**Ocean Science
Discussions**

Interactive comment on "Modelling temperature and salinity in Liverpool Bay and the Irish Sea: sensitivity to model type and surface forcing" *by* C. K. O'Neill et al.

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Author response to Referee 2 for manuscript os-2012-12

Thank you to Referee 2 for his/her comments.

General comments: The treatment of the observational data concerning the removal of the influence of tides is not described adequately. For the CEFAS buoy the authors explain that a Doodson filter was used. For the CTD and Ferrybox data the treatment remains unclear, so it cannot be judged for which reason the differences between the CTD observations and the Ferrybox measurements displayed for example in figure 11 for the salinity gradients are occurring and whether the tidal influence is excluded or

not. That makes it difficult to evaluate the paper.

Generally, very little attention is paid by the authors on the fact that NEMO is underestimating the strong variability that is typical for the ROFI regions. In contrast POLCOM-Sis overestimating. However, in the conclusions the authors come to the statement that the models perform well in predicting surface temperature when measured by objective metrics. It remains so questionable whether the right measures were used.

The models do perform well over most of the Irish Sea and even in the more difficult Liverpool Bay area the seasonal cycle is well represented. The coarsest of these models, at 12km resolution, is of course incapable of accurately modelling the dynamics of Liverpool Bay, however that is not to say that the data there should not be used. Though much of the data is from a ROFI, the intention is not to assess the skill in reproducing ROFI effects in these models. We are limited by the data area coverage.

The overall impression of the paper is that the authors didn't investigate enough or at least didn't explained enough the possible explanations that can cause differences between the predictions itself and between model and observations on the other hand. The manuscript is mostly written in form of a technical report and lacks a deeper discussion of the factors impacting the results

This manuscript was submitted as part of a collection on the MyOcean project. We have presented the material in the way that we thought appropriate for the wide target audience.

The figure captions should be written that the reader understands what is displayed in the figure without reading the whole text.

The captions have been expanded

Specific comments:

On page 655, line 13: It took time to realize, that here the authors describing the POLCOMS 12 km resolution AND the NEMO model. An explanatory sentence would

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be helpful

This has been clarified.

On page 657, line 10: A ten minute sub sample resulting in 7 km resolution (20 knots ferry velocity) may be too coarse within the areas of large salinity or temperature gradients.

The model outputs are only hourly, so it would not be meaningful to interpolate the data too much more than the ten minutes used here. Also, two of the three models have resolution ≤ 7 km so it wouldn't be appropriate to try and interpolate to observations with a much finer horizontal resolution.

On page 657, line 11. Exemplary for several occurrences in the text and Figures. The unit PSU is not longer used for Salinity and should be removed

PSU has been removed

On page 659 and 661: Numbering of the paragraphs dealing with the results for t and s should be consistent with the previous chapters. (2.4.1 for T and 2.4.2 for S)

Yes this was an error and has been corrected.

On page 662, lines 20-23: The authors make a general statement of the validity of the temperature and salinity prediction of the models applied in the study. It is desirable to include a wider view of the state of the art within the modeling community whether that is a common challenge or dedicated to the area of investigation.

It is certainly true of Liverpool Bay processes modelling that the principal driving influence is the poorly determined freshwater forcing. We have added a citation (Polton et al 2011) that can be followed up with included references.

On page 664, line 18: How can a 'tidal variability' cost function be defined when the tidal influence is filtered out?

I think that we presented this in a confusing way. By virtue of their definitions Chi squared is biased towards skill, or lack thereof, of the short timescale (i.e. tidal) signal, whereas r^2 is biased towards measuring the skill in the annual cycle. Here we have a low Chi value so the skill in measuring the short timescale, ie tidal, SST is high. The tides are not filter out in this diagnostic. Chi squared is biased towards skill, or lack thereof, in the tidal signal. whereas r^2 is biased towards skill in the annual cycle. This is clear from their equations.

Table 3: The use of an overall cost function (in my understanding integrated over the whole water column, in contrast to the surface temperature cost function from page 663, lines 3-4) for the Ferrybox comparison remains in my view meaningless, since the observations are limited to the surface.

In this context the 'overall' cost function means a spatial average over the horizontal area, not vertically integrated. This has been clarified.

Figure 6 The figure captions should be written that the reader understands what is displayed in the figure without reading the whole text. That is difficult here.

They have been updated.

Figure 8: The same range should be used for x- and y-axes

This has been changed

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