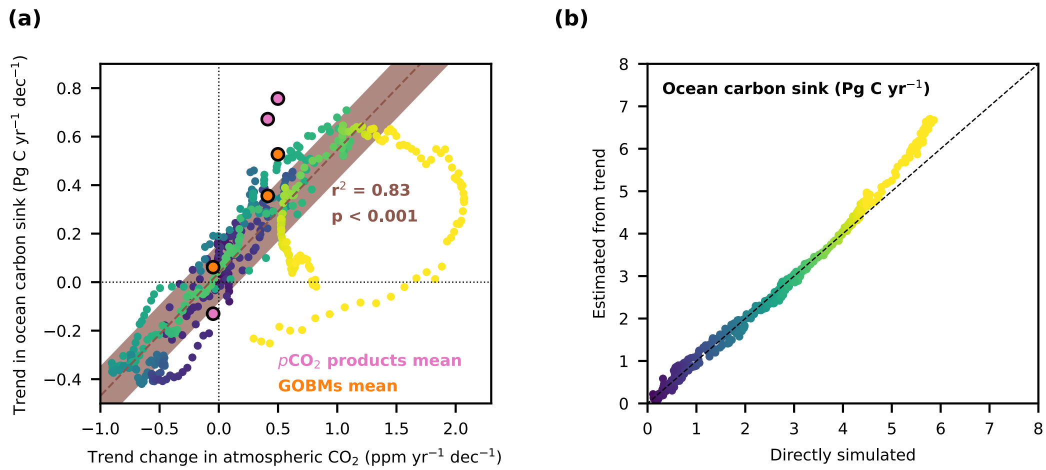


**Figure 3:** The relationship between changes in the atmospheric CO2 growth rate and decadal trends of the global ocean carbon sink for the multi-model mean. **(a)** Decadal trends of the multi-model mean ocean carbon sink compared to changes in decadal trends in atmospheric CO2, which represent the decadal averaged growth rate of atmospheric CO2. The dark blue to yellow circles without a surrounding black line show multi-model averages for all years of the historical period from 1850 to 2014 and for all years from 2015 to 2100 for all four SSPs. All decades over from 1850 to 2100 are shown, i.e., 2000-2009, 2001-2010, 2002-2011, etc. The brown line shows a linear fit for all years when the global ocean carbon sink is smaller than 4.5 Pg C yr-1 and the brown shading is the 1-s projection uncertainty. The dots with black lines around them show values from the respective ensemble means of the pCO2 products (pink) and GOBMs (orange) from the Global Carbon Budget 2023 (Friedlingstein et al., 2023) for the three decades between 1990 and 2020. **(b)** The simulated ocean carbon sink in comparison to the expected ocean carbon sink based on the relationship in **(a)** and the prescribed trend change in atmospheric CO2 in the simulations.

- The author focuses the introduction on the weaknesses of pCO2 products and GOBMs, but does not adequately acknowledge that ESMs also have weaknesses. The fact that the author will adjust and detrend the ESMs before doing his analysis needs to be acknowledged here, as just one example of a weakness. Please adjust this discussion to be more balanced.

**Response:** As suggested by the reviewer, we have added the following text about ESMs to the Introduction:

*"Here, I use an ensemble of 12 ESMs to provide a new perspective on potential drivers of the decadal trends of the ocean carbon sink from phase 6 of the Coupled Model Intercomparison Project (CMIP6) (Table 1). Fully coupled ESMs are another tool to quantify and understand the ocean carbon sinks (e.g., Joos et al., 1999; McNeil and Matear, 2013; Frölicher et al., 2015; Goris et al., 2018; Terhaar et al., 2022b, 2021b). As ESMs are fully coupled and not forced with atmospheric reanalysis data, they do not simulate the same inter-annual internal climate variability as pCO2 products and GOBMs do and their biases of the surface ocean physics and biogeochemistry are thus larger than surface ocean biases of GOBMs (Terhaar et al., 2022b;*

*Terhaar et al., 2024). However, ESMs have distinctive advantages compared to pCO2 products and GOBMs for the analyses of decadal drivers of the ocean carbon sink because (1) they cover a period of 251 years from 1850 to 2100, (2) cover at least four different future scenarios, and (3) they all have a different internal climate state."*

- The author needs to be more precise about ESMs vs. GOBMs. Papers such as Gruber et al 2023 and the associated literature, as well as RECCAP2, focus on comparing pCO2 products to GOBMs, not to ESMs as indicated on Line 458 and below. Please check throughout the paper and make sure the discussion does not confuse.

**Response:** I believe there has been a misunderstanding. Although Gruber et al. (2023) did not focus on ESMs, but on pCO2 products and GOBMs as explained by the reviewer, their discussion extends to ESMs (here called coupled carbon-climate models):

"An ocean sink that is more sensitive to climate change than currently assumed in coupled carbon-climate models[52] would imply that the ocean will take up less $CO_2$ from the atmosphere in the future than anticipated."

Reference 52 (Arora et al., 2020) in Gruber et al. (2023) is about idealized scenarios with steadily increasing atmospheric $CO_2$ and carbon-climate feedbacks. However, here I have shown that the variability of the ocean carbon sink is mainly driven by variability in atmospheric $CO_2$ growth, which does not exist in these idealized scenarios, and not to climate change. Furthermore, I have demonstrated in Fig. 7 that the ESMs simulate equal or larger trends in the major climate modes. Moreover, several past studies have shown that the variability and trends in $p$$CO_2$ products are instead overestimated (e.g., Gloege et al., 2021; Hauck et al., 2023) further questioning the discussion above by Gruber et al. (2023).

Hence, I believe that the hypothesis by Gruber et al. (2023) that the decadal trends in the ocean carbon sink in ESMs is too small and that this means that the sensitivity to climate change is to small is not supported by Arora et al. (2020) and that the information in this manuscript challenges that hypothesis. Hence, I think that sentence in line 458 was correct (*"Thus, there is no indication that the decadal variability of the ocean carbon sink in ESMs (Fig 7a) might be too small because of a too small internal climate variability in ESMs as previously hypothesized (Gruber et al., 2023)"*).

To avoid any further misunderstandings, I have nevertheless changed it to:

*"As the decadal trends in climate mode are larger or equal to the observed ones, there is no indication that the decadal variability of the ocean carbon sink in ESMs (Fig 7a) might be too small because of a too small internal climate variability in ESMs as previously hypothesized by Gruber et al., (2023) based on small carbon-climate feedbacks in idealized scenarios without variability in the atmospheric $CO_2$ growth (Arora et al., 2020)."*).

In addition, I have read the manuscript carefully again to make sure that all statements are correct.

- There is a lot of discussion of detailed features from the figures that are very difficult for the reader to see due to a lack of annotation. For example, on figures 1 and 2 where atmospheric CO2 concentration or growth rate are plotted against the ocean sink, the author discusses features at specific dates. It is not possible to see these dates on such a figure. The author needs to make sure the reader can identify the features he discusses.

**Response:** As suggested by the reviewer, the discussed time periods are now marked on the respective panels as suggested by the reviewer. The figures were revised to:

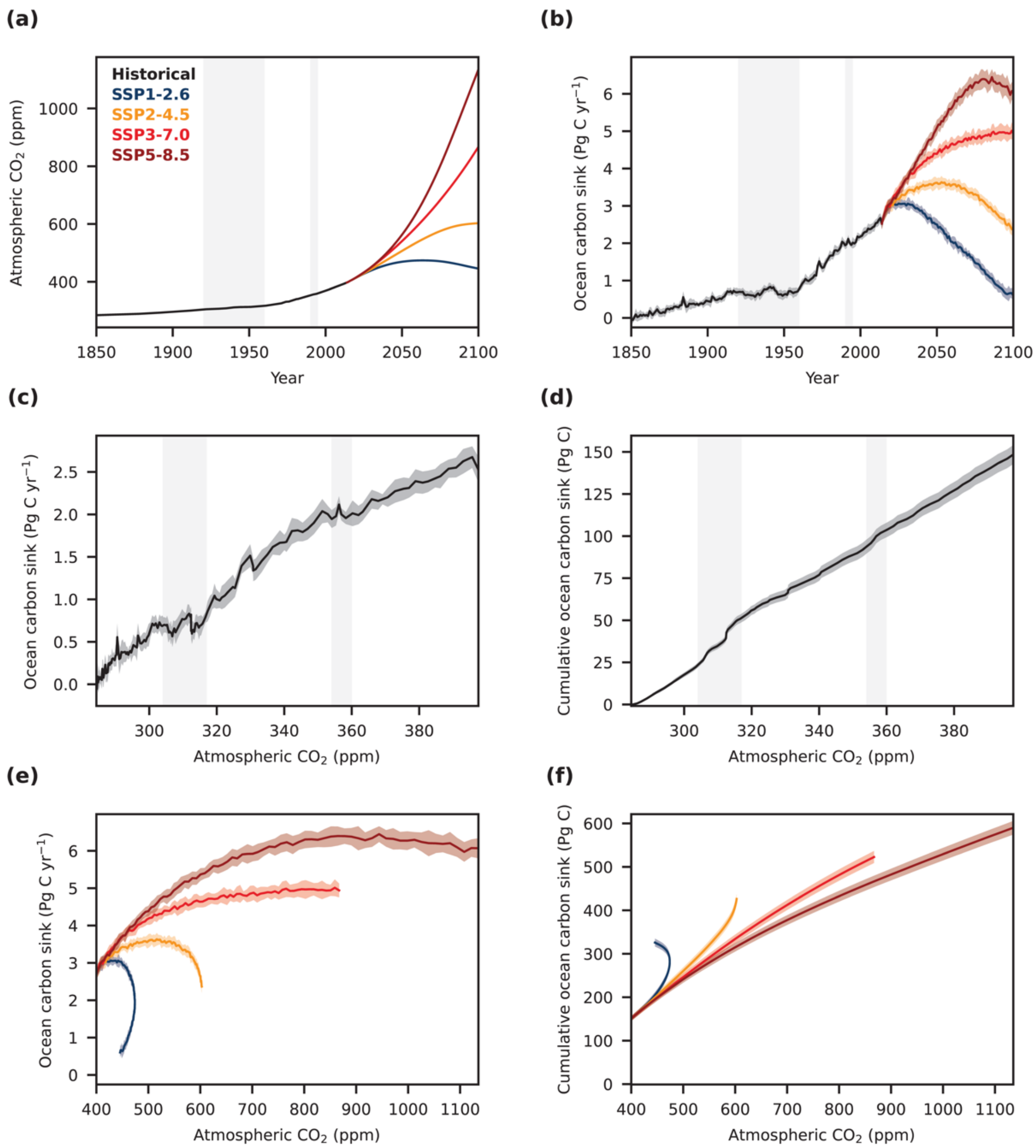


**Figure 1: The relationship between atmospheric $CO_2$ and the global ocean carbon sink. (a)** The annually averaged atmospheric $CO_2$ that was used to force the ESMs from CMIP6 based on observation-based estimates from 1850 to 2014 (black) and based on four different SSPs (SSP1-2.6 in blue, SSP2-4.5 in orange, SSP3-7.0 in red, and SSP5-8.5 in brown) from 2015 to 2100. **(b)** The resulting ocean carbon sink as simulated by 12 ESMs (Table 1) after being adjusted for biases in circulation and surface ocean carbonate chemistry following Terhaar et al. (2022). The thick lines indicate multi-model means and the shading the 1-σ standard

deviation across the model ensemble. Relationships between atmospheric $CO_2$ and the annually averaged ocean carbon sink **(c)** for the historical period until 2014 and **(e)** for the 21[st] century from 2015 onwards, as well as between atmospheric $CO_2$ and the cumulative ocean carbon sink **(d)** for the historical period until 2014 and **(f)** for the 21[st] century from 2015 onwards. The light grey shadings in **(a)** – **(d)** indicate the time periods from 1920 to 1960 and from 1990 to 1995.

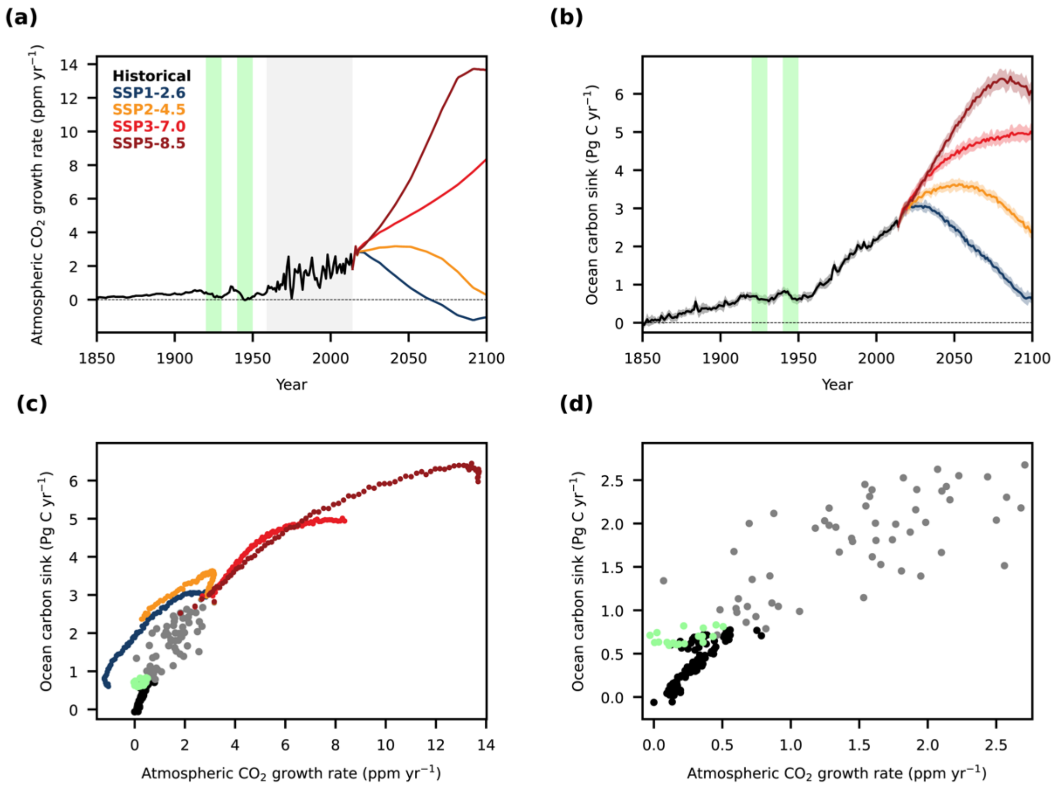


**Figure 2: The relationship between the atmospheric $CO_2$ growth rate and the global ocean carbon sink. (a)** The annually averaged atmospheric $CO_2$ growth rate based on atmospheric $CO_2$ forcing files from CMIP6, which are based on observation-based estimates from 1850 to 2014 (black) and based on four different SSPs (SSP1-2.6 in blue, SSP2-4.5 in orange, SSP3-7.0 in red, and SSP5-8.5 in brown) from 2015 to 2100. **(b)** The ocean carbon sink as simulated by 12 ESMs (Table 1) after being adjusted for biases in circulation and surface ocean carbonate chemistry following Terhaar et al. (2022). The thick lines indicate multi-model means and the shading the 1-σ standard deviation across the model ensemble. Relationships between atmospheric $CO_2$ growth rate and the annually averaged ocean carbon sink **(c)** for the entire period from 1850 to 2100 and **(d)** only for historical period until 2014. The light grey shading in **(a)** indicates the period where direction atmospheric CO2 observations are available and the pale green shading in **(a)** and **(b)** and the pale green dots in **(c)** and **(d)** indicate the 1990s and 1940s. The zero growth rate and ocean carbon sink in **(a)** and **(b)** are shown as black dashed lines.

Minor

Line 33 - 12 members is not a "large ensemble" in the common understanding of this terms. This would be at least many 10s of members. Please revise.

**Response:** The sentence was changed to "The robust relationship over an ensemble of 12 different ESMs" as suggested by the reviewer.

Line 59 – Ridge and McKinley 2021 Biogeosciences should also be cited

**Response:** The reference was added as suggested.

Line 78 – Please add Gloege et al. 2022 JAMES, Bennington et al. 2022 GRL, Bennington et al. 2022 JAMES

**Response:** The references were added as suggested.

Line 80 – Please add LaCroix et al. 2020

**Response:** The reference was added as suggested.

Line 87 – Says "pCO2 products" here, should be GOBMs

**Response:** Thank you. The mistake was corrected as suggested.

Line 93 – unclear to what "both products" refers

**Response:** For clarification, "Both products" was replaced by "$p\mathrm{CO_2}$ products and GOBMs".

Line 94 – Gloege et al. 2021 did not use GOBMs; large ensembles of ESMs were used.

**Response:** Thank you. The mistake was corrected as suggested.

Line 110 – Please reference McKinley et al. 2023 ERL as a study that considers the full CMIP6 suite.

**Response:** The reference was added as suggested.

Line 193 – Clarify here briefly that 2014 is the end of the historical period of forcing - i.e. "After 2014, when SSP scenario forcing begins,… " or similar

**Response:** As suggested by the reviewer, the beginning of the sentence was changed to:

*"After 2014, when the historical period in CMIP6 ends and SSPs start,…"*

Line 218 – A square root relationship should be dependent on the units. Please include units. Please also mark this square root relationship on the figure

**Response:** As the sentence appeared to have created more confusion than clarity and the relationship was weak, we have removed any mention such a square root relationship from the manuscript.

Line 225 – "ocean carbon sink does not go back close to zero but remains almost stable (Fig. 2b)." This cannot be easily seen on the plot

**Response:** For clarification, dashed lines were added where the atmospheric $CO_2$ growth rate (Fig. 2a) and ocean carbon sink (Fig. 2b) are zero (please see revised Fig. 2 above).

Section 2.1 Some of these ESMs provide multiple ensemble members to CMIP6. How are the ensembles used here? Just the first one taken? An average made? If the latter, then it could impact results by averaging out some of the internal variability of individual members, and this would need to be discussed. Please make this clear, and discuss any impacts on results.

**Response:** Only the first ensemble member is used. The reasoning is now discussed in the revised manuscript:

*"For each ESM, only the first ensemble member is used as averaging over multiple ensemble members would have removed variability and using different numbers of ensemble members per ESM would have biased results towards the ESMs with more ensemble members."*

Section 3.2, and Line 251-262. It is difficult to follow these discussions. Adding annotation on figures (as suggested below), increasing the size of the figures so that these features can be seen, and/or revising the text to more clearly describe the features being discussed.

**Response:** The discussed time periods are now marked on the respective panels as suggested by the reviewer. Please see response to major comment above.

Line 245-246. The first phrase of this sentence is incomplete, and also and above it was said there is a square root relationship. Please revise.

**Response:** The sentence was revised as suggested by the reviewer, and any mention of a square root relationship was removed.

Line 260. Replace "done" with "down"

**Response:** The word was changed as suggested by the reviewer.

Line 264. It is not true that it is "not possible" to compare to a linear trend. For example, it would be possible to calculate linear trends for 30 years, and use this as comparison. The author needs to find a better way to justify the approach taken here.

**Response:** I disagree with the reviewer. The linear trend might be possible as long as the increase in atmospheric $CO_2$ can be approximated by a linear trend. Where this is not possible, i.e., when atmospheric $CO_2$ peaks or the increase is strongly exponential, this is not possible. The sentence has been revised following a major comment above by a reviewer.

Line 270. What is the mechanism of this delayed response?

**Response:** As described in the first major comment, I believe that is a misunderstanding that has been clarified in the response to that comment.

Line 318. SSP1-2.6 scenario is not clearly labeled in figure 3a. Please ensure the reader can follow this discussion.

**Response:** The periods are now marked in the figure. The revised figure looks as follows:

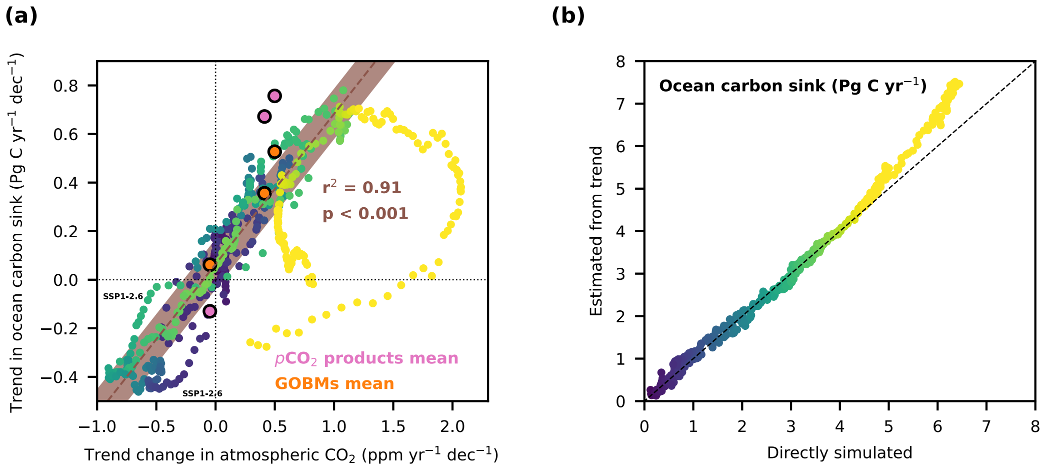


**Figure 3: The relationship between changes in the atmospheric $CO_2$ growth rate and decadal trends of the global ocean carbon sink for the multi-model mean. (a)** Decadal trends of the multi-model mean ocean carbon sink compared to changes in decadal trends in atmospheric $CO_2$, which represent the decadal averaged growth rate of atmospheric $CO_2$. The dark blue to yellow circles without a surrounding black line show multi-model averages for all years of the historical period from 1850 to 2014 and for all years from 2015 to 2100 for all four SSPs. All decades over from 1850 to 2100 are shown, i.e., 2000-2009, 2001-2010, 2002-2011, etc. The brown line shows a linear fit for all years when the global ocean carbon sink is smaller than 4.5 Pg C $yr^{-1}$ and the brown shading is the 1-σ projection uncertainty. The dots with black lines around them show values from the respective ensemble means of the $p$CO$_2$ products (pink) and GOBMs (orange) from the Global Carbon Budget 2023 (Friedlingstein et al., 2023) for the three decades between 1990 and 2020. Small deviations from the relationship in SSP1-2.6 are marked by 'SSP1-2.6'. **(b)** The simulated ocean carbon sink in comparison to the expected ocean carbon sink based on the relationship in **(a)** and the prescribed trend change in atmospheric $CO_2$ in the simulations.

Line 390. Strike "to"

**Response:** The word "to" was removed as suggested by the reviewer.

Line 455-482. ESMs may get the climate modes, but not necessarily the ocean carbon sink response to these modes. The following discussion of biogeochemical vs physical driven variability in GOBMs and of higher resolution models does try to address this, but still it is not conclusive because these are still models being used as the point of comparison. There is circularity in this discussion that needs to be acknowledged – i.e. though sampling of pCO2 may lead the pCO2 products to overestimate decadal variability, it also remains possible that the pCO2 products are capturing real signals that we are not modeling.

**Response:** In the revised manuscript, it will be acknowledged that it remains possible that the $p$CO$_2$ products are capturing real signals that we are not modeling as suggested by the reviewer:

*"Overall, the dominance of physical variability over biogeochemical variability and the larger decadal trends of climate modes in ESMs than in the real world suggest that the ESMs do not*

*underestimate the natural variability of the ocean carbon sink although it always remains possible that the pCO2 products are capturing real signals that are not yet simulated."*

Line 486. "ESMs used here"

**Response:** The words were reordered as suggested by the reviewer.

Line 523. Again, what is the mechanism of the decadal lag in the ocean response to the atmospheric growth rate?

**Response:** As described in the first major comment, I believe that is a misunderstanding that has been clarified in the response to that comment. For further clarification, this sentence was changed to:

*"While McKinley et al. (2020) have focused on the last decades and suggested that the trends in the ocean carbon sink depends on differences on the atmospheric growth rate of $CO_2$ compared to the long-term trend of the growth rate, I could generalize this idea here and show that it is the change in the growth rate compared to the previous decade that drives the trends of the ocean carbon sink over a wide range of timescales and SSPs."*

Figure 1.

- Please add a legend on the figure. Please make the blue more clearly distinguishable as not black/gray.
- Consider marking 1920, 1960, 1990, 2000 on panel c, d. This is needed to more easily follow the discussion about "jumps" at line 189-191.

**Response:** The periods are now marked as suggested by the reviewer (please see Figure above). However, the colors were not changed as these are the official colors from the IPCC reports that I prefer to keep making the SSPs easier recognizable.

Figure 2

- It is not clear where 1920 and 1940 are in panels c and d; please these clearly mark on the plots

**Response:** The annotations were added as suggested.

Figure 3

- The text suggests that here it is ocean sink in decade 2 compared to atm CO2 trend in decade 1, but it isn't stated in this way in the caption. The caption suggests they are concurrent trends. This needs clarification.

**Response:** I believe this misunderstanding has been clarified in the response to the major comments.

## References


Arora, V. K., Katavouta, A., Williams, R. G., Jones, C. D., Brovkin, V., Friedlingstein, P., Schwinger, J., Bopp, L., Boucher, O., Cadule, P., Chamberlain, M. A., Christian, J. R., Delire, C., Fisher, R. A., Hajima, T., Ilyina, T., Joetzjer, E., Kawamiya, M., Koven, C. D., Krasting, J. P., Law, R. M., Lawrence, D. M., Lenton, A., Lindsay, K., Pongratz, J., Raddatz, T., Séférian, R., Tachiiri, K., Tjiputra, J. F., Wiltshire, A., Wu, T., and Ziehn, T.: Carbon–concentration and carbon–climate feedbacks in CMIP6 models and their comparison to CMIP5 models, Biogeosciences, 17, 4173–4222, https://doi.org/10.5194/bg-17-4173-2020, 2020.

Fay, A. R., Gregor, L., Landschützer, P., McKinley, G. A., Gruber, N., Gehlen, M., Iida, Y., Laruelle, G. G., Rödenbeck, C., Roobaert, A., and Zeng, J.: SeaFlux: harmonization of air–sea CO2 fluxes from surface pCO2 data products using a standardized approach, Earth Syst. Sci. Data, 13, 4693–4710, https://doi.org/10.5194/essd-13-4693-2021, 2021.

Friedlingstein, P., O'Sullivan, M., Jones, M. W., Andrew, R. M., Bakker, D. C. E., Hauck, J., Landschützer, P., Le Quéré, C., Luijkx, I. T., Peters, G. P., Peters, W., Pongratz, J., Schwingshackl, C., Sitch, S., Canadell, J. G., Ciais, P., Jackson, R. B., Alin, S. R., Anthoni, P., Barbero, L., Bates, N. R., Becker, M., Bellouin, N., Decharme, B., Bopp, L., Brasika, I. B. M., Cadule, P., Chamberlain, M. A., Chandra, N., Chau, T.-T.-T., Chevallier, F., Chini, L. P., Cronin, M., Dou, X., Enyo, K., Evans, W., Falk, S., Feely, R. A., Feng, L., Ford, D. J., Gasser, T., Ghattas, J., Gkritzalis, T., Grassi, G., Gregor, L., Gruber, N., Gürses, Ö., Harris, I., Hefner, M., Heinke, J., Houghton, R. A., Hurtt, G. C., Iida, Y., Ilyina, T., Jacobson, A. R., Jain, A., Jarn\'\iková, T., Jersild, A., Jiang, F., Jin, Z., Joos, F., Kato, E., Keeling, R. F., Kennedy, D., Klein Goldewijk, K., Knauer, J., Korsbakken, J. I., Körtzinger, A., Lan, X., Lefèvre, N., Li, H., Liu, J., Liu, Z., Ma, L., Marland, G., Mayot, N., McGuire, P. C., McKinley, G. A., Meyer, G., Morgan, E. J., Munro, D. R., Nakaoka, S.-I., Niwa, Y., O'Brien, K. M., Olsen, A., Omar, A. M., Ono, T., Paulsen, M., Pierrot, D., Pocock, K., Poulter, B., Powis, C. M., Rehder, G., Resplandy, L., Robertson, E., Rödenbeck, C., Rosan, T. M., Schwinger, J., Séférian, R., et al.: Global Carbon Budget 2023, Earth Syst. Sci. Data, 15, 5301–5369, https://doi.org/10.5194/essd-15-5301-2023, 2023.

Frölicher, T. L., Sarmiento, J. L., Paynter, D. J., Dunne, J. P., Krasting, J. P., and Winton, M.: Dominance of the Southern Ocean in Anthropogenic Carbon and Heat Uptake in CMIP5 Models, J. Clim., 28, 862–886, https://doi.org/10.1175/JCLI-D-14-00117.1, 2015.

Gloege, L., McKinley, G. A., Landschützer, P., Fay, A. R., Frölicher, T. L., Fyfe, J. C., Ilyina, T., Jones, S., Lovenduski, N. S., Rodgers, K. B., Schlunegger, S., and Takano, Y.: Quantifying Errors in Observationally Based Estimates of Ocean Carbon Sink Variability, Global Biogeochem. Cycles, 35, e2020GB006788, https://doi.org/https://doi.org/10.1029/2020GB006788, 2021.

Goris, N., Tjiputra, J. F., Olsen, A., Schwinger, J., Lauvset, S. K., and Jeansson, E.: Constraining Projection-Based Estimates of the Future North Atlantic Carbon Uptake, J. Clim., 31, 3959–3978, https://doi.org/10.1175/JCLI-D-17-0564.1, 2018.

Gregor, L. and Gruber, N.: OceanSODA-ETHZ: a global gridded data set of the surface ocean carbonate system for seasonal to decadal studies of ocean acidification, Earth Syst. Sci. Data, 13, 777–808, https://doi.org/10.5194/essd-13-777-2021, 2021.

Gruber, N., Bakker, D. C. E., DeVries, T., Gregor, L., Hauck, J., Landschützer, P., McKinley, G. A., and Müller, J. D.: Trends and variability in the ocean carbon sink, Nat. Rev. Earth Environ., https://doi.org/10.1038/s43017-022-00381-x, 2023.

Halsey, L. G.: The reign of the p-value is over: what alternative analyses could we employ to fill the power vacuum?, Biol. Lett., 15, 20190174, https://doi.org/10.1098/rsbl.2019.0174, 2019.

Hauck, J., Nissen, C., Landschützer, P., Rödenbeck, C., Bushinsky, S., and Olsen, A.: Sparse observations induce large biases in estimates of the global ocean CO2 sink: an ocean model subsampling experiment, Philos. Trans. R. Soc. A Math. Phys. Eng. Sci., 381, 20220063, https://doi.org/10.1098/rsta.2022.0063, 2023.

Held, L. and Ott, M.: On p-values and Bayes factors, Annu. Rev. Stat. Appl, 5, 393–419, https://doi.org/10.1146/annurev-statistics-031017-100307, 2018.

Joos, F., Plattner, G.-K., Stocker, T. F., Marchal, O., and Schmittner, A.: Global Warming and Marine Carbon Cycle Feedbacks on Future Atmospheric CO2, Science, 284, 464–467, https://doi.org/10.1126/science.284.5413.464, 1999.

McKinley, G. A., Fay, A. R., Eddebbar, Y. A., Gloege, L., and Lovenduski, N. S.: External Forcing Explains Recent Decadal Variability of the Ocean Carbon Sink, AGU Adv., 1, e2019AV000149, https://doi.org/https://doi.org/10.1029/2019AV000149, 2020.

McNeil, B. I. and Matear, R. J.: The non-steady state oceanic CO2 signal: its importance, magnitude and a novel way to detect it, 10, 2219–2228, https://doi.org/10.5194/bg-10-2219-2013, 2013.

Lovenduski, N. S., Chatterjee, A., Swart, N. C., Fyfe, J. C., Keeling, R. F., and Schimel, D.: On the detection of COVID-driven changes in atmospheric carbon dioxide, Geophysical Research Letters, 48, e2021GL095396, https://doi.org/10.1029/2021GL095396, 2021

Orr, J. C., Kwiatkowski, L., and Pörtner, H.-O.: Arctic Ocean annual high in pCO2 could shift from winter to summer, Nature, 610, 94–100, https://doi.org/10.1038/s41586-022-05205-y, 2022.

Terhaar, J., Torres, O., Bourgeois, T., and Kwiatkowski, L.: Arctic Ocean acidification over the 21st century co-driven by anthropogenic carbon increases and freshening in the CMIP6 model ensemble, Biogeosciences, 18, 2221–2240, https://doi.org/10.5194/bg-18-2221-2021, 2021a.

Terhaar, J., Frölicher, T., and Joos, F.: Southern Ocean anthropogenic carbon sink constrained by sea surface salinity, Sci. Adv., 7, 5964–5992, https://doi.org/10.1126/sciadv.abd5964, 2021b.

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

Terhaar, J., Goris, N., Müller, J. D., DeVries, T., Gruber, N., Hauck, J., Perez, F. F., and Séférian, R.: Assessment of global ocean biogeochemical models for ocean carbon sink estimates in RECCAP2 and recommendations for future studies, Global Biogeochem. Cycles, 16, e2023MS003840, https://doi.org/10.1029/2023MS003840, 2024.

Yasunaka, S., Manizza, M., Terhaar, J., Olsen, A., Yamaguchi, R., Landschützer, P., Watanabe, E., Carroll, D., Adiwara, H., Müller, J. D., and Hauck, J.: An assessment of CO2 uptake in the Arctic Ocean from 1985 to 2018, Global Biogeochem. Cycles, 2023.

Zeng, J., Iida, Y., Matsunaga, T., and Shirai, T.: Surface ocean CO2 concentration and air-sea flux estimate by machine learning with modelled variable trends, Front. Mar. Sci., 9, 989233, https://doi.org/10.3389/fmars.2022.989233, 2022.