Ocean Science Discussions

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Comment on os-2021-87

Anonymous Referee #2

Referee comment on "Tracer and observationally derived constraints on diapycnal diffusivities in an ocean state estimate" by David S. Trossman et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-87-RC2, 2021

Comments on "Tracer and observationally-derived constraints on diapycnal diffusivities in an ocean state estimate" by Trossman et al.

In this study, the authors used the ECCO framework to explore a few ways to better constrain the estimates of diapycnal diffusivity. Based on a set of sensitivity analyses, they investigated the impacts of including diapycnal diffusivity estimates that are obtained from microstructure measurements or inferred from CTD measurements, as well as dissolved oxygen measurements. They concluded that both ways could improve the presentation of diapycnal mixing in ECCO. I think this is potentially a very important paper, and I really appreciate the authors put tremendous efforts to address this interesting yet difficult question. The paper should eventually be published. However, before I recommend acceptance, I would like the authors to clarify a few questions and concerns I list below.

My major concern is the definition of ECCO diapycnal diffusivity used in this study. From what I know about ECCO v4, there are many components involved in the calculation of vertical fluxes as well as diapycnal diffusivity. The diapycnal diffusivity at least consists of three parts: the background diffusivity, which was adjusted through the adjoint process; the parameterized part based on Gaspar et al. (1990); and convective adjustment. In this paper, the authors briefly described some of those terms. But it is still not clear to me what the exact definition of the diapycnal diffusivity the authors analyzed is. The combination of all or some of the components mentioned above? or just the adjusted background diffusivity? This information is critical for the interpretation of almost all the results presented in this study. And the authors should make that information more explicitly presented.

It is good that the authors reminded the readers a couple of times through the text that the "observed" diapycnal diffusivities based on either microstructure measurements or fine-scale parameterization with CTD measurements include uncertainties as well.

Detailed comments:

Lines: 3-5: The authors concluded that “the assimilation of existing in situ temperature, salinity, and pressure observations is not sufficient to constrain κ_ρ estimated with ECCO”. However, since the number of iterations or the adjoint runs in ECCO is limited, is it possible that by running more iterations, the ECCO diffusivities will be better adjusted to the truth, even without including other new datasets? From what has been presented in this paper, I don’t believe that possibility has been ruled out, and therefore the statement above is not solid, IMO.

Line 14: I did not find how this conclusion was reached.

Line 141: The background κ_ρ is time-invariant. But if the parameterized part and/or convective adjustment were included, it should be time-variant.

Line 152: What do “14-day adjustments” mean? Could the authors explain that?

Lines 156-158: This information is important but confusing. Could the authors elaborate on how the ECCO κ_ρ used in this study was obtained? Is it based on parameterization (Gaspar et al., 1990)? or is it adjusted through the adjoint? or other ways?

Lines 174-176: It seems that the authors were likely asked by other reviewers to add the appendix. I personally think it is not necessary. But I am OK if the authors choose to keep it.

Line 200: It would be helpful if the authors could explicitly state the differences between the two runs E_κ and E_ε.

Lines 214-223: I am confused here. Why does using a previously derived product as an initial condition minimize model drifts? Also, how did the authors conclude that using other products as initial conditions would be worse than using that one?

Lines 234-237: Since uncertainties associated with undersampling (spatially and temporally) were not considered, the prescribed uncertainties are likely lower bounds. If so, the readers should be reminded about this.

Lines 296-299: As commented above, is it possible that with more iterations $\kappa_{\rho,ECCO}$ will be closer to the observationally-derived κ_ρ?

Lines 356-363: Now I understand the purpose of the run $E\varepsilon$. It would help the readers if some of the information here were added around line 200.

Line 389: What is the amount of the available DO measurements? Is it comparable to or much larger than T/S data? If not, not sure it will be that useful.

Line 420: "microstructure CTD-derived κ_ρ"? Do you mean CTD-derived κ_ρ?

Lines 345-437: The last sentence is disconnected from the sentences before it.

Figure 4: since there are many data gaps (white area) in the right panels, it might make sense to use a different colorbar.