

Earth Syst. Dynam. Discuss., referee comment RC1
<https://doi.org/10.5194/esd-2022-12-RC1>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on esd-2022-12

Anonymous Referee #1

Referee comment on "Contrasting projections of the ENSO-driven CO₂ flux variability in the equatorial Pacific under high-warming scenario" by Pradeebane Vaittinada Ayar et al., Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2022-12-RC1>, 2022

Vaittinada Ayar et al. examined a divergent projection of the ENSO-CO₂ flux relationship between CMIP6 ESMs in the SSP5-8.5 scenario. Half of the ESMs simulate a reversal ENSO-CO₂ flux relationship while the other half ESMs have a consistent ENSO-CO₂ flux relationship from historical to future period. They found the reversal relationship in the first half ESMs is due to a faster-increasing surface DIC, enhanced primary production variability, and pCO₂ increase. The manuscript is well written and well structured. These findings are very interesting and have significant implications for understanding the different CMIP6 model projections and model bias. However, I have some concerns about the missing terms in the analysis of the contribution to the CO₂ flux and ocean pCO₂ variability. I would recommend it for publication in ESD after the concerns and comments are considered by the authors.

General comments

- Since the air-sea CO₂ flux is related to three terms: ocean pCO₂, air pCO₂, and wind-solubility coefficient, the authors only analyze the ocean pCO₂ in the manuscript. I am very interested in the role of wind-solubility coefficient and air pCO₂ in explaining the divergence in two groups of ESMs. Different models might have different wind and temperature variability which might contribute to the CO₂ flux variability. It is worth quantifying and discussing the wind and solubility terms. All the ESMs might use the same air pCO₂, so the air pCO₂ might have a very little contribution. However, it is needed to be at least discussed in the manuscript.
- Ocean pCO₂ is sensitive to four terms: temperature, DIC, alkalinity, and salinity (Takahashi et al., 1993). The authors only discuss the temperature and DIC. Although

the DIC is dominant in the ocean pCO₂ variability, the alkalinity has a very large compensation. The alkalinity might partly contribute to the model divergence. In addition, the precipitation probably changes a lot under global warming (Cai et al., 2015), this might drive a relatively large variability of alkalinity and salinity in the future. It would be convincing if the author could discuss/quantify the contribution of alkalinity and salinity to the model divergence?

- Line 245-247. The authors found two differences between two group of ESMs (Large increase of surface DIC and lower range of DIC changes). Fig.9 could show the large increase in surface DIC. However, I could not see a figure showing the lower range of DIC changes during ENSO phases. I would suggest one such figure in the main text or supporting information.
- Fig. 9. Except for surface DIC difference between preserved and reversed models, I also see the difference in subsurface DIC (e.g., 200-300 m) between two groups of ESMs. What is the role of subsurface DIC difference in the model CO₂ flux-ENSO relationship divergence? Why is the subsurface DIC also different in the two groups of models?

Minor comments:

- Line 41-43. The tropical Pacific ocean CO₂ flux anomaly is not only related to the upwelling strength but also related to the poleward Ekman transport driven by easterly trade wind. One more reference (Liao et al., 2020 GBC) is suggested.
- Line 80. What is the re-grid method?
- Line 91. The ENSO variability is usually an interannual variability ranging from 3 to 7 years. Could the author plot the total CO₂ flux and CO₂ flux anomaly at a sample point to show how well is the detrend method? Could the detrend method remove the decadal variability?
- Line 100. Why do you use 1981-2010 as the climatological period instead of 1985-2014 which is the contemporary period defined in the manuscript.
- 2 Caption. What is the observed data of SST and CO₂ flux? I know the authors state them in the method section. However, it would be clearer for the readers if the authors

could detail them in the caption. For example, SST is JRA.

- 3. Why do the authors use 5N-5S instead of 2N-2S? This is not consistent with the method section.
- What is the CMIP6 ensemble anomalies one standard deviation?
- Line 379. The text reads like Liao et al. (2021) selected the model subjectively and got a partial and biased conclusion. Actually, Liao et al., (2021) use a strict and reasonable constrain method to select the model. The results are physically rational and convincing. I would suggest rephrasing the words. A suggested way would be: "With a strict constrain method based on contemporary observations, the model tends to show a weaker future CO2 flux anomalies during ENSO phases (Liao et al., 2021)."
- Lines 381-382. The increasing Revelle factor and ocean pCO2 sensitivity to temperature would be a general result in my opinion. This point is discussed by many studies. I would rephrase or delete this point.

References:

Cai, W., Borlace, S., Lengaigne, M., van Rensch, P., Collins, M., Vecchi, G., et al. (2014), Increasing frequency of extreme El Niño events due to greenhouse warming, *Nature Climate Change*, 4, 111.

Liao, E., Resplandy, L., Liu, J., and Bowman, K. W. (2020), Amplification of the Ocean Carbon Sink During El Niños: Role of Poleward Ekman Transport and Influence on Atmospheric CO2, *Global Biogeochemical Cycles*, 34(9), e2020GB006574.

Takahashi, T., Olafsson, J., Goddard, J. G., Chipman, D. W., and Sutherland, S. C. (1993), Seasonal variation of CO2 and nutrients in the high-latitude surface oceans: A comparative study, *Global Biogeochemical Cycles*, 7(4), 843-878.