Invited review of “Turbulent mixing in the seasonally-stratified western Irish Sea: a Thorpe Scale perspective” by K. L. Stansfield, M. R. Palmer, T. R. Rippeth and J. H. Simpson.

The title of this paper suggests new information about turbulent mixing in a specific location, the western Irish Sea, during the summer season when mixing is restricted by stratification. However the major focus is actually on comparisons of direct measurements of kinetic energy dissipation rates from microprofiler shear probe measurements (ε) with estimates derived indirectly from measurements of Thorpe scale (L_T) using

$$\varepsilon_T = (0.8L_T)^2 N^3 \ , \qquad (1)$$

derived from a relationship

$$L_O = 0.8L_T \qquad (2)$$

between Thorpe and Ozmidov (L_O) scales originally suggested by Dillon (1982). Two Thorpe scales are computed, one from temperature measurements on the same microprofiler that carried the shear probes, the other from standard CTD casts. Adding measurements of vertical shear from a shipborne ADCP, the ε data set is used to show that TKE dissipation rate increases with decreasing stratification over the entire range of shear values measured. As outlined below, this paper provides neither convincing comparisons between ε and ε_T, nor significant information useful for furthering understanding of turbulence in coastal waters.

1. A major problem is that there is considerable disagreement, not mentioned, about the universality of equation (2) above. LES of turbulence generated by Kelvin-Helmholtz (K-H) instability (Smyth et al. 2001), where N evolves with time as a consequence of mixing, show that L_O / L_T decreases almost monotonically with time, i.e with the “age” of K-H turbulence. More recently, Mater et al. (2013) used direct numerical simulation of stably stratified turbulence to compare L_T to various other scaling lengths, including the Ozmidov scale L_O. They found that relationships are a function of a buoyancy strength parameter NT_L, where N is (constant) buoyancy frequency and $T_L = k/\varepsilon$ is a decay time scale of turbulent kinetic energy k. When buoyancy effects are dominant (NT_L >1), L_T is found to be linearly correlated with a scale $L_{kN} \equiv (k/N)^{1/2}$, rather than with L_O. Quoting from their abstract, “ . . . observable overturns in strongly stratified flows are more reflective of k than of ε. In the context of oceanic observations, this implies that inference of k, rather than ε, from measurements of L_T is fundamentally correct when $NT_L \approx 1$ and most appropriate when $NT_L > 1$.” Mater et al. also show that relationships similar to those found in their DNS are also seen in the towed grid turbulence measurements of Itsweire et al. (1986). Lacking an independent means of determining T_L from the data, uncertainty in the relationship (2) above may be a major contributor to any disagreement

between ε and ε_T, whether the latter is derived from temperature data from the profiler (which I will call ε_{TP} from T_P) or from the CTD (ε_{TC} from T_C).

2. The experimental design and analysis used is not appropriate for comparison between ε and ε_{TC}. As stated on p.2145, l.10, "The sampling strategy was to make consecutive FLY dropsonde profiles for a period of about 30 minutes followed by two back-to-back CTD profiles . . .". Although drop speeds are not given for either the FLY or the CTD, we are told that the data set consisted of 172 microstructure profiles and 58 CTD profiles. It is not acceptable to compare data sets of unequal size for a parameter like ε that has a profoundly non-normal distribution. The best that could be done would be to compare CTD profile results with those from the immediately previous (or subsequent) FLY profile. A clear example of the type of issue arising with the comparison that has been done, i.e. of 172 values with 58 values, is seen in Fig.3, where TKE dissipation rates are binned into stratification/shear bins. Because ". . .(only bins containing 3 or more dissipation estimates have been plotted) . . ." (p.2148,l.19), the smaller total number of values of ε_{TC} means that many fewer bins pass this criterion, so that Fig.3(c) looks completely different from Fig.3(a), despite the fact that the same data in Fig.2(d) bears at least some resemblance to that in Fig.2(c).

3. What <u>could</u> be compared are the two same-size data sets (ε and ε_{TP}) derived from the microscale profiler. However the description of the processing of T-data from the profiler (T_P) is inadequate to allow assessment of the resulting Thorpe scales, for a number of reasons:

(a) p.2145, l.28: the value used for the cut-off frequency Ω_c of the (low-pass?) filter applied to T_P , the combination of (low+high)-frequency FLY temperature channels, is not given. Presumably this value is what determines use of a 0.1m vertical average for the effective vertical sampling interval? But why is this interval necessarily the same for FLY and CTD temperatures?

(b) Is the non-dimensional parameter Q given as eqn.(1) in the paper (p.2146, l.19) calculated for FLY or CTD measurements? Again, there seems no reason for these to be the same.

(c) If values of profiler-derived ε_{TP} are to be compared with CTD-derived ε_{TC} (but see point 1. above), it would be useful to see a comparison of actual profiles from both, i.e. to see how closely T_P , the combination of FLY (low+high)-frequency channels, compares with T_C from an immediately previous (or subsequent) CTD. Such a comparison might shed light on the result (p.2148, l.1) " That the CTD-derived TKE dissipation estimates show the quarter-diurnal periodicity more clearly than the fast-thermistor derived estimates is somewhat surprising as the CTD data has only one-third the temporal sampling resolution of the FLY data sets." "somewhat surprising" needs explanation, as does the visual impression in Fig.2 that the temporal resolution of (d)= ε_{TC} is finer than that of (e)= ε_{TP} (unless the caption is wrong?).

4. The main conclusion of this paper (p.2149,l.21) is that " . . . where tidal stresses are the dominant input of mechanical energy into the turbulence the TKE dissipation rate increases with decreasing stratification over the entire range of shear values measured." However this is a "chicken-and-egg" result - one might either argue that turbulence (associated with finite dissipation rates) is stronger when stratification is low or, alternately, that it is causing the lower stratification, as in the LES simulations of Smyth et al. mentioned previously – and as such doesn't contribute to understanding of turbulence in the coastal ocean. It also brings up a related data analysis issue, of whether the stratification associated with a turbulent patch should be taken as " . . . the mean value of buoyancy frequency over the re-ordered region of the overturn . . ." (p.2146, l.11) as done here, or calculated from the density difference between locations just outside this region - and what difference this latter choice would make to the results shown in Fig.3(a).

5. Finally, although the Abstract and Introduction focus on the need to understand mixing across the thermocline in stably stratified shelf-seas, the Conclusions state (p.2150,l.3) "Thus the transition to turbulence, for whatever process is driving thermocline mixing, is likely not only under resolved but completely ignored." (I'm assuming that "ignored" here means "not known"). There is thus a disconnect between stated objectives and results. Moreover, even if the vertical resolution issue could be resolved in the thermocline, a 29 hour time series is totally inadequate to determine dominant processes in this interior region where mixing is not likely driven (solely) by tides.

References not in manuscript:

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Smyth, W. D., J. N. Moum and D. R. Caldwell. 2001. The efficiency of mixing in turbulent patches: inferences from direct simulations and microstructure observations. J. Phys. Oceanogr., 31,1969-1992.