Dear editor,

We are resubmitting the manuscript, "Tracer and observationally-derived constraints on diapycnal diffusivities in an ocean state estimate," to *Ocean Science*. We have changed some of the text to clarify how we separate the background diapycnal diffusivity (now denoted by $\kappa_{\rho,bg}$), Gaspar et al. (1990) scheme, and (3,3) entry of the along-isopycnal tensor. We also note that convective adjustment does not act through a diapycnal diffusivity in the MITgcm. We address each of the reviewer's points below.

1 Reviewer #2 (Comments to Author (shown to authors):

- In this round of revision, the authors added more materials and rewrote some of the text. Those efforts clearly improved the readability of this difficult paper. The authors also tried to address the few questions I had about the previous version of the paper. I appreciate their hard work and effort. However, I am confused about their reply to one of my major concerns that if the "ECCO diapycnal diffusivity" the authors used is actually comparable to the directly observed or inferred diapycnal diffusivity. I consider this a key issue that should be examined and clarified before I recommend acceptance.

- **Thanks for your additional feedback.**

- In their reply and new text, the authors added "Vertical mixing–diapycnal plus the vertical component of the along-isopycnal tensor–is determined according to the Gaspar et al. (1990) mixed layer turbulence closure, simple convective adjustment, and estimated background κ_ρ for internal wave-induced mixing." Does this mean the vertical component of the Redi parameter is affected by the other processes, like the convective adjustment? I don't think that is the case.

- **Each component of the along-isopycnal diffusivity tensor is time-invariant, as is the background diapycnal diffusivity. However, the Gaspar et al. (1990) mixed layer turbulence closure is not time-invariant. While simple convective adjustment is not time-invariant either, convective adjustment does not act through a diapycnal diffusivity in the MITgcm. Thus, the vertical diffusivity is affected by the (3,3) entry of the along-isopycnal tensor, but not the other way around (other than indirectly due to how the Gaspar et al. (1990) scheme affects the ocean state estimate). We have edited the above quote to read: "Vertical mixing is the sum of diapycnal mixing and the vertical component of the along-isopycnal tensor, where diapycnal mixing is determined according to the *Gaspar et al.* (1990) mixed layer turbulence closure and estimated $\kappa_{\rho,bg}$. Convective adjustment does not act through κ_ρ in the MITgcm. Here, κ_ρ represents a combination of processes, including–but potentially not limited to–internal wave-induced mixing. $\kappa_{\rho,bg}$, the Redi coefficient, and the Gent-McWilliams coefficient are time-independent..."**

- The authors also mentioned that ".. to transform this value into a diapycnal diffusivity that's equivalent to the observational product values, we need to subtract out the (3,3) entry of the along-isopycnal diffusivity tensor, which is not perpendicular to the isopycnal contours but parallel to them. After subtracting this from the vertical diffusivity, we are left with the diapycnal diffusivity. As for what this coefficient physically represents, it is the adjusted background diffusivity via the adjoint estimation process." Unless I misunderstood these sentences, subtracting the vertical component of the redi parameter from the vertical diffusivity won't give us the background diapycnal diffusivity κ_ρ, but a term including background diapycnal diffusivity, parameterized mixing based on Gaspar et al. (1990) and convective adjustment.

- **This quote was from a previous version of the manuscript. The vertical diffusivity includes the (3,3) entry of the along-isopycnal tensor, the parameterized mixing based on Gaspar et al. (1990), and background diapycnal diffusivity. Thus, we need to subtract the (3,3) entry of the along-isopycnal tensor to get the full diapycnal diffusivity. We explain why we said that what's left physically represents the adjusted background diffusivity below, but you're right that we should rephrase the sentence in the quote, which is what we did with the version you just read.**

- My understanding is that in this study the authors just compared the directly available background diapycnal diffusivity κ_ρ with the observed or inferred diapycnal diffusivity. I don't think they are directly comparable. A more sensible choice would be the full diapycnal diffusivity including the other parts rather than just the background diapycnal diffusivity. I understand that a lot of diffusivity terms used in this paper and it is possible that the confusion is simply a presentation issue. If that is the case, this should be easily addressed by rewriting a few sentences. If that is not the case, this will be a critical issue and require a lot of extra work.

- **We used the full diapycnal diffusivity but in the regions where we're comparing the simulated diapycnal diffusivities from observations, the full diapycnal diffusivity field is approximately the same as the background diapycnal diffusivity field. Microstructure-inferred diapycnal diffusivities include the full sum of process, but are not reliably measured near the boundaries. On the other hand, Argo/CTD-derived diapycnal diffusivities are only valid away from the surface because they represent the internal wave-related background mixing. The *de Lavergne et al.* (2020) product represents internal tide-related background mixing. Note that the regions with the highest level of agreement between the adjoint sensitivities from J_O and J_κ are away from the boundaries. This is either because there are no observations near the boundaries, as is the case with microstructure and Argo, or because there are other factors not included in the observational product impacting the diapycnal diffusivities near the boundaries, as is the case with the *de Lavergne et al.* (2020) product. Thus, the background diapycnal diffusivity is an appropriate description for the model parameter we compare with each of the observational products away from the surface, but we now clarify that we use the full diapycnal diffusivity field. We have edited the text to read: "The resulting $\kappa_{\rho,bg}$ field in the ECCOv4r3 solution–plus the *Gaspar et al.* (1990) contribution–will be referred to as $\kappa_{\rho,ECCO}$ hereafter and is shown in Fig. 2–depth-averaged below the model's average mixed layer**

depth. Note that the initial guess for $\kappa_{\rho,bg}$ in ECCO is 10^{-5} m^2 s^{-1} and in the absence of observation-driven adjustments, $\kappa_{\rho,bg}$ in ECCO remains at or is close to its initial value in the ECCOv4r3 solution, at least in its depth-average. Also note that in regions away from ocean boundary layers, $\kappa_{\rho,ECCO}$ is approximately the $\kappa_{\rho,bg}$ in ECCO."

- In addition, it would be very helpful if the authors can provide references or documents indicating diffKr is indeed a combination of diapycnal diffusivity and the vertical component of the redi mixing rate. This is kind of technical but essential for the whole story.

- **The variable diffKr is just the background diapycnal diffusivity but the sensitivities we used for this study are for the full diapcynal diffusivity (background plus Gaspar et al. (1990)), not including the (3,3) entry of the along-isopycnal tensor. The *Forget et al.* (2015) study in Geoscientific Model Development we cite at least partially describes this (hence their notation with perpendicular et cetera).**