EGUsphere, referee comment RC2
https://doi.org/10.5194/egusphere-2022-1067-RC2, 2022


# Comment on egusphere-2022-1067

Anonymous Referee #2

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Referee comment on "Seasonal to interannual variabilities of sea–air $CO_2$ exchange across Tropical Maritime Continent indicated by eddy–permitting coupled OGCM experiment" by Faisal Amri et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1067-RC2, 2022

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Amri et al. examined pCO2 and air-sea CO2 fluxes variability over the Tropical Maritime Continent (TMC) using a regional ocean biogeochemical (BGC) model. Surface pCO2 patterns across the TMC have not been well constrained, so this study represents a valuable effort to better understand carbon system dynamics in the region. However, I have three major concerns about the model results and analysis:

1) It is not clear to me whether the model is getting realistic pCO2 patterns or not. The comparison with Bakker et al. (2016), Iida et al. (2022), and LandschuÌ□tzer et al. (2016) suggests a significant overestimation of surface pCO2, especially in the open ocean region. I wonder to what degree the initial and boundary conditions for the BGC model, derived through an analytical (regression models) approach, were properly resolved. Since the authors do not provide a model validation, neither physic or biogeochemistry –putting aside pCO2 and CO2 fluxes–, it is difficult be confident in their results. I think this study requires a proper model validation, which should include model-data comparisons for horizontal and vertical patterns of temperature, salinity, nutrients, and carbon system variables when available.

2) The authors claim that changes in sDIC and sAlk represent biological processes, which is not correct. DIC and alkalinity also can change due to advection and mixing, and air-sea flux in the case of DIC. This wrong assumption led to a wrong interpretation for the Taylor decomposition analysis. The authors need to revise that interpretation, making clear that process like wind-driven upwelling of DIC-rich subsurface water could play an important role in the pCO2 variability off Java.

3) The analysis of the interannual pCO2 variability is interesting, but the link to ENSO and IOD need a better explanation. If the patterns are properly described, this could be the most interesting part of the study. Please provide a better description. One thing that catch my attention was the negative trend in the CO2 flux. The authors did not offer any explanation for this trend. I wonder whether this a model pCO2 drift or not.

**Specific comments.**

69: I would rather use the name "regional ocean biogeochemical model" instead of OGCM.

90: I would indicate that "coccolithophores decrease alkalinity, as they produce a body shelf structure made of $CaCO_3$"

122-125: I do not understand why you are indicating this Taylor series decomposition here. Need to explain the motivation.

157: I wonder how your estimated fields for the biogeochemical (BGC) variables compare with the WOA2019 ($NO_3$, $PO_4$, $O_2$). Also, I wonder if you made any comparison between your BGC estimates and BGC fields from reanalysis products (e.g., GLORYS Mercator Ocean).

Table 2. Alkalinity usually co-varies with salinity. I wonder why you left alkalinity as a function of temperature instead of salinity.

188: It would be helpful to show similar map to Fig. 2 ($\partial pCO_2$) in Kartadikaria et al (2015). Most likely your model is overestimating $pCO_2$ in the open ocean region.

188: that higher => that were higher

190-197: There is a significant bias in surface $pCO_2$, especially in the open ocean region surrounding the TMC. This likely explains the much greater carbon outgassing you obtained compared to previous studies.

220: I wonder what you consider strong $CO_2$ outgassing. Maybe you could refer to the region(s) with the strongest $CO_2$ outgassing.

236: "The biological processes, represented by SSDIC and SSAlk" This is a wrong statement. Changes in sDIC and sAlk are also affected by advection and mixing, and air-sea flux in the case of sDIC. Besides, I would not expect important biology-driven changes in sAlk.

238-239: This is a wrong conclusion based in the wrong assumption that changes in sDIC and sAlk represent biological processes. Consider the upwelling season off Java during summer. sDIC promotes an increase (and sAlk a decrease) in pCO2. Which biological process could explain this? It is not respiration. Most likely, the signature is associated with the upwelling of subsurface waters with higher DIC and alkalinity concentration than the surface waters. During fall, you have a negative impact of sDIC on pCO2, which could reflect a weakening in coastal upwelling. Remember that in Fig. 5 you are visualizing dpCO2 not pCO2. I would expect a maximum biological uptake of DIC around September. This uptake contributes to decrease sDIC, so its impact should be opposed to the DIC-rich subsurface waters due to upwelling.

Second comment on 238-239, after reading discussion: You mentioned "supply of subsurface inorganic" as a factor impacting pCO2 off Java in the Discussion section (line 339), so I wonder why you did not mention anything of that in the Result section.

268: I wonder why the long-tern negative trend in the fluxes. What does it drive this trend? It may be a model flux drift. Need explanation. Specially if you are highlighting that ENSO and IOD contributed to attenuate this trend.

Figure 6b: It is hard to discriminate the color of the lines. Please increase line width.

270: Why do you think it confirms? You are not stating any mechanisms linking the ENSO or IOD variability.

285: Why are you using standard deviation instead of the mean index value? I got lost.

Figure 8b. I wonder why you did not use a smaller colorbar interval for the flux anomalies.

339: "accelerated gas exchange and an abundant supply of subsurface inorganic". You should try to mention these two processes when describing Figs. 5 & 6 in the Result section.

350-358: It is not clear to me how the anomalous divergence in the West Pacific affects the air-sea CO2 flux. Could you develop more this idea? I think you need to explain better the counteracting effect of this divergence/convergence with the increased/decreased solar heating during El Nino/La Nina.