

Earth Syst. Dynam. Discuss., referee comment RC1
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Comment on esd-2022-19

Anonymous Referee #1

Referee comment on "Time varying changes and uncertainties in the CMIP6 ocean carbon sink from global to regional to local scale" by Parsa Gooya et al., Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2022-19-RC1>, 2022

The authors investigate the future ocean carbon sink in CMIP6 models under several SSP scenarios. They quantify the uncertainty in the future sink as a function of model, internal and scenario uncertainty for the global and regional scales. They calculate time of emergence for the forced signal to emerge from internal variability. They find that the future ocean carbon sink is most uncertain in regions of currently highest flux.

The methods used are solid and, with a few minor exceptions, adequately explained. Reasonable assumptions are made. The Supplementary provides useful additional information.

The paper is a contribution to the literature on the CMIP6 models. Most of the calculations done here have been done before for CMIP5 models in several papers, so this is a useful update. It is appropriate for ESD readership. Conclusions are justified by the analysis.

Major Comments

On the whole, the paper lacks depth and clarity in the discussion of mechanisms on the ocean carbon sink and how it should evolve in the future. The references to the literature are also somewhat sparse. I encourage the authors to review some more of the literature and to add more mechanistic discussion and connection to previous studies. To do so will make the paper a more useful contribution. Possibilities include Crisp et al 2022 in Reviews of Geophysics, Ridge and McKinley 2021 in Biogeosciences, Hauck et al. 2020 in Frontiers, McKinley et al. 2020 in AGU Advances, Bushinsky et al 2019 in GBC, Schwinger and Tjiputra 2018 in GRL.

I also recommend changing away from the "hotspots" terminology. For the carbon sink, this term is often used for very small regions, such as western boundary current mode water formation regions. To use this for all of the Southern Ocean, North Atlantic, etc. is also just not a very good choice of words also since these are large, basin scale regions not "spots".

Minor

Pg 2, line 46. Bushinsky et al. 2019 should be added to this list

Pg 3, line 84. ESMs are based in fundamental equation such a Navier Stokes. Yes, there are many details that differ, but there is also a lot of basis in physics! This statement suggests that models are much more of a potpourri than they actually are. Please add some more discussion to more accurately represent ESMs.

Pg 8, line 95. Need to clarify that SOM-FFN is just one realization, not the forced signal. Of course, it is all that we have, so the comparison to the multimodel mean is reasonable. The authors just need to make sure that the text here helps the reader to understand that observations are not the forced component.

pg 8 line 7-8, the "hotspots" terminology is too vague, making it hard for the reader to follow.

Table 1. Note in caption that internal is from CanESM only

Both Tables are difficult to read. Labels in column 2 are too small. The distinction between the scenarios is not clear enough.

Figure 2. correct spelling in words in 2b

pg 11, line 65-66, "test from Santer et al. (2018)" should be defined in methods

pg 11, line 69. Strike "in the models" and replace with "in CanESM"

pg 12, line 77. Figure 2 (and also Figure 4) makes it evident that "model uncertainty" is much the mean spread across the models. Please mention this connection explicitly.

Pg 17, line 80-83. This section is poorly worded. Please rephrase to avoid "are mostly within hotspots but are not confined to them and do not include all of them".. and to be more specific about the regions to which you refer.

Pg 17, line 87-93. This is just the mean sink, i.e. where low anthropogenic carbon is being brought to the surface. These regions continue to be ventilated from the deep and this is why the sink persists. There is no need to invoke teleconnections.

Pg 17, Line 95-96. This description of the scenarios is not sufficiently precise. Scenarios are designed primarily to represent potential futures that socio-economic modeling indicates have potential to be realized. Within this range, there is a selection made of a representative pathways that are not too similar. But this is not the same as to say that they are "designed to deviate". See Riahi et al. 2017
<http://dx.doi.org/10.1016/j.gloenvcha.2016.05.009>

pg 18 line 01-03. please discuss what are these processes.

pg 19, line 26 "fixed inactive regions"? please clarify. These regions are not "fixed" or "inactive"

pg 20, line 41-51. The size of the forced trend is critical in the time of emergence. The scenarios with smaller forced trend emerge later. Please include this in the discussion

Pg 20, line 53 "mostly in a few hotspot regions" suggests a few small spots when in fact the ocean carbon sink is diffuse and occurring really everywhere (see figure 1c). See Major Comment.

Pg 21, line 72. Strike "basins", replace with "regions"

Supplementary

Below Eqn S4. "Section S2", instead of "Appendix B"

Page numbers are needed in the Supplementary