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Ocean Science
Discussions

# *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 Anonymous Referee #2**

We appreciate the reviewer for the comprehensive 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.



***General comments***

*The authors suggest that the turbulent properties measured by the microstructure profiler will aid in understanding the phytoplankton dynamics. Although the full details of how phytoplankton might respond given these observations is out of the scope of this manuscript, can the authors make any simple estimates / statements at the end of the manuscript on what might be expected? Do plankton favor highly turbulent/mixed waters that likely have more nutrients?*

We do not think it is appropriate to make simple statements at the end of the manuscript on how mixing will affect phytoplankton. On the one hand, the relationship between mixing and phytoplankton distribution is not straightforward. Different studies suggest that mixing can both retard and accelerate phytoplankton sinking (Ross 2006, Behrenfeld 2010), and the extent of the effect of mixing on phytoplankton distribution will depend on additional factors such as the light conditions, nutrient environment, aggregation mechanisms, etc. (Mann and Lazier 2006). We will add a short description of this issue into the revised discussion of the paper.

*The main conclusion of the paper is that increased $K_T$ and $\varepsilon$ are found at the most northerly stations. In Fig. 8, is this conclusion still valid given the size of the uncertainties of $\varepsilon$ (without considering panel a)? I think this figure would be much improved if it were plotted in a realistically scaled x-axis, by latitude, rather than station number.*

The conclusion is still valid given the size of the uncertainties of $K_T$ and $\varepsilon$, and it is still valid without considering panel a. It is better observed in Figure BR1, which is the Figure 8 but with the tenth-logarithm of the turbulence quantities. In Figure BR1, the inter-station differences of the uncertainties of the turbulent quantities are better assessed. In the Figure 8 of the article, we prefer to show the actual magnitude of the turbulent quantities instead of the tenth-logarithm. By showing the actual magnitude of the turbulent quantities, the abrupt increase of the turbulent quantities after 48°N is better observed.

The suggestion of the reviewer to plot Fig. 8 in a realistically scaled x-axis, by latitude rather than station number, is a good suggestion. We have modified Fig. 8 accordingly.

*The authors test possible mechanisms that might be causing these increased turbulent quantities, such as winds, double diffusion, stratification stability. None of these possible mechanisms are mentioned in the abstract (perhaps they should be) and the readers are left wondering which of these are actually important. For example, it is said that double diffusion may be important in 22% of the bins but in the end, given the other observations of wind induced mixing and stratification, is this actually important?*

This is a good point as indeed double diffusion may justify the increase of mixing. It can be observed by comparing the amount of bins with *Tu* between -90° and -45° in Figure 5 and the magnitude of $K_T$ in Figure 6. However, making a detailed analysis is difficult because we would need more profiles in each station, and we would need shear measurements to compare to other sources of mixing. Furthermore, the relationship between double diffusion and mixing is not fully clear from the literature (Large et al. 1994, St Laurent and Schmitt 1998). In the manuscript we do not intend to assess the relative importance of the different processes causing mixing (instabilities due to shear, internal wave breaking, and double diffusion) because it is extremely difficult due to the large amount of stations with different meteorological conditions and lack of time series at each station. We have therefore decided to delete the paragraphs dealing with the double diffusion and to delete Figure 5.



***Detailed comments***

*p. 2, l. 10-11: remove "to it" in abstract: should mention whether or not profiling through MLD? will this be important?*

We will replace "of it" (we assume this is what the reviewer refers to) by "of the transition zone".

Information about the extent of the MLD is important to the reader and we realise that it is not derived from the information in the abstract. We will slightly modify the sentence 5-7 in the abstract to introduce information of the extent of the MLD, e.g., "The derived turbulent quantities show a transition between weakly stratified (Mixed Layer Depths, MLD, $<$100) and well-mixed waters (MLD$>=$100) , which was centered at about 48°N."

*p. 2, l. 13: where is the scaling factor in this equation? unclear without reading manuscript 14-15, no mention as to why increase $K_T$ was observed? which mechanisms?*

The equation does not include a scaling factor because the equation represents the wind stress similarity variable. To clarify the meaning, we have rephrased the sentence: "The station-averaged $\varepsilon$ values throughout the mixed layer scale with the wind stress similarity variable with a scaling factor of about $1.8$ in the wind-dominated stations ($\varepsilon \approx 1.8\ u_*^3/(-\kappa z)$)." The reason for the larger values of $\varepsilon$ in Spring will be described in detail in the revised manuscript.

*p.2, l. 19: likely references of models could be included here*

We will add two additional references: Sarmiento et al. 1998 and Levitus et al. 2000.

*p.2, l. 21: "Changes in stratification" are these vertical changes? lateral? which are more important to phytoplankton dynamics?*

The sentence in p.2 l. 21 refers to vertical stratification changes. Vertical stratification changes are more important to phytoplankton distribution than the lateral ones because the former determine to a major degree the proximity of phytoplankton to light and nutrients. We will modify the sentence in p. 2 l. 21 to clarify it: “Changes in vertical stratification patterns determine the proximity of phytoplankton to light and nutrients and therefore influence the capacity for primary production (Behrenfeld et al. 2006)”.

*p. 3, l.12: references for lower mixing away from boundaries*

We will add the reference of Ferrari and Polzin (2005) for the magnitude of mixing in the deep ocean away from boundaries. The sentence will become: “Measured values of $K_T$ range from $10^{-2}$ m$^2$ s$^{-1}$ in the mixed layer to $10^{-5}$ m$^2$ s$^{-1}$ in the deep ocean away from boundaries (Ferrari and Polzin 2005).”

*p. 3, l. 13: remove "s" in vertical structure*



Suggestion followed.

*p. 3, l. 28-29: remove “the” between “between” and “atmospheric”; remove “the” between “and” & “turbulence”*

Suggestion followed.

*p. 4, l. 1: add dates of new cruise*

We will add the dates of the cruise STRATIPHYT-II p. 4 l. 1 and we will omit them in p. 4 l. 15

*p. 4, l. 10: $K_T$ of $10^{-1}$ is not low*

The range of the measured station-averaged $K_T$ ($10^{-6}$ to $10^{-1}$ m$^2$ s$^{-1}$) is low compared to other reported ranges of $K_T$ (references in the paper). The sentence might be more comprehensible if we showed the cruise-average, but the cruise-average is not a reliable indicator because of the large latitudinal coverage of the cruise.

*p. 4, l. 14: what is meant by similarity variable?*

The similarity variable is explained later in the section 4.3 and it is also explained in the reference Jurado et al. 2012 (JDW12). We will add the reference JDW12 in page 4 l. 14 to be more precise.

*p. 4, l. 29: comma after results, comma after conclusions*

Suggestion followed.

*p. 6, l. 4: define what is meant by segments*

With segments we indicate the whole beginning and ending of the profiles that have a very variable profiler descent rate (larger than a certain threshold). We will modify the sentence to: "If required, the beginning and ending of the temperature-conductivity-pressure profiles were rejected to avoid spectra contaminated by variations of the profiler descent rate."

*p. 6, l. 5: remove ing of ending*

Suggestion followed.

*p. 6, l. 13: remove also*

Suggestion followed.



*p. 6, l. 14: add profiles after temperature; remove ", and the" after temperature, add "."*

Suggestion followed. The sentence becomes: " The salinity was derived from the trimmed-smoothed-sharpened-filtered and depth-binned conductivity and temperature profiles. The density was computed using the (UNESCO, 1981) equation of state for sea water, using salinity and temperature data. "

*p. 6, l. 19: remove "The values of of"*

Suggestion followed.

*p. 7, l. 1: remove "ing" of falling*

Suggestion followed.

*p. 7, l. 3: references after studies*

We will add two references: Soloviev et al. (1988) and Jonas et al. (2003).

*p. 7, l. 8-9: unclear what is meant in this sentence*

We will modify the sentence to: "The maximum value of the TKE dissipation rate $\varepsilon$

was found equal to 2 $10^{-5}$ $m^2$ $s^{-3}$ (Sect. 4.2). The turbulence velocity, $u$, resulted a maximum value of 0.06 m $s^{-1}$, which was lower than the probe free-fall velocity."

*p. 7, l. 15: "averages of the time" was is meant ?*

We mean averages of the turbulent quantities over the time of each SCAMP cast. We will not follow the suggestion of the reviewer because it has a different meaning.

*p. 7, l. 18: unclear*

The cloud cover percentage and wave height were calculated approximately from the observations made by the captain at the deck bridge. Modifying the sentence in p. 7 l. 18 would mean to give additional details that are not relevant. We think that the sentence is clear in the manuscript.

*p. 8, l. 27: in the cases where the MLD was hard to distinguish, did the authors also try a density criteria? See Holte and Talley, 2009*

The density criteria gave unreliable MLDs for those cases. We both used the de Boyer Montégut et al. (2004) criterion (density difference of 0.03 kg $m^{-3}$ with respect to the density value at 10 m depth) and the Levitus et al. (2000) criterion (density difference of 0.125 kg $m^{-3}$ with respect to the density value at the surface).



*p. 11, l. 25: remove "in the cruise" and change "around" to "at"*

Suggestion followed.

*p. 12, l. 2, add ", as" between "Atlantic" and "reported"*

Suggestion followed.

*p. 12, l. 3-5: sentence is unclear*

What we want to convey is that the MLD is not well determined because it gives a low quality index in Lorbacher et al. 2006. The quality index compares the variance of the temperature profile above MLD to the variance to a depth of 1.5 x MLD. We have decided to modify slightly the sentence to: "At most of the stations in the first half of the cruise, the measured thermocline was not in the same position during the profiling time, the temperature profiles presented small steps, and the computed MLD was not always well defined from the temperature profiles (low quality index in Lorbacher et al. 2006)."

*p. 12, l. 9: figure 4 grey boxes are very hard to see from the figure unless zoomed in by a large amount. is it more clear if they are made white?*

If the grey bins are made white, the bins with static instabilities are mixed with the bins that have been trimmed or that have not been measured. We are conscious of the difficulty to see the grey boxes, but we have not found a better solution.

*p. 13, l. 3-10: so in the end, is this an important process?*

We have decided to delete this paragraph and the previous one (see the reasons above, in the section of General Comments).

*p. 13, l. 13: reference after "upper ocean"*

In the Introduction we provide references for the range of values of $K_T$ and $\varepsilon$. On p. 13, l. 13, we do not think that it is needed to again mention those references.

*p. 15, l. 1-3: these figures show the relationship between the wind and $K_T$ nicely at the stratified stations. Does this agree with the statement on p. 9, line 1?*

The relationship between $K_T$ and $u_{10}$ in our measurements and in the measurements in JDW12 does not seem directly affected by depth of the LMO, and thus it is independent of the statement on p. 9, l. 1. The relationship between $\varepsilon$ and $u_{10}$ is, instead, affected by the depth of the LMO. At the stations with a larger LMO, $\varepsilon$ follow better the law of the wall and has a lower proportionality constant.



*p. 15, l. 13: expand on what is meant by "memory effect of the previous winter"*

We will clarify the reason for the differences in $\varepsilon$ in Spring and Summer in much detail in the revised manuscript. We will also extend the captions in Figure 9 and 10 to make clear that the stations north of 48°N in STRATIPHYT-II had larger MLDs than the maximum depth measured by the SCAMP and that, at those stations, we performed the column-averages down to 100 m. Additionally, we will add "down to 100 m depth" after "column-averages" in p. 17 l. 26.

*p. 18, l. 13: again, unclear what is meant by a memory effect of the previous winter?*

We recognise that the term "memory effect" is unclear and will delete it in the revised version.

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

ARlog10LatitudeKTeSt2.pdf

**Fig. BR1.** Station- and column-averaged turbulence quantities, together with the station-averaged wind speeds. Graphs presented for (a) wind speed at 10 m height $u_{10}$, (b) temperature eddy diffusivity $K_T$, and (c) TKE dissipation rate $\varepsilon$. The standard deviation of the station-averaged wind speeds and turbulence quantities is also depicted. The angle brackets indicate a station-average, the notation $_{\mathrm{COL}}$ indicates a column-average down to 100 m



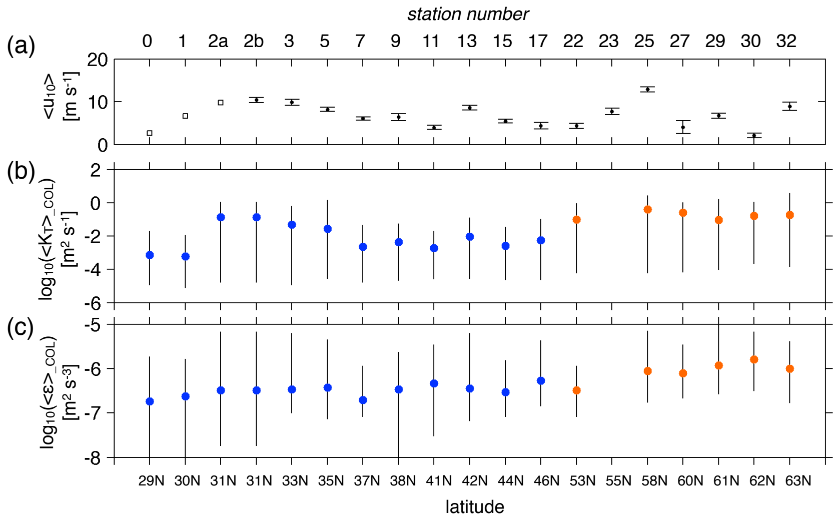


**Fig. BR2.**