



EGUsphere, referee comment RC3
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Comment on egusphere-2022-387

Anonymous Referee #3

Referee comment on "A modeling framework to understand historical and projected ocean climate change in large coupled ensembles" by Yona Silvy et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-387-RC3>, 2022

General comments:

This manuscript presents a modeling framework for investigating the effect of individual surface flux perturbations on transient ocean climate change, specifically focused on temperature and salinity trends. The modeling framework is implemented for the IPSL-CM6A-LR model such that the ocean component is forced with perturbations extracted from the historical+ssp245 ensemble of coupled model simulations. Overall, the structure and writing of the manuscript are clear, and most of the set-up is well validated. However, as discussed by previous reviewers, the scientific question that the new modeling framework seeks to address needs to be made more explicit. Choices for the framework set-up should be justified more clearly in the context of the scientific questions. Finally, the section on passive tracers should include validation plots, and passive anomaly salinity needs to be more thoroughly described. This manuscript could be publishable after revisions.

Specific comments:

Line 65: What is scientifically added by using fixed fluxes from sea ice rather than coupling the sea ice model? Is this the same justification for not coupling to the atmosphere - i.e. to prevent retroactions that may change the phase of the internal variability?

Line 94: Include validation on going from 32 members to 11 members after 2060, such as a plot showing mean and variability of forcings. As reviewer 2 asked, how many members are necessary to obtain the forced surface flux perturbation?

Section 3.5.2/Fig 4: Here, validation of the method for prescribing the chlorophyll field is by comparing global SST for each option. Does temperature in the vertical agree?

Line 250: It would be helpful to have more details here on why the unconstrained case is better for the scientific questions.

Fig 7d shows that for most depths below 1000-2000m, the zonal mean salinity difference is larger than 2 times interannual standard deviation of piControl. More justification on why this still validates the CTL experiment for the intended use would be helpful.

Line 361: Some mismatch between ALL and the fully coupled ensemble could also be due to eliminating atmosphere-ocean feedbacks. If the goal for the ALL experiment is to simulate a climate consistent with the coupled model ensemble, is this a limitation of this framework?

Fig 9c and 9d: Visually, the freshwater fluxes for this study appear to be stronger than the FAFMIP anomalies especially in the Arctic. Is this due to a stronger model response than the CMIP5 ensemble mean or due to other differences in experimental set-up?

Line 423-425: There are also some significant differences in salinity in the Southern Ocean between ALL and the coupled model (Fig 11f and l)

Line 428: This manuscript just shows results from ALL. It is stated that comparing HEAT+WATER+STRESS with ALL is in the companion paper, but it would be helpful to show it here.

Line 436: Which studies were compared to?

Sec 5: Validation plots for passive tracers needed.

Line 495 to 500: Equations equivalent to Equations 6 and 7 for salt and PAS would help for understanding the additional complexities here.

Line 505: Similar to for PAT, is there PAS in all experiments which experiences F? If so, make this a bit clearer.

Technical corrections:

Line 21: "associated with" rather than "associated to"

Line 125: "bears similarities" rather than "bares similarities"

Line 266: capitalize "deacon"

Line 293: says stipules represents less than piControl interannual variability in Fig 7 but the figure caption says 2 times interannual standard deviation

Line 610: "worse" rather than "worst"

Line 628: "constraints" rather than "constrains"