Ocean Sci. Discuss., 11, C1215–C1218, 2014
www.ocean-sci-discuss.net/11/C1215/2014/

Ocean Science
Discussions

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

S. Reckinger (Referee)

Scott_Reckinger@Brown.edu

Received and published: 23 December 2014

The paper "Exploring the isopycnal mixing and helium-heat paradoxes in a suite of Earth System Models" by A. Gnanadesikan, R.P. Abernathey, and M.A. Pradal attempts to resolve a helium-heat paradox, where estimates of mantle helium are much lower than needed to balance the observed geothermal heat flux, by imposing higher diffusivities in the ocean interior, which fixes the isopycnal paradox. Whereas the paradoxes are not resolved from this study, important steps are made toward a better understanding of the proper scaling of mesoscale eddy tracer diffusivities used in coarse-resolution ocean models.

This study is more than just "another possible resolution" (quoted from page 2541, line 22) to the paradoxes, since anisotropy is complimentary to an accurate background dif-

fusivity. The theoretical scalings for the effects of (anisotropic) transport mechanisms typically rely on an enhancement or suppression of a background turbulent eddy diffusivity. Thus, studies such as this are necessary, regardless of the success of the inclusion of anisotropy in the tracer transport within ocean models. That is, while anisotropy may alleviate some of issues addressed in the paper, the complete resolution to the paradoxes must include a better handle on the magnitude and spatial variation of the "isotropic" eddy diffusivity.

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

1 Specific Comments

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?

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.

Page 2547, line 19 Page 2543, line 9: Can you briefly explain why increasing A_{Redi} has a destabilizing (destratifying) effect?

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?

2 Technical Corrections

Page 2536, line 4: . . . eddy kinetic "energy" . . .

Page 2538, line 18: . . . fluxes "of" mantle ...

Page 2538, line 26: I am either misinterpreting the figure or the statement is incorrect. Should it be the lowest levels of radiocarbon in the "North Pacific" OR the "highest" levels of radiocarbon in the North Atlantic?

Page 2541, line 20: reference should be S. J. Reckinger, et. al. 2015

Page 2549 line 5: erase "the" in "... between the where He is. . ."

Figure 1: please define $\sigma_2 = 36.95$.

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