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# *Interactive comment on* "Exploring the isopycnal mixing and helium-heat paradoxes in a suite of Earth System Models" *by* A. Gnanadesikan et al.

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We appreciate the Dr. Reckenger's support for this paper and understanding about what we are trying to do.

*One general comment I have regards the mismatch in depth between the observations used to motivate the sensitivity study and the region that has the greatest effect on mantle helium transport. The isopycnal paradox regards a comparison of observations for along-isopycnal eddy diffusivity largely near the surface to the values used in the ocean interior. In addition to a set of simulations using constant diffusivities, one simulation uses a diffusivity distribution from Abernathy and Marshall (2013), which only calculates $A_{Redi}$ near the surface. It is valid that $A_{Redi}$ is not necessarily equal to $A_{GM}$, and that $A_{Redi}$ is likely greater than $A_{GM}$, but the lack of estimates away from the sur-*



*face makes it impossible to have an expected best value for diffusivity in the ocean interior.*

We agree that the lack of vertical structure is a problem, but one which we feel must be tackled in future studies. We believe that this currently covered in the second paragraph on p. 2541, but will add the word "observational" before "studies" on line 9 as well as the following sentence

"However, if the surface values of $A_{Redi}$ are too low, forcing $A_{Redi}$ to decrease with depth may actually give *less* realistic deep values than the vast majority of parameterizations which simply prescribe values of 500-1000 $m^2s^{-1}$ uniformly throughout the water column. Evaluating the extent to which this is the case is a major thrust of this paper."

: Response to major points

1.*Page 2536, line 20: It is stated that, based on various other studies, the mesoscale eddy diffusion processes have a strongly anisotropic nature. It would be beneficial to briefly mention whether the measurements of eddy diffusivity are able to pick up the entire anisotropic diffusion tensor? Are the various measurements of an isotropic diffusivity measuring the same aspects of the anisotropic tensor (minor diffusivity, major diffusivity, or an arbitrary combination)? How well do the studies that measure zonal vs. meridional diffusivities represent the full anisotropy (minor diffusivity vs. major diffusivity), that is how accurate is the assumed orientation of east-west vs. north-south?*

We will add the following comment

"It is not clear, however, how well the observational estimates capture the details of anisotropy, given that a particular challenge in making such measurements is removing imperfectly known background shear flows (see for example LaCasce et al., 2014, who resolved this issue by looking at the cross-stream diffusivity only). Such shear flows may also cause tilts in the major and minor axes of the dispersion tensor that reflect resolved flows rather than details of the turbulence. Employing higher subgridscale diffusivities in the alongstream direction in models where one already models the large scale flow may thus count the impact of the shear dispersion twice. "

2. *Page 2544, line 25: It would be nice to see how the model responds to the adjustment to a new diffusivity, as it asymptotes to a new equilibrium. Is it possible to include a figure to show how effective the spin-up process is? For example, one could plot the evolution of a scalar metric, such as global mean temperature.*

This is a useful point, also echoed by the other reviewer. We are currently in the process of extending these runs out to 1000 years. This will require close to a month. At this point the runs will be comparable to the spinups done for the majority of operational climate models (almost none of which are actually spun up from observations to equilibrium). We believe that by showing that our key results are robust at this time scale and by adding the following language we will be able to make the case that the results are still useful.

Spinup is an issue with coupled models. As described in Pradal and Gnanadesikan (2014) the surface temperatures are close to equilibrium after a few hundred years. This may not be the case for the deep ocean however, as the time scale for equilibrating radiocarbon can be many thousands of years, far longer than the spinups used in the majority of coupled climate models (which are generally from a few hundred to a few thousand years). . Caution should thus be used in interpreting mean results. Since the models are initialized from data, changes which enhance errors are likely to be significant, while it is possible that changes in diffusion which seem to reduce errors may in fact produce an overshoot in the opposite direction after many thousands of years. The key metrics which we use here, in particular the *relationships* between helium and radiocarbon and the sharpness of the plumes adjust much more rapidly, with time scale of about 100 years.



*3.Page 2547, line 19 Page 2543, line 9: Can you briefly explain why increasing ARedi has a destabilizing (destratifying) effect?*

We will add the following sentences

"Models with more intense eddy mixing stir more salt into the high latitude surface layer. This reduces the salinity contrast the across winter halocline, which is the dominant factor in determining vertical exchange in the subpolar North Pacific and Southern Ocean."

*4. Page 2552: In addition to anisotropy, the conclusions highlight an important expectation for improved parameterization development: flow-awareness through horizontal and vertical variability of eddy diffusivities. Do you expect any significant effects from the relative coarse resolution of the simulations? Is there potential in extending the simulations to a 1 model with greater vertical resolution?*

This is a great question, though one to which we'd be hesitant to speculate too much in this paper. Gnanadesikan's experience has been that the differences in changing constant coefficients across horizontal resolutions ranging from 3 to 1 degree are pretty similar. But this may not be the case when one starts looking at length scale suppression, which may require actually resolving high velocities in the boundary currents. In terms of vertical resolution, we don't expect a huge impact, qualitatively the impact of lateral diffusion on low-latitude oxygen and high-latitude temperatures is pretty similar to what we see in the contrast between CM2.0 and CM2.1, with about twice the vertical resolution.

Minor comments:

Thanks for the suggestions, which will be fixed in the final version.

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Interactive comment on Ocean Sci. Discuss., 11, 2533, 2014.