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Ocean Science Discussions

OSD

9, C943–C949, 2012

Interactive Comment

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

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**Response to comments by Referee #1 Kelvin Richards**

We appreciate the reviewer for the insightful comments on our manuscript "Microstructure observations during the spring 2011 STRATIPHYT-II cruise in the Northeast Atlantic" (Paper OS-2012-49). A detailed answer to the issues raised by the reviewer and the corresponding changes in the text are presented below.

### ***Issues with the section relating the turbulence properties to the atmospheric forcing***

*The authors try to relate $K_T$ and $\varepsilon$ 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).*

We find a similar fit of $K_T$ with the wind for the Summer and Spring cruises (Summer: $log_{10} < K_T >\_{\mathrm{MLD}} = 0.14 < u_{10} > -3.68$; Spring: $log_{10} < K_T >\_{\mathrm{MLD}} = 0.22 < u_{10} > -3.34$ ). The fact that $K_T$ has a clear and similar correlation with the changes of wind stress in both cruises indicates that $K_T$ responds quickly to changes of the instantaneous wind stress forcing.

We find a different fit of $\varepsilon$ with the wind for the Summer and Spring cruises (Summer: $\varepsilon/\varepsilon_{s*} \sim 1.8$; Spring: $\varepsilon/\varepsilon_{s*} \sim 0.2$), the main reason being that the value of $\varepsilon$ in all the stations is much larger in Spring than in Summer. In addition, $\varepsilon$ does not correlate with the changes of wind stress (see Figure 6 and 7, and Figure AR1).

The larger values of $\varepsilon$ during Spring must be due to earlier convective or strong wind mixing events. When surface temperatures increase in Spring, the upper layer effectively shields the lower layers from direct atmospheric influences, and decaying turbulence can exist over time scales much longer than those associated with the changing surface conditions. These convective events are not taken into account by the scaling of $\varepsilon$. We used the term "memory effect from the previous winter" but we realize that this term is not accurate and will delete it in the revised paper. The issue of the different values of $\varepsilon$ for both cruises will be discussed at length in the revised paper.

In the manuscript of Brainerd and Gregg (1995), which presented measurements of $\varepsilon$ in the PATCHEX and COARE experiments, $\varepsilon$ was more responsive to atmospheric forcing than in our measurements. For example, the $\varepsilon$ presented in Brainerd and Gregg was similar to the distribution to the scale $L_C$. In our work $\varepsilon$ and $L_C$ have a different distribution (See Figure 7 and Figure SM1, the latter in the supplementary material, and see JDW12).

*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.*

We are not able to relate $K_T$ to MLD because for some stations (stations *0*, *11*, *17*) the MLDs could not be not clearly determined from the temperature profiles. In addition, stations *22* to *30* had unknown MLDs, which were deeper than the maximum depth measured by the SCAMP and the CTD. However, the fact that $K_T$ is related to $u_{10}$ is supporting a relation of $K_T$ to the MLD because $u_{10}$ is related to the MLD (see upper panel and Figure 6a, and Lozovatsky et al. 2005). This is mentioned in the revised manuscript.

*The authors should note that the expression (5) relating $\varepsilon$ 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 $\varepsilon$ profiles for z$<$L, rather than the full profile to see if $C_s$ is less variable.*

The behaviour of Figure 10a for MLD/LMO$>>$1 has similar shape to other works that assess the atmospheric forcing of $\varepsilon$ (see the similarity between the Figure 10a in our manuscript and the Figure 10a in Lombardo and Gregg 1989). We will add a sentence in the revision that explains it in more detail. Fitting the $\varepsilon$ profiles for z$<$LMO (we assume that the Reviewer means LMO instead of L) does give bad results because at most of the stations LMO is lower than the used cutoff of 5 m.

*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. (2005) use 15m. You could varying the cutoff depth.*

This issue was addressed above. The magnitude of $C_s$ does not change significantly if we modify the cutoff depth to 15 m. The profiles of $\varepsilon$ are fairly uniform below 5 m.

### *Additional Point*

*Section 4.1, Fig 5. The authors present the Turner angle to show regions susceptible to double diffusion, but do nothing with 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.*



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In the manuscript we do not assess the mixing due to double diffusion because the measured data does not allow to draw definite conclusions on the importance of this process. We have a limited number of profiles at each station and we do not perform shear measurements. Besides, the literature on turbulence does not present a conclusive and clear relationship (Large et al. 1994, St Laurent and Schmitt 1998), adding difficulty to the assessment of this process. We have decided to delete the section on double diffusion and to delete Figure 5.

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



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ARCorrelationKTeWind.pdf

**Fig. AR1.** Station- and column-averaged turbulence quantities in the mixed layer versus the wind speed at 10 m height, $u_{10}$, and for the STRATIPHYT-II cruise. Graphs presented for (a): Temperature eddy diffusivity $K_T$ and (b) TKE dissipation rate $\varepsilon$.



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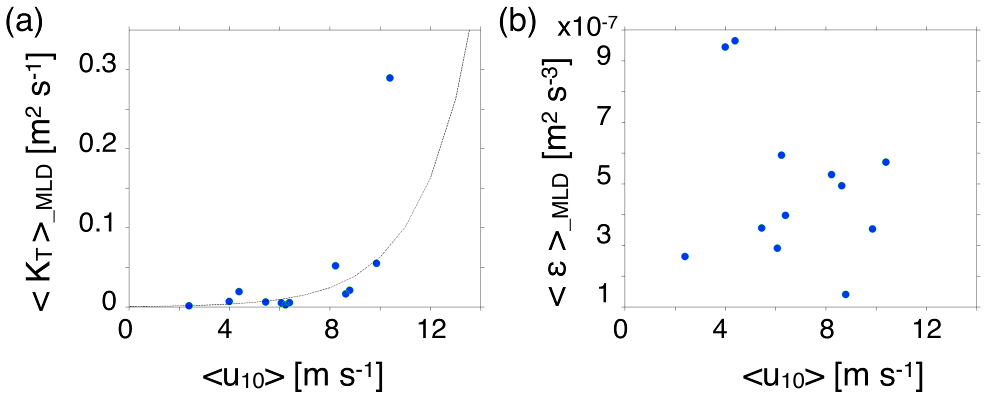


**Fig. AR2.**



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