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Discussions

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

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Review of Paka et al., "Microstructure measurements and estimates of entrainment in the Denmark Strait overflow plume"

The authors describe and analyze CTD / LADCP / microstructure measurements from the Denmark Strait overflow. The microstructure measurements are based on an innovative and very clever approach. The profiler rides on the CTD/LADCP/sampler frame and is released only at a distance above the bottom where the length of cable paid out can be short enough to prevent cable drag from ruining the shear probe data.

Overall, I find the paper highly interesting and a good contribution to topics relevant to large parts of oceanography. Excellent work! - But, of course, I have some quibbles.

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, highly constrained and more or less artificial lab and numerical flows can't. All the high "turbulence activity", eps / (nu Nˆ2), data in Shih et al. come from unsteady, growing turbulence.

(In this comment, eps denotes the dissipation rate. I am also denoting eps/(nu Nˆ2) 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 Nˆ2 –>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 « 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.

(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.)

The Hudson river data are further interesting in that they have been used for a highly idiosyncratic form of turbulence closure modeling by Peters and Baumert (2007, Ocean Modelling). The spring ebb state of the flow, small Ri but no growth of the turbulence, is indeed consistent with simple turbulence models. In Peters and Baumert, the simulated turbulence stays close to a production-dissipation-buoyancy flux balance at all times with small, but equally as important time derivative and vertical diffusion terms in the TKE balance.

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.

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.

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.

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Interactive comment on Ocean Sci. Discuss., 10, 1067, 2013.