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Interactive comment on "Microstructure measurements and estimates of entrainment in the Denmark Strait overflow plume" *by* V. Paka et al.

V. Paka et al.

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First of all we would like to thank the reviewer for profound, highly professional comments.

We have responded to the reviewer's criticism/suggestions as follows. (For convenience, quotation of the review is hereafter given in double angle brackets, « . . . »).

« The most serious problem I see is the application of the Shih et al. (2005) recipe for the flux Richardson number (or the misnamed "mixing efficiency, usually denoted as Gamma). Applying the Shih recipe to energetic geophysical flows is either highly questionable or dead wrong, depending on how conservatively one wants to express oneself. The reason that Shih et al. does not (generally) apply to geophysical flows

is that real flows are far more complex and can "do" things that much simpler, highlyconstrained and more or less artificial lab and numerical flows can't. All the high "turbulence activity", eps / (nu NËĘ2), data in Shih et al. come from unsteady, growing turbulence. (In this comment, eps denotes the dissipation rate. I am also denoting eps/(nu NËĘ2) as Re_b.) In my interpretation, the reason for comparatively small Gamma at lag Re_b is that the turbulence preferentially funnels energy available from shear production into growth of TKE rather into the buoyancy flux. But what if the is quasi steady state at very large Re_b? In contrast to Shih et al., geophysical flows typically, or at least often, merrily flow along at very high Re_b without significant growth of the turbulence. The best documented case that I am aware of is the tidally driven, stratified, highly energetic turbulence in the Hudson River (Peters and Bokhorst, 2000, 2001, JPO). During spring ebbs, the flow has Richardson numbers, Ri, as low as 1/10 and very large Re_b without systematic growth in the turbulence. The preceding does not imply that Gamma, or the flux Richardson number Rf, is constant. But it does put three bold case question marks behind the use of the Shih et al. recipe. Rf canNOT be constant for reasons more basic than what is addressed by Shih et al. A simple look at the steady state TKE equation shows that Rf –> 0 as NËĘ2 –>0. Rf has to be a function of Ri. (And Shih et al. know that). Peters and Bokhorst (2001) show that making Rf linearly dependent on Ri for small Ri Âń 1/4 has at least qualitative merit. With Rf=const (Gamma=const.), the vertical turbulent salt flux estimated from eps increases toward the bottom - which cannot be right as the flux is 0 at the bottom. With Rf dependent on Ri, the salt flux decreases toward the bottom, as it should. Maybe Paka et al. should try out Peters and Bokhorst's simple little recipe for Gamma. They might still find much smaller buoyancy fluxes than with Gamma=const because the Denmark Strait flow shows large regions of low Ri<1/4. »

We agree with the Reviewer in that the approach of Peters and Bokhorst (2001) to parameterize eddy diffusivity taking Gamma a function of Ri based on steady state TKE balance is a better choice for ocean outflows than the parameterization by Shih et al. (2005) corresponding to growing turbulence. Unfortunately, we cannot apply Peters and Bokhorst's recipe because we do not possess simultaneous vertical profiles of Ri and eps: the LADCP/CTDO casts used to estimate Ri were taken approximately half an hour ahead of the MSP casts. In such circumstances, it seems worthwhile to calculate Krho both from Osborn (1980) formula Krho=0.2 eps / Nˆ2, and from Shih et al. (2005); the former is used for the upper estimate of eddy diffusivity and the latter for the lower estimate. We added a discussion on the issue to the revised MS.

« (By the way, Osborn (1980) caNOT be held responsible for Gamma = const = 0.2. His statement is Gamma <= 0.2! Inequalities being inconvenient for actual computations, folks (including this reviewer) have subsequently oversimplified Osborn.) »

We use formula Krho=0.2 eps / Nˆ2 to estimate the upper limit of eddy diffusivity, just in accordance with the above remark.

« Something else. I see no reason why actual geophysical flows should adhere to supposed "accepted" values of the drag coefficient somewhere around 0.003. In the, to my knowledge still only direct measurements of the Reynolds stress in an overflow (at least a deep one), Peters and Johns (2006 with correction 2007, JPO) found c_d as large as 0.008-0.009 at one location. Real flows are complex and harbor a range of processes that may defy acceptability. »

We added to the revised MS a mention of such a large estimate of c_d in the Red Sea Outflow.

« The comparison of Paka et al.'s dissipation-based c_d with that of Girton and Sanford is meaningless. This throws apples and oranges together in one pot. »

Girton and Sanford (2003) reported on a log-velocity-profile-based c_d estimate in DSO plume at approx. 0.003, while our dissipation-based c_d estimate was found to be approx. one third of that. Why not to discuss, following Johnson et al. (1994), why the latter estimate is considerably smaller that the former one? We do not think this is meaningless. To our mind, estimates of the same physical quantity (i.e. c_d) obtained

with different approaches, in contrast to apples and oranges, should be compared.

« Something else, yet. There is another reason why bulk entrainment rates can be much bigger than local values derived from turbulence measurements. Nash et al. (2012, GRL) have pointed at the importance of smallscale features in the bottom topography. Choke points for outflows can harbor turbulence orders of magnitude more intense than elsewhere; the turbulence mixing and entrainment can be concentrated in hotspots - as they are in the Mediterranean Outflow. »

We fully agree with this remark and take a liberty to reproduce it the revised MS with a reference to the Reviewer.

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