# ANSWER TO REVIEWERS

## J. Garcia Lafuente's review

1) Why did the authors select the layer 100-400m as representative of LIW?

The choice of depths and thicknesses is indeed somehow arbitrary. It was driven in this paper by the vertical structure of the stratification. The temperature and salinity (T&S) reach a maximum around 400m due to the LIW, and the stratification decreases significantly below. Therefore the layer 100m-400m is appropriate to be able to separate the relative contribution of the vertical T&S profiles on the stratification.
We carried out some sensitivity tests by changing the depth from 400m to 500m and to 600m. The results were not qualitatively different. Figure 9 of the paper showing the buoyancy fluxes necessary to reach certain depths, including 400m and 500m, confirms this.

2) Connection with the salinification of the inflow at Gibraltar from 2003 and 2006

It is indeed possible that the trends observed at the Dyfamed site and at the strait of Gibraltar have a common cause; hence this trend is now mentioned in the last paragraph of the introduction in the revised manuscript.

3) Specific comments

The authors agreed with all of these comments and each point is addressed in the revised manuscript, apart from the following question:

Why is the anti-correlation between the density and the stratification not so good before 2000?

The anti-correlation for the intermediate layer seems indeed less strong before 2000. We think this is because the density in the top layer varies less smoothly before 2000. The average stratification in the intermediate layer is proportional to the difference in density between 100m and 400m but most of the change occurs in the upper part of the layer, and so it is expected to be correlated with the upper-layer density and anti-correlated with the middle layer density.

## J. Font's comments

1) Cascading as shown in time series of Font et al., 2007 (P53, L27-29)

This reference should and is now included in the introduction.

2)&4) Definition of the significance of a trend (P57, L24-25)

The visual check is indeed not a clear metric and is moreover unnecessary in our cases. Any reference to it is removed in the revised manuscript. We do indeed mean that the trend times 10 should be much larger than the std calculated over 10 years, and this is clarified by using mathematical notations in the revised manuscript.

3) 2004 displays a more pronounced drop than 2005 (P58, L3)
7) Should specify "less than 200 W/m2" refers to September (P63, L15)
8) In the table I don't see slightly larger, but smaller. (P64, L19-20)
9) The discussion of information from table 8 could be maybe expanded for clarification. (P67, L7-21)
11) Better 400-2000m than <400m to indicate the deep layer. (Fig 4)

These corrections are made in the revised manuscript.

5) Why should advection be responsible for S drop in Dec-Jan? (P58, L19)

Surface processes are unlikely to be responsible for it because the water column is not vertically mixed yet and there is more evaporation than precipitation at that time of year.

6) In winter 2005 the stratification was really quite weak, but you can not say that more than in the previous winter (missing data). (P60, L26)

We do not understand this point. We do have data in the intermediate layer for winters 2002-03 and 2003-04. Could you please explain?

10) The disruption of trends mentioned here has to be considered with caution, as the authors were presenting data from a very particular year (with a huge volume of dense water formed) that should be examined considering data from the following years (P68, L11-12)

This is a fair point. This could well be a one-off event. The text is reworded in the relevant sub-sections of section 3, and removed from the conclusion in the revised manuscript.

## V. Artale's reviews

Vincent Artale made a lot of useful suggestions on further possible studies. The idea behind the work presented in this paper was to isolate one of the processes of convection (preconditioning) rather than considering them all at once. The convection phenomenon is indeed very complex and interesting and we think that addressing the other processes described by the reviewer would be of great interest, but would constitute separate studies and papers. As commented by J. Font, the present manuscript is already quite dense.

However, it is true that little background is given in the introduction on the other processes occurring during convection, particularly the advection-convection feedbacks that occur during the violent mixing phase. In the revised manuscript the introduction is expanded to describe the other processes occurring during convection in more detail.

The 1D assumption made throughout the paper is a major limitation of the study, as mentioned P65 L12-17, because of baroclinic instability occurring during and after the deepening. This latter process was also studied as part of the main author's PhD thesis (Chapters 4 and 5), available online at:
eprints.soton.ac.uk/72147
and will be the subject of a paper still in preparation at the moment.

Minor corrections

a) Comparison with Marty et al., 2002

This is a fair point. However the mixed layer depth Marty et al. 2002 calculated rarely reach below 100m, while we find the stratification maximum in the 100m-400m layer.
In the revised version, we cite the more recent work of Marty and Chiaverini 2010 and compare their trends found at 2000m at Dyfamed with ours.

b) Indicate how you compute explicitly the probability that convection reaches some depth.

We calculate the mean and standard deviation of $X_{BF}$-$X_h$ and assume that $X_{BF}$-$X_h$ follows a normal probability distribution. A property of the normal distribution is that 68.2% of the values are within 1 std of the mean, for example. We have done the reverse calculation and calculated the probability that $X_{BF}$-$X_h$<0.
This is described in the text P65, L7-9.

c) Equation 2 cut one parenthesis

This is addressed in the revised manuscript (Equation 4).