Ocean Sci. Discuss., 9, C817–C819, 2012
www.ocean-sci-discuss.net/9/C817/2012/

Ocean Science Discussions

Interactive comment on "Microstructure observations during the spring 2011 STRATIPHYT-II cruise in the Northeast Atlantic" *by* E. Jurado et al.

K. Richards (Referee)

rkelvin@hawaii.edu

Received and published: 1 August 2012

This is a second manuscript on results from the STRATIPYT cruises. In general the manuscript is well written and presents the results clearly. Even though there are issues with regard the limited number of turbulence profiles at each station and the measurements are restricted to around midday, the large meridional range of the data, sampling different conditions in spring time is a nice complimented to the published results from STRATIPYT-I which was conducted in the summer. I have a major issue with the section relating the turbulence properties to the atmospheric forcing, and therefore suggest the authors undertake a major revision before the manuscript is suitable for

publication.

The authors try to relate K_T and epsilon to the wind. They find very different fits for the summer and spring cruises suggesting the scaling they are applying is not correct. Putting the differences down to such things as the "memory effect of the previous winter" is not a valid reason since the turbulence very quickly responds to changes in the surface forcing (see e.g. Brainerd and Gregg, DSR I, 1995). I am surprised that the expression used to relate the mixed layer averaged K_T to the wind is independent of the depth of the mixed layer whereas the expectation is that the diffusivity is directly proportional to MLD - see for instance eqn (10) of Large et al (Reviews Geophys. 1994). The authors should see if the Large et al scaling helps collapse the data better.

The authors should note that the expression (5) relating epsilon to u* and z is valid for z<LMO, as suggested by Fig 10a where C_s is close previous estimates, i.e. O(1) when MLD/LMO is relatively small. I am not totally familiar with the literature so I am sure what to expect when MLD/LMO»1. The authors should try and find published results for this case. The authors also could try fitting the epsilon profiles for z<L, rather than the full profile to see if C_s is less variable.

Again, the differences between the summer and spring results (where C_s is found to be an order of magnitude smaller for the former) cannot be ascribed to the turbulence the previous winter. Turbulence does not linger. The authors could try varying the depth of the upper cutoff (at present set to 5m) which is used to try to eliminate wave affected turbulence. I note that Lozovatsky et al (JGR 205) use 15m. You could varying the cutoff depth.

Additional point:

Section 4.1, Fig 5. The authors present the Turner angle to show regions susceptible to double diffusion, but do nothing wit the information. Care is needed in the interpretation as shear induced mixing very readily destroys the dd structures. They could in principle compare the implied dd diffusion coefficient with that they estimate from turbulence measurements (see section 4 of Large et al for references), but it is probably best to delete the section on dd.

Kelvin Richards

Interactive comment on Ocean Sci. Discuss., 9, 2153, 2012.